Educational Interventions and Labor Market Success

Inauguraldissertation zur Erlangung des
akademischen Grades eines Doktors der
Wirtschaftswissenschaft (doctor rerum politicarum)
des Fachbereichs Wirtschaftswissenschaft
der Freien Universität Berlin

vorgelegt von

Dipl.-Volksw. Nils Peter Saniter

Berlin, April 2014

Erstgutachter:
Prof. Dr. Klaus F. Zimmermann, Universität Bonn und FU Berlin

Zweitgutachterin:
Prof. Dr. Katja Görlitz, FU Berlin

Tag der Disputation:
14. Juli 2014

Zusammenarbeit mit Koautoren und Vorveröffentlichungen

Kapitel 2

- Kein Koautor
- Eigenleistung: 100 %
- Vorveröffentlichungen:
  - IZA Discussion Paper No. 6813
  - DIW Discussion Paper No. 1213
  - SOEPpaper No. 458

Kapitel 3

- Koautor: Thomas Siedler
- Eigenleistung: 50 %
- Keine Vorveröffentlichung

Kapitel 4

- Koautor: Thomas Siedler
- Eigenleistung: 50 %
- Keine Vorveröffentlichung

Educational Interventions and Labor Market Success

Dissertation

Nils Peter Saniter

Acknowledgments

First of all, I would like to thank my supervisor Klaus F. Zimmermann for his guidance, continuous support and valuable comments on my papers. I am also indebted to Katja Görlitz for immediately agreeing to be my second supervisor and lending her expertise from the field of empirical labor economics.

A special thank goes to my co-author and mentor Thomas Siedler for our productive and delightful cooperation. My work immensely gained from his enthusiasm and encouragement. Furthermore, I am grateful to Helmut Lütkepohl for being a wonderful employer.

I wrote this thesis while being a doctoral student in the Graduate Center of DIW Berlin. This program's academic training and stimulating research environment helped tremendously in completing this thesis. Thanks to my colleagues and friends at DIW Berlin for our fruitful discussions, mutual support and making the institute a fun place.

I also benefited from being an IZA Research Affiliate. IZA's policy events and research network were great sources of inspiration. Many thanks also to numerous researchers at conferences, seminars, and workshops—particularly to the participants of the BeNA seminar series—for feedback that truly helped to improve my papers.

Moreover I would like to thank Simon Diehl and Felix Kersting for research assistance, Adam Lederer and Deborah Bowen for proofreading, SOEP, IAB, and DZHW for data provision, and the German National Academic Foundation for awarding me a doctoral scholarship.

Last but not least, I would like to thank my parents for their trust and unconditional support.

Berlin, April 2014
Nils Peter Saniter

Contents

1 Introduction

1.1 Motivation

It is an undisputed fact that education is one of the best instruments for individuals to hedge against the risk of poor wage income. Work by the Research Institute of the Federal Employment Agency (IAB) based on German registry entries shows that investments in education have profitable returns measured in life-time earnings. For example, on average university graduates earn 2.7 times more than persons without a tertiary degree. Across all ages in a person's lifespan and given various educational degrees, there is a simple but concise relationship: The more education one attains, the higher his or her average earnings are in later life (Schmillen and Stüber, 2014). Almost without controversy, this finding is backed up by corresponding observations not just for Germany (Weishaupt, 2012) but also for other industrialized countries and developing countries around the globe (OECD, 2013; Peracchi, 2006; Psacharopoulos and Patrinos, 2004).

Education also guards against the risk of becoming unemployed. Weber and Weber (2013) compare unemployment rates in Germany for holders of different degrees in 1975 through 2011 based on census data. The authors find the same ordering of favorable outcomes across degree types as above: University graduates have the lowest risk and persons without a tertiary degree have the highest risk of becoming unemployed. In 2011 the unemployment rates in these two groups diverged considerably, reaching 2.4 percent at one end and 19.6 percent at the other. Groups with other educational attainments take middle positions between these two extremes, sorted by the rank of the degree. This points at a solid negative relationship between educational attainment and unemployment risk. Again, this finding is reflected by many studies from around the world (OECD, 2013).

Moving beyond earnings and employment, economic research also documents correlations between education and various nonmonetary outcomes for individuals. Examples for such nonmonetary outcomes include health (Cutler and Lleras-Muney, 2006), fertility (Black et al., 2008), intergenerational transmissions (Behrman and Rosenzweig, 2002), criminal activity (Machin et al., 2011), and civic engagement (Dee, 2004), among others.

It is this outstanding importance of a person's level of education as a predictor for such different measures of wellbeing that is fueling the extensive investigation into education's causes and consequences. Some scholars argue that the significance of education on individual performance in the labor market has steadily grown and has never been as pronounced as today (Card and Lemieux, 2001).

Reaping the benefits of education comes at the cost of investing into education. Such investments can comprise direct costs like tuition fees or a decrease in utility based on an intrinsic dislike of learning. They can also comprise indirect costs that result from foregone opportunities whilst studying, for example, foregone earnings due to the absence from the labor market while being enrolled at school. Individuals must decide on how much to invest in education, given the expected costs and returns of the investment and being bound by a set of restricting parameters. This choice setting gives way for economic reasoning, which most prominently has been realized by Becker (1993). Becker put the ideas related to educational choices into a coherent framework, the human capital theory. The human capital theory departs from the premise that investments in education augment a person's stock of human capital, which yields a return, similar to any other kind of capital. For example, education is likely to increase a person's productivity, which typically leads to higher wage compensations in the labor market. Individuals have an interest in investing in education as long as the marginal discounted return is higher than the marginal costs they have to incur. Optimal investment (for instance, the optimal number of years of schooling) is where marginal returns equal marginal costs. Different optimal investments across individuals reflect differences in the inherent stock of human capital at birth and differences in the aptitude (i.e. the costs) of acquiring education, the latter being sometimes conceptualized as a person's ability. In calibration exercises the ability parameter in the human capital model has proven to have a crucial role in mirroring education-earnings profiles observed in real data (e.g. Ben-Porath, 1967 and Heckman, 1976).

Another prominent theory that explains the returns to education is the signaling theory (Spence, 1973). Different from the human capital theory, the signaling theory puts forward the idea that productivity may not be the result of educational efforts but of innate quality based on a person's ability and other fixed background characteristics. These intrinsic characteristics determine individual cost-benefit calculations of pursuing educational attainments. The educational system merely works as a filter that selects individuals into different degrees. If productive efficiency is unobservable to a prospective employer, then the degree can serve to credibly signal the worker's productivity. Individuals have an interest in investing in education because of the expected return from signaling their ability to employers.

Both theories have in common that they model individual behavior as the result of optimizing investments in education. In order to solve the optimization problem, individuals need to form rational expectations about the investments' returns. In real life, however, this can be a difficult task as expectations about returns usually come with a significant share of uncertainty. There is also reason to suspect that some of education's positive associations with favorable individual outcomes can indeed be spurious or at least significantly biased. This idea will be laid out below. The uncertainty involved with deciding for how much of one's time and resources to invest in education contrasts with what is suggested by the introductory statements, namely that education can have significant and long-lasting impacts on various spheres in a person's life. Therefore, when deciding about education, much is at stake.

It is this juxtaposition of uncertainty and potential impacts that motivates the myriad of empirical investigations of the returns to education. It also motivates this dissertation, which is a contribution to the literature on the returns to education. For individuals the value of this research is clear: it helps them to make better decisions. But benefits of this research also occur for policy makers. It contributes to evaluating the effects of institutional settings, policy reforms or programs in the field of education, training and labor markets. For example, in the course of the debate about shortening upper track secondary education in Germany from nine to eight years (G8 reform, see e.g. Büttner and Thomsen, 2010) knowing about the returns of one extra year of schooling is an important piece of information in weighing the pros and cons of this reform before implementing it. Another example is publicly funded programs that tackle the problem of early school dropouts or provide special support to low performers in school (e.g. Oreopoulos, 2007). Similarly, any kind of publicly facilitated education or training program for workers in the labor market is typically based on some belief about its returns. Therefore, investigating the returns of education is key for designing policy interventions.

Most of early work in the literature on the returns to education followed the lead of Mincer (1974) and aimed at estimating the increase on the wage bill induced by additional years spent in schooling. Mincer proposed an estimable form of an earnings function that he derived from the human capital model. In this equation the coefficient for schooling measures the internal rate of return to educational investments and can be calculated by means of statistical regression. However, methodological difficulties arise when this coefficient is meant to have a causal interpretation. Indeed, the theories of human capital and signaling show that individual capacities positively affect both educational attainments and wages. Estimated positive correlations between schooling and wages may then not represent a causal link but simply reflect the fact that high-ability persons stay longer in school and receive higher wages. If one cannot control for ability, then OLS estimates are inherently biased. Given that the omitted variables are positively correlated with the model's error term, the true (causal) coefficient is overestimated. In the language of signaling theory, we can also talk of selection bias as individuals select themselves into educational attainments based on characteristics unobservable to the researcher.

While most of the discussion revolving around ability bias takes Mincer's earnings equation as a departing point (take Griliches, 1977 as a famous example), concerns about omitted variable bias and selection bias apply to any reduced form specification for any outcome in the field of returns to education. This relates to the "fundamental problem of causal inference" (Holland, 1986), which alludes to the general fact that in non-experimental settings it is impossible to observe counterfactual outcomes. In the returns to education framework this means that empirical researchers cannot observe (and compare) outcomes for same individual for different values of education. For obvious reasons, conducting laboratory experiments with random assignment of educational degrees to real persons is usually not feasible. Similarly, participation in some educational program or labor market training typically results from individual choice and not from random assignment. If the underlying determinants of participation are not included in the estimation model, then parameter estimates are prone to selection bias.

A bridge between laboratory experiments and observational data can be established by what is called a "natural experiment". A natural experiment occurs when a subset of the population is subject to a shift in a variable of interest that is exogenous to individual unobservable characteristics. Examples include natural events, administrative rules, or changes in legislation. Borrowing from the terminology of the program evaluation literature (going back to Roy, 1951, and Rubin, 1974), some individuals are treated whereas others remain untreated. The untreated ones belong to the control group. Alternatively, individuals may differ by the intensity of the treatment. If remaining self-selection into and out of the treatment and into treatment intensity is not systematically correlated with components of the error term, then identification of the estimation parameters can be achieved by various estimation techniques. Possible "identification strategies" (Angrist and Krueger, 1999) include, among others, instrument variables, difference-in-difference estimations, regression discontinuity design, within-difference estimators, as well as less common methods, for example, identification through heteroskedasticity. There has been a surge in studies analyzing returns to education that rely on these and related methods (see Card, 1999 for an early survey; for Germany see Flossmann and Pohlmeier, 2006). And causal inference from observational data is still in vogue as evidenced by the large and continuous flow of publications of this type. However, there remains room for methodological advances and many research questions have not been answered yet. This is where this dissertation fits in.

1.2 Contribution of this Thesis

This dissertation makes three contributions to the empirical returns-to-education literature, each investigating individual decisions and their consequences for labor market success. The decisions of interest are about secondary and tertiary education, occupational choice, and labor market trainings through internships. The subjects under investigation are young adults at different stages in their life course: in school, shortly before moving from school to work, and in university. Each contribution fills a gap in the existing literature by introducing novelties with respect to research questions, methods, or a combination of the two. Aside from academic gains, this dissertation also delivers relevant insights for policy making. It contributes to designing effective institutional settings in school education, occupational counseling, and labor market training at universities.

Table 1.1 provides an overview of the three contributions that are presented in Chapters 2, 3 and 4 of this dissertation. The table contains information about the research questions, methods, data sources, and main results. It also informs about co-authorships and my contribution to the work.

The succeeding paragraphs briefly introduce each chapter, followed by a discussion about the connecting links between them. While each paper is specific in terms of research questions, data and methods, I argue that each one contributes to an overarching, common research question.

Chapter 2 studies the earning returns to years of education for different educational groups in Germany. Entitled "*Estimating Heterogeneous Returns to Education in Germany via Conditional Heteroskedasticity*" this paper arrives at estimates with causal interpretation by means of a reasonably new identification method that goes back to Klein and Vella (2010). In a control function framework, this method purges endogeneity from the model by introducing a control term. The control term carries exogenous variation in the form of nonlinearity, which allows identification of the parameters of interest. The nonlinearity, in turn, hinges on the presence of heteroskedasticity in at least one of the two equations in the system. If the variances of the error terms fulfill what will be introduced as the variable impact property, then consistent estimation of the returns to education is feasible through an iterative multi-step procedure.

A major advantage of this approach is that identification does not rely on additional regressors that generate exclusion restrictions as for the case of IV methods. Instead, it uses identifying variation from inside the model caused by the presence of heteroskedastic error terms. Moreover, given that the heteroskedasticity is present across the entire distribution of the covariates, parameters represent average treatment effects (ATE) and not local average treatment effects (LATE) as for IV.

Table 1.1: Overview of Chapters

	Chapter 2	Chapter 3	Chapter 4
Title	Estimating Heterogeneous Returns to Education in Germany via Conditional Heteroskedasticity	The Effects of Occupational Knowledge: Job Information Centers, Educational Choices, and Labor Market Outcomes	Door Opener or Waste of Time? The Effects of Internships on Early Labor Market Outcomes
Main research question	What is the marginal return to education?	How does knowledge about the returns to education and occupational knowledge affect young adults' educational and occupational choices?	What is the return of job training through internships during university with regard to the transition into the labor market and early career outcomes?
Data	GSOEP	ALWA, SIAB, own data, Federal Statistical Office data	DZHW Graduate Surveys
Identification method	Control function approach; identification based on presence of heteroskedasticity	Difference-in-difference method	Instrumental variables method
Main results	The causal return to education is about 1 percent for graduates from the basic school track and about 8 percent for graduates from a higher school track. Across these groups the endogeneity bias in simple OLS regressions varies significantly.	Higher educational attainments and smoother transfer to the labor market. No positive effects on individuals' earnings in their first job or later in life.	Positive and significant earnings returns five years after finishing university of about 6 percent. Effect driven by intermediary variables describing job characteristics and the transition from university to work.
Co-Author	–	Thomas Siedler	Thomas Siedler
Author's Contribution	100%	50%	50%
Published Versions	IZA Discussion Paper No. 6813; DIW Discussion Paper No. 1213; SOEPpaper No. 458	–	–

Using data from the German Socio-Economic Panel Study (GSOEP) the study calculates a return of 8 percent for the entire sample, 1.1 percent for graduates from the basic school track and 8.3 percent for graduates from a higher school track. Across these groups, the bias in simple OLS estimates varies significantly. These results are compared to three different studies for Germany that rely on IV methods and arrive at seemingly contradicting results. I argue that the diverging results can be reconciled by their LATE interpretation. I provide support for this view by showing that my results replicate the findings of the IV studies when I run the regressions on the subsample of individuals that arguably belong to the studies' group of compliers.

The results of Chapter 2 are interesting in that they indicate smaller returns to education for graduates from the basic school track than for graduates from the higher school tracks. From a policy perspective, this implies that increasing the minimal number of years of schooling in Germany would only have minor effects on the graduates' earnings. A similar reasoning applies to extending the duration of apprenticeships by one year, because it is usually low school track graduates that proceed with apprenticeships after school. The diverging returns seem also interesting from a theoretical point of view, because they go against the implication of the human capital theory of decreasing marginal returns. However, as will be argued in the paper, interpretation of this kind ignores self-selection into the two subsamples. The identification approach only allows for causal inference within the examined subgroups, not between them. For this reason, the paper does not make a strong case about conformity of the results with theory.

Chapter 3 is entitled "*The Effect of Occupational Knowledge: Job Information Centers and Labor Market Outcomes*". This paper focuses on the link between young adults' occupational knowledge and both their educational choices and early career outcomes. The treatment under investigation is an increase in the availability of labor market related information. This information can help individuals to form rational expectations about prospects of income and employment given different educational and occupational choices. Distinct from the preceding study, interest lies not on the return to education itself but rather on the return to an increase in knowledge about the returns to education.

The paper is motivated by four lines of reasoning about why job information may positively affect individuals' labor market outcomes: First, knowing about the number and types of occupations and jobs available might influence one's educational and occupational choices in a way that mitigates the risk of skill mismatch. Similarly, occupational knowledge may entail efficiency gains if it induces young adults to choose a job that better fits their skills and interests. Third, job information may help to loosen the restraints of peer pressure and gender roles. And fourth, there is empirical evidence that students' educational decisions are influenced by their expectations about the monetary returns of their choices.

We proxy occupational knowledge with mandatory visits to job information centers (JIC) for pupils in Germany. Between 1976 and 2000, 181 JICs were introduced in different regions at different times as part of a nationwide program agreed upon by the Council of the German Employment Agency. Wherever available, school authorities agreed to make one-day class trips to JICs obligatory for all students. We argue that for a given year and locality the availability if JICs is exogenous to individual characteristics. This lends itself to a difference-in-difference approach and the estimation of an intention-to-treat effect.

In order to detect whether an individual was exposed to the informational program during their youth, we combine self-collected data on the location and introduction of job information centers with survey data from the ALWA study (*Arbeiten und lernen im Wandel*, Working and Learning in a Changing World), which contains history data on schooling and place of residence. For earnings outcomes later in life, registry entries from the SIAB (Sample of Integrated Labor Market Biographies) are matched with ALWA respondents and inform about daily earnings.

The results suggest that treated individuals, who went to a low- or intermediate-track school when a JIC was available in their district of residence, have a significantly higher chance of experiencing upward educational mobility, that is, changing to a higher school track and finishing with a better degree. Moreover, we find negative effects on the likelihood of becoming unemployed and involuntarily losing the job during the five years after entering the labor market. We do not find evidence for an effect on daily earnings.

The policy conclusions of these findings are straightforward: First, providing free information about jobs and career paths to young adults pays off in terms of educational attainments, smoother transition into the labor market and higher job stability. And second, more specifically, the introduction of job information centers in Germany has proven to be an effective initiative to provide such information.

On a more general level, this paper allows to conclude one interesting detail about theory: human capital theory and signaling theory both allow for uncertainty in decision making but take rational expectations about returns as given. This study provides evidence that expectations are not fixed over time. Instead they are updated in response to new information and therefore subject to policy intervention. We conclude that making choice-related knowledge available to young adults plays a significant role in their ultimate decisions.

The title of Chapter 4 is "*Door Opener or Waste of Time? The Effects of Internships on Labor Market Outcomes*". This paper examines the benefits of completing an internship while studying at university as a mode for vocational exploration and gathering of practical experience. The research is motivated by a surge in popularity of internships, not only among students, who seek to improve their labor market opportunities, but also among policy makers in higher education. In the course of the Bologna Reform, employability of graduates became a central objective of higher education and the institutional promotion of internships was considered to be one vehicle to achieve employability.

We utilize several theories to derive hypotheses about the effects of internships for early career outcomes: The theories of human capital, social capital, signaling, and screening make favorable predictions, such as smaller likelihood of being unemployed, better job match and increased earnings. However, internships also incur costs. Individuals have to invest time, effort and sometimes money. And institutions that promote internships have to bear the costs of corresponding programs. Quantifying the benefits of internship experience is therefore important for both individual decision-making and public policy evaluation.

We employ longitudinal data from three graduate surveys collected by the German Centre for Research on Higher Education and Science Studies (DZHW) that contain information on internships and labor market outcomes up to five years after the last exam. In order to account for the endogeneity bias caused by students' self-selection into internship experience, we estimate treatment effects in a two-stages least squares approach (2SLS). Exogenous variation in the first stage regression comes from the introduction and abolishment of mandatory internships at the university-subject level. The results show that internship experience leads to higher earnings of about six percent, both in OLS and IV regressions.

We then proceed by investigating the relevance of different channels through which the practical work experience may affect earnings. The transmission channels under inspection fall into two categories: pathways to the first job and job characteristics. The former category includes the occurrence of different activities in the years between graduation and the measurement of earnings (e.g. engaging in doctoral studies or employment experience). The latter category comprises attributes of the current job position (e.g. the maturity of the work contract, part- and full-time employment and whether one is self-employed or not). The results show that the positive earnings returns are mainly driven by a higher propensity to work full-time and a lower propensity to be unemployed at the beginning of one's labor market career. As to the analysis of heterogeneous effects, there is evidence that internships yield greatest returns for individuals and study areas with a weak labor market orientation.

The preceding descriptions of the three chapters reveal that each contribution is specific in terms of the addressed research question, the employed data, and the identification method. On a more general level, however, they all contribute the common goal of quantifying causal returns to investments in human capital. The treatments under consideration – formal education, labor market knowledge and labor market training – are all subject to individual choice. They share the property of being costly investments in human capital with potential returns at a later time. These returns are typically not perfectly predictable; hence investing involves a certain degree of risk. Quantifying the average returns of these investments delivers important information to individual decision-making in favor or against investing or the choice of optimal investment intensity. Moreover, the findings of the three papers are linked by their immediate applicability to policy making. All evaluated treatments have effectively been utilized by public interventions, be it minimal schooling legislation or early dropout prevention (Chapter 2), improving the provision of occupational knowledge (Chapter 3), or introducing mandatory internships at university (Chapter 4). Finally, from a methodological viewpoint, the papers are united in their use of microeconometric methods that aim at causal inference.

On a more detailed level, the papers are connected by the choice of outcome variables. All three chapters put wage earnings at the center of investigation. In Chapters 2 and 3 the focus is not only on final realizations of wages but also on the transition phases from school to work and university to work, respectively, which potentially operate as intermediating channels. Consequently, these papers add further outcome variables to the analysis that help to understand what happens during this period of early career consolidation, like time spent in unemployment, geographical mobility, or stability of jobs. With respect to the subjects under investigation, that is young adults, the contributions can further be connected on an imaginary time-line along a person's biography. Chapters 2 and 3 deal with educational and occupational decisions at relatively early ages. In contrast, Chapter 4 investigates internships while being enrolled in university and thus targets at individuals at later ages.

2 Estimating Heterogeneous Returns to Education in Germany via Conditional Heteroskedasticity

Chapter Abstract

In this paper I investigate the causal returns to education for different educational groups in Germany by employing a method by Klein and Vella (2010) that bases identification on the presence of conditional heteroskedasticity. Compared to IV methods, key advantages of this approach are unbiased estimates in the absence of instruments and parameter interpretation that is not bounded to local average treatment effects. Using data from the German Socio-Economic Panel Study (GSOEP) I find that the causal return to education is 8 percent for the entire sample, 1.1 percent for graduates from the basic school track and 8.3 percent for graduates from a higher school track. Across these groups the endogeneity bias in simple OLS regressions varies significantly. This confirms recent evidence in the literature on Germany. Various robustness checks support the findings.

2.1 Introduction

Ever since Mincer (1974) laid out the methodological foundation to estimate wage equations, a tremendous amount of work has been dedicated to finding the causal return to education. The causal return to education is the extra amount of wage income a randomly selected individual receives from an additional year of education. Knowing the causal return is important for policy makers. It directly informs about the utility of educational programs in terms of monetary payoffs for its beneficiaries. Estimating the causal returns, however, is not trivial: While simple wage regressions correctly produce correlations between, say, years of schooling and wages, they do not report causal returns to education as the schooling variable is likely to be endogenous due to omitted ability variables. Given the typical belief that the omitted ability variables influence wages and schooling in the

same direction, simple OLS parameters are upward biased (Griliches, 1977).[1]
One well-established route to circumvent the endogeneity problem is to use instrument variable (IV) methods. While theoretically appealing, IV may not always be easily implemented in practice as it relies on the availability of valid and significant instruments. Moreover, when the effects are heterogeneous, the interpretation of IV parameters is bounded to the local average treatment effect (LATE) along the lines of Imbens and Angrist (1994): The estimated coefficients represent causal effects only for the subsample of compliers, i.e. individuals who are actually affected by variations in the instrument. Inference on the average treatment effect (ATE), i.e. the entire population, is generally not valid. Therefore, different instruments typically produce different results and it requires case-specific judgment to determine which subgroup of the population the estimates are representing (Ichino and Winter-Ebmer, 1999, do this exercise for Germany).

The guiding question of this paper is how years of education affect wages. I estimate the causal returns to education with ATE interpretation without using IV methods. Instead, I make use of a novel identification strategy by Klein and Vella (2010) (hereafter, KV), which is realized with a control function approach. Identification relies on the nonlinearity of the control term induced by heteroskedasticity. Arguing that the presence of heteroskedasticity is not limited to a subsample of the population, this allows me to estimate the ATE. Data are taken from the German Socio-Economic Panel Study (GSOEP) and results are produced for individuals of different educational groups – less well educated, better educated, and all individuals.

I compare my results to three IV studies for Germany that use different instruments: Pischke and Wachter (2008) use the extension of compulsory schooling years across states and years as an instrument and find no returns to schooling. Their IV parameters are significantly smaller than their OLS coefficients, hinting at upward biased parameters in simple OLS regressions. Becker and Siebern-Thomas (2007) identify the returns to education using the urbanization of the place of childhood as an instrument to proxy the availability of higher secondary schools. They find downward biased OLS coefficients. Finally, Ichino and Winter-Ebmer (2004) instrument schooling with own and father's World War II involvement and also find downward biased OLS estimates. While these results seem to contradict each other, they can be reconciled taking into account their LATE interpretation: Each IV result resembles the effect for the respective instrument-specific subpopulation of compliers. Arguably, for Pischke and Wachter (2008), these compliers are individuals with basic school education. For Becker and Siebern-Thomas (2007) they are individuals who are better educated. Finally, for Ichino and Winter-Ebmer (2004) they are individuals with any level of schooling.

[1] This disregards potential attenuation bias from measurement error.

The subsamples used in my study—low educated, better educated, and all individuals ñ— resemble these complier groups. In fact, using these different subgroups, the three studies' results can approximately be replicated using a parametrized version of the approach by KV, first implemented by Farré et al. (2010). This allows two suppositions: First, my method correctly identifies the causal education parameter. Second, the puzzle of seemingly conflicting evidence for Germany can be solved by accounting for IV's LATE interpretation. To preview results, I find that the wage premium of one additional year of education in Germany is 8 percent for the whole sample. It is 8.3 percent for students with one of the two highest possible school diplomas, while for those who achieve a lower high school diploma the return is only 1.1 percent.

The remainder of this paper is organized as follows: Section 2.2 briefly describes the educational system in Germany. Section 2.3 reviews the relevant empirical literature. Section 2.4 presents the econometric model and the implementation of the estimator. Section 2.5 describes the data. Section 2.6 presents the empirical results. Discussions and robustness checks are provided in Section 2.7. Section 2.8 concludes.

2.2 Education in Germany

In Germany, children typically start elementary school at the age of 6. After four years they move on to a secondary school, entering one out of three school tracks that differ with respect to their curriculum and academic standards. The lower track (*Hauptschule*) is the least demanding one. Students finish this track after a total of 9 years of schooling. Besides basic academic content, this track contains various elements of vocational training. After finishing, graduates usually engage in an apprenticeship leading to a blue collar occupation. The middle track (*Realschule*) is more demanding than the lower track and ends after 10 years of schooling. Graduates of this track usually engage in an apprenticeship leading to a white collar occupation. The higher track (*Gymnasium*) is the most academic track. It ends after 13 years of schooling with the *Abitur* degree or after 12 years with the slightly less academic *Fachabitur* degree. While the former qualifies for university studies, the latter allows individuals to study at a polytechnic.[2]

The assignment to one of the school tracks is a combination of elementary school

[2]The emergence of new school forms and reductions in the duration of *Gymnasium* for *Abitur* graduates from 13 to 12 years in almost all German states are not relevant for my study, since my sample consists of early birth cohorts that were not affected by this change. Different years of schooling in the former German Democratic Republic do not play a role either as I focus only on West Germany. Besides the classical three school tracks, in some states comprehensive schools exist that comprise all the mentioned school tracks and may award all degrees. Numerically, however, comprehensive schools are not significant and receive no extra consideration in this study.

performance, teacher recommendation and parental choice. The exact mechanisms differ across states and years. Yet, everywhere in Germany and throughout history, school track choice is subject to considerable self-selection on ability and parental background (e.g. Dustmann, 2004). The first two years of secondary school are often considered to be an orientation phase allowing for mobility between the tracks. If a student of a higher school track accomplishes the minimum number of years required for a lower school track, he or she can leave the school with the lower school degree (e.g. a student can finish *Realschule* after 9 instead of 10 years with a degree from *Hauptschule*).

What makes the German educational system different from many other countries is the important role of its vocational training (Gang and Zimmermann, 2000; Zimmermann et al., 2013). For graduates of the lower and middle school tracks it seems plausible that success in the labor market depends more on the type and quality of an apprenticeship than on the school degree itself (Pischke and Wachter, 2008). Similarly, for individuals holding an *Abitur* degree, labor market success considerably improves with a university degree. Hence, a sole measure of years of schooling does not have sufficient explanatory power as to satisfactory predict wages. This is why in my application I enhance the measure of schooling with information about vocational trainings, apprenticeships, polytechnics and university studies arriving at a measure of *total years of education*.[3]

2.3 Relevant literature

One major methodological avenue to estimate the returns to education free of endogeneity bias is instrumental variable methods. The IV approach builds upon the postulation that there exists an instrument variable that is correlated with the endogenous regressor but not with the error term. Causal effects can then be estimated using the exogenous variation of this variable. One often employed instrument with an arguably strong stance of validity is a compulsory schooling law change that brings about variation in the minimal number of schooling years across space and time. The pioneers of this strand of literature are Angrist and Krueger (1991), who find returns to schooling in the US labor market of 6-10 percent for different birth cohorts that lie well above their OLS estimates of 5-7 percent. This result is corroborated by Oreopoulos (2007), who calculates causal returns in the magnitude of 13 percent as compared to lower OLS returns of almost 8 percent. In contrast, Acemoglu and Angrist (2000) estimate a return to schooling of 10 percent and fail to find evidence for biased OLS estimates. For the UK, Harmon and Walker (1995) as well as Oreopoulos (2006) find roughly 15 percent higher earnings from one

[3]See Section 2.5 for a more detailed description.

additional year of compulsory schooling, a result refuted by Devereux and Hart (2010), who calculate only 3 percent returns on average. Similar studies exist for a whole range of other countries. Interestingly enough, many IV estimates range 20-40 percent higher than the corresponding OLS results (Card, 1999). This hints at downward biased OLS estimates, which is counterintuitive given the above outlined interpretation of omitted ability variables. One IV study on returns to education for Germany that uses school reform as an instrument is by Pischke and Wachter (2008). In the 1950s and 60s the duration of the basic track was extended from 8 to 9 years. The exact timing of the policy intervention varied across time and states allowing the authors to apply a difference–in–difference framework. Using the two data sets Micro Census and Qualification and Career Survey (QaC), Pischke and von Wachter establish the result of zero returns to schooling. That is, while OLS estimations yield returns in the order of 6-7 percent, this parameter drops to a number not significantly different from zero using the IV approach. This result is remarkable as it contradicts most of the evidence from other countries.[4]

Another much acknowledged instrument for education is schooling infrastructure: Card (1995) breaks new ground by exploiting the regional and temporal variation in college proximity. The idea is that the cost of attending a college rises with distance, making the geographical closeness a sufficiently strong indicator of college education. Card uses data from the U.S. National Longitudinal Survey and finds relatively high IV returns of 13.2 percent compared to 7.3 percent OLS returns. Subsequent studies that use the same instrument are Kane and Rouse (1995), Conneely and Uusitalo (1998), and Kling (2001). In a similar spirit, Becker and Siebern-Thomas (2007) calculate the returns to education for Germany. Based on GSOEP data they use the urbanization of the place of childhood as a proxy for the availability of higher secondary schools. They report returns of about 13 percent that lie well above their OLS estimates of 6.6 percent.

A third instrument is employed by Ichino and Winter-Ebmer (2004). They use father's involvement in World War II and own educational disruptions due to the war period to instrument education in Germany. Using GSOEP data they calculate IV coefficients of 11.3 percent (own involvement) and 9.4 percent (father's involvement).

Table 2.1 reports the main findings of the three German studies. While the OLS coefficients are quite similar across studies, the IV coefficients differ considerably, even hinting at different signs of endogeneity bias. A possible explanation is that the rates of return to education differ across heterogeneous individuals (Card, 1999) and that each study correctly identifies the LATE for its respective subsample of compliers (Ichino and Winter-Ebmer,

[4]Pischke and von Wachter's favorite explanation for this fact is that the basic skills needed for the labor market are learned earlier in Germany than in other countries. An alternative explanation is that the signaling of school track choice, vocational training or apprenticeships is far more important than the actual number of years in schooling.

1999): In Pischke and von Wachter's (2008) study this subsample of compliers consists of individuals who receive one more year of schooling due to the increment in compulsory minimal schooling. Arguably, these were basic school track pupils who wanted to leave school early. Pupils of higher tracks did not receive an additional year of schooling due to the reform, as they would have attained more than minimal schooling anyway. For them the IV estimate does not hold. Similar considerations apply for the other two IV studies: Becker and Siebern-Thomas (2007) argue that their instrument provides an interpretation only for schooling differences in grade 10 and above. This is because their instrument, the degree of urbanization of the place of childhood, only affects students of higher tracks because schools of the higher tracks were more likely available in urban places while basic track school were available everywhere. Ichino and Winter-Ebmer (2004) do not document at which grade their instrument affects schooling. Since it is reasonable that World War II involvement did not affect certain educational groups exclusively, their results are likely to apply to a broader subpopulation.

2.4 Estimation approach

2.4.1 Econometric model

Interest lies in parameter δ of the linear wage equation

$$W_i = X_i\beta + \delta S_i + u_i$$

where W_i stands for hourly log wages, S_i for years of education and X_i for a $1 \times k$ vector of exogenous regressors for individual i. Identification difficulties arise as $\hat{\delta}$ likely suffers from an endogeneity bias caused by omitted variables, like unobserved ability. To illustrate this paper's estimation strategy that produces estimates free of bias, it is useful to reformulate the endogeneity problem in a control function setting. We can rewrite the model as a system of equations (and omit the subscripts for notational convenience):

$$W = X\beta + \delta S + u \tag{2.1}$$

$$S = X\varphi + v. \tag{2.2}$$

X may be identical for both equations. We refer to (2.1) as the wage or primary equation and to (2.2) as the education or secondary equation. Endogeneity is present if and only if $cov(u, v) \neq 0$. This can be represented by $u = \lambda v + e$, where the error terms v and e are uncorrelated. Note that one can use this linear combination to replace u in

(2.1) in order to derive the controlled function

$$W = X\beta + \delta S + \lambda v + e, \tag{2.3}$$

in which v is called a *control term*. Its impact λ represents the degree of endogeneity in the system. Since $cov(v, e) = 0$ the controlled equation is free of endogeneity. However, from (2.2) we know that v is a perfect linear combination of S and X. The regressors are collinear and OLS is infeasible. A standard way to solve this problem is to consider additional regressors z that restore the orthogonality of S, X and v. If we could estimate v with an additional instrument z so that (2.2) becomes $S = X\varphi + \gamma z + v$ while z is not part of (2.1), the collinearity problem would be solved. The resulting estimates are equivalent to IV.

Rather than pursuing an IV approach, however, this paper follows the approach by Klein and Vella (2010), who draw upon the presence of heteroskedasticity for establishing identification. The key idea of this approach is the notion that one is able to identify the coefficients of interest if the impact of v is not constant across X but variable, and that it is possible to estimate this impact. First, replace the unknown v by its empirical version $\hat{v}$ that we derive as the residual of (2.2). Then, transform the control term in (2.3) as detailed in appendix A.1.1 to derive the final estimation equation

$$W = X\beta + \delta S + \rho\frac{H_u(X_u)}{H_v(X_v)}\hat{v} + \varepsilon. \tag{2.4}$$

The parameter ρ is a correlation coefficient between u and v and $H_u(X_u)$ as well as $H_v(X_v)$ represent heteroskedasticity functions of the errors conditional on $X_u \subseteq X$ and $X_v \subseteq X$, respectively. X_u and X_v may be different or identical in both equations. Assuming now that the enhanced control term $(H_u(X_u)/H_v(X_v))\,\hat{v}$ is not constant across X, the regressors are no longer collinear and their parameters can be estimated with OLS. Hence, the identifying condition is that $H_u(X_u)/H_v(X_v) \neq const$ across X. That is, identification relies on non-linearity of the control term. KV (2010) call this the *variable impact property* (VIP). A second condition for identification requires that the errors correlation be independent of the regressors and constant, that is $corr(uv|X) = corr(uv)$ and $\rho = const$. This is what the authors call the *constant correlation condition* (CCC).[5] Given that both the VIP and the CCC hold, KV prove that identification is established and equation (2.4) can consistently be estimated.

As of the writing of this paper, few applications make use of identification through conditional heteroskedasticity as outlined above. Klein and Vella (2009a) estimate the return to endogenous schooling decisions for a sample of Australian workers. Farré et al.

[5] See appendix A.1.1 for further comments on the CCC.

(2010) perform a similar assessment on a sample of young adults from the U.S. Longitudinal Survey of Youth 1979. On the same dataset, the authors assess the intergenerational mobility of education (Farré et al., 2012). An application for educational and occupational mobility in China comes from Emran and Sun (2011). Finally, Schroeder (2010) uses conditional second moments to estimate the impact of microcredit borrowing on household consumption in Bangladesh.

2.4.2 Implementation

KV propose estimating the heteroskedasticity functions $H_u(X_u)$ and $H_v(X_v)$ without imposing any functional form, while the Xs enter the H functions as linear indices. A suitable estimator for this is Ichimura's (1993) semiparametric least squares (SLS) method. With the predicted version of $H_v(X_v)$ plugged into (2.4), $\hat{H}_u(X_u)$ and the final parameters of $\Phi = [\beta, \delta, \rho]$ are then simultaneously derived by minimizing two objective functions simultaneously in a maximum likelihood framework. Such semiparametric approach is theoretically attractive because it makes results not reliant on specific functional forms of the heteroskedasticity. However, running the nonparametric estimator several times in multiple rounds of iteration is computationally burdensome. Large-k X, big sample sizes, and bootstrapping the standard errors quickly render computation infeasible. For this reason, I follow Farré et al. (Farré et al., 2010, 2012) in treating both $H_u(X_u)$ and $H_v(X_v)$ as exponential functions of an index with unknown parameters:[6]

$$H_u{}^2 = exp(X_u\theta) \tag{2.5}$$

$$H_v{}^2 = exp(X_v\pi) \tag{2.6}$$

where θ and π are unknown parameters of the linear indices to be estimated. Parametrizing the H functions drastically reduces computational demands as nonparametric methods are no longer involved and there is no further need to condition on two objective functions simultaneously (Farré et al., 2010). Given the parametrizations of the H functions in (2.5) and (2.6) the estimation procedure is as follows:

1. Estimate the residuals of (2.2) as $\hat{v} = S - X\hat{\varphi}_{OLS}$.

2. Estimate π via non linear least squares using (2.6) and specifying $ln(\hat{v}^2)$ as the dependent variable. Then compute $\hat{H}_v = \sqrt{exp(X_v\hat{\pi})}$.

[6] In Monte Carlo simulations the authors show that results are robust to misspecifying the true functional form. I assess this proposition in Section 2.7.

3. In a final step, the parameters for Φ and θ are simultaneously estimated: For a candidate value of Φ , say Φ_c, we define the residual to be $\hat{u}(\Phi_c)$. Using these residuals, regress $ln(\hat{u}(\Phi_c)^2)$ on $X_u\theta_c$, where θ_c is a candidate value for θ. Then compute $\hat{H}_u(\Phi_c)$ as $\sqrt{exp(X_u\hat{\theta}_c)}$ and estimate ρ_c as

$$\min_{\rho_c} \sum \left(\hat{u}(\Phi_c) - \rho_c \frac{\hat{H}_u(\Phi_c)}{\hat{H}_v} \hat{v} \right)^2. \tag{2.7}$$

   Consistent estimates of the parameters in (2.7) are obtained by searching over Φ_c, θ_c and ρ_c by means of a standard iterative procedure.

Standard errors are obtained by repeating steps 1-3 on bootstrapped samples with 1,000 replications.

2.5 Data

My analysis draws on the data from the German Socio-Economic Panel Study (GSOEP) (Wagner et al., 2007). The GSOEP is an annual longitudinal representative household survey. It entails detailed information on income, labor market status, education and a big range of other socio-economic characteristics. Besides information on the month preceding the interview, it also contains retrospective data on a person's biography. For my analysis the GSOEP has two key advantages that makes it preferable over other data sets: First, the GSOEP contains a wide variety of socio-economic and biographical variables, which are indispensable for consistently estimating the wage and education equations as well as the two heteroskedasticity functions. A second argument in favor of the GSOEP is its panel structure, which allows for calculating time-averaged wage incomes within units, thus alleviating potential inefficiencies due to measurement error in the dependent variable (Solon, 1992).[7] For the purpose of analysis, I use all available annual waves from 1984-2009. The focus rests on full and part time employed workers, excluding self-employed. To reduce censoring of ongoing education activities, I employ information only from respondents who are at least 30 years old and who can safely be assumed to have completed their education. Individuals older than 65 are also excluded because 65 is the legal retirement age for most employees in Germany. The sample is further restricted to persons born after 1939 to exclude potential World War II influences to educational attainment after the age of 6.[8] Moreover, this guarantees that all individuals in the sample

[7] See Section 2.7 for a more thorough discussion of potential measurement error in variables.

[8] Ichino and Winter-Ebmer (2004) report that the cohorts who were in schooling age during World War II have significantly lower educational attainment than other cohorts.

received their secondary schooling after the foundation of the Federal Republic of Germany in 1949. East Germany is omitted from the analysis due to the different organization of its educational system during the GDR era. Likewise, the special status of West Berlin during that time makes me exclude this region from the analysis, too.

Table 2.2 provides an overview of all variables used in this study. The earnings variable LWAGE reports the logarithm of hourly wages. Its information comes from self-reported monthly gross wage income. In order to derive at a measure of hourly wages, the income is divided by the numbers of weekly hours agreed upon in the work contract, times the average number of weeks in a month. To mitigate potential random measurement error that is typically inherent to self-reported information, LWAGE is a three years moving average. Three observations in consecutive years constitute one averaged observation for the year in the middle. Hence, only those observations that have a precedent and a subsequent non-missing value within one unit enter the estimation. This excludes periods where workers are unemployed. To approximate the educational attainment, I use an augmented measure of years of schooling, *total years of education* (YRSEDUC), that carries additional information on the length of a person's post schooling degree, such as degrees from vocational training, apprenticeships, or universities. This generated GSOEP variable contains more variation compared to *years of schooling* and, arguably, reflects better the important role of the apprenticeship system and other post-secondary training in the German labor market. This education measure slightly differs from that of Pischke and Wachter (2008), but it follows the lines of other studies for the German labor market (e.g. Gang and Zimmermann, 2000). The generated YRSEDUC is the best approximation for educational attainment in the data set. However, it is not robust against measurement error as further explored in Section 2.7.

While most variables in Table 2.2 are self-explanatory, brief comments on STATE, MIGBACK, and AGEIMMIG are warranted: The STATE of residency dummies comprise a set of 9 binary variables indicating German federal states. For the education equation, I assume that the individuals received most of their education in the state in which they currently live.[9] MIGBACK reports a migration background for all persons of non-German nationality who immigrated to Germany themselves and for those who are of migrant origin but born in Germany. Age at immigration (AGEIMMIG) has a positive integer value for all individuals who have a migration background and immigrated to Germany themselves. For all native Germans and descendants of immigrants born in Germany this variable is coded zero. This way, some individuals have a MIGBACK value of one but an AGEIMMIG value of zero.

[9]Pischke (2007) and Siedler (2010) both report that about 85 percent of the Germans still live in their state of birth, which makes this claim reasonable.

The claim of this paper is that the cited IV studies by Ichino and Winter-Ebmer (2004), Pischke and Wachter (2008) and Becker and Siebern-Thomas (2007) have a LATE interpretation bounded to the respective subsample of compliers. For Ichino and Winter-Ebmer they are individuals with all levels of schooling, for Pischke and von Wachter the compliers are individuals with no or basic school education, and for Becker and Siebern-Thomas they are individuals with higher school education. In order to substantiate this claim I run separate regressions for the entire sample (A), for the subsample of individuals holding no school degree or a degree from the lower track (*Hauptschule*) (B), and for the subsample of individuals holding a degree from one of the higher tracks (*Realschule, Fachgymnasium, Gymnasium*) (C). This information is taken from a GSOEP variable that reports the highest school degree attained. For each individual I use the last available observation, thus disregarding the panel structure nature of the data and keeping the estimation simple. Moreover, numerous observations for one individual add little to the estimation precision as there is no within-variation in the key explanatory variable YRSEDUC. The sample consists of all observations that have non-missing values for all variables. This amounts to a total of 6,066 observations. The subsample of basic track graduates carries 2,482 observations and the subsample of higher tracks graduates 3,584. Table 2.3 displays sample summary statistics.

KV's (2010) econometric approach and its parametric version by Farré et al. (2010, 2012) work for an identical set of control variables in both the wage and the education equation. In practice, however, efficiency gains can be exploited by differently specifying both equations according to the nature of their subject. It is self-evident that, for example, job TENURE needs to be included in the wage equation while it does not have any explanatory power in the education equation. The opposite is true, say, for the variable SIBLINGS. While the number of siblings seems to be an important piece of information for the education equation,[10] it needs not to be included in the wage equation. While this way some of the regressors may constitute exclusion restrictions in an IV sense, the chosen identification strategy remains unaffected.[11]

[10] See Becker's (1991) model of the quality-quantity trade-off of children. It explains the parental choice of the number of children and the human capital investments in each child.

[11] A variant of the preferred specification with all education regressors included in the wage equation leaves the point estimates nearly unchanged.

The final estimation model takes the following form:

$$\begin{aligned}\text{LWAGE} =& \beta_0 + \beta_1\text{YRSEDUC} + \rho\text{COR} + \beta_2\text{BIRTH} + \beta_3\text{AGE}+ \\ & \beta_4\text{AGE}^2 + \beta_5\text{TENURE} + \beta_6\text{TENURE}^2 + \beta_7\text{FEMALE}+ \\ & \beta_8\text{MIGBACK} + \beta_9\text{PUBLIC} + \beta_{10}\text{FULLTIME} + \beta_{11}\text{EXPER}+ \\ & \beta_{12}\text{EXPER}^2 + \beta_{13}\text{UNEMPL} + \beta_{14}\text{UNEMPL}^2+ \\ & \sum_{j=1}^{8}\beta_{14+j}\text{STATE} + \sum_{j=1}^{9}\beta_{22+j}\text{INDUSTRY} + \sum_{j=1}^{3}\beta_{31+j}\text{FIRMSIZE} + \varepsilon\end{aligned} \tag{2.8}$$

$$\begin{aligned}\text{YRSEDUC} =& \beta_0 + \beta_1\text{BIRTH} + \beta_2\text{FEMALE} + \beta_3\text{SIBLINGS}+ \\ & \beta_4\text{MIGBACK} + \beta_5\text{AGEIMMIG} + \beta_6\text{RURAL}+ \\ & \sum_{j=1}^{8}\beta_{6+j}\text{STATE} + v\end{aligned} \tag{2.9}$$

Equation (2.8) is specified in log-linear form. Squares of TENURE, EXPER and UNEMPL are included to control for non-linear effects (Topel and Ward, 1992). Note that the equation contains the correction term COR $= [H_u(X_u)/H_v(X_v)]\,\hat{v}$ as an additional regressor and that the variables SIBLINGS, AGEIMMIG, and RURAL appear in (2.9) but not in (2.8). Note further that in the baseline setting for both equations the regressions of the conditional mean and the heteroskedasticity index are based on the same set of explanatory variables.

Before proceeding it is useful to have a closer look at the key identifying assumptions, namely the VIP and CCC. The VIP requires that there is heteroskedasticity in either one or in both equations in a fashion that the quotient of the two functions is non-constant across X. Various variables of the dataset like AGE, job TENURE or the PUBLIC sector dummy are potential candidates of causing heteroskedasticity in the wage equation, but not in the education equation. In the education equation, corresponding candidates are number of SIBLINGS and RURAL. Take job TENURE and the variable RURAL as examples: The length of job tenure is likely to decrease an employer's uncertainty about the level of worker ability in the wage setting process. As a consequence, $var(u)$ should decrease in TENURE while $var(v)$ remains unchanged. The variable RURAL indicates whether an individual grew up in a rural as opposed to an urban area. This may impart differences in individuals' distances from their homes to the nearest school, as discussed in Card (1995). This way, RURAL might negatively impact $var(v)$, while $var(u)$ does not change. I provide evidence on these propositions in Section 2.6.

Turning to the CCC, it does not seem like this condition is overly restrictive either. The CCC calls for a degree of endogeneity that is constant across regressors. For this to hold, it is shown in appendix A.1.1 that one has to assume that the correlation between the homoskedastic (unscaled) errors $\rho = \text{cov}(u^* v^*)$, i.e. the degree of endogeneity in the model, is independent of X. In other words, the impact of the unobserved ability on education and wage must be independent of the circumstances. While potentially a weak spot, this does not make KV's approach less credible than IV as the CCC is essentially inherent to any IV strategy. Although rarely spelled out explicitly, IV estimates are derived under the presumption of a homogenous degree of endogeneity across the population.

2.6 Results

Following the three-step procedure outlined in Section 2.4.2, I report four different estimation outputs: (1) The OLS estimation of the education equation; (2) the non linear least squares estimation of the education equation's heteroskedasticity index; (3) the iterative estimation of the wage equation; and (4) the non linear least squares estimation of the wage equation's heteroskedasticity index. In order to test my hypothesis that there are heterogeneous returns to education across graduates from different school types, I perform all estimations on the whole sample (A), the subsample of graduates from the basic track (B), and the subsample of graduates from higher school tracks (C).

Following the sequence of estimation, we first consider the OLS estimates of the education equation in Table 2.4. The dependent variable is YRSEDUC. The adjusted R^2 measures show that the selected socio-economic variables possess a reasonable amount of explanatory power. Only the higher tracks sample has a weak overall goodness-of-fit of 5 percent. Four of the variables are highly significant predictors of education across all samples: Being FEMALE and having a migration background negatively pays off in terms of education. Having many SIBLINGS reduces education, too. The time trend variable birth is positive for samples (A) and (C); it turns negative for sample (B). The remaining variables have different effects across the three samples: Children who grew up in RURAL areas are educationally disadvantaged, except for the basic track sample where the coefficient is insignificant. A possible explanation is that having a rural background is itself an indicator for the track choice as also visible in the summary statistics of Table 2.3. The AGEIMMIG coefficient is positive but small but becomes insignificant in the higher educated subsample.

The results of the non linear least squares estimates of $H_v^2(X_v\pi)$ are reported in Table 2.5. The dependent variable is the log squared residual from the education equation. The statistical significant coefficients and the overall goodness-of-fit measures indicate that heteroskedasticity is at work in all three samples. As presumed above, the variable RURAL exerts a negative influence on the error variance, however not for the individuals in lower school tracks. Other variables, including the STATE variables, also have significant impacts. Further evidence for the presence of heteroskedasticity comes from the Breusch-Pagan test and the White test reported at the bottom of the table. The joint χ^2 test statistics reject the null hypothesis of homoskedasticity at any common significance level. Graphical analyses in Figure A.1.1 in the appendix substantiate the claim of heteroskedasticity being at work, too. For the whole sample, panels (a) and (b) show that the variance of the residuals decreases in both the number of SIBLINGS and the year of BIRTH. Similarly, panel (c) suggests that having a migration background decreases the variance. The dispersion in education levels is higher for natives than for immigrants. For the variable RURAL in panel (d) there is no clear relationship visible although its parameter in the NLS estimation is negative and strongly significant.

Table 2.6 displays the main results for the wage equation. For each sample two outputs are presented: one from a naïve OLS regression ignoring the endogeneity of education, and one from the control function approach with the control term COR $= [H_u(X_u)/H_v(X_v)]\,\hat{v}$ as an additional regressor. In both the OLS and the CF settings the wage equation is specified as a linear function. Its semi-log character allows for interpreting small-valued coefficients as percentage changes in wage income given a unit change in the regressors. Except for the control term coefficient ρ, the interpretation of the CF parameter estimates is identical to OLS. The CF standard errors come from bootstrapping across the whole procedure with 1,000 replications. The p-values are derived by means of the percentile method. All regressions include controls for STATE, INDUSTRY, and FIRMSIZE. The sign and size of ρ allows direct inference on the direction and size of the endogeneity in the model. $\rho > 0$ indicates a positive correlation between the unobservables in the wage and education equations. The OLS parameters of YRSEDUC are upward biased. A negative coefficient reports a downward bias. The level of statistical significance informs about the extent to which the model successfully employs the VIP for identification. If ρ is significant, then there is endogeneity and the estimator performs well in purging it from the model. Conversely, an insignificant ρ hints at identification failure, which goes back to either poor performance of the estimator or a lack of endogeneity. While a detailed examination of each estimation step may reveal insights to the performance of the estimator and theory may guide considerations about the existence of endogeneity, one cannot test formally which one of the two effects is at work.

The central result of my paper becomes apparent from the estimates of the key parameters β_1 and ρ, which belong to YRSEDUC and COR, respectively. The results are presented in the first two rows of Table 2.6. The OLS estimation for the sample (A) yields a 7.4 percent return from one additional year of education. In the controlled setting, this parameter increases by only 0.6 percentage points to 8 percent, hinting at only a small downward bias in the naïve model. The corresponding coefficient of the correction term, ρ, is only weakly significant at the 10 percent level. The picture changes for the two subsamples: Both start off with positive returns to education from OLS estimations, with 4.5 percent for the basic track graduates and 7 percent for the higher track graduates. When applying the CF estimation procedure, however, the results diverge considerably. For the lower educated individuals of sample (B) ρ is positive and strongly significant. It shrinks the education coefficient to 1.1 percent and removes its precision. That is, after accounting for the endogeneity in the model, there remains hardly any return to one additional year of education for graduates from the basic school track. Opposite results are found with better educated individuals in sample (C). Here, the control term parameter is negative and weakly significant. It pushes the OLS estimates from 7.0 percent to 8.3 percent retaining its precision. Altogether, the diverging results of the two subsamples suggest that there are heterogeneous returns to education across the population.

The remaining parameter estimates of the control variables display a rather homogeneous pattern across the three samples, and also across the OLS and CF estimations. In line with most wage regressions, AGE positively affects wages, however not significantly. Most likely, this is due to overlapping effects with the experience variables. The variables BIRTH cohort, job TENURE, FULLTIME employment and job market experience (EXPER) also increase hourly wages. Instead, being FEMALE, having a migration background (MIGBACK) and possessing unemployment experience (UNEMPL) depress wages. For the basic track subsample the parameter for PUBLIC is insignificant. This may stem from the fact that in the basic track sample only a share of 20 percent is employed in the public sector (cf. Table 2.3).

The results of the wage equation's heteroskedasticity estimation are reported in Table 2.7. Note that they come from the final iteration after achieving convergence. As presumed above, the variable TENURE exerts a negative influence on the variance of u, however this cannot be measured for the subsample of higher educated. Other variables have significance impacts on $log(\hat{u}^2)$ as well, hinting at the presence of heteroskedasticity. This proposition is further supported by formal tests shown at the bottom of the table. The joint χ^2 test statistics strongly reject the null hypothesis of homoskedasticity. The graphical analyses in Figure A.1.2 in the appendix visualize exemplary how these variables might influence the squared residual. Visual inspection of panel (a) reveals the expected negative relationship between TENURE and the error variance. Panel (b) shows that between AGE and the error variance there seems to be a nonlinear relationship with different slopes across the variable's range. The scatter plot for the dummy variables PUBLIC and MIGBACK indicate that working in the public sector or being an immigrant, respectively, decrease the variance in wages. However, for MIGBACK there is no conclusive indication for this from the regression results in Table 2.7.

Reviewing the regressions and visual evidence suggests that the ATE interpretation of my estimates is justified. My parameters are ATEs only if the VIP holds for the whole sample. In other words, there must be heteroskedasticity across the whole range of at least one variable in at least one equation, and if present in both equations, the conditional variance functions must differ from one another. Visual inspection of the scatterplots suggests that the heteroskedasticity is not restricted to a certain range of the regressors' values. Moreover, identical variance functions in the wage and education equations are very unlikely. The simple fact that SIBLINGS and RURAL cause heteroskedasticity in the education equation, but are not part of the wage equation, supports this notion. Variables shared by both equations also reveal a diverging pattern across equations, as the variable MIGBACK shows exemplarily. Final support for the validity of the VIP comes from the fact that for the two subsamples the control term parameter ρ is well identified. This shows that the heteroskedasticity functions of the education equation and the wage equation are not identical and that the VIP holds.

The OLS and CF results in Table 2.6 confirm the IV literature on returns to education: Starting with the benchmark OLS results for the whole sample, my 7.4 percent returns lie well in the range of 6-7.6 percent defined by the above discussed literature (cf. Table 2.1). For the two subsamples, I do not draw comparisons with respect to OLS results because most IV studies do not split the sample into educational subgroups. Moving on to the more interesting CF estimates, recall the argument that my three different samples approximate the LATE subgroups of Ichino and Winter-Ebmer (2004), Pischke and Wachter (2008), and Becker and Siebern-Thomas (2007), respectively. Beginning with Ichino and Winter-Ebmer, one can view the treatments of their instruments, own and father's World War II involvement, to be not restricted to one particular subgroup of the population. Given this is true, their LATE represents the true ATE. Their two instruments yield returns of 9.4 percent and 11.3 percent, respectively. My CF estimate for the whole sample of 8 percent is smaller than these numbers, yet in the same ballpark. Moving on to Pischke and Wachter (2008), I draw upon the subsample of the lower educated. Just like the authors I find strong evidence for upward biased OLS estimates that diminish dramatically once the endogeneity is controlled for. The causal return to education is practically zero as the coefficient of 1.1 percent is not significant at any conventional level. This is in line with Pischke and von Wachter's "zero returns to schooling". I share the authors' conclusion that the returns for lower educated persons are considerably lower than naïve OLS suggests. Finally, Becker and Siebern-Thomas (2007) calculate a causal return of 13 percent. They acknowledge that this result can, most likely, only be interpreted for graduates from grade 10 and above. The comparison group in my study is, therefore, the individuals who graduated from higher school tracks. Just like in the cited paper my calculations indicate downward biased OLS estimates that are raised to 8.3 percent when controlling for the endogeneity. While this finding contrasts with the interpretation of omitted ability bias, it matches the tendency of the cited paper and also reflects much of the international IV literature (Ichino and Winter-Ebmer, 1999). I conclude that Pischke and von Wachter's 0 percent return to schooling and Becker and Siebern-Thomas' 13 percent return to schooling are just two sides of the same coin. Both findings just reflect different parameters for different subsamples of the population. The study that most likely finds the population's ATE is the one by Ichino and Winter-Ebmer with a range of return of 9.4-11.3 percent.

Before concluding this section, it is worth noting that, at first glance, my results appear to contradict the common hypothesis of diminishing returns to education, i.e. the assumed concave relationship between education and labor market income (Becker, 1993). The hypothesis predicts that individuals with few years of education receive a higher wage premium for one additional year than individuals with many years of education. In contrast, I find a low wage premium for lower educated and a high wage premium for higher educated. However, this contradiction is spurious. The identification approach employed in this paper, as introduced in Klein and Vella (2010), essentially builds on the assumption of linearity of the primary equation. This allows identifying only one single slope parameter for the entire sample. Causality is established *within* the sample. Splitting the sample in two parts still allows for causal inference within each sample. It does not, however, allow for causal inference *across* the samples as self-selection into higher and lower education is still a problem (cf. Section 2.2). In other words, while the estimated parameters of 1.1 percent and 8.3 percent are average returns for a randomly selected individual *within* the group of lower and higher educated individuals, respectively, they cannot inform about different returns at different points of the educational scale of a randomly selected individual from the *entire* sample.

2.7 Robustness checks

This section discusses various aspects to evaluate the robustness of my results: First, I vary the specification of the primary equation in order to assess the estimates' sensitivity to a changing number of control variables. Second, I vary the specification of the heteroskedastic index estimations. Third, I use different sample definitions, which allows closer resembling the literature's samples. Fourth, I split the sample into two halves across birth cohorts to check for variations over time. Finally, I conject about the potential problems of measurement error in the variables.

1. *Varying the specification*: A major difference of my approach to IV studies is that it

requires a relatively large number of exogenous variables in order to guarantee a sufficient degree of precision at every stage of the estimation procedure. To gain some insights into the extent to which my CF results hinge on a rich specification, Table 2.8 reports the estimation results for different sets of control variables while holding the sample size constant. As I opt to employ the same variable matrix for the level regressions as for the heteroskedasticity regressions, this also affects the non linear least squares estimation of the squared residuals. One effect I expect is a loss in precision due to a poorer fit in the heteroskedasticity estimation. An opposing effect might occur if the correction term is correlated with left-out control variables in the level estimation. In this case, part of the captured variance may translate into increased significance of ρ. The overall effect on the precision of parsimonious specifications remains ambiguous. Now turning to changes not in the precision but in the value of parameters, the stepwise omission of control variables allows to disentangle the partial effect of education from its total effect. For this reason I successively remove all variables that are potentially correlated with years of education (PUBLIC, FULLTIME, TENURE, TENURE^2 EXPER, EXPER^2, UNEMPL, UNEMPL^2, FIRMSIZE, INDUSTRY and STATE) and leave all variables in the model that are considered to be predetermined with respect to education (BIRTH, AGE, AGE^2, FEMALE, MIGBACK). Table 2.8 displays the estimation results for the two key variables YRSEDUC and COR for four different specifications (columns 1-4) and the three samples (A), (B) and (C). Column 1 mirrors the results from the preferred specification of equations (2.8) and (2.9) is known from Table 2.6. This specification employs the full set of available control variables. Moving from left to right the number of controls decreases. One can see that the key parameters do not change much across specifications: For sample (A), COR is negative and weakly significant. Reducing the number of controls decreases the parameter value of YRSEDUC slightly, while the standard errors remain nearly unchanged. For subsample (B) there is a decline in the YRSEDUC parameter towards zero across columns 1-4 as the control term gains explanatory power. The economic interpretation, however, remains unchanged. A similar conclusion holds for subsample (C). All in all, reducing the number of controls affects precision only slightly. The point estimates are robust, too. Striking differences between the partial and total effect of education cannot be detected.

2. *Varying the heteroskedastic indices*: The correct estimation of the functions $H_v(X_v)$ and $H_u(X_u)$ is crucial for successfully applying the CF approach. In order to assess the results' reliance on particular specifications of the indices in (2.5) and (2.6), Table 2.9 reports the CF estimates for different $X_u \subseteq X$ and $X_v \subseteq X$. Columns (1) and (2) show the well-known OLS and CF baseline results, respectively. The model in column (3) differs in that the non linear least squares estimator misses out the dummies for STATE, FIRMSIZE and INDUSTRY. The same is true for column (4). Different from (3), however,

this model includes additional second order controls of BIRTH, SIBLINGS and AGEIMMIG. One observes that the changes in the functional form of the heteroskedasticity estimation neither significantly influences the point estimates nor the precision of YRSEDUC and COR. Thus, I conclude that my results are not sensitive to alternative specifications of the heteroskedastic indices.[12]

3. *Varying the sample*: My sample definition is typical for the returns-to-education literature but does not perfectly match the ones used in the cited literature. Pischke and Wachter (2008) just like Ichino and Winter-Ebmer (2004), for example, exclude individuals with migration background from their analyses. Angrist and Krueger (1991) restrict their analysis to men. Becker and Siebern-Thomas (2007) limit their sample to the full-time employed. Such narrow definitions clearly circumvent potential problems of unexplained heterogeneity from the peculiarities of, say, female and immigrant employment or non-linear effects in part-time occupation. Different from such IV studies, however, my approach relies on a rich set of control variables that potentially induce, and detect, heteroskedastic error terms. Restricting my sample to native full-time employed males in, say, the private sector removes four variables (MIGBACK, FULLTIME, FEMALE, and PUBLIC) from the model, all of which proved to be a source for heteroskedasticity and are therefore important for identification. In order to investigate the robustness of my results not only to different specifications but also to different sample definitions, Table 2.10 reports the OLS and CF estimates for four different samples: Column (1) excludes immigrants from consideration, column (2) restricts the sample to full-time employed individuals, column (3) focuses on males only, and column (4) excludes public sector employees. For each sample definition, one control variable must be dropped from the model. For example, only focusing on natives makes the dummy MIGBACK redundant. Looking across columns 1-4 for all three samples (A), (B), and (C) shows that the point estimates do react to varying sample definitions to some extent. This is connected to the changing precision of the correction term. This feature is most pronounced for the sample of natives (1), where COR does not succeed in purging the endogeneity from the model of any education group. It turns out that the variable MIGBACK is crucial to estimating the education heteroskedasticity function. In the sample of full-time employed individuals the signs of the biases, that were identified in the baseline model, are replicated. For samples (A) and (C), however, COR is not significant. Most resemblance to the preferred specification can be found in the models of column (3) that exclude females from the regressions. Excluding the public sector from consideration in column (4) accentuates the result of "zero returns to education" for low skilled even more. For the entire sample (A) and the high skilled

[12] However, when excluding too many variables (not reported) identification through second moments eventually fails.

(C) identification through the control function approach fails.

4. *Splitting the sample across time*: One concern is that the returns to education in Germany changed over time and that my estimations only apply to certain cohorts of the population. In order to assess the sensitivity of my results across time, I split each of the educational subsamples (A), (B), and (C) into two groups using the group-specific median birth year as the cutoff point. This way, two groups are generated that allow a comparison between early and late born individuals. As the instruments used by Pischke and Wachter (2008), Becker and Siebern-Thomas (2007) and Ichino and Winter-Ebmer (2004) predominantly affect early birth cohorts, this is another way of validating the results in the literature. Table 2.11 displays the median cutoff values, the number of observations in each group and the OLS and CF estimation results. The comforting feature of this intertemporal comparison is that the results for the early birth cohorts are very similar to the baseline results in terms of level estimations and precision, which corroborates the link between my results and the existing literature. My main results hold for the cohorts born between 1939 and 1956/59/60. Note that the coefficient for the entire sample of 9.1 percent now draws near Ichino and Winter-Ebmer's range of 9.4-11.3 percent. When moving on to the late birth cohorts, however, the situation is different. The OLS returns decrease considerably and so do the causal returns, except for the lower educated in sample (B), where the causal returns stay close to zero. In (A) and (B) the endogeneity correction parameter loses its impact. It remains unclear, however, if the failure to correct for endogeneity for the later-born results from the fact that the returns to education have decreased over time, or from the fact that the degree of endogeneity has decreased, or because there is a violation of the VIP assumption.

5. *Assessing potential measurement error*: One issue not sufficiently addressed so far is measurement error (ME) in the variables. Both of this study's key variables, hourly wages and years of education, are self-reported and may hence suffer from imprecision. Potential ME in the dependent variable *lwage* has been addressed throughout the analysis by employing three-years moving averages. Remaining ME adds noise to the regression, but does not bias the estimates (cf. also appendix A.1.1). For the right-hand side variable YRSEDUC the issue of ME is more serious as it may lead to biased estimates. The observed years of education might differ from true years of education for three reasons: First, the discrete assignment of years to educational attainments might not correctly mirror the actual time spent to achieve them. Consider the example of an individual with a middle school track degree and an apprenticeship. For the school degree a fixed value of 10 years is assigned, regardless of how many years the individual actually spent in school. Similarly, the apprenticeship scores 2 years although some apprenticeships take longer and some take shorter than that. This discrete assignment results in a random or *classical*

measurement error. Second, the variable YRSEDUC is censored at both tails. At the lower end, individuals with less than 7 years of schooling are nevertheless assigned 7 years resulting in a positive ME. At the upper end, the maximum score is 18 years. For individuals with more than 18 years of education, this produces a negative ME. Hence, outside the range of allowed years of education the ME is negatively correlated with YRSEDUC resulting in *mean-regressive measurement error*.[13] A third source of ME has very similar characteristics. Questionnaire respondents may simply misreport their educational attainments. At intermediate levels of the variable's range this error may be random. At the lower and upper limits, however, it is more plausible to assume a mean-regressive ME. Specifically, individuals with very low levels of education cannot under-report education, whereas individuals with very high levels of education cannot over-report. Again, the ME is negatively correlated with YRSEDUC and therefore mean-regressive. IV estimations for non-categorical variables are usually consistent under classical measurement error. IV does not, however, guard against mean-regressive ME. In contrast, my CF approach is vulnerable to both classical and mean-regressive measurement error. Classical ME leads to an attenuation bias in the coefficient towards zero. The magnitude of this attenuation increases upon the inclusion of other independent variables that are correlated with the wrongly measured independent variable. The bias of mean-regressive ME is more difficult to determine. In general, however, in absolute terms, the coefficient of the variable with mean-regressive ME lies above the coefficient that would result with only classical ME (Bound et al., 2001). Whether it also exceeds the true coefficient depends on the strength of the relationship between the true years of education and the ME. As long as the relationship is weak, it does not. This happens for a few bottom and top codings. Then the ME is random for most observations. The share of observations that is neither bottom nor top coded amounts to 86.2 percent for the whole sample, 95 percent for the basic school track sample, and 80.1 percent for the higher school track sample. Correspondingly, I suppose that the impact of mean-regressive measurement error varies across the samples. It is highest for the higher track sample and lowest for the basic track sample. The latter has almost no bottom and top codings. Conclusions for my parameter estimates are hardly derivable from these numbers. I know, however, that the attenuation bias in both the OLS and the CF estimates are bigger for the basic track graduates than for higher tracks graduates. Since the share of bottom and top coded observations in neither sample is excessively large, I suspect that true parameters lie slightly above my estimates.

[13] Mean-regressive measurement error has, in fact, received some acknowledgment in the return to education literature e.g. by Kane et al. (1999), Bound and Solon (1999), and Black et al. (2000).

2.8 Conclusion

This article estimates the causal returns to education for the population of West Germany after World War II. Simply regressing wages on a measure of education produces the well-known endogeneity bias as omitted ability variables are likely to upward shift the coefficients. One potential remedy for this problem is IV methods. If the education effect is heterogenous across the population, however, the interpretation of IV estimates is bounded to LATE. Different instruments may yield different coefficients according to the characteristics of the population's subsample of compliers. This interpretation delivers a key to understanding diverging IV estimates of the returns to schooling in Germany by Ichino and Winter-Ebmer (2004), Pischke and Wachter (2008), and Becker and Siebern-Thomas (2007). My study reconciles these seemingly conflicting results. Using GSOEP data, I estimate the returns to schooling for each of the corresponding subsamples that presumably represents the compliers of each instrument. These samples are (A) all available individuals, (B) graduates from the basic school track, and (C) graduates from the higher school tracks, respectively. My calculations confirm the directions of the studies' results and hence contribute to externally validate them.

I use a control function approach to regress averaged log hourly wages on a measure of education that includes post-secondary education and a set of control variables. Identification is established through the nonlinearity of the control term, which is driven by heteroskedasticity. Tentative evidence is provided that the necessary conditions for deriving causal estimates, the variable impact property and the constant correlation condition, are likely to hold. Arguing that the presence of heteroskedasticity is not bounded to a subsample of my observations, the final coefficients sidestep the limited LATE interpretation while instead possessing ATE interpretation. For the entire sample, one additional year of education increases wages by 8 percent. The CF approach reveals that the OLS parameter is only slightly downward biased. The picture changes for the subsample of basic school track individuals. For them, one additional year of education increases wages by 1.1 percent after controlling for endogeneity. The OLS estimate of 4.5 percent is strongly upward biased, which is in line with the notion of omitted ability variable bias. In stark contrast, for the subsample of higher school tracks graduates, the estimated return to education is 8.3 percent. The corresponding OLS estimate of 7 percent is downward biased. The robustness section shows that these results are fairly robust across different specifications of the main equations and the heteroskedastic indices, while being vulnerable to different sample definitions. Splitting the samples in two across time reveals that the estimations are most reliable for early born individuals that belong to the birth cohorts from 1939 until the late 1950s. Finally, assessing the impact of measurement error in the education variable leads to the conclusion that the CF estimates are likely to represent a lower bound of the true parameter.

Before concluding, a word of caution about the interpretation of the coefficients in the return to education literature, specifically for studies about Germany, is warranted: The conventional concept of measuring education in years of schooling closely resembles the concept of human capital accumulation. Even so, it is not necessarily convincing for a structured school system like the one in Germany where different tracks lead to different degrees with little mobility from one to another. The problem arises when researchers claim to measure the *quantity* of schooling while holding the *quality* of schooling constant. For Germany this is most likely not the case. One cannot assume that, for example, one year of the basic track (*Hauptschule*) is equivalent to one year of the highest track (*Gymnasium*). Regression results for Germany should therefore not be interpreted from the perspective of a change in years of schooling but rather from the perspective of changes across different school tracks. One way to alleviate this problem while sticking to the established concept years of education is to enhance this measure by post-secondary educational activities. Another option is to run separate regressions for different school types. Both strategies were realized in this study.

Tables

Table 2.1: Selected IV Studies for Germany

	Data (wave)	Birth cohorts	Instrument	OLS	IV
Piscke & von Wachter (2008)	Micro Census (1989,91,93,95–2004), QaC (1979,85,91,98)	1930–60	compulsory school reform	6–7%	0%
Becker & Siebern-Thomas (2007)	GSOEP (1985)	1930–65	urbanization place of childhood	6.6%	13%
Ichino & Winter-Ebmer (2004)	GSOEP (1984-86)	1925–49	own ... father's ... World War II involvement	7.6% 7.2%	11.3% 9.4%

Note: QaC: Qualifcation and Career Survey, GSOEP: German Socio-Economic Panel Study

Table 2.2: Variables Description

LWAGE	three-years moving average of log hourly gross wage in euros (deflated to 2005)
YRSEDUC	length of secondary and post-secondary education in decimal years
FEMALE	female (yes/no)
BIRTH	year of birth in decimal form (e.g. 1959 = 5.9)
AGE	age at interview
TENURE	length of time with same employer in decimal years
STATE	9 federal state dummies for residency
INDUSTRY	10 NACE classification dummies for sector of employment
FIRMSIZE	4 dummies for number of employees in firm
FULLTIME	working 35 hours weekly or more (yes/no)
EXPER	length of full-time employment experience in decimal years
UNEMPL	length of unemployment experience in decimal years
PUBLIC	employed in the public sector (yes/no)
MIGBACK	migration background (yes/no)
AGEIMMIG	age at immigration (= 0 if born in Germany)
SIBLINGS	number of siblings including half brothers and sisters
RURAL	raised in the countryside (yes/no)

Table 2.3: Sample Summary Statistics

	(A) ALL		(B) BASIC TRACK		(C) HIGHER TRACKS	
	mean	sd	mean	sd	mean	sd
LWAGE	17.87	(9.02)	14.58	(6.13)	20.22	(10.06)
YRSEDUC	12.30	(2.77)	10.16	(1.00)	13.81	(2.59)
BIRTH	5.82	(0.89)	5.61	(0.90)	5.96	(0.85)
AGE	46.51	(8.23)	47.68	(8.13)	45.49	(8.19)
TENURE	14.41	(10.67)	15.01	(11.01)	13.84	(10.37)
EXPER	19.25	(11.00)	21.82	(11.41)	17.32	(10.21)
UNEMPL	0.42	(1.22)	0.57	(1.52)	0.32	(0.91)
SIBLINGS	2.30	(1.98)	2.85	(2.24)	1.90	(1.66)
FEMALE	0.46		0.41		0.49	
RURAL	0.39		0.44		0.34	
MIGBACK	0.16		0.26		0.10	
PUBLIC	0.30		0.20		0.38	
share in basic school track	0.42		100.00		0.00	
n	6,066		2,482		3,584	

Note: SOEP, 1984–2009. The sample includes all individuals employed in West-Germany (excluding self-employed and West Berlin) born after 1939, and aged between 30 and 65 years, for whom all relevant variables are non-missing.

Table 2.4: OLS Estimates—Education Equation

Dependent variable: YRSEDUC	ALL	BASIC TRACK	HIGHER TRACKS
	β_{OLS}	β_{OLS}	β_{OLS}
BIRTH	0.249 ***	0.102 ***	-0.275 ***
	(0.038)	(0.022)	(0.050)
FEMALE	-0.165 **	-0.200 ***	-0.463 ***
	(0.067)	(0.039)	(0.084)
SIBLINGS	-0.264 ***	-0.067 ***	-0.156 ***
	(0.018)	(0.009)	(0.026)
MIGBACK	-2.164 ***	-0.992 ***	-0.815 **
	(0.207)	(0.098)	(0.330)
AGEIMMIG	0.031 ***	0.010 ***	-0.001
	(0.008)	(0.004)	(0.012)
RURAL	-0.548 ***	-0.013	-0.475 ***
	(0.070)	(0.039)	(0.091)
c	12.014 ***	10.049 ***	16.253 ***
	(0.238)	(0.132)	(0.317)
n	6,066	2,482	3,584
adj. R^2	0.12	0.17	0.05

Note: SOEP, 1984–2009. Standard errors in parentheses. The sample includes observations for all persons employed in West-Germany (excluding self-employed and West Berlin) born after 1939, and aged between 30 and 65 years, for whom all relevant variables are non-missing. All regressions include STATE dummies. *=p< 0.10, **=p< 0.05, ***=p< 0.01.

Table 2.5: Heteroskedasticity Index—Education Equation

Dependent variable: $log(\hat{v}^2)$	ALL		BASIC TRACK		HIGHER TRACKS	
	β_{SLS}	s.e.	β_{SLS}	s.e.	β_{SLS}	s.e.
c	1.747	(0.176)***	-2.297	(0.327)***	2.069	(0.207)***
BIRTH	-0.021	(0.028)	-0.264	(0.054)***	-0.129	(0.033)***
FEMALE	-0.197	(0.049)***	0.897	(0.096)***	0.005	(0.055)
SIBLINGS	-0.164	(0.013)***	0.229	(0.022)***	-0.080	(0.017)***
MIGBACK	-1.225	(0.153)***	2.183	(0.243)***	-0.360	(0.216)*
AGEIMMIG	0.023	(0.006)***	0.002	(0.009)	-0.010	(0.008)
RURAL	-0.497	(0.052)***	0.000	(0.097)	-0.193	(0.059)***
Schleswig-Holstein	-0.602	(0.136)***	-0.103	(0.275)	0.074	(0.148)
Hamburg	0.131	(0.184)	-0.718	(0.439)	0.087	(0.185)
Lower Saxony	-0.175	(0.086)**	-0.357	(0.170)**	0.016	(0.094)
Bremen	0.260	(0.256)	0.852	(0.547)	0.463	(0.269)*
Hesse	0.175	(0.091)*	-0.049	(0.189)	0.125	(0.098)
North Rhine-Westphalia	-0.286	(0.100)***	-0.355	(0.180)**	0.109	(0.119)
Baden-Württemberg	0.046	(0.076)	0.343	(0.144)**	0.189	(0.086)**
Bavaria	-0.201	(0.074)***	-0.166	(0.140)	0.219	(0.083)***
n	6,066		2,482		3,584	
adj. R^2	0.08		0.25		0.03	
Breusch-Pagan test	144.35		732.04		53.64	
White test	222.03		358.11		194.41	

Note: See Table 2.4. The standard errors are calculated from pairwise bootstrapping with 1,000 replications.

Table 2.6: OLS & CF Estimates—Wage Equation

Dependent variable: LWAGE

	ALL				BASIC TRACK				HIGHER TRACKS			
	β_{OLS}		β_{CF}		β_{OLS}		β_{CF}		β_{OLS}		β_{CF}	
YRSEDUC	0.074	***	0.080	***	0.045	***	0.011		0.070	***	0.083	***
	(0.002)		(0.004)		(0.006)		(0.007)		(0.002)		(0.007)	
COR			-0.074	*			0.108	***			-0.159	*
			(0.040)				(0.020)				(0.087)	
BIRTH	0.060	***	0.058	***	0.073	***	0.081	***	0.043	***	0.046	***
	(0.007)		(0.006)		(0.009)		(0.006)		(0.010)		(0.007)	
AGE	0.003		0.002		-0.003		-0.003		0.008		0.008	
	(0.005)		(0.006)		(0.008)		(0.005)		(0.007)		(0.007)	
AGE2	0.000		0.000		0.000		0.000	*	0.000		0.000	
	(0.000)		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)	
TENURE	0.010	***	0.010	***	0.007	***	0.007	***	0.011	***	0.011	***
	(0.001)		(0.001)		(0.002)		(0.001)		(0.002)		(0.002)	
TENURE2	-0.000	***	-0.000	***	-0.000		-0.000		-0.000	**	-0.000	***
	(0.000)		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)	
FEMALE	-0.168	***	-0.167	***	-0.223	***	-0.236	***	-0.142	***	-0.137	***
	(0.010)		(0.010)		(0.016)		(0.010)		(0.012)		(0.010)	
MIGBACK	-0.079	***	-0.066	***	-0.051	***	-0.084	***	-0.138	***	-0.124	***
	(0.011)		(0.013)		(0.014)		(0.012)		(0.017)		(0.015)	
PUBLIC	-0.021	*	-0.021	*	0.017		0.016		-0.040	***	-0.041	***
	(0.011)		(0.011)		(0.019)		(0.012)		(0.014)		(0.011)	
FULLTIME	0.026	**	0.025	*	0.046	**	0.044	***	0.025		0.024	*
	(0.012)		(0.013)		(0.019)		(0.014)		(0.016)		(0.013)	
EXPER	0.014	***	0.014	***	0.010	***	0.010	***	0.014	***	0.014	***
	(0.002)		(0.002)		(0.002)		(0.002)		(0.002)		(0.002)	
EXPER2	-0.000	***	-0.000	***	-0.000	***	-0.000	***	-0.000	***	-0.000	***
	(0.000)		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)	
UNEMPL	-0.037	***	-0.036	***	-0.032	***	-0.032	***	-0.053	***	-0.052	***
	(0.006)		(0.007)		(0.008)		(0.005)		(0.012)		(0.011)	
UNEMPL2	0.002	***	0.002	***	0.002	**	0.002	***	0.003		0.003	**
	(0.001)		(0.001)		(0.001)		(0.000)		(0.002)		(0.002)	
c	0.531	***	0.467	***	0.922	***	1.236	***	0.558	***	0.372	**
	(0.132)		(0.142)		(0.202)		(0.145)		(0.180)		(0.179)	
n	6,066		6,066		2,482		2,482		3,584		3,584	
adj. R^2	0.53		0.53		0.43		0.44		0.50		0.50	

Note: SOEP, 1984–2009. Standard errors in parentheses. The sample includes observations for all persons employed in West-Germany (excluding self-employed and West Berlin) born after 1939, and aged between 30 and 65 years, for whom all relevant variables are non-missing. The CF standard errors and p-stastistics are calculated from pairwise bootstrapping with 1,000 replications. All specifications of the wage equation additionally control for STATE dummies, INDUSTRY sector dummies and FIRM SIZE dummies. The school heteroskedasticity equation is estimated using the same variables as in Table 2.4 (results in Table 2.5). The wage heteroskedasticity function is estimated using the same variables as above (results in Table 2.7). *=p< 0.10, **=p< 0.05, ***=p< 0.01.

Table 2.7: Heteroskedasticity Index—Wage Equation

Dependent variable: $log(\hat{u}^2)$ of final iteration	ALL		BASIC TRACK		HIGHER TRACKS	
	β_{SLS}	s.e.	β_{SLS}	s.e.	β_{SLS}	s.e.
c	-5.917	(1.040)***	-7.076	(1.625)***	-7.025	(1.427)***
BIRTH	0.221	(0.053)***	0.334	(0.078)***	0.244	(0.079)***
AGE	0.028	(0.043)	0.040	(0.067)	0.056	(0.060)
AGE^2	0.000	(0.000)	0.000	(0.001)	0.000	(0.001)
TENURE	-0.021	(0.010)**	-0.036	(0.016)**	0.001	(0.015)
TENURE^2	0.000	(0.000)	0.001	(0.000)*	-0.001	(0.000)
FEMALE	-0.178	(0.076)**	-0.032	(0.131)	-0.188	(0.098)*
MIGBACK	0.032	(0.084)	-0.326	(0.112)***	0.337	(0.136)**
PUBLIC	-0.300	(0.088)***	-0.100	(0.155)	-0.359	(0.111)***
FULLTIME	-0.221	(0.097)**	-0.437	(0.160)***	-0.133	(0.127)
EXPER	-0.018	(0.013)	0.011	(0.020)	-0.017	(0.019)
EXPER^2	0.000	(0.000)	0.000	(0.001)	0.001	(0.001)
UNEMPL	0.004	(0.050)	0.078	(0.066)	-0.111	(0.092)
UNEMPL^2	0.000	(0.005)	-0.009	(0.006)	0.017	(0.014)
Schleswig-Holstein	0.114	(0.163)	-0.003	(0.265)	0.003	(0.215)
Hamburg	-0.282	(0.220)	-0.042	(0.423)	-0.010	(0.269)
Lower Saxony	0.054	(0.103)	0.068	(0.163)	-0.021	(0.137)
Bremen	-0.482	(0.307)	0.068	(0.529)	-0.343	(0.390)
Hesse	0.182	(0.109)*	-0.002	(0.183)	0.151	(0.142)
North Rhine-Westphalia	-0.166	(0.119)	-0.170	(0.171)	-0.182	(0.172)
Baden-Württemberg	-0.065	(0.091)	-0.098	(0.138)	-0.124	(0.125)
Bavaria	0.013	(0.088)	-0.052	(0.134)	0.081	(0.121)
agriculture	-0.004	(0.365)	0.415	(0.506)	-0.200	(0.543)
energy	-0.189	(0.289)	0.286	(0.508)	-0.016	(0.366)
mining	-0.322	(0.465)	1.087	(0.560)*	-0.918	(0.852)
construction	-0.588	(0.141)***	-0.394	(0.180)**	-0.582	(0.237)**
trade	-0.032	(0.106)	-0.067	(0.147)	0.100	(0.159)
transport	-0.110	(0.146)	0.196	(0.216)	-0.200	(0.206)
banking & insurance	-0.022	(0.143)	-0.163	(0.343)	0.099	(0.171)
services	0.152	(0.096)	0.073	(0.157)	0.311	(0.129)**
other industries	0.028	(0.367)	-0.217	(0.471)	0.663	(0.603)
$20-200$ employees	-0.200	(0.089)**	0.094	(0.131)	-0.245	(0.125)*
$200-2000$ employees	-0.429	(0.097)***	-0.047	(0.146)	-0.456	(0.134)***
>2000 employees	-0.257	(0.097)***	-0.179	(0.151)	-0.206	(0.132)
n	6,066		2,482		3,584	
adj. R^2	0.02		0.02		0.02	
Breusch-Pagan test	544.25		268.59		403.84	
White test	857.88		823.81		717.99	

Note: See note of Table 2.5.

Table 2.8: Varying the Specification

Dependent variable: LWAGE	(1)		(2)		(3)		(4)	
	β_{OLS}	β_{CF}	β_{OLS}	β_{CF}	β_{OLS}	β_{CF}	β_{OLS}	β_{CF}
(A) ALL SCHOOL TRACKS								
YRSEDUC	0.074 ***	0.080 ***	0.077 ***	0.086 ***	0.071 ***	0.076 ***	0.073 ***	0.079 ***
	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)
COR		-0.074 *		-0.097 **		-0.051		-0.066
		(0.040)		(0.040)		(0.039)		(0.041)
n	6,066		6,066		6,066		6,066	
(B) BASIC SCHOOL TRACK								
YRSEDUC	0.045 ***	0.011	0.044 ***	0.008	0.048 ***	0.003	0.048 ***	0.001
	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.008)	(0.006)	(0.008)
COR		0.108 ***		0.111 ***		0.128 **		0.137 **
		(0.020)		(0.019)		(0.020)		(0.021)
n	2,482		2,482		2,482		2,482	
(C) HIGHER SCHOOL TRACKS								
YRSEDUC	0.070 ***	0.083 ***	0.074 ***	0.093 ***	0.064 ***	0.079 ***	0.065 ***	0.090 ***
	(0.002)	(0.007)	(0.002)	(0.007)	(0.002)	(0.007)	(0.002)	(0.009)
COR		-0.159 *		-0.234 **		-0.179 **		-0.298 **
		(0.087)		(0.088)		(0.084)		(0.108)
n	3,584		3,584		3,584		3,584	
Control variables:								
PUBLIC	✓	✓	✓	✓	✓	✓		
FULLTIME	✓	✓	✓	✓	✓	✓		
TENURE+TENURE^2	✓	✓	✓	✓				
EXPER+EXPER^2	✓	✓	✓	✓				
UNEMPL+UNEMPL^2	✓	✓	✓	✓				
FIRMSIZE dummies	✓	✓						
INDUSTRY dummies	✓	✓						
STATE dummies	✓	✓						

Note: SOEP, 1984–2009. Standard errors in parentheses. The sample includes observations for all persons employed in West-Germany (excluding self-employed and West Berlin) born after 1939, and aged between 30 and 65 years, for whom all relevant variables are non-missing. The CF standard errors and p-statistics are calculated from bootstrapping with 1,000 replications. All specifications of the wage equation control for the predetermined variables BIRTH, AGE, AGE^2, FEMALE and MIGBACK. $\hat{H}_v$ is estimated using the same variables as in Table 2.4 (results in Table 2.5). $\hat{H}_u$ is estimated using the same variables as in the level estimation of the wage equation. *=p< 0.10, **=p< 0.05, ***=p< 0.01..

Table 2.9: Alternative Specifications of Heteroskedasticity Indices

CF ESTIMATION—WAGE EQUATION				
Dependent variable: LWAGE				
	(1)	(2)	(3)	(4)
	β_{OLS}	β_{CF}	β_{CF}	β_{CF}
	(A) ALL SCHOOL TRACKS			
YRSEDUC	0.074 ***	0.080 ***	0.083 ***	0.084 ***
	(0.002)	(0.004)	(0.004)	(0.004)
COR		-0.074 *	-0.104 **	-0.119 **
		(0.040)	(0.043)	(0.047)
	(B) BASIC SCHOOL TRACK			
YRSEDUC	0.045 ***	0.011	0.008	0.013 *
	(0.006)	(0.007)	(0.007)	(0.007)
COR		0.108 ***	0.119 ***	0.104 ***
		(0.020)	(0.020)	(0.018)
	(C) HIGHER SCHOOL TRACKS			
YRSEDUC	0.070 ***	0.083 ***	0.086 ***	0.085 ***
	(0.002)	(0.007)	(0.008)	(0.008)
COR		-0.159 *	-0.208 **	-0.188 *
		(0.087)	(0.107)	(0.106)
HET. INDEX—WAGE EQUATION				
BIRTH		✓	✓	✓
AGE+AGE^2		✓	✓	✓
TENURE+TENURE^2		✓	✓	✓
FEMALE		✓	✓	✓
MIGBACK		✓	✓	✓
PUBLIC		✓	✓	✓
FULLTIME		✓	✓	✓
EXPER+EXPER^2		✓	✓	✓
UNEMPL+UNEMPL^2		✓	✓	✓
STATE dummies		✓		
FIRMSIZE dummies		✓		
INDUSTRY dummies		✓		
HET. INDEX—EDUCATION EQUATION				
BIRTH		✓	✓	✓
FEMALE		✓	✓	✓
SIBLINGS		✓	✓	✓
MIGBACK		✓	✓	✓
AGEIMMIG		✓	✓	✓
RURAL		✓	✓	✓
STATE dummies		✓		
BIRTH^2				✓
SIBLINGS^2				✓
AGEIMMIG^2				✓

Note: See note of Table 2.6, except for different specification of heteroskedastic index. Heteroskedasticity models include constant.

Table 2.10: Varying the Sample

Dependent variable: LWAGE								
	(1) ONLY NATIVES		(2) ONLY FULL-TIME EMPLOYED		(3) ONLY MALES		(4) ONLY PRIVATE SECTOR	
	β_{OLS}	β_{CF}	β_{OLS}	β_{CF}	β_{OLS}	β_{CF}	β_{OLS}	β_{CF}
	(A) ALL SCHOOL TRACKS							
YRSEDUC	0.078 ***	0.077 ***	0.072 ***	0.076 ***	0.068 ***	0.082 ***	0.069 ***	0.070 ***
	(0.002)	(0.004)	(0.002)	(0.004)	(0.003)	(0.004)	(0.002)	(0.004)
COR		0.016		-0.046		-0.164 ***		-0.001
		(0.047)		(0.044)		(0.043)		(0.031)
n	5,073		4,577		3,294		4,226	
	(B) BASIC SCHOOL TRACK							
YRSEDUC	0.078 ***	0.063 ***	0.048 ***	0.020 ***	0.043 ***	0.013 **	0.039 ***	0.000
	(0.009)	(0.011)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)
COR		0.023		0.087 ***		0.076 ***		0.141 ***
		(0.014)		(0.018)		(0.015)		(0.020)
n	1,859		1,920		1,456		1,999	
	(C) HIGHER SCHOOL TRACKS							
YRSEDUC	0.072 ***	0.081 ***	0.068 ***	0.072 ***	0.067 ***	0.089 ***	0.064 ***	0.063 ***
	(0.002)	(0.008)	(0.003)	(0.007)	(0.004)	(0.006)	(0.003)	(0.006)
COR		-0.126		-0.056		-0.280 ***		0.020
		(0.101)		(0.084)		(0.072)		(0.063)
n	3,214		2,657		1,838		2,227	
Control variables:								
MIGBACK	no	no	✓	✓	✓	✓	✓	✓
FULLTIME	✓	✓	no	no	✓	✓	✓	✓
FEMALE	✓	✓	✓	✓	no	no	✓	✓
PUBLIC	✓	✓	✓	✓	✓	✓	no	no

Note: SOEP, 1984–2009. Standard errors in parentheses. The sample includes observations for all persons employed in West-Germany (excluding self-employed and West Berlin) born after 1939, and aged between 30 and 65 years, for whom all relevant variables are non-missing. The CF standard errors and p-statistics are calculated from bootstrapping with 1,000 replications. All specifications of the wage equation control for the checkmarked variables as well as BIRTH, AGE, AGE^2, TENURE, TENURE^2 EXPER, EXPER^2, UNEMPL, UNEMPL^2, FIRMSIZE dummies, INDUSTRY dummies, and STATE dummies. $\hat{H}_v$ is estimated using the same variables as in Table 2.4 (results in Table 2.5). $\hat{H}_u$ is estimated using the same variables as in the level estimation of the wage equation. *=p< 0.10, **=p< 0.05, ***=p< 0.01.

Table 2.11: Comparing Early and Late Birth Cohorts

Dependent variable: LWAGE				
	(1)		(2)	
	≤ MEDIAN		> MEDIAN	
	β_{OLS}	β_{CF}	β_{OLS}	β_{CF}
	(A) ALL SCHOOL TRACKS (median=1959)			
YRSEDUC	0.080 ***	0.091 ***	0.067 ***	0.067 ***
	(0.002)	(0.004)	(0.003)	(0.004)
COR		-0.121 ***		0.002
		(0.039)		(0.042)
n	3,187		2,879	
	(B) BASIC SCHOOL TRACK (median=1956)			
YRSEDUC	0.059 ***	0.013 **	0.029 ***	0.008
	(0.008)	(0.007)	(0.008)	(0.008)
COR		0.150 ***		0.063 ***
		(0.022)		(0.017)
n	1,294		1,188	
	(C) HIGHER SCHOOL TRACK (median=1960)			
YRSEDUC	0.075 ***	0.096 ***	0.067 ***	0.062 ***
	(0.003)	(0.007)	(0.003)	(0.006)
COR		-0.271 ***		0.061
		(0.088)		(0.073)
n	1,799		1,785	

Note: See Table 2.6.

3 The Effect of Occupational Knowledge: Job Information Centers and Labor Market Outcomes*

Chapter Abstract

This study examines the causal link between individuals' occupational knowledge, educational choices, and labor market outcomes. We proxy occupational knowledge with mandatory visits to job information centers (JICs) in Germany while still attending school. Exogenous variation in the location of JICs and timing of when they opened makes it possible to estimate causal effects in a difference-in-difference setup. Combining linked survey-administrative data with the data on JICs allows us to detect whether individuals benefited from the comprehensive information service when they were young. The results suggest that individuals, who went to school in administrative districts with a JIC, have higher educational attainments and a smoother transfer to the labor market than students who did not have access to these facilities. However, we find no positive effects on individuals' earnings in their first job or later in life. Overall, our results tend to confirm the importance of policies that promote occupational knowledge among youths and young adults.

3.1 Introduction

In most industrialized countries, students have to decide early in life which educational track to follow and what type of job to learn. The educational and occupational choices are often made under uncertainty. Students may be uncertain about their own abilities and tastes, skills needed for particular work, local labor market conditions, future job prospects, or earnings profiles. A key ingredient for making a well-informed labor market decision is the availability of job-related information. While there are different ways to

*This chapter is based on joint work with Thomas Siedler.

retrieve job-related information privately, in many countries it is publicly provided in the form of occupational education in classrooms or information facilities outside schools.

Among educators and policy makers, there is typically little doubt about the general societal value of publicly providing job-related information (OECD, 2004). However, research to date has been limited in estimating potential benefits of occupational information programs. In particular, there is hardly any evidence on interventions at the national level. We make use of a comprehensive and nationwide reform of occupational education in Germany that involves the establishment of job information centers (JICs) at different times in different regions. Job information centers provide young people with very detailed and comprehensive information on occupations, employment and income prospects, local labor market conditions and educational pathways. Exogenous variation in the location and timing of opening JICs allow causal inference in a difference-in-difference framework. For the present study, linked survey-administrative data were used to analyze various outcomes of students at different points over the life-course, including education and educational mobility, job search and job matching, unemployment experience, as well as earnings.

Theory suggests several reasons why providing comprehensive job information in general—and the availability of JICs in particular—might influence individuals' choices and outcomes. First, providing information about the number and types of occupations and job vacancies available to young people in their region of residence might improve the quality of the match between workers and companies (Miller, 1984). Similarly, providing information about job prospects and future earnings profiles may lead to efficiency gains if young individuals are more likely to find an occupation that matches their skills and interests (Borghans et al., 2013). Third, job counseling might help students to make choices that are less affected by their parents, prevailing gender roles, individuals' perception of their own identity (Akerlof and Kranton, 2000; Favara, 2012; Borghans et al., 2013). Fourth, recent empirical studies suggest that educational choices are influenced by students' expectations about their economic returns to educational fields (Zafar, 2011; Stinebrickner and Stinebrickner, 2011; Papay et al., 2011). Thus, there is reason to believe that job market knowledge acquired through visiting a JIC might alter students' expectations and, as a result, impact on their educational choices and long-term labor market outcomes.

Our empirical estimates suggest that the availability of a job information center causes a large and significant increase in the likelihood of experiencing upward educational mobility, and improves chances of acquiring the highest general school degree. For instance, the likelihood of experiencing upward educational mobility increases by around 8-12 percentage points. Individuals who went to school in an administrative district with a JIC are also less likely to become unemployed or to involuntarily lose their job at the beginning of their labor market career. Further, the estimates appear to indicate that visiting a JIC improves job matching. However, we find no positive impact on individuals' earnings in their first job or later in life. These findings are robust to the inclusion of various explanatory variables, for various sub-samples and different time trends. Our results are consistent with previous studies reporting that information programs are effective at improving individuals' educational choices. In particular, our findings are in line with a recent study by Rodríguez-Planas (2012) who reports positive effects of a randomized mentoring program in the United States on educational choices, but no effects on labor market outcomes. Exploiting variation across regions and over time enables us to measure general equilibrium effects. As such, we complement the local estimates from randomized experiments among upper school students from selected colleges or universities (Booij et al., 2012; Wiswall and Zafar, 2011; Oreopoulos and Dunn, 2013).

In the difference-in-difference research design, a key assumption for identification is that there are no contemporaneous shocks or changes on the district level that are correlated with the establishment of JICs and the various outcome measures. We present detailed evidence to verify this assumption. First, we present graphical evidence supporting the common trend assumption for various educational and labor market variables. Second, we estimate survival analysis models showing that the timing of JIC openings is uncorrelated with contemporaneous or pre-determined variables on the district level. The estimates show that the timing of JICs opening is not statistically significantly correlated with initial educational and labor market characteristics. For example, JICs were not more likely to open sooner in districts with a high youth unemployment rate, or in regions with a high proportion of educated people. Further, we find no empirical evidence that pre-existing trends in education or local labor markets are correlated with the timing of JICs opening.

The remainder of this paper is structured as follows: Section 3.2 summarizes the relevant literature. Section 3.3 provides details on the institutional background of job information centers, their role in occupational education in schools, how they work, and when they opened (across years and districts). Section 3.4 describes the data and presents summary statistics. Section 3.5 provides details on the estimation method and identifying assumptions, which are further discussed on the basis of graphical evidence and survival estimates in Section 3.7. Section 3.6 presents the results, Section 3.8 presents robustness checks, and Section 3.9 concludes.

3.2 Related Literature

This paper is closely related to the literature on the effects of counseling and mentoring on individuals' educational choices and labor market outcomes (Borghans et al., 2013;

Rodríguez-Planas, 2012). Rodríguez-Planas (2012) examines the effects of a randomized program offering mentoring by case workers, educational services, and financial awards over a five-year period to low-performing high school students in the United States. The study reports positive effects on high school completion and post-secondary education, but little evidence on labor market outcomes five years after the end of the program (e.g., hours worked, having a full-time job, and pay). Borghans et al. (2013) study the impact of counseling at high school on the quality of tertiary educational choices in the Netherlands. The authors use variation in the intensity of counseling practices between high schools as exogenous variation for students' decisions whether or not to seek career counseling. The study finds a positive influence of counseling on students' own evaluation of their educational choices 18 months after graduating from tertiary education. The effects of visiting a study counselor are particularly beneficial for graduates with a migrant background and for those whose parents have lower levels of education.[1]

A second related literature studies the importance of information and subjective expectations. Several papers report that individuals modify their beliefs and behavior in response to new information (Dominitz, 1998; Hastings and Weinstein, 2008; Jensen, 2010; Martínez and Dinkelman, 2011; Nguyen, 2008; Oreopoulos and Dunn, 2013; Stinebrickner and Stinebrickner, 2011; Wiswall and Zafar, 2011; Zafar, 2011), whereas others find no evidence that providing information changes people's behavior (Booij et al., 2012). Many studies in this strand of literature exploit exogenous variation through randomized trials to study the impact of interventions on individuals' educational choices and revealed preferences.[2] The studies by Zafar (2011), Stinebrickner and Stinebrickner (2011), Oreopoulos and Dunn (2013) and Wiswall and Zafar (2011) are particularly relevant to our analysis. Zafar (2011) examines how students form expectations about major-specific outcomes and how they resolve uncertainties in their educational decisions. Using longitudinal survey data on a sample of university undergraduates, the author finds that learning plays an important role for students' educational choices and that students' updating process is consistent with a Bayesian learning model. Similarly, Stinebrickner and Stinebrickner (2011) study how low-income college students update their beliefs about choice of college major. The authors find that students are quite open to majoring in math or science at the start of college, but that many move away from these majors after learning about their own skills and realizing that their grades will be lower than originally expected if they focus on these subjects.

[1]Note that we do not review the counseling and monitoring literature for unemployed people here. Recent studies that examine the effects of job-search counseling for unemployed individuals are, for example, Hainmüller et al. (2009) and Vikström et al. (2011).

[2]See, for example, Hastings and Weinstein (2008); Nguyen (2008); Jensen (2010); Martínez and Dinkelman (2011); Booij et al. (2012); Wiswall and Zafar (2011); Oreopoulos and Dunn (2013).

Oreopoulos and Dunn (2013) study the effect of information provision on educational attainments. Students from high schools in low-income neighborhoods in Toronto are being provided accurate information by means of a promotional video about the costs and benefits of attaining higher education and loan eligibility. The authors report that students who have been exposed to the educational information report higher expectations of their own returns to post-graduate education, express a higher likelihood that they are eligible for a grant, and are more likely to say that they aim to completing a college degree three weeks after the intervention. The authors also report evidence that the intervention changes students' behavior: those who are shown the video are more likely to download additional information about higher education and specific colleges and universities from the Internet. Wiswall and Zafar (2011) examine whether providing new information alters students' beliefs and their choice of college major. Their longitudinal analysis shows that individuals make substantial errors in population beliefs and revise their self-beliefs in response to new information. Among other things, the authors conclude: "This suggests that information campaigns focused on providing accurate information on returns to schooling could have a large impact on beliefs and choices of students." (Wiswall and Zafar, 2011, p. 35).

The aim of the present paper is to contribute to this literature by studying the effects of introducing a comprehensive and nationwide occupational information program on individuals' educational choices and labor market outcomes by opening JICs in Germany. We are able to study short-term (e.g., educational attainment of secondary schooling, educational upward mobility), medium-term (e.g., labor market outcomes in the first five years after entering the labor market) and long-term effects (e.g., earnings and geographic mobility 15 to 20 years after entering the labor market). Further, this study uses quasi-experimental evidence on young people from different types of school and with heterogeneous educational backgrounds. Unlike authors of other studies, we are able to estimate effects for an entire country. Thus, we complement experimental studies based on university student populations (Wiswall and Zafar, 2011; Booij et al., 2012) or students from high schools in one city (Oreopoulos and Dunn, 2013). Another important contribution is that we examine the effects of information provision that is very detailed and comprehensive, since job information centers provide information media on hundreds of different occupations, vocational training and apprenticeships, higher education, job descriptions, earnings and employment prospects, and information about the local labor market.

3.3 Job Information Centers: Description, Survey Evidence, and Development over Time

This section provides an overview of the functioning and aims of job information centers in Germany, and summarizes key findings from various surveys conducted among visitors. It also discusses the development of information centers over time and across regions.

3.3.1 Description, Aims, and Institutional Background

A job information center (*Berufsinformationszentrum*) is a public establishment that provides detailed and comprehensive information on occupations, vocational training and apprenticeships, higher education, job tasks, earning prospects, and local labor market conditions. The goal of job information centers is to provide specific, up-to-date, detailed occupational information and, if required, counseling to facilitate individuals' labor market-related choices. The centers are designed to combine visitors' autonomous retrieval of information with assistance from professional job counselors (Jenschke, 1979c; Weitzel, 1988). Entry to JICs and counseling interviews are free of charge.

There are several ways visitors can access the resources to acquire the information they need. First, visitors can use various media, such as information folders, computers, career videos, books, slide shows and documentaries.[3] In a representative survey of around 10,000 visitors to job information centers in 1997, 90 percent report consulting information folders, 72 percent used computer programs, 62 percent books and 29 percent slide shows (Kretschmer and Perrey, 1998). Hence, the most important type of media are folders.[4] Each folder contains detailed descriptions of the work available, responsibilities involved, educational requirements, availability of training positions in the local labor market, and income and employment prospects for one particular occupation. For example, Figure A.2.3 in the Appendix displays a copy of the contents page of the information folder for the occupation of a salesperson in the food trade (*Verkäufer im Nahrungsmittelhandwerk*), copied from Weitzel (1987). The JICs supply a folder like this for almost every occupation.[5] Apart from the provision of stationary media, JICs also serve as venues for job choice-related events, such as seminars and talks by occupational counselors, educators, or trade organizations, training, and job fairs (Jenschke, 1979b; Lohmann, 1988; Weitzel,

[3]On average, visitors consult 3.2 different forms of media (Kretschmer and Perrey, 1998).

[4]Schweikert and Meissner (1984), Hermanns (1989) and Perrey (1995) also report that around nine out of ten young people read information in folders during their visit to a job information center.

[5]For example, Jenschke (1979b) points out that the very first job information center in Berlin had 260 information folders, covering more than 90 percent of all occupations, apprenticeships, and study degree programs on offer in Berlin at that time.

1988). Visitors can also take away reading and information material. Massute (1984) points out that over 50 percent of all visitors report collecting leaflets with descriptions of occupations to continue reading at home.

The primary target group of JICs are young people, mostly students in secondary school, who will soon enter the labor market (Massute, 1984; Weitzel, 1988). Typically, teachers take their classes to visit a JIC. But older adults, who would like to seek new professional opportunities or return to the labor market after an absence, are also welcome to retrieve the job market-related information at the centers. Around 70 percent of all visitors are students who visit a JIC with a school class and their (head) teacher (Kretschmer and Perrey, 1998). They typically spend a full school day there. At the start of the visit, a job counselor explains how the job information centers works, gives an overview of the media available, and invites students to come back to the JIC at any time in the future. After the introductory presentation, students are usually left on their own to autonomously retrieve any information they wish (Beinke, 1988). In most cases, students will already know the job counselor, since she or he normally visits the class in school prior to the trip to the job information center. Hence, education about occupations begins with a job counselor's visit to a school and through information provided by teachers. In fact, 80 percent of all visitors to JICs say that they first heard about the JIC in school (Massute, 1984).

The German Federal Employment Agency (*Bundesagentur für Arbeit, BA*) is responsible for the conceptual development, implementation, and running of the job information centers as publicly funded institutions. JICs are physically separated from other BA facilities, but often located in the same building or near BA job centers. This study makes use of the fact that the BA agreed with the Standing Conference of the Ministers of Education and Cultural Affairs (*Kultusministerkonferenz*) to make a school trip to a JIC an integral part of occupational education at school (Rahmenvereinbarung, 2004). Regional agreements with the regional school administrations stipulate that eligible students have to visit a JIC no later than two years before they leave secondary education. For instance, the agreement with the state of Bremen reads: "The minimum occupational counseling for each school grade is one session of occupational orientation at school and one at a job information center".[6] We chose the administrative unit of German districts (*Kreise*) as the geographical area to distinguish between *treated* and *untreated areas*. Using the level of Germany's 413 districts[7] for the analysis is superior to using the next higher level of the 16 federal states, since districts are of smaller size and better reflect local labor market areas. In contrast, using a lower level of 12,263 municipalities would lead to inaccuracies

[6] *Vereinbarung über die Zusammenarbeit von Schule und Berufsberatung zwischen der Senatorin für Bildung und Wissenschaft des Landes Bremen und der Regionaldirektion Niedersachsen-Bremen der Bundesagentur für Arbeit*, citation translated from German

[7] The number of geographical units reflects the status as of December 31, 2008.

as the catchment area of JICs stretches beyond municipal boundaries. Finally, from Jenschke (1979a) we know that, from the outset, schools and JICs were urged to cooperate in order to reach 100% coverage in hosting all students from the locality where the JIC was established.

3.3.2 Existing Survey Evidence

Over the years, several surveys have been conducted among visitors to job information centers. The most relevant ones are the representative surveys of all existing job information centers commissioned by the Federal Employment Agency. These surveys were conducted in the years 1988, 1991, 1992, 1994, and 1997 (Hermanns, 1989, 1992; Perrey, 1995; Kretschmer and Perrey, 1998). In addition, Massute (1984) interviewed 369 visitors to the job information center in the city of Hanover in August 1983, and Schweikert and Meissner (1984) conducted a survey of 3,032 respondents who visited the JIC in Berlin in 1980. The surveys include questions on age, gender, nationality, employment status, and various questions about the visit to the JIC (e.g., level of satisfaction, knowledge gained, usefulness of the resources, length of stay, number of visits). The key findings of these surveys can be summarized as follows:

1. **The majority of visitors are students.** Kretschmer and Perrey (1998) report that 69 percent of all visitors to JICs are students, with 57 percent of all visitors being aged 16 or younger, and 13 percent aged 17 to 18. Similarly, Massute (1984) reports that 71 percent of all visitors to JICs in the city of Hanover are 12 to 18 years old.

2. **Most students are attending low- or intermediate-track secondary schools.** The majority of students are attending low (*Hauptschule*) or intermediate-track secondary schools (*Realschule* or *Gesamtschule*) when they visit a job information center. For example, Kretschmer and Perrey (1998) report that 16 percent of the students attend low-track and 48 percent intermediate-track schools. Only around 26 percent attend upper-track schools (*Gymnasium*).[8]

3. **Positive spillover effects.** The study by Massute (1984) points to important positive spillover effects. He writes (translated into English): "But inferences about a possible multiplier effect can also be drawn. Visits with friends make up around 30 percent of all visits to job information centers. That is, in many cases, a visit to

[8] In line with these findings, Massute (1984) reports that around 80 percent of all visitors are students attending low- or intermediate-track schools when visiting a JIC.

a JIC with a school class is followed by one or more further visits with friends, for whom the information facility may be new" (Massute, 1984, 197).[9]

4. **Multiple visits and length of stay.** Schweikert and Meissner (1984) interviewed 1,021 students who visited the job information center in Berlin in 1980 together with their teacher and classmates during school hours, and 2,011 young people who visited the JIC in Berlin on their own initiative. Among those who came on their own (or together with a friend), 75 percent said that they had visited a job information center previously. On average, individuals visit the JIC twice. Overall, the authors report that 24 percent stated that they were visiting the JIC for the first, 36 percent for the second, and 40 percent responded that they were visiting a JIC for the third or more times (Schweikert and Meissner, 1984). On average, people spend between one and two hours at the information facility, with 29 percent of all visitors staying for two hours or more (Kretschmer and Perrey, 1998).

5. **Positive feedback from visitors.** The majority of visitors give positive feedback about their visit to a job information center. Schweikert and Meissner (1984) interviewed visitors about their level of satisfaction with the visit in general, and about their level of satisfaction with the resources and media used in particular. Among respondents who attended a talk about a particular occupation, 75 percent claimed that they learned new facts, and 60 percent described the talk as "easy to understand." In addition, 90 percent of all respondents gave a positive response to the question "Can you recommend the talk at the job information center?" (Schweikert and Meissner, 1984).[10] Similarly, 78 percent said they were "very satisfied" or "satisfied" with the visit (Kretschmer and Perrey, 1998). The authors also report that 97 percent of visitors were able to find the information they wanted. Table 3.1 gives an overview of the gains in knowledge among students who visited a JIC together with their school class. The table shows that−after visiting a job information center−over 60 percent of all students said that they knew more about many aspects of occupations they were interested in: the school degree generally required, how an apprenticeship is structured, the availability of trainee positions, and the manual and intellectual skills needed. However, the responses also indicate that only one in three visitors claimed to know more about earning prospects after their visit to a JIC.

[9]Similarly, Schweikert and Meissner (1984) report that 26 percent of all visitors who visited the JIC on their own learned about the job information center through friends.

[10]The positive evaluation of the media is very stable over time. Respondents to the representative surveys in the years 1988, 1991, 1994, and 1997 all evaluated the information folder as "good", on average (Kretschmer and Perrey, 1998).

6. **Visitors come from nearby.** There is also evidence that most visitors to job information centers come from the district or city where the JIC is located. For instance, Massute (1984) reports that almost all visitors to the job information center in the city of Hanover actually come from Hanover (92 percent).

Beside these stylized facts, many educators and occupational counselors argue that the establishment of job information centers considerably improved occupational education in Germany (Jenschke, 1979c) and cooperation between local schools and career counselors, and that students are very eager to acquire new information during their visits to job information centers. Lohmann (1988) writes: "Previous experiences with the existing occupational information centers show that the Federal Employment Agency has hit the mark with this service. The facility has been received extremely well by users of all ages and is highly valued by the professionals in career counseling, employment services, and job placement as an aid to their work" (Lohmann, 1988, 125-126). Jenschke (1979b) quotes from a report by a school class after visiting a JIC: "The class was interested throughout the entire time and would like to take advantage of the opportunity to gather further information there, either alone or with their parents, or to visit events there" (Jenschke, 1979b, 139).

3.3.3 Development of Job Information Centers over Time and across Regions

The Federal Employment Agency first contemplated the idea of permanently providing information materials about occupations and career paths outside existing job centers in 1970. In November 1976, the first JIC opened in Berlin. As it was widely considered to be a success (Siebert, 1979), in October 1979, the council of the BA decided to establish similar JICs throughout Germany. It was agreed that the JICs would first open in cities with over 500,000 inhabitants (including catchment area) and that there should be a regionally balanced distribution throughout Germany.[11] It was also decided that all JICs should have the same structure and equipment (Nieder, 1980). Beside these criteria, the timing of the establishment of job information centers seems to be as good as random. On June, 5, 2013, the authors of this paper had a meeting with the team leaders of the department "Berufsinformationszentrum (BIZ)" at the headquarters of the Federal Employment Agency in Nuremberg. The experts confirmed that the timing of the opening of the JICs was not influenced by external factors (e.g., local labor market conditions, local educational requirements etc.) and happened in a rather unsystematic, random fashion.

[11] Hirsch (1974) writes with respect to the first JICs in large cities that at least one job information center should open in each federal state.

It is also important to point out that the decision when to open a JIC in a particular area was not made by local politicians. Hence, it is unlikely that districts more in favor of investing in education, or more supportive of active labor market policies, are also more likely to open a JIC earlier than others. In fact, a key identification assumption of our difference-in-difference research design is the exogeneity of the timing of job information centers being opened. This issue will be dealt with in more detail in Section 3.7 below.

Figure 3.1 shows the evolution of job information center openings over time. Panel (a) displays the development of the number of JICs, and panel (b) plots the percentage of districts with a JIC. The figure shows that the establishment of JICs varies considerably over time. In particular, there was a steady and large increase in the number of JICs in West Germany between 1976 and 1990. After German reunification in 1990, JICs also opened in former East Germany. Overall, 181 job information centers were opened in 175 out of 413 districts.[12]

Figure 3.2 shows the regional distribution of job information centers at the district level over time. Shaded areas indicate districts with a JIC, and white areas districts without a JIC. Panel (a), for example, shows that in 1980, there are 13 districts with a job information center. By the year 2000, job information centers had been opened in 42 percent of all districts. It appears that the regional distribution is indeed balanced, although the West German Ruhr area seems to show a higher density of treated districts than elsewhere.[13] It can also be observed that small-area, urban districts are more frequently treated than sparsely populated, rural districts.

3.4 Data, Variables and Descriptive Statistics

3.4.1 Datasets

We use information from four different datasets. The primary data source is the ALWA study (*Arbeiten und Lernen im Wandel*, Working and Learning in a Changing World) provided by the Research Data Center of the German Federal Employment Agency (IAB) (Antoni et al., 2011). The ALWA dataset contains detailed longitudinal information on 10,404 randomly selected individuals who were interviewed in 2007 and 2008. Monthly spell data allow us to trace out the respondents' trajectories in the fields of education, training, employment, and unemployment, as well as their residential history. The survey also contains information on parental background and labor market outcomes elicited at

[12]Note that Figure 3.1 only displays the opening of 175 job information centers since we have no valid information on the opening date for four JICs and two JICs in Berlin were later closed.

[13]In the sensitivity analysis in section 3.8, we examine the robustness of the estimates when we exclude the Ruhr area from the sample.

the time of the interview. Most importantly, ALWA includes information on the administrative district of residence at any point in the respondents' lives. We make use of this information in order to assign each individual to a district either with or without a JIC while they are in secondary school.[14]

We make use of a version of ALWA that is complemented by administrative data from the social security records, forming the ALWA-ADIAB dataset (Antoni and Seth, 2011), as shown in Figure A.2.4 in the Appendix. These administrative records comprise daily spell data ranging back to 1975 for western Germany, and 1992 for eastern Germany. Further variables in the administrative data contain information on employment, gross daily pay, type of job, occupational and industrial classifications, and some employer characteristics. The survey and administrative data are linked through a matching algorithm and this is conditional on the respondent's consent. Out of all ALWA respondents, 8,166 individuals (78%) are successfully linked with the administrative data.

The administrative data ADIAB is a subsample of a much larger dataset called the Sample of Integrated Labor Market Biographies (SIAB) (Dorner et al., 2010) that is unrestricted to successful matching with ALWA. The SIAB, in turn, is a two percent random sample drawn from the Integrated Employment Biographies (IEB). The IEB consist of all individuals known to the German social insurance agency, including those who are receiving unemployment benefit or are searching for a job (excluding the self-employed and civil servants). The observations go back to 1975 (for West/western Germany) and 1992 (for eastern Germany) and are reported on the district level. The SIAB comprises approximately 40.5 million daily spell entries for 1.66 million individuals. We use this remarkably large dataset to assess the common trend assumptions and to generate district-level variables for the survival analysis regressions in Section 3.7. We match the ALWA and ALWA-ADIAB data with self-collected data on the location and opening dates of the job information centers. Moreover, we merge in data on the the population density (on the district level) from the German Federal Statistical Office.[15]

3.4.2 Outcome Measures and Treatment Variable

This section describes the definition of the various outcome measures and the treatment variable. An overview and description of the variables can be found in Table A.2.1 in the Appendix. We categorize the outcome variables under the following four topics: education and educational mobility, labor market attachment, job search and job matching, and

[14] In the dataset available to researchers, the code of the district of residence is anonymized. Fortunately, the IAB agreed to run our Stata program on the original dataset, thereby generating our treatment variable. We are very grateful for this assistance.

[15] We use the population size of the year 1995 as earlier records are not available at the district level.

wages and income.

Education and educational mobility. In Germany, there are three main types of secondary schooling which may be described approximately as: low-track school (*Hauptschule*), intermediate-track school (*Realschule*) and grammar school (*Gymnasium*). *Hauptschule* is the lowest level of secondary schooling and ends after a minimum of nine years of schooling, at the age of 15 or 16, with the qualification *Hauptschulabschluss*. Students who successfully finish the intermediate school track after ten years of formal schooling receive the school certificate *Mittlere Reife*. Students from the low- and intermediate-track schools normally proceed to vocational training and begin an apprenticeship or move on to the next highest school track. The grammar school is the most prestigious and academic school track. It ends after 12 or 13 years of schooling with the *Abitur* certificate, which is the highest secondary-school qualification and enables individuals to enter technical colleges and universities.[16]

The educational outcome measures are defined as follows. The dichotomous variable *low-track school degree* means having completed the general school track. *Intermediate-track school degree* is also a dichotomous outcome variable equal to one if the individual completed ten years of schooling and received *Mittlere Reife*, and zero otherwise. The dichotomous variable *upper-track school degree* equals one if the individual received an *Abitur*, and zero otherwise. The three outcome measures low-, intermediate- and upper-track school degree are mutually exclusive and always measure the highest general school degree attained. Our fourth outcome, *upward mobility*, is equal to one if an individual experienced upward educational mobility, and zero otherwise.[17] Finally, the educational outcome *university degree* is a dichotomous variable equal to one if an individual has a technical college or university degree, and zero otherwise.

Labor market attachment. To capture labor market attachment at the beginning of individuals' careers, we measure whether they experience part-time employment, full-time employment or unemployment during the first five years after completing their education. For example, the variable *part-time employment* equals one if an individual works part-time for at least one month during the first five years after finishing the last episode of formal education, be it general schooling, vocational training, or higher education. Similarly, the dichotomous outcome variable *full-time employment* (*unemployment*) is equal to one if an individual works full-time (experienced unemployment) for at least one month during the

[16] For a more detailed description of the German school system, see, for example, Winkelmann (1996), Dustmann (2004) and Francesconi et al. (2010).

[17] For example, if an individual attended a low-track school (*Hauptschule*) in the year of potentially visiting a JIC and later completed his or her general schooling with a *Mittlere Reife* or an *Abitur*, we define this person as experiencing upward educational mobility. Another possibility of upward mobility is attending an intermediate-track school initially and later leaving school with the *Abitur* certificate.

first five years after completing formal education, and zero otherwise. We not only measure employment incidences, but also the time spent by individuals' in part-time employment, full-time employment and unemployment, measured in months. For instance, the variable *unemployment duration* measures the number of months individuals are unemployed in the first five years after entering the labor market. Note that the duration variables are restricted to values between 0 and 60.

Job search and job matching. The variable *search duration* measures the number of months individuals search for employment after completing vocational training or higher education. It is coded zero when the transition into a job immediately follows education, or when the education spell is still ongoing when entering regular employment. As proxy variables for job match quality, three different outcomes were generated. First, it was measured whether, at the time of the interview in 2007 or 2008, individuals still lived in the same district (or federal state) as when they were young, i.e., at the age of potential visiting a JIC. The basic idea of these two geographic outcome measures is that if the availability of and visits to JICs improves matching between employers and employees in the local labor market, the need to move away is lower than without the proximity of a job information center. Hence, if a higher proportion of individuals staying in the local area is observed when there was a local job information center during their youth, this is interpreted as indirect evidence of a better job matching. Our final proxy variable for job match quality measures the *share of involuntary job changes* in the first five years after completing formal education. We make use of an item in the ALWA questionnaire that asks for the reason for the end of an employment spell. Respondents can report whether employment was terminated by themselves, by the employer, or whether the contract ended officially according to prior agreement. For the first five years of individuals' labor market careers, we count the number of times they report losing a job involuntarily and relate this to the total number of times each individual changes employers. Thus, the outcome variable *share involuntary job change* takes on values between 0 and 1.

Wages and income. We use the administrative data from ALWA-ADIAB to measure daily pay in an individual's first job and at the age of 35.[18] The pay at the age of 35 is simply the average daily salary in the year an individual turns 35 years old. Hence, individuals whom we cannot observe at the age of 35 are excluded from the regression.[19]

[18]The wage of the first job is taken from the first observable monetary compensation from regular employment observed in the administrative part of the data. However, the first spell in the ADIAB may be subject to left censoring and may follow earlier spells of unreported self-employment or civil service. To make sure it really is the wage of the first job, we compare the spell's year to the year, for which the individuals report their first regular employment in the questionnaire. We only consider observations, for which the difference between these two dates is not more than $\pm$ 1 year.

[19]Individuals with fewer than 180 employment days in the respective year are considered to have instable jobs and are also excluded.

Finally, survey respondents were interviewed about their pay in their last job and their net income at the time of the survey in 2007/2008. The income and wage measures are in logarithms and are trimmed at the 1st and 99th quantiles.

Treatment effect—Job information center. Selection into treatment is based on the availability of a JIC in the administrative district where the individual went to school. Since no variable in the survey directly informs about the availability of a JIC during an individual's youth, we use monthly spell information on school track, school degree, the district of residence, and the location and opening dates of job information centers. We know that schools send their students to JICs in different grades according to the school track attended, normally two to three years before they leave school (Schweikert and Meissner, 1984, and Section 3.3 above). Hence, students from the same cohort are typically treated at different ages if they attend different school tracks. Relative to the year of graduation from school, we calculate a potential treatment year, t.[20] Table A.2.2 in the Appendix gives an overview of how we define t for different combinations of school tracks and school degrees. Typically, the school type defines the type of qualification (e.g., intermediate-track school leads to *Mittlere Reife* and a grammar school leads to *Abitur*). Table A.2.2 shows that, for instance, students who attend an intermediate-track school and successfully receive an intermediate-track school degree are defined as being treated two years before leaving school. In real life, the accordance between school track and school degree often holds, but not always. For example, a student may drop out of grammar school after 11 years with a *Mittere Reife*, which is a lower qualification than an *Abitur*. Different reasoning applies to this constellation. The early drop-out is treated closer to graduation because the teachers still take their students to the JICs two or three years prior to *typical* graduation. The early drop-out from a grammar school leaves two years earlier than his classmates. He or she is therefore treated $3 - 2 = 1$ year before the end of his or her spell.

Using potential treatment years relative to the year of graduation has consequences for the educational composition of the treatment and control group. To illustrate this, consider a district which hosts a job information center. Assume that this district has only two residents, both born in the same year. One attends the lowest school track, and the other the highest. Assume further that a JIC openes when both are aged 16. Now, using the treatment rule relative to the time of graduation, only the higher educated individual is treated, while the lower educated is not (the typical graduation age in the lower track is 15 or 16). Hence, treatment status depends on the age and school track attended. Therefore, in the regressions below, we always control for the school type attended when individuals

[20] We call it *potential* because we yet have to determine whether a JIC was actually available in the district of residence or not.

are potentially treated and dummy variables for the year of birth.

3.4.3 Sample Selection and Descriptive Statistics

The final estimation sample consists of all individuals in the ALWA dataset for whom the treatment variable and the outcome measures can be derived and who were born between 1960 and 1982. The 1960 birth cohort is the earliest that might have been treated by visiting the first JIC in the city of Berlin after it opened in 1976, given the typical graduation age in the lowest school track. Individuals born after 1982 would be too young for meaningful outcome measures.

Three different samples are distinguished. The *Full sample* includes all individuals for whom we have valid information for the key variables. The *Reduced sample I* comprises all individuals who attended either the low- or intermediate-track schools at the time of potentially visiting a job information center. These two samples are distinguished because educators and occupational counselors argue that the information media at JICs are more beneficial and comprehensive for students in low- and intermediate-track schools than for those in the upper-track schools (Massute, 1984). Moreover, the majority of students visiting a JIC attend a low- or intermedium-track school (Perrey, 1995; Kretschmer and Perrey, 1998). For example, Schweikert and Meissner (1984) report that only nine percent of all students who attended talks about particular occupations were at grammar school. Hence, the magnitude of the treatment effects is likely to be stronger among individuals from the low- or intermediate-track schools. Finally, the *Reduced sample II* comprises only individuals who attended either low- or intermediate-track schools and, at the time of potential treatment, also lived in one of the 175 districts where a JIC opened between 1976 and 2010. This implies that all individuals who grow up in a district where no JIC ever opened are excluded from the *Reduced sample II*. It therefore captures the within dimension of the variation. The main reason for including this third sample is that some individuals who were defined as part of the comparison group in the *Full sample* and *Reduced Sample I* (i.e., individuals living in districts without a JIC at the time of potential treatment) might actually have visited a JIC in another district. Hence, if positive treatment effects of visiting a job information center are found for educational choices and labor market outcomes in the *Full Sample* and the *Reduced Sample I*, the estimates might be lower bounds, since some individuals in the comparison group might also have been treated. The key advantage of the *Reduced Sample II* is that the definition of the comparison group is likely to be "cleaner". This is because the comparison group only consists of individuals from districts where a JIC eventually opened, but was not in existence when they went to school.

Table 3.2 presents summary statistics for the key variables by sample and treatment status. For each variable and sample, we also present p-values for treatment and control differences. The upper panel in Table 3.2 reports the summary statistics for the outcome measures, and the lower panel for selected explanatory variables. There are significant differences in the unconditional means for many outcome variables between treated and non-treated individuals. For example, among those who grew up in a district with a JIC, a lower proportion completed their schooling by obtaining the lowest school degree, compared to those who grew up in a district without a JIC. The difference of five to seven percentage points is statistically significant at the one percent level in all three samples. In addition, a larger proportion of the individuals treated received the highest school degree and experienced educational upward mobility. However, the unconditional means also point toward a higher likelihood of experiencing unemployment and a slightly longer unemployment duration (0.5 to 1.2 months), on average, among treated individuals during their first five years after entering the labor market. In Section 3.3 above, it was pointed out that one resolution of the council of the Federal Employment Agency was that JICs should open in large cities first. In line with this decision, the mean comparisons for the explanatory variable population density at the bottom of the table shows a significantly higher population density among the treatment group than the comparison group. Moreover, there is a higher proportion of individuals with a migrant background or with a higher socio-economic background (e.g., mother or father has an upper-track school degree) among treated than non-treated individuals.

3.5 Estimation Method

To estimate the effect of the availability of job information centers on individuals' educational choices and long-term labor market outcomes, their openings are treated as a quasi-natural experiment in a difference-in-differences setup, thus making use of both regional and time variation in the availability of JICs. The estimation equation takes on the form

$$y_{idt} = \alpha + \beta JIC_{idt} + \sum \lambda_{district} + \sum \phi_{birth} + \sum \delta_{track} + X_i\gamma + X_d\pi + \varepsilon_i, \qquad (3.1)$$

where y_{idt} is one of the outcome variables for individual i who lived in district d in the year of the potential treatment t. JIC_{idt} is an indicator variable for JIC availability equal to one if district d at time t had a job information center, and zero otherwise. $\sum \lambda_{district}$ captures 413 district fixed effects, $\sum \phi_{birth}$ captures 22 birth cohort fixed effects, $\sum \delta_{track}$

are fixed effects of the school track attended at the time of potential treatment, and the vector X_i includes dummies for gender, migrant background, and parents' education. In addition, the vector X_d consists of dummy variables capturing differences in population density between districts. This setup makes it possible to control for effects specific to treated and untreated districts (captured by $\lambda_{district}$) and aggregate macro trends common to all districts (captured by ϕ_{birth}). The standard errors are clustered on the district level to adjust for district-specific elements in the error term.

The key parameter of interest is β. For identification, it must be ruled out that the estimates are driven by district-time-specific effects, such as reforms or regional shocks that happened at the same time and in the same regions the JICs opened. In other words, the common trend assumption must hold (cf. Section 3.7 for evidence on this presupposition). A more precise formulation of the identifying assumption is given by the conditional independence assumption. It states that, conditional on all covariates $\mathbf{Z} = [\sum \lambda_{district}, \sum \phi_{birth}, \sum \delta_{track}, X_i, X_d]$, the potential outcomes with and without treatment, y_1 and y_0, respectively, are independent of selection into treatment:

$$y_1, y_0 \perp T | \mathbf{Z} \tag{3.2}$$

The implication of (3.2) is that visiting a JIC must not depend on outcomes, after controlling for the variation in outcomes induced by differences in $\mathbf{Z}$. If valid, this establishes a population-average treatment effect on the treated (ATT) for β. As we do not observe who among the eligible students actually visited a JIC, the ATT reduces to an average intention to treat effect. We argue that, conditional on a rich specification of district and time fixed effects as well as individual and district characteristics, the variation in the availability of JICs over time and within districts is exogenous to individuals' unobserved characteristics, such as ability, intelligence, or motivation. It is also exogenous to district-specific characteristics and common macro trends.

3.6 Results

Tables 3.3 to 3.6 report the OLS results from estimating equation (3.1) for different dependent variables and samples. Table 3.3 displays the estimates for the outcomes on education and educational mobility, Table 3.4 for labor market attachment, and Table 3.5 shows the results for job search and job matching. The estimates from wage and income regressions are reported in Table 3.6. Due to the high number of outcome and control variables, we only report β and its standard error along with the adjusted R^2 and the sample size.[21] In

[21] More detailed results are shown in Table A.2.3 in the Appendix.

all tables, separate results are reported for the three different samples (panels A to C).

Table 3.3 reports the first set of results from linear probability models.[22] Columns 1 to 3 show how being exposed to a job information center influences the likelihood of obtaining a certain school degree, given the school track enrolled in at potential treatment time t. Column 4 presents the estimates on the probability of experiencing educational upward mobility, and column 5 contains the results on the likelihood of obtaining a technical college or university degree. The estimates of panel A in Table 3.3 show that there is no significant effect of visiting a JIC on the likelihood of obtaining a particular school or university degree in the full sample. However, there is a positive and significant effect of visiting a JIC on the likelihood of experiencing educational upward mobility by three percentage points.

Panels B and C in Table 3.3 contain the estimated results from similar specifications for individuals who attended a low- or intermediate-track school. In both panels, JIC availability decreases the likelihood of obtaining an intermediate-track school degree (*Realschulabschluss*) and increases the likelihood of a higher school qualification (*Abitur*). For example, a person who grows up in a district with a JIC when attending school is between 7 and 12 percentage points more likely to obtain the highest general school degree. The size of the estimated coefficients indicates that, ceteris paribus, being exposed to the job information program leads to the same increase in the likelihood of obtaining the highest general school degree as when the father has the highest school degree (*Abitur*) compared to a father with no school qualification. Consistent with this finding, the results in panels B and C point toward a positive and statistically significant effect on the likelihood of experiencing educational upward mobility, and the chance of obtaining a university degree. The point estimates are also in line with descriptive survey evidence among visitors to a JIC in Berlin. Schweikert and Meissner (1984) report that 21 percent of students who are attending a low-track school at the time of visiting a JIC report aiming to acquire an intermediate-track school degree (*Mittlere Reife*) and one percent aim to obtain the highest general school degree.

In sum, the results in Table 3.3 indicate a positive effect of the presence of a job information center on the likelihood of experiencing educational upward mobility, of obtaining the highest general school degree, and of successfully completing a technical college or university degree. The estimates are important from a policy perspective because it is precisely for students in low- and intermediate-track schools that educators and job counselors expect the highest potential from visiting a JIC. Students in the highest school track

[22] In unreported regressions, we also estimated logit models for all dichotomous outcome measures. The marginal effects from these models are very similar to the present OLS coefficients and are available from the authors upon request.

may already have acquired a wealth of information and knowledge from their parents or other media prior to visiting a JIC.

Next, labor market outcomes are studied. Table 3.4 reports estimates on the incidence and duration of part-time employment, full-time employment, and unemployment during the first five years after ending formal education. The most salient result is that being treated decreases the chances of experiencing unemployment by about eight (panels A and C) or ten percentage points (panel B), but these effects are imprecisely estimated in all three samples. Furthermore, the results in column 5 suggest that JIC treatment increases the duration of full-time work by around 2.8 months (panel A). This effect is significantly different from zero at the five percent level. In sum, the results in Table 3.4 only suggest small differences in labor market attachment in the first five years of the individuals' labor market career.

Table 3.5 provides information about whether a visit to job information centers positively impacts on young adults' job search success and the quality of their job choice. Search duration in column 1 measures the time in months that passed between completing the last episode of education and beginning the first episode of regular employment. The geographic mobility measures in columns 2 and 3 of Table 3.5 are dichotomous outcome variables indicating whether, at the time of the interview, an individual still lives in the district or state where he or she lived while going to school. The outcome variable *share involuntary job change* in the last column measures the proportion of involuntary job losses in the first five years of the labor market career.

The magnitude of the estimated coefficients in column 1 of Table 3.5 suggest that individuals who visit a JIC spend around one to two months less searching for their first job. Note, however, that none of the estimates is different from zero at conventional significance levels. In contrast, the estimates for the likelihood of staying in the same district are precisely estimated in panels A and B. The results from the linear probability models indicate that individuals who visit a JIC while young have a seven to eight percentage points higher likelihood of still living in the same district when they are adults. We interpret this as a strong effect, given that visiting a job information center positively influences individuals' education and recent research also points to a causal positive impact of education on regional labor mobility (Machin et al., 2012). Assuming that most individuals have a preference for staying in close geographic proximity to where they grow up, one possible interpretation of these results is that treated individuals experience less pressure to look for employment in distant places, since JICs help improve the quality of the matching between employers and employees in the local labor market. The estimates in column 4 of Table 3.5 are also consistent with an improvement in job matching. They indicate that individuals' risk of losing their job involuntary decreases significantly if they visit a job information center during their youth. The effect is strongest for individuals who attended a low- or intermediate-track school at the time of treatment.

Finally, Table 3.6 reports results for wage and income measures. The outcome variables in columns 1 and 2 come from the administrative data, and those in columns 3 and 4 from the survey. All outcomes in Table 3.6 are measured in logarithms. In addition to the control variables in the regressions in Tables 3.3 to 3.5, we add some further explanatory variables (e.g., a dummy variable for part-time employment and a maximum set of district of residence dummy variables at the time of measuring the outcome variable).[23] Overall, the results in Table 3.6 do not suggest that visiting a job information center increases individuals' short- and long-term wages and income. For example, all estimated coefficients in panel A have a negative sign and are close to zero. Similarly, none of the estimates in panel B are positive and statistically significant from zero at conventional levels. The only estimates pointing toward positive returns from visiting a job information center are those in panel C of Table 3.6. However, the magnitude of the estimated effects are not very plausible and only one coefficient is significant at the five percent level. Overall, we can conclude from Table 3.6 that there is no convincing evidence of positive wage returns resulting from the occupational information provided by JICs in Germany.

3.7 Timing of the Opening of Job Information Centers and Common Trend Assumptions

The validity of our estimation methods requires exogeneity of the timing of the opening of job information centers. Moreover, the difference-in-differences strategy only allows an average treatment interpretation if the common trend assumption holds. We now shed further light on the validity of these identification assumptions.

In order to understand the timing of the opening of JICs, we estimated discrete-time logistic hazard models on the district-year level using rich administrative data from the SIAB. The enormous number of observations (40.5 million spells for 1.66 million individuals) makes it possible to calculate reliable district- and year-specific averages for the variables of interest. Our analysis focuses on the opening of job information centers and we treat spells (district-years) where no JIC was opened as right-censored. The sample consists of around 7,000 district-year observations, of which around 40 percent end with the opening of a JIC.

In our first model, we measure all district-level variables in the year 1975, on the eve

[23]In columns 1 and 2 of Table 3.6, we include a full set of district dummies at the beginning of the first job and at the age of 35, respectively. In column 3, the district dummies are measured at the last spell of employment. In column 4, the district dummies are measured at the time of the interview.

of the opening of the first JIC in Berlin. For eastern Germany, we use the year 1992, the first year for which we have valid district-level information available. In the second model, we control for a rich set of lagged time-varying covariates. Finally, in the third model, we investigate potential correlations between changes in explanatory variables over time (e.g., percentage change between $t-2$ and $t-1$) and the timing of the opening of job information centers.

The results are presented in Table 3.7. Two different specifications are presented for each model. In the first specification, we control for the log population size and the log area size of the districts, average wages, the proportions of the population with a certain school qualification, and the unemployment rate. In the second specification, various labor market characteristics for young individuals aged 25 to 30 were also included to capture differences in local labor markets across districts (and over time). Overall, the estimates show that there is no systematic and statistically significant relationship between the timing of the opening of job information centers and average wages, various educational levels, and the unemployment rate. Further, none of the local labor market characteristics are significantly related to the hazard of opening a JIC. The only explanatory variables that are statistically significantly related to the hazard of opening a JIC at the one percent level are the population and the size of the district. The estimates suggest that districts with a larger population have a higher hazard, and districts with a larger surface area, i.e., rural areas, have a lower hazard of opening a JIC. These results are in line with the decision by the Federal Employment Agency to open JICs in urban areas first. Overall, we interpret the results in Table 3.7 as strong supportive evidence for the identification strategy that the variation in the timing of the opening is unlikely to be endogenous. However, it should be pointed out that unobservable regional shocks coinciding with the timing of the opening could still bias the estimates.

Next, comprehensive graphical evidence on the common trend assumption are presented in Figures 3.3 to 3.6. The common trend assumption states that, in the absence of treatment, the outcome variables would have parallel trends for the treatment and control group. This implies that in the years before the introduction of a job information center, districts that were eventually treated and districts that were never treated must display the same trend in the average values of the outcome variables. However, since the JICs opened at different points in time, there is no single cut-off point to separate pre-treatment and post-treatment years. The further we move along the time axis in, for example, Figure 3.3, the fewer districts, in which a JIC opened at some later stage, actually remain untreated at the observation time. Conversely, moving backwards on the time axis increases the number of yet untreated districts in the treatment group. The calculation of the averages becomes more precise, but as the number of pre-treatment years decreases, the graph becomes less informative. Figures 3.3 to 3.6 deal with this trade-off by comparing pre-treatment trends using three different cut-off years (1980, 1985, and 1990, as indicated by the vertical lines). For the year 1980, for example, the solid red line shows the pre-treatment trend in outcome variables for all 130 districts ($c = 130$) where a job information center opened in 1980 or later. Analogously, the solid red line for the year 1990 represents the pre-treatment trend for the smaller sample of 41 districts ($c = 41$) that opened a JIC in 1990 or later. The blue solid line always shows the trend in the average outcomes among all districts in which a JIC never opened.

Ideally, Figures 3.3 to 3.6 would present graphs for all outcome variables. However, while its sample size is large, the SIAB only contains a limited number of variables. It allows us to draw graphs of common trends for the school and university qualification measures, labor market behavior at the beginning of the employment career (part-time employment, full-time employment and unemployment experiences at ages 25 to 30), unemployment rates, and wages. For example, panels (a)-(c) in Figure 3.3 show the development in the proportion of individuals with a low- or intermediate-track school degree between 1975 and 2008. The figure shows the pre-treatment trends (solid line) and post-treatment trends (dotted line) for the treatment group (red) and control group (blue) for the cut-off years 1980, 1985, and 1990, respectively. Figure 3.3 shows the trends for the educational variables, and Figure 3.4 for the variables duration in part-time employment, full-time employment, and unemployment (in months) at ages 25 to 30. Figure 3.5 displays the trends for the unemployment rate and youth unemployment rate (ages 20 to 25), and Figure 3.6 shows the development of average wages separately for the treatment and comparison group.[24]

Overall, the trends in the outcome variables between treatment and comparison group are quite similar. In particular, Figures 3.5 and 3.6 make the case not only for the common trend assumption (same trend) but also for same levels. For example, all panels in Figure 3.5 show an almost identical development of the overall unemployment rate and the youth unemployment rate over time in treated and untreated districts before and after the treatment. Therefore, it appears that the unemployment rate was not a driving factor in policy makers' decision where and when to open job information centers. In sum, we argue that the common trend assumption is likely to hold.

[24]Note that the vertical lines are more informative for displaying pre-treatment rather than post-treatment levels. This is because they indicate the first year in which districts of the treatment group potentially could have been treated. As we move from the vertical line to the right, however, the number of treated districts increases only gradually. Hence, the first observations to the right of the dotted red line are characterized by a relative low treatment intensity. Moreover, the vertical lines are mainly informative for displaying trends on the county, rather than on the individual level, because there might be a time gap between treatment and realization of the outcome.

3.8 Sensitivity Checks

In Table 3.8, we explore the sensitivity of the results. For the sake of brevity, we only report estimates for selected outcome variables based on the *Restricted Sample I*. We begin by investigating whether the key results hold for important subgroups in the population, i.e., western Germany, rural areas, and early cohorts. Further, we examine whether there are heterogeneous effects by gender.

Panel A of Table 3.8 presents results for individuals living in western Germany. Overall, the estimates are quite similar to those in Tables 3.3 to 3.6. For instance, the estimate for the outcome *upward mobility* in Table 3.3, panel B, suggests that growing up in a district with a job information center increases the probability of experiencing upward educational mobility by 7.5 percentage points, compared to 8.3 percentage points in panel A, Table 3.8. One notable exception is the finding for the outcome *low-track school degree*. In the sample for western Germany, the point estimates suggest a statistically significant reduction in the likelihood of leaving school with the lowest school degree by eight percentage points. The corresponding estimate in the overall sample is -0.005.[25]

It was mentioned in Section 3.3 above that one of the agreements of the Federal Employment Agency, when deciding to establish JICs, was that they should first open in large cities. Hence, there is the risk that our estimates might be driven by differential time trends between urban and rural areas. To examine this, panel B in Table 3.8 presents estimates excluding urban districts from the regressions.[26] This decreases the sample size by around 30 to 35 percent. However, the sign and magnitude of the estimates in panel B is in line with the corresponding results in Tables 3.3 to 3.6. This suggests that the findings are unlikely to be driven by differential unobserved trends between rural and urban areas.

Figure 2 shows that the timing of the opening of job information centers was geographically balanced, with the exception that a JIC opened in almost all districts in the Ruhr area. Hence, we investigate whether some of the effects differ once we exclude districts in the Ruhr area. The estimates in panel C of Table 3.8 do not change much as a result of this sample restriction, suggesting that the findings are unlikely to be driven by unobserved local influences in that region.[27]

[25]In unreported regressions, we also estimated the regressions only for individuals with German nationality. The estimates were very similar to those in panel A of Table 3.8. Note that we do not present separate regressions for eastern Germany because of small sample sizes.

[26]The data allows us to distinguish between the following types of administrative districts: (1) rural districts (*Landkreise*); (2) districts (*Kreise*); (3) Free Hanseatic City (*Freie Hansestadt*), (4) urban municipalities (*kreisfreie Städte*); and (5) city boroughs (*Stadtkreise und Stadtverbände*). In the present sample, we exclude district types (3) to (5).

[27]The estimates are also unaffected if we exclude both urban districts and the districts in the Ruhr area.

Job information centers in Germany opened over a long period of time, spanning more than 20 years. Clearly, one very important innovation in these two decades was the introduction and dispersion of the Internet. Hence, visiting a JIC might be less beneficial or important for educational and occupational choices if young people were able to do Internet research. We therefore examine the impact for early cohorts, who made their educational and occupational decisions when access to the Internet was very unlikely or restricted (i.e., those born between 1960 and 1975). The results in panel D in Table 3.8 show that most of the effects are indeed larger in magnitude and significance among the older cohorts. For example, the estimate on the outcome variable *university degree* suggests that the likelihood of obtaining a technical college or university degree increased by 12 percentage points among those born between 1960 and 1975, compared to seven percentage points in Table 3.3.[28]

Next, we explore the sensitivity of the estimates to various econometric specifications. In panel E in Table 3.8 we also control for an additional linear time trend. This does not change the estimates considerably. Panel F reports estimates when we restrict the sample to individuals for whom the potential treatment years are within a range of 10 years around the opening of a JIC and also control for decade-state interaction terms. Again, the results are in line with the previous findings in Tables 3.3 to 3.6.

The key identification assumption embodied in the estimation is that there were no other changes at the time JICs opened, relative to the areas where there were no JICs, that also influenced individuals' educational choices and labor market outcomes. In panel G, we test for the identification assumption with a falsification exercise. We estimate placebo regressions by moving the year of the opening of JICs forward six years. If our identification strategy is valid, then the measure for the availability of a JIC should have no impact on the various outcomes, since the individuals already made the educational choice many years before the JIC opened. Indeed, none of the point estimates in panel G of Table 3.8 is different from zero at conventional significance levels. Moreover, the magnitude of most estimates is considerably lower compared to those in Tables 3.3 to 3.6.

In sum, the various robustness exercises confirm that the opening and availability of job information centers impacts on individuals' educational choices and influences their labor market outcomes to some extent. The results are robust to various sample selections and different econometric specifications.

[28]In unreported regressions, we also estimated the regressions for individuals born between 1975 and 1982. None of the point estimates were precisely estimated for these younger cohorts.

3.9 Conclusions

In 1976, the German Federal Employment Agency opened its first job information center in West Berlin. Since then, 181 job information centers have opened throughout Germany. This paper quantifies the effects of the public and free provision of occupational knowledge through JICs on young adults' educational choices and labor market outcomes. More specifically, it assesses the argument of educators and policy makers that the provision of information on job types, employment prospects, earning profiles, and educational pathways to various occupations improves the quality of educational and labor market choices.

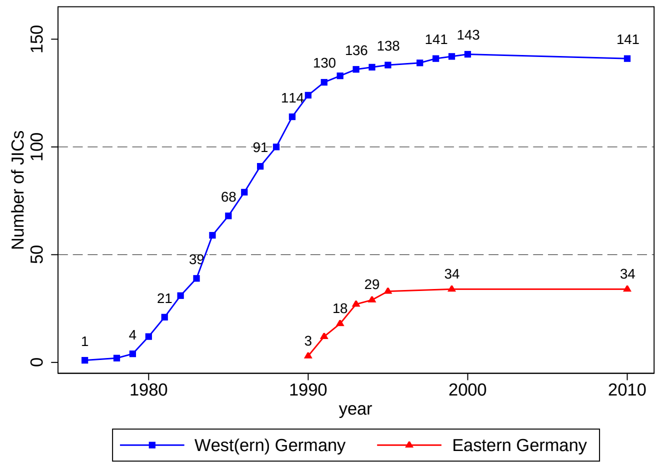
We exploit the variation across time and regions of the opening of job information centers in Germany using a difference-in-differences strategy. Our estimates suggest that individuals, who went to a low- or intermediate-track school when a JIC was available in their district of residence, have a significantly higher probability of obtaining the highest school degree, and of experiencing upward educational mobility. Those who grow up in an administrative district with a JIC are also less likely to become unemployed, or to involuntarily lose their job in the first five years after entering the labor market. Moreover, as adults, they have a higher likelihood of still living in the same district and federal state where they lived when at school. However, no empirical evidence was found that the information program significantly increased individuals' wages and income.

Our findings point toward improved welfare gains in the short-run, since people's upward educational mobility has been increased. However, the long-term impact of the information program is rather mixed. There is some evidence of improved job match quality, but no positive impact on wages and income. Overall, the results reveal the importance of policies that promote occupational information for young people's educational choices. As countries such as Finland and Luxembourg are considering implementing similar information programs—and Australia, Austria, and Switzerland also opened job information centers in the last two decades (Hirsch, 1974; Gödl, 1986; Nowak, 1996)—our findings provide useful insights that might guide future educational and vocational policy decisions.

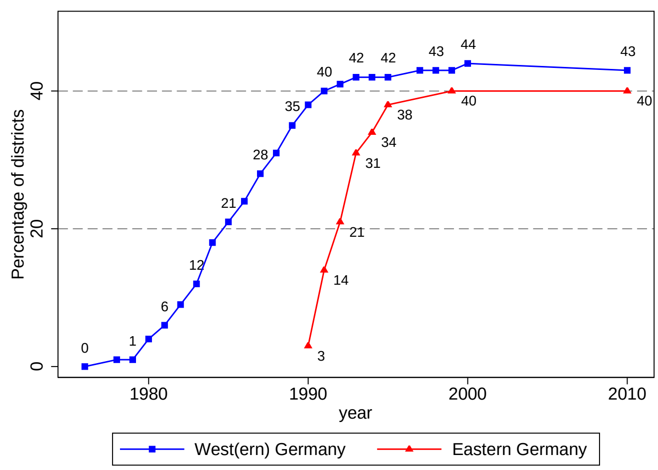
Figures and Tables

Figure 3.1: Development of Job Information Centers in West(ern) and Eastern Germany, by Year

(a) Number of JICs

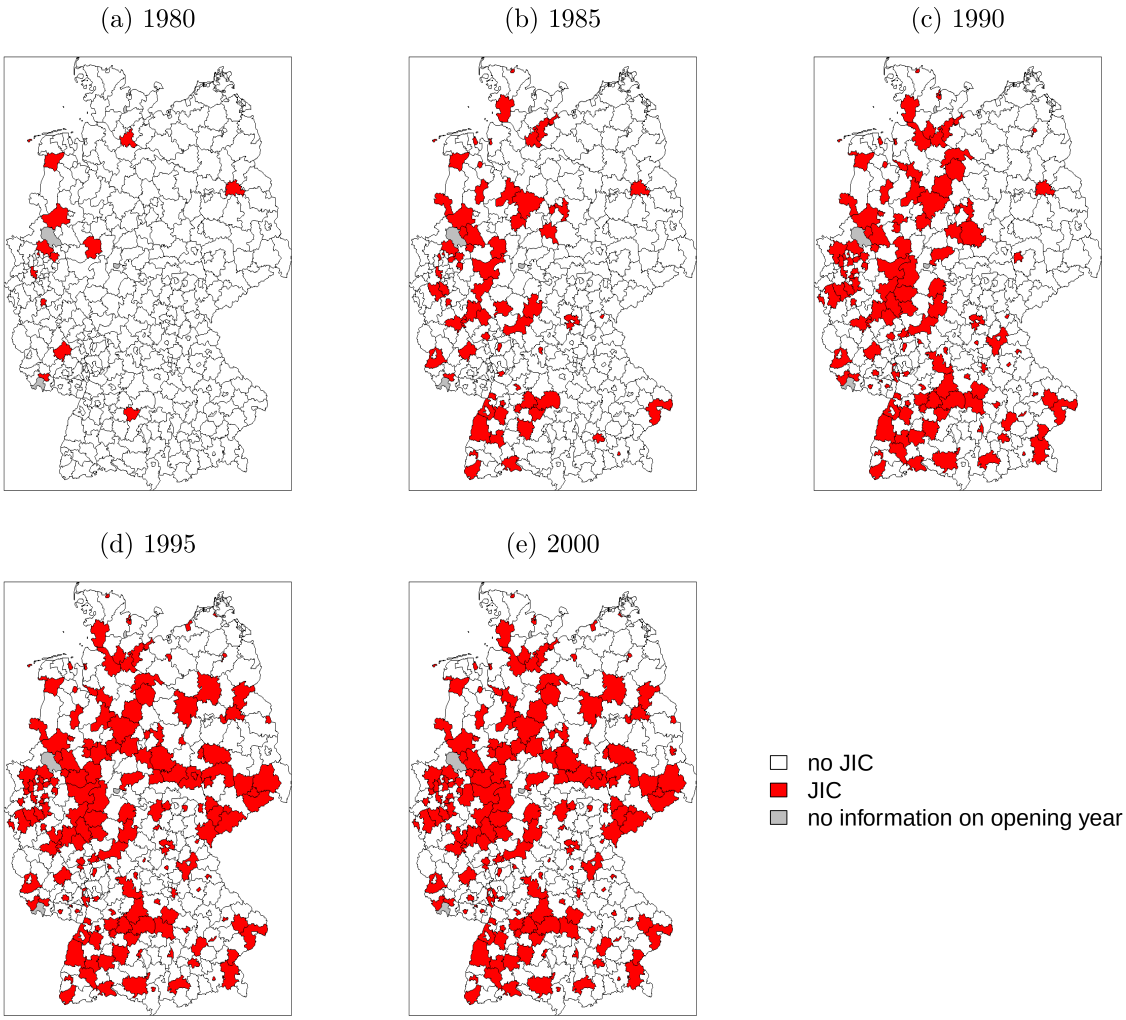


(b) Percentage of districts with a JIC



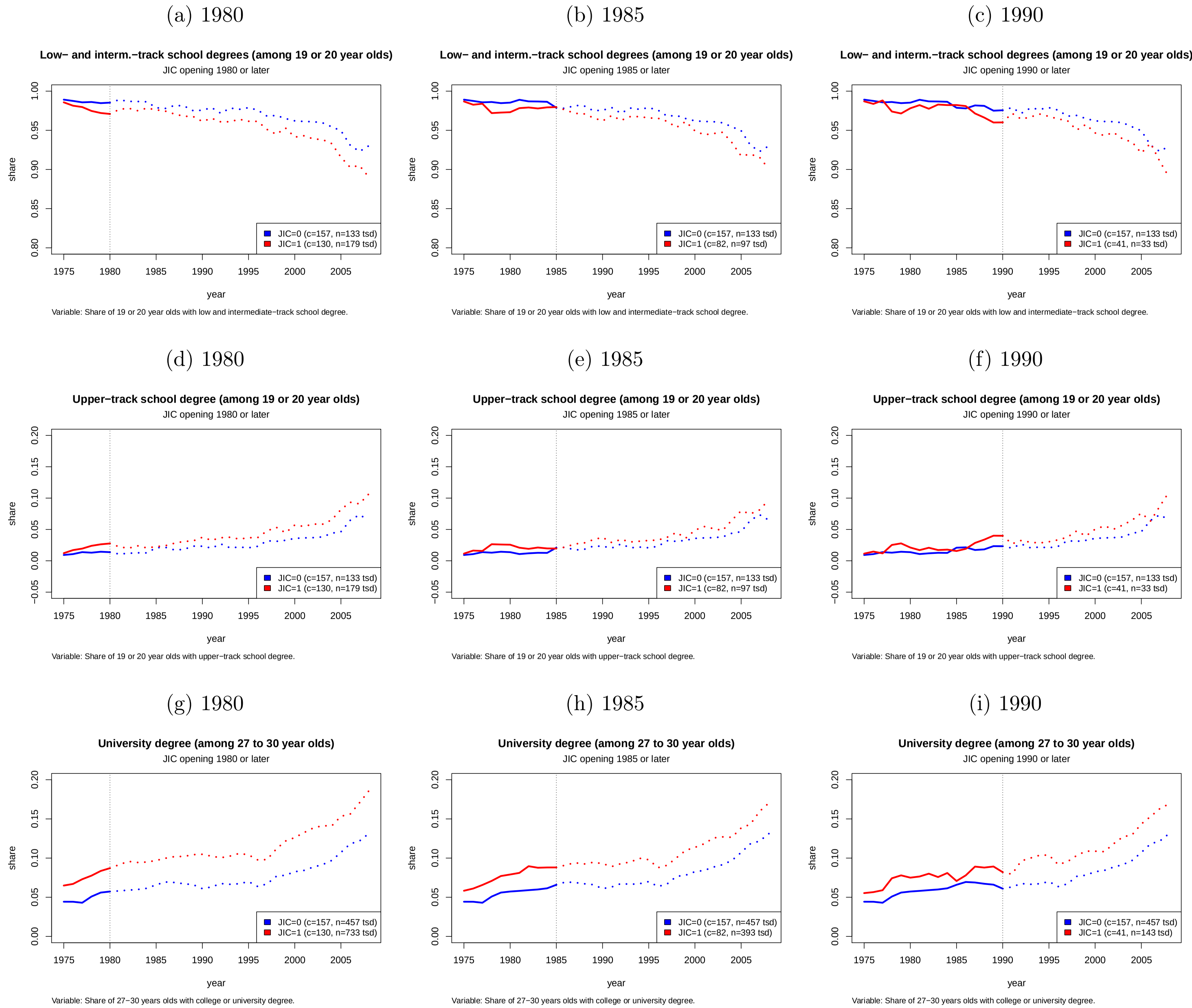
Note: Own data collection. Since the mid-1970s, 181 job information centers have opened in Germany, of which 147 opened in West/western Germany, and 34 in eastern Germany. In Berlin, two out of six JICs closed in 2005 and 2006. For four JICs (Neumünster, Coesfeld, Kassel and Saarbrücken) no data on the opening year are available. This results in 141 JICs in western Germany, and 34 in eastern Germany in 2010, respectively, as shown in panel (a). Panel (b) is based on 413 districts in the whole of Germany in 2008, of which 327 were in western Germany and 86 in eastern Germany. Berlin is defined as western Germany.

Figure 3.2: Regional Distribution of Job Information Centers in Germany, on the District Level



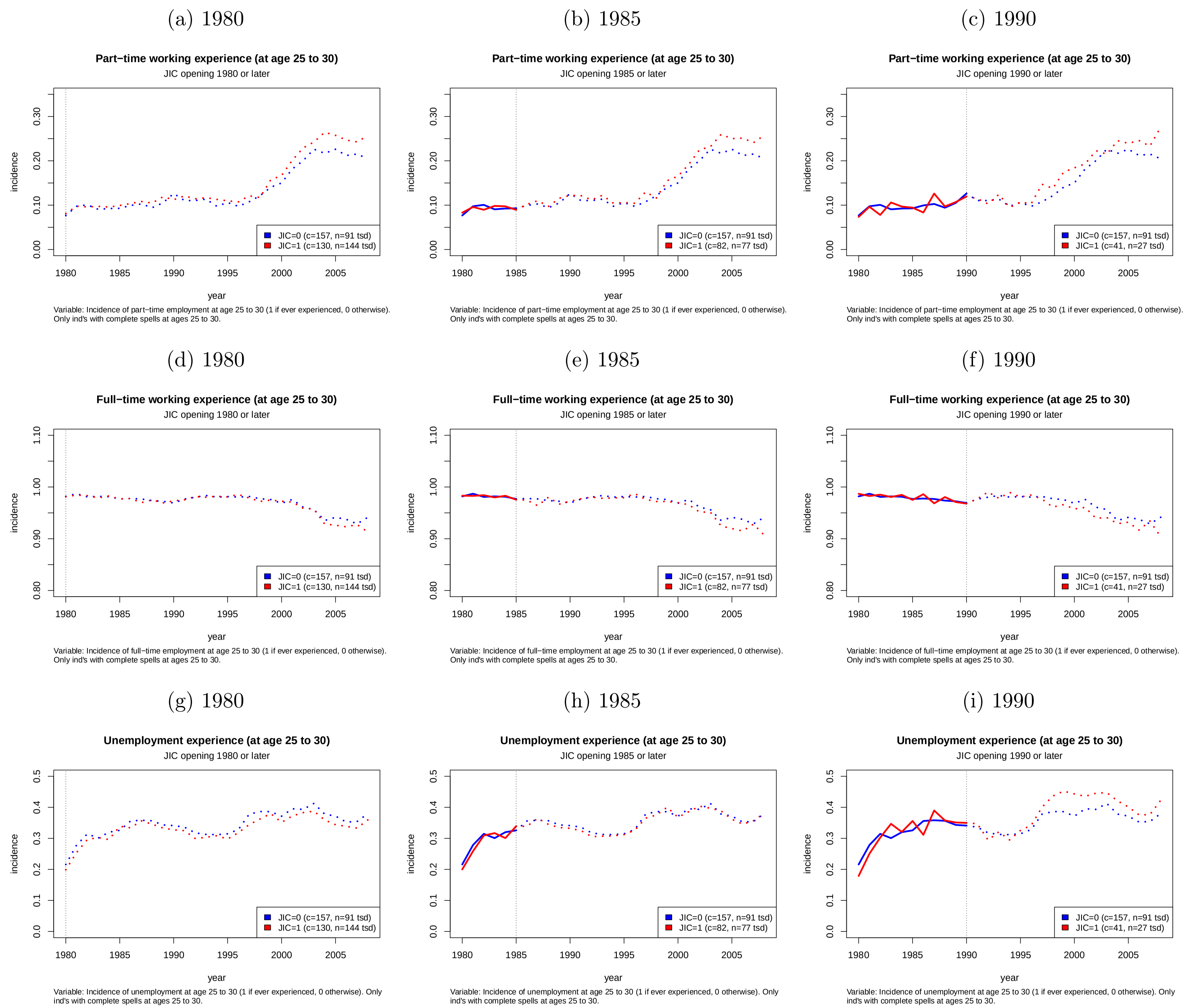
Note: Own data collection. The maps are based on the shapefile VG250 provided by the Federal Agency for Cartography and Geodesy. Its administrative borders are as of December 31, 2008.

Figure 3.3: Common Trends I, Education



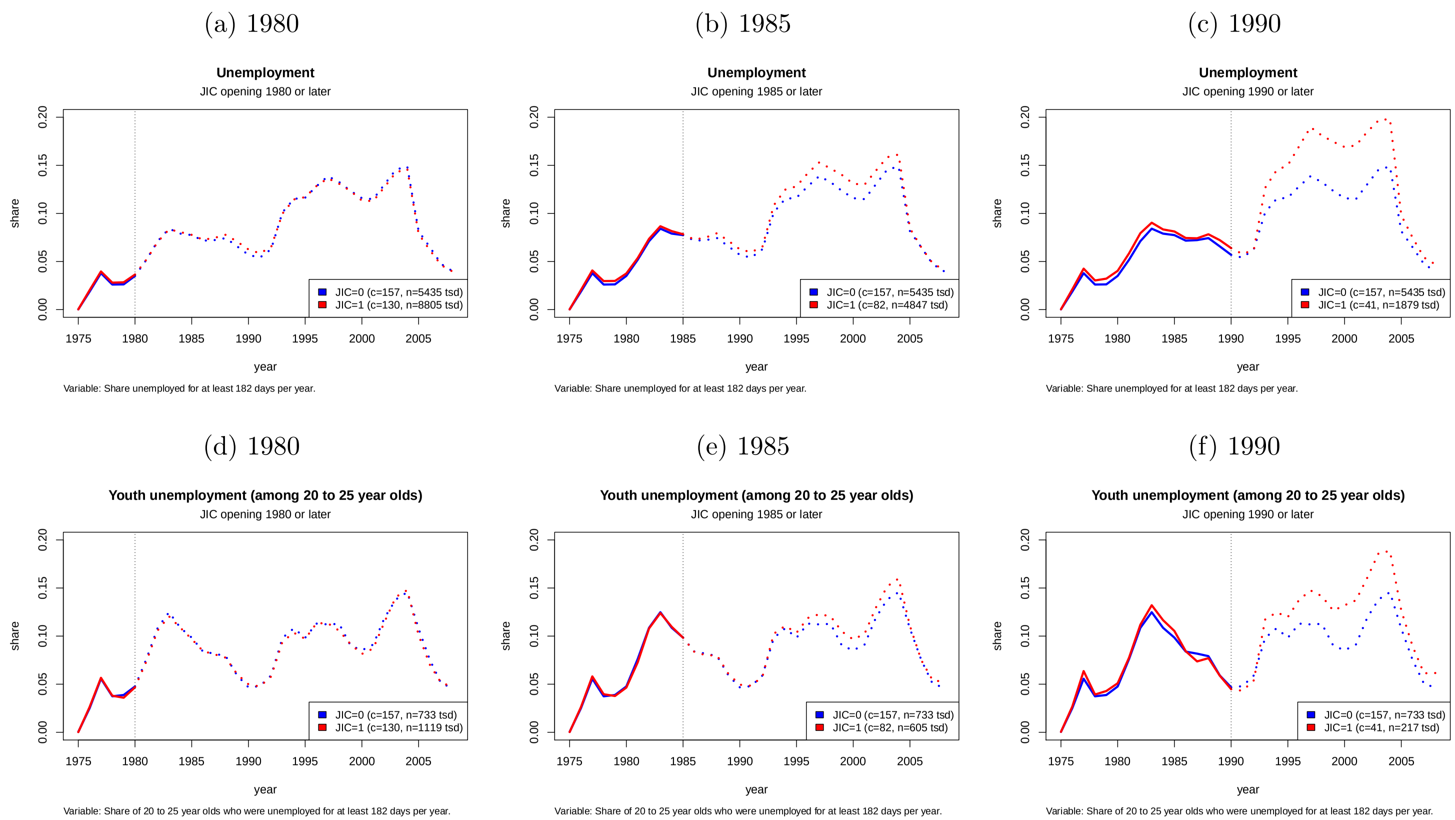
Note: SIAB-R 7508, own calculations. All available observations. Reunified Germany since 1992. Graphics are based on 290 administrative districts that are distinguishable in the dataset, of which 133 include valid information on the opening of a JIC. Three JICs opened before 1980.

Figure 3.4: Common Trends II, Five Years into the Labor Market (Incidence)



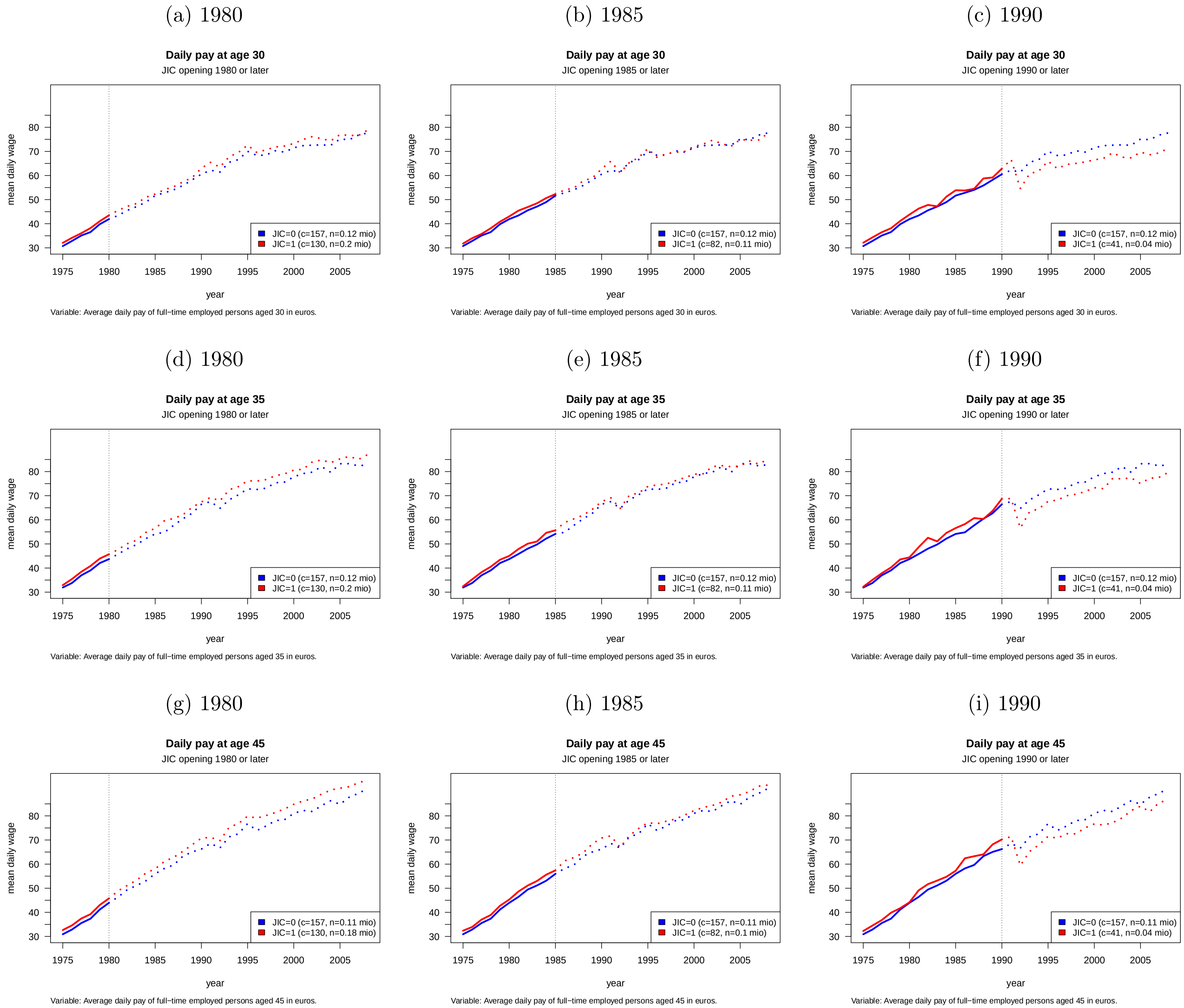
Note: SIAB-R 7508, own calculations. All available observations. Reunified Germany since 1992. Graphics are based on 290 administrative districts that are distinguishable in the dataset, of which 133 include valid information on the opening of a JIC. Three JICs opened before 1980.

Figure 3.5: Common Trends III, Unemployment



Note: SIAB-R 7508, own calculations. All available observations. Reunified Germany since 1992. Graphics are based on 290 administrative districts that are distinguishable in the dataset, of which 133 include valid information on the opening of a JIC. Three JICs opened before 1980.

Figure 3.6: Common Trends IV, Daily Pay



Note: SIAB-R 7508, own calculations. All available observations. Reunified Germany since 1992. Graphics are based on 290 administrative districts that are distinguishable in the dataset, of which 133 include valid information on the opening of a JIC. Three JICs opened before 1980.

Table 3.1: Survey about the Increase in Knowledge

I now know more about ...	Percentage of respondents
which school-leaving certificate you need for a particular occupation.	67
how the apprenticeship is structured and what you have to do in it.	61
how many places are available in the training program.	67
what you earn during an apprenticeship or how much the training costs.	69
which physical and mental capabilities one ought to have to go into this field.	59
what the workplace is like and what the job consists of.	55
how much you earn after training.	31
what kind of stress you can encounter in this field (dust, noise, and health risks).	36
how high the risk of unemployment is.	19
what you can do to advance in this occupation.	60
which higher-level occupations one can obtain qualifications for through further training.	55

Note: Schweikert and Meissner (1984), p. 143, Table 59. Translated into English.

Table 3.2: Summary Statistics

	Full Sample			Reduced Sample I			Reduced Sample II		
	No JIC	JIC	Δ	No JIC	JIC	Δ	No JIC	JIC	Δ
Outcome variables									
Education and educational mobility									
Low-track school degree	0.17	0.10	-0.07**	0.28	0.23	-0.05**	0.29	0.23	-0.07**
Intermediate-track school degree	0.43	0.31	-0.12**	0.65	0.62	-0.03	0.63	0.62	-0.01
Upper-track school degree	0.40	0.59	0.19**	0.07	0.15	0.08**	0.08	0.15	0.07**
Upward educational mobility	0.10	0.12	0.02**	0.15	0.26	0.11**	0.16	0.26	0.11**
University degree	0.23	0.24	0.01	0.05	0.05	0.00	0.06	0.05	-0.01
Labor market attachment									
Incidence part-time employment	0.13	0.20	0.07**	0.08	0.12	0.04**	0.07	0.12	0.05**
Incidence full-time employment	0.97	0.95	-0.02*	0.98	0.98	-0.01	0.98	0.98	-0.01
Incidence unemployment	0.28	0.32	0.04*	0.27	0.34	0.06**	0.26	0.34	0.07**
Duration part-time employment	4.24	6.71	2.47**	2.25	3.45	1.20*	2.12	3.46	1.34*
Duration full-time employment	46.81	45.25	-1.57*	48.41	46.14	-2.27*	48.37	46.11	-2.26*
Duration unemployment	2.22	2.76	0.54*	2.30	3.18	0.88*	1.99	3.19	1.20**
Job search and job matching									
Search duration	3.25	3.58	0.32	3.24	3.91	0.67	3.42	3.92	0.51
Stayed in district	0.64	0.74	0.10**	0.71	0.81	0.10**	0.66	0.81	0.15**
Stayed in state	0.83	0.87	0.04**	0.87	0.92	0.05**	0.85	0.92	0.07**
Share involuntary job changes	0.16	0.15	-0.00	0.18	0.18	-0.00	0.21	0.18	-0.03
Daily Pay									
Pay first job	47.54	50.74	3.20**	43.25	48.98	5.73**	42.16	48.91	6.75**
Pay at age 35	75.03	84.23	9.20**	66.48	75.27	8.79**	68.50	75.27	6.77*
Pay last job	1,815	1,649	-166*	1,547	1,520	-26.51	1,583	1,517	-66.03
Net income	1,575	1,203	-372**	1,401	1,175	-226**	1,514	1,173	-341**
Main explanatory variables									
Year of birth	1969	1978	9.04**	1968	1978	10.02**	1964	1978	13.55**
Female	0.52	0.46	-0.05**	0.51	0.40	-0.10**	0.50	0.40	-0.10**
Migrant background	0.01	0.04	0.03**	0.01	0.06	0.04**	0.01	0.06	0.05**
Lower-track school[a]	0.22	0.16	-0.06**	0.35	0.34	-0.01	0.38	0.34	-0.03
Intermediate-track school[a]	0.40	0.30	-0.11**	0.65	0.66	0.01	0.62	0.66	0.03
Upper-track school[a]	0.38	0.55	0.17**	0.00	0.00	0.00	0.00	0.00	0.00
Father with upper-track school degree	0.19	0.35	0.15**	0.09	0.19	0.10**	0.09	0.19	0.11**
Mother with upper-track school degree	0.11	0.25	0.14**	0.05	0.13	0.08**	0.05	0.13	0.08**
Population density[b]	4.85	6.90	2.05**	4.54	6.60	2.06**	6.32	6.60	0.28*
Number of individuals	3,699	1,533		2,214	652		814	651	

Note: IAB-ALWA. Columns 3, 6 and 9 report p-values from two-group mean comparison t-tests. [a] Measured at the time of potential treatment. [b] Measured for the district of residence at time of potential treatment. $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 3.3: Education and Educational Mobility

Dependent variable:	Low-track school degree (1)	Interm.-track school degree (2)	Upper-track school degree (3)	Upward mobility (4)	University degree (5)
Panel A: Full sample					
Job information center	-0.004	-0.015	0.019	0.033*	0.032
	(0.014)	(0.019)	(0.014)	(0.014)	(0.021)
Adjusted R^2	0.595	0.563	0.734	0.154	0.344
Number of individuals	5,232	5,232	5,232	5,232	4,972
Panel B: Reduced sample I					
Job information center	-0.005	-0.064+	0.069**	0.075**	0.054**
	(0.028)	(0.034)	(0.023)	(0.029)	(0.021)
Adjusted R^2	0.544	0.362	0.055	0.093	0.065
Number of individuals	2,866	2,866	2,866	2,866	2,737
Panel C: Reduced sample II					
Job information center	-0.025	-0.095*	0.120**	0.121**	0.108**
	(0.031)	(0.042)	(0.035)	(0.039)	(0.036)
Adjusted R^2	0.491	0.318	0.094	0.114	0.080
Number of individuals	1,465	1,465	1,465	1,465	1,387

Note: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. ALWA-ADIAB. OLS regressions. Each estimate represents the coefficient from a different regression. Standard errors (in parentheses) are clustered on the district level. All specifications control for a female and migrant dummy, dummies for year of birth, dummies for district of residence and school track attended at time of potential treatment, dummies for mothers' and fathers' highest school qualification, and dummies for population density on the district level at time of potential treatment (10 groups). See Table A.2.3 in the Appendix for detailed regression output. *Reduced sample I* only includes individuals in low- and intermediate-track schools (*Hauptschule* and *Realschule*) at the time of potential treatment. *Reduced sample II* consists of *Reduced Sample I* individuals, but only those who lived at the time of potential treatment in districts in which a job information center opened between 1976 and 2010.

Table 3.4: Labor Market Attachment

Dependent variable:	Incidence (yes/no)			Duration (months)		
	Part-time employed (1)	Full-time employed (2)	Unem-ployment (3)	Part-time employed (4)	Full-time employed (5)	Unem-ployment (6)
Panel A: Full sample						
Job information center	-0.010	-0.010	-0.077^{+}	0.056	2.811*	-0.658
	(0.028)	(0.014)	(0.041)	(1.157)	(1.257)	(0.547)
Adjusted R^2	0.095	0.034	0.043	0.088	0.046	0.021
Number of individuals	3,286	3,286	3,359	3,286	3,286	3,359
Panel B: Reduced Sample I						
Job information center	0.008	0.009	-0.101^{+}	0.083	1.541	-0.767
	(0.033)	(0.016)	(0.055)	(1.285)	(1.613)	(0.679)
Adjusted R^2	0.053	0.017	0.080	0.007	0.027	0.052
Number of individuals	2,160	2,160	2,198	2,160	2,160	2,198
Panel C: Reduced Sample II						
Job information center	-0.030	0.006	-0.080	-1.183	2.390	-0.061
	(0.040)	(0.018)	(0.071)	(1.750)	(2.122)	(0.645)
Adjusted R^2	0.075	0.045	0.069	0.029	0.007	0.054
Number of individuals	1,075	1,075	1,094	1,075	1,075	1,094

Note: $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001. ALWA-ADIAB. OLS regressions. Each estimate represents the coefficient from a different regression. Standard errors (in parentheses) are clustered on the district level. All specifications control for a female and migrant dummy, dummies for year of birth, dummies for district of residence and school track attended at time of potential treatment, dummies for mothers' and fathers' highest school qualification, and dummies for population density on the district level at time of potential treatment (10 groups). See Table A.2.3 in the Appendix for detailed regression output. *Reduced sample I* only includes individuals in low- and intermediate-track schools (*Hauptschule* and *Realschule*) at the time of potential treatment. *Reduced sample II* consists of *Reduced Sample I* individuals, but only those who lived at the time of potential treatment in districts in which a job information center opened between 1976 and 2010.

Table 3.5: Job Search and Job Matching

	Search duration (1)	Stayed in district (2)	Stayed in state (3)	Share invol. job changes (4)
Panel A: Full sample				
Job information center	-0.968	0.072**	0.027	-0.051+
	(0.801)	(0.025)	(0.019)	(0.027)
Adjusted R^2	0.001	0.242	0.181	0.034
Number of individuals	3,366	5,213	5,215	2,427
Panel B: Reduced sample I				
Job information center	-1.431	0.082*	0.040+	-0.079+
	(1.016)	(0.032)	(0.022)	(0.044)
Adjusted R^2	0.048	0.244	0.223	0.007
Number of individuals	2,205	2,864	2,864	1,479
Panel B: Reduced sample II				
Job information center	-1.821	0.018	0.012	-0.123+
	(1.503)	(0.045)	(0.032)	(0.063)
Adjusted R^2	0.096	0.202	0.133	0.016
Number of individuals	1,098	1,463	1,463	731

Note: + $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$. ALWA-ADIAB. OLS regressions. Each estimate represents the coefficient from a different regression. Standard errors (in parentheses) are clustered on the district level. All specifications control for a female and migrant dummy, dummies for year of birth, dummies for district of residence and school track attended at time of potential treatment, dummies for mothers' and fathers' highest school qualification, and dummies for population density on the district level at time of potential treatment (10 groups). See Table A.2.3 in the Appendix for detailed regression output. *Reduced sample I* only includes individuals in low- and intermediate-track schools (*Hauptschule* and *Realschule*) at the time of potential treatment. *Reduced sample II* consists of *Reduced Sample I* individuals, but only those who lived at the time of potential treatment in districts in which a job information center opened between 1976 and 2010.

Table 3.6: Wages and Income

	Register data (ALWA-ADIAB)		Survey data (ALWA)	
Dependent variable:	Daily pay first job (1)	Daily pay at age 35 (2)	Daily pay last job (3)	Net monthly income (4)
Panel A: Full sample				
Job information center	-0.013	-0.106	-0.010	-0.003
	(0.033)	(0.071)	(0.034)	(0.040)
Adjusted R^2	0.233	0.315	0.568	0.349
Number of individuals	2,496	1,831	2,777	4519
Panel B: Reduced sample I				
Job information center	0.002	-0.116	0.078	-0.046
	(0.043)	(0.101)	(0.052)	(0.057)
Adjusted R^2	0.258	0.325	0.563	0.306
Number of individuals	1,574	1,201	1,692	2,531
Panel C: Reduced sample II				
Job information center	0.067	-0.125	0.172*	0.065
	(0.077)	(0.124)	(0.079)	(0.086)
Adjusted R^2	0.237	0.260	0.562	0.330
Number of individuals	812	611	859	1,285

Note: $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001. ALWA-ADIAB. OLS regressions. Each estimate represents the coefficient from a different regression. Standard errors (in parentheses) are clustered on the district level. All specifications control for a female and migrant dummy, dummies for year of birth, dummies for district of residence and school track attended at time of potential treatment, dummies for mothers' and fathers' highest school qualification, and dummies for population density on the district level at time of potential treatment (10 groups). See Table A.2.3 in the Appendix for detailed regression output. *Reduced sample I* only includes individuals in low- and intermediate-track schools (*Hauptschule* and *Realschule*) at the time of potential treatment. *Reduced sample II* consists of *Reduced Sample I* individuals, but only those who lived at the time of potential treatment in districts in which a job information center opened between 1976 and 2010. Compared to the baseline specification the regressions include full sets of county dummies for the time when the monetary flow was realized. Models (1)-(3) additionally control for part-time employment, and models (1) and (2) additionally control for the number of days worked in the year of the respective age. Finally, models (3) also controls for age and age^2.

Table 3.7: Discrete Time Logistic Hazard Models of Opening a Job Information Center on the District Level

	Pre-determined variables[a]		Time-varying covariates[b]		Prop. changes between t-1 and t-2[c]	
	(1)	(2)	(1)	(2)	(1)	(2)
Log population [×1000]	1.149**	1.176**	1.062**	1.201**	1.081**	0.963**
	(0.196)	(0.207)	(0.162)	(0.189)	(0.157)	(0.218)
Log size of area [km²]	-0.477**	-0.501**	-0.421**	-0.574**	-0.465**	-0.719**
	(0.109)	(0.115)	(0.099)	(0.109)	(0.090)	(0.116)
Daily pay	-0.073	-0.106	0.019	-0.065+	7.306	-1.270
	(0.074)	(0.069)	(0.031)	(0.034)	(8.025)	(8.519)
Percent in the population with:						
low or interm.-track school degree	-0.406	14.479	-13.030	-7.180	-2.963	17.472
	(19.228)	(24.543)	(15.110)	(15.928)	(24.401)	(29.738)
upper-track school degree	5.717	31.279	-21.667	-4.889	-0.682	-0.357
	(28.338)	(33.529)	(20.940)	(21.399)	(1.443)	(2.411)
university degree	1.012	-19.240	17.660	4.252	0.641	0.191
	(22.951)	(27.139)	(15.833)	(16.028)	(1.188)	(1.932)
Unemployment rate	4.422	-2.236	-0.252	-4.890	0.529	0.670
	(6.981)	(6.877)	(2.047)	(3.298)	(0.407)	(0.602)
Number of months (age 25 to 30):						
employed full-time		-0.013		0.002		0.245
		(0.017)		(0.019)		(0.344)
employed part-time		-0.049		0.038		0.045
		(0.062)		(0.050)		(0.035)
unemployed		-0.156		-0.011		0.010
		(0.095)		(0.059)		(0.040)
Percentage of the population (age 25 to 30):						
employed full-time		-2.382		-1.329		-3.408+
		(3.569)		(2.456)		(1.831)
employed part-time		-0.662		-2.394		0.028
		(2.154)		(1.571)		(1.132)
unemployed		-0.582		0.200		0.001
		(1.141)		(1.102)		(0.245)
Adjusted R^2	0.151	0.172	0.152	0.166	0.158	0.198
Number of district-year observations	7,041	5,567	7,041	5,567	6,415	2,905

Note: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. ADIAB (SIAB), 1975-2008. The table reports coefficients from logit models using a time-to-event dataset on the the district level. Standard errors in parentheses are corrected for clustering on the district level. Explanatory variables are averages on the district level. [a] Pre-determined variables are measured in the first year available on the district level, corresponding to 1975 in West Germany and to 1992 in eastern Germany. [b] Time-varying covariates vary on the annual level, except for log population and log size, due to data limitations. [c] Explanatory variables measure proportionate changes between $t-2$ and $t-1$. All specifications also control for yearly duration dependence parameters.

Table 3.8: Various Samples and Specifications (*Reduced Sample I*)

Low-track school degr. (1)	Interm.-track school degr. (2)	Upward mobility (3)	University degree (4)	Unempl. incidence (5)	Unempl. duration (6)	Stayed in district (7)	Share invol. job change (8)	Pay 1st job (9)
Panel A: Western Germany								
-0.080*	0.085**	0.083*	0.057*	0.006	-0.037	0.058	-0.037	-0.006
(0.038)	(0.027)	(0.035)	(0.023)	(0.060)	(0.672)	(0.035)	(0.047)	(0.045)
Panel B: Cities excluded								
-0.075	0.084**	0.103**	0.067*	-0.106	-0.600	0.048	-0.118*	0.012
(0.047)	(0.028)	(0.038)	(0.026)	(0.069)	(0.780)	(0.041)	(0.056)	(0.058)
Panel C: Ruhr area excluded								
-0.080*	0.065**	0.063*	0.057**	-0.059	-0.341	0.081*	-0.081+	0.021
(0.036)	(0.024)	(0.031)	(0.022)	(0.061)	(0.714)	(0.034)	(0.047)	(0.045)
Panel D: Early cohorts only (1960-1975)								
-0.089*	0.131**	0.125**	0.120**	-0.116+	-1.428*	0.022	-0.090	0.065
(0.045)	(0.042)	(0.048)	(0.038)	(0.070)	(0.686)	(0.050)	(0.058)	(0.061)
Panel E: Additional linear time trend								
-0.056+	0.064**	0.071*	0.053**	-0.096+	-0.640	0.085**	-0.063	0.010
(0.033)	(0.023)	(0.029)	(0.020)	(0.055)	(0.675)	(0.031)	(0.043)	(0.044)
Panel F: Within-variation, potential treatment years $+/-10$ around JIC opening								
-0.061	0.089+	0.077	0.110*	0.008	0.037	0.009	-0.097	0.119
(0.055)	(0.050)	(0.058)	(0.044)	(0.091)	(0.807)	(0.068)	(0.107)	(0.097)
Panel G: Placebo Test: JIC opening years manipulated by -6 years								
-0.034	0.026	0.030	0.009	-0.039	-0.027	0.022	-0.065	0.013
(0.033)	(0.021)	(0.032)	(0.018)	(0.050)	(0.704)	(0.035)	(0.049)	(0.057)

Note: $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001. ALWA-ADIAB. OLS regressions. Each estimate represents the coefficient from a different regression. Standard errors (in parentheses) are clustered on the district level. All specifications control for a female and migration dummy, dummies for year of birth, dummies for district of residence and school track attended at time of potential treatment, dummies for mothers' and fathers' highest school qualification, and dummies for population density on the district level at time of potential treatment (10 groups). See Table A.2.3 in the Appendix for detailed regression output. Based on *Reduced sample I* that only includes individuals in low- and intermediate-track schools (*Hauptschule* and *Realschule*) at the time of potential treatment.

4 Door Opener or Waste of Time? The Effects of Internships on Labor Market Outcomes*

Chapter Abstract

This paper studies the causal effect of student internship experience on labor market choices and wages later in life. We use variation in the introduction and abolishment of mandatory internships at German universities as an instrument for completing an internship while attending university. Employing longitudinal data from graduate surveys, we find positive and significant wage returns of about six percent in both OLS and IV regressions. This result is mainly driven by a higher propensity of working full-time and a lower propensity of being unemployed in the first five years after entering the labor market. Moreover, former interns pursue doctoral studies less frequently. The positive returns are particularly pronounced for individuals and areas of study that are characterized by a weak labor market orientation. Heterogeneous effects are not found across other subgroups of the population.

4.1 Introduction

Internships have become a widespread phenomenon among university students in many countries throughout North America and Europe. Callanan and Benzing (2004), for example, argue that internships in the US have become increasingly popular as a way to bridge the transition from education to work, with three out of four college students completing an internship in 2004, compared to fewer than 40 percent of students in 1980. In Germany, 55 percent of students who are currently enrolled in a university report having completed an internship during the past twelve months (Krawietz et al., 2006). By the time students finish their studies, nearly 80 percent report at least one absence from university to complete an internship (Scarletti, 2009).

*This chapter is based on joint work with Thomas Siedler.

What motivates students to complete internships while enrolled at university? First and foremost, students expect internships to pay off after graduation when they enter the labor market. Indeed, when asked for their main motivation for undertaking an internship, most cited the desire to get to know the work environment and gather practical work experience. Many also hope that an internship will help them to find employment later. The desire to earn money as an intern appears to be only a secondary motivator (Krawietz et al., 2006).

The surge in popularity of internships in higher education is not only a consequence of individual choices; it is also the result of universities emphasizing the importance of internships as part of the broader educational experience. Following the policy changes implemented as part of the Bologna Reform, making graduates employable has become a central objective of higher education across Europe (Teichler, 2011). Universities have been called upon to prepare their graduates better for the transition to work by focusing on competencies that are relevant to the job market. Internships have been identified as an effective means of building these competencies (Wolter and Banscherus, 2012; Teichler, 2011). As a consequence, many universities urge students to complete internships or even make internships an integral part of the curriculum (Krawietz et al., 2006).

Internships are believed to help students build work-relevant skills, gain specific knowledge of their future occupations, develop a clearer self-concept, and confirm or redirect individual career goals (Brooks et al., 1995). Most of the skills acquired during internship are general and transferable (Busby, 2003). These attributes may then translate into various favorable outcomes for the transition into the labor market and early career success, for example, shorter job search duration, lower probability of unemployment, more stable job positions, better job match, greater job satisfaction, and increased earnings. However, internships also produce costs due to the investment of time, effort and sometimes even money. Interns have to accept educational opportunity costs and often enter the labor market later than non-interns. Considering the fact that most internships are poorly paid or not paid at all, it is not surprising that some debate has arisen about the potential downside effects of internships, namely the allegation that firms exploit highly qualified students as cheap workers (Wolter and Banscherus, 2012). The overall effect of internships on individual labor market outcomes is unclear, and empirical research is needed to provide a basis for sound conclusions.

In this paper, we examine the effect of student internships on subsequent labor market outcomes among university graduates in Germany. The investigation focuses on wages, but also aims at tracing the different channels by which internship experience affect wages. The key research questions are:

1. What is the causal effect of student internships on wages later in life?

2. To what extent do intermediary outcomes serve as transmission channels for wage returns?

Based on economic theory, we anticipate student internships to have positive wage returns. Human capital theory (Becker, 1993; Mincer, 1974) predicts that the additional knowledge, skills and competencies accumulated as an intern result in higher pay. Signaling theories point out that employers' hiring decisions are made under uncertainty since the productivity of potential workers is unknown. Job seekers may therefore use internships and positive reference letters provided to them upon completion of the internship to signal high ability, which may result in improved job matching and higher earnings (Spence, 1973; Akerlof, 1970; Schnedler, 2004). Screening theory predicts that firms use such signals to more accurately assess workers' hidden productivity (Stiglitz, 1975). Social capital theory (Bordieu, 1986; Coleman, 1988) also foresees positive labor market returns of internships because of the opportunity they provide to establish relationships with co-workers and potential employers. These social ties might lead to better jobs after graduation (Granovetter, 1995).

For the empirical investigation, we use longitudinal data from graduate surveys conducted by the German Centre for Research on Higher Education and Science Studies (DZHW) that provide information on students internships and income later in life. In order to account for the endogeneity of students' decisions to undertake an internship, we employ a two-stage least squares (2SLS) approach and instrument internship completion with the occurrence of mandatory internships. Exogenous variation comes from the introduction and abolishment of mandatory internships at the university level. The first-stage regressions suggest that the occurrence of mandatory internships has a large and significant impact on the likelihood of acquiring internship experience. In fact, students have a 58 percentage points higher likelihood of completing an internship during the course of their studies if the internship is mandatory. Internship experience causes wages to rise by around six percent, both in OLS and IV regressions. This result is mainly driven by a higher propensity to work full-time and a lower propensity to be unemployed during the first five years after graduating from university. Moreover, former interns begin and complete doctoral studies less frequently. The positive returns are particularly pronounced for individuals and areas of study with a weak labor market orientation.[1] Across other

[1] At the individual level, we distinguish between students for whom labor market aspects played an important role in the choice of what to study. Further, following Scarletti (2009), we distinguish between areas of study with a strong labor market orientation (areas of study that lead to a particular profession, e.g. medicine and architecture) and areas of study with a weak labor market orientation (areas of study that teach more general skills and qualify graduates for a wide range of different jobs, e.g. history, philosophy, languages). See Table A.3.6 in the appendix for a complete classification of areas of studies into weak and strong labor market orientation.

subgroups of the population, however, we do not detect heterogeneous treatment effects.

Despite the prevalence of student internships and their significance for vocational exploration, the empirical literature on causal effects of internship experience remains scant. Several studies draw conclusions based on opinion polls among interns about the perceived benefits of their work experiences (Beck and Halim, 2008; Cook et al., 2004; Shoenfelt et al., 2013; Krawietz et al., 2006). Another strand of literature compares treatment and control groups, but does not account for potential self-selection into the treatment group. Some studies have found internships to be positively correlated with interns' self-crystallization of interests and values (Taylor, 1988) and self-efficacy (Brooks et al., 1995). Moreover, interns are reported to be more likely to adopt employer-oriented values (Pedro, 1984), to acquire job relevant competencies (Garavan and Murphy, 2001), and to possess interpersonal skills that are typically not part of the study curriculum (Crebert et al., 2004). Studies also report positive correlations of internships with shorter job search duration (Gault et al., 2000), higher job stability (Richards, 1984), more and better quality job offers (Taylor, 1988), a higher chance of choosing a career-oriented job (Callanan and Benzing, 2004), and wage increases (Gault et al., 2000; Reimer and Schröder, 2006; Scarletti, 2009). To our knowledge the only papers that aim at estimating causal effects of internship experience are Nunley et al. (2014) and Klein and Weiss (2011). Nunley et al. (2014) conduct a résumé-audit study in the US and randomly assign three-month internship experience to fictitious job seekers. They find that applicants with internship experience receive about 14 percent more interview requests than those who were not assigned an internship. The effects are larger for non-business degree holders than for business degree holders. Klein and Weiss (2011) study wage effects of mandatory internships among university graduates in Germany. The authors employ matching estimation methods and find no positive effects on wages. Similar to our study, the authors argue that the introduction of mandatory internships is independent of unobservable characteristics. However, the scope of interpreting their results is limited. First, they use cross-sectional data and do not utilize changes in the occurrence of mandatory internships over time, which makes their identification less robust and less credible. Second, they elicit the effect of mandatory internships, not voluntary internships, and their findings are based on relative small sample sizes.

The remainder of this paper is structured as follows: Section 4.2 describes the data, and section 4.3 lays out the empirical strategy. Section 4.4 presents the main results for the effects of internship experience on wages later in life. Section 4.5 discusses various aspects of identification. Section 4.6 inspects whether the effects differ for various subgroups of the population. Section 4.7 sheds light on potential intermediary outcomes that channel positive effects toward wages. Various robustness checks are presented in Section 4.8.

Section 4.9 concludes.

4.2 Data, Variables, and Descriptive Statistics

We use longitudinal data from surveys of university graduates conducted by the German Centre for Research on Higher Education and Science Studies (DZHW).[2] Each survey is a random sample of the student population at German universities. We employ information from three different cohorts that comprise persons who graduated in the years 2001, 2005, and 2009, respectively. For each cohort, an *initial survey* was conducted around one year after graduation from university. Around five years later, a *follow-up survey* was conducted. For the cohorts 2001 and 2005, data are available for both waves, the initial and the follow-up survey. For the 2009 cohort, only the first wave is available. Figure 4.1 visualizes the timing of the data collection.

In the initial survey, students were asked whether they did a voluntary and/or mandatory internship during the course of their studies. We use this information to generate the key dummy variable for whether students did an internship and the instrument dummy variable for whether the study regulations included a mandatory internship. Further information was collected on details of the area of study and universities as well as on the graduates' opinions about their university studies. The surveys also include comprehensive demographic, socioeconomic and educational information, and information on the parental background. The main outcome variable, gross monthly wages, is self-reported for the job at the time of the interview and measured in euros adjusted to 2005 prices.

Throughout the analysis, we differentiate between two samples. We focus on *Sample I*, which measures wages in the second waves for the graduate cohorts 2001 and 2005, as indicated by the shaded areas in panel A of Figure 4.1. This sample allows us to detect effects of internships on wages five to six years after graduating. We observe that most individuals have completed the transition from university to work by this time. For wage reports from the initial survey, provided about 12 months after graduation, we suspect that some respondents have not yet entered the labor market. Some may still be looking for a job or may not be in the labor market for other reasons, for example, because they are pursuing further education. However, to use all available information from the surveys, we also work with a pooled sample, referred to as *Sample II*. The composition of this sample is depicted in panel B of Figure 4.1. It comprises all available waves for the three graduate cohorts. This sample helps to increase the precision of the estimates, which will become particularly relevant when studying heterogeneous effects in section 4.6. We borrow the

[2] See Rehn et al. (2011), for a thorough description of the survey and data. Recent studies that have also used DZHW data are, for example, Parey and Waldinger (2011) and Grave and Goerlitz (2012).

idea of pooling the data from Parey and Waldinger (2011).

A typical feature of some university subjects and degrees is that they imply an obligatory second phase of education. For example, prospective teachers take a first state exam upon completing their university studies and then have to complete a 1.5 year practical training phase in the classroom before taking a second state exam, which then enables them to work as a teacher. Similar obligatory second educational phases of varying duration exist for lawyers, clerics and medical doctors. During this period, individuals are outside the regular labor market. For this reason, we exclude all individuals from our sample who finished university with a state exam (lawyers, clerics, pharmacists, teachers, and physicians) or reported having to complete an obligatory second phase of education. Furthermore, we exclude graduates who finished university with a bachelor's degree.[3] Bachelor's degrees imply a shorter duration of study than other university degrees (Diplom, Magister, Master) and are less accepted by employers in Germany. Finally, we only keep observations in the estimation samples that have non-missing values for all relevant variables. This results in a sample size of 6,424 graduates for *Sample I* and 19,218 observations for *Sample II*.

Tables 4.1 and 4.2 report means for the two samples, differentiated by graduation cohort and internship experience. The numbers in column 1 in Table 4.1, for example, show that in *Sample I* the average year of birth is 1976, 54 percent are female, around one in three graduates completed an apprenticeship before studying, and the final high school grade is 2.2 (on a scale 1-5 with 1 signifying "excellent" and 5 "failing"). Further, many students come from highly educated families, with 36 percent of mothers and 49 percent of fathers having graduated from an upper secondary school. Five to six years after graduating from university, 88 percent of the respondents are employed, 85 percent are employed full-time, and 70 percent have a permanent position. With respect to the main outcome variable—monthly wages—the unconditional means show that students with internship experience have slightly higher mean values than their fellow graduates.

[3] Bachelor graduates were only interviewed in 2009.

4.3 Estimation Method

To estimate the effect of internship experience on labor market outcomes we use a 2SLS setup and instrument internship experience with the presence of mandatory internships. The two main equations are:

$$\begin{aligned} Log(Wage) =& \beta_0 + \beta_1 Internship + \beta_2 Female + \beta_3 GradCohort + \beta_4 BIRTHYEAR + \\ & \beta_5 AREA + \beta_6 UNIVERSITY + X\gamma + \epsilon \end{aligned} \tag{4.1}$$

$$\begin{aligned} Internship =& \alpha_0 + \alpha_1 Mandatory + \alpha_2 Female + \alpha_3 GradCohort + \alpha_4 BIRTHYEAR + \\ & \alpha_5 AREA + \alpha_6 UNIVERSITY + X\gamma + \varepsilon, \end{aligned} \tag{4.2}$$

where $log(Wage)$ is the logarithm of wages, $BIRTHYEAR$ is a 22×1 vector that comprises indicators for year of birth, $AREA$ is a 53×1 vector that comprises fixed effects for students' area of study, and $UNIVERSITY$ is a 262×1 vector that comprises university fixed effects.[4] $Female$ and $GradCohort$ are dummy variables indicating gender and the graduation cohort.[5] Depending on the particular specification, the vector X contains different sets of additional explanatory variables. In equation (4.1), the variable $Internship$ equals one if the student did an internship while studying, and zero otherwise. In the first-stage equation (4.2), the dichotomous variable $Mandatory$ equals one if an internship was mandatory during the course of studies, and zero otherwise.

We present results for two different specifications. In our baseline model, we control for individuals' year of birth fixed effects, area of study, and university fixed effects, a female and graduation cohort dummy, as well as a dummy variable for graduating from a university of applied sciences. We call this the parsimonious model. In the second specification—called the full model—we add several predetermined variables that are likely to be good proxy variables for students' intelligence, ability, and labor market orientation. We control for students' final high school grade (*high school grade*), whether they completed an apprenticeship before studying (*apprenticeship*), the self-reported influence of labor market aspects on their choice of what career to pursue and thus what to study at the university (*labor market orientation*), as well as a full set of dummy variables for

[4]Note that for $AREA$, the data only allow us to observe the areas of study, which are referred to as *Studienbereiche* in the nomenclature of the Federal Statistical Office (2012), but not the exact subject. For example, we can observe whether someone studied Romance philology, but not whether the subject was French, Italian, Spanish, or Portuguese.

[5]When estimating the above regressions for *Sample II*, we control for two dummy variables for graduate cohorts, as the sample includes graduates from the 2001, 2005, and 2009 cohorts.

mother's and father's highest general educational degree (four groups each).[6]

4.4 Results

The OLS and IV results for equation (4.1) are presented in Table 4.3. Each column shows the estimated coefficients and standard errors from a different regression. The first four columns present results for wages measured about five years after graduating from university (*Sample I*), and columns 5-8 show the estimates from pooled regressions that also include wages measured one year after graduating from university (*Sample II*). In the *Sample I* regressions, standard errors are clustered at the university level. In the *Sample II* regressions, standard errors are clustered at the individual level.[7] In the robustness section 4.8, we also present results when clustering at the level of the area of study or the departments, where departments are defined as unique combinations of area of study and university.

All regressions in Table 4.3 show a positive and significant relationship between internship experience and wages. The OLS coefficients for both samples suggest that a student who gained labor market experience through an internship during the course of his or her studies has around 6 percent higher wages later in life. The coefficients are statistically significant at the 1 percent level. Importantly, the IV estimates also point to a positive and significant relationship between internship experience and graduates' labor market wages, with estimated effects of around six percent. The comparison of OLS and IV estimates from *Sample I* reveals a small upward bias in the OLS regressions, which is potentially due to ability bias. However, the estimates based on *Sample II* do not suggest an upward bias in the OLS regressions. Taken as a whole, the estimates in Table 4.3 suggest positive wage returns of student internship experience of around six percent.

Table 4.3 also shows the estimated effects for other selected explanatory variables. Female graduates have around 17-20 percent lower wages than male graduates. These results are broadly consistent with previous findings for Germany (Machin and Puhani, 2003; Leuze and Strauß, 2009). Moreover, the estimates for the variable *apprenticeship* reveals that graduates who completed an apprenticeship before studying have around five to eight percent higher wages. In the IV regressions, the magnitude of the estimate is

[6] Mincer type wage equations typically control for age and age^2 to proxy work experience. Age variables have been omitted from the baseline specification because they are likely to be outcome variables themselves. This is because internship experience might delay labor market entry due to the extra time working rather than attending university. We experimented with the inclusion of age variables and found that this leaves our results unchanged.

[7] While the former accounts for suspected error correlation at the level of universities, the latter accounts for the fact that for many individuals in *Sample II*, we use repeated observations at the individual level, one from the initial survey and one from the follow-up survey.

quite similar to the effect of internship experience. Note, however, that apprenticeships last on average around three years, whereas student internships last on average twelve weeks (Scarletti, 2009). A comparison of these two estimates underlines the economic relevance of the positive wage returns of internships.

First-stage results based on equation (4.2) are presented in Table 4.4. We again report estimates for the parsimonious and full model for *Samples I and II*, respectively. As expected, the estimated coefficient for the instrumental variable $Mandatory$ is always positive and precisely estimated at the 1 percent significance level. The estimates suggest that a compulsory student internship increases the likelihood of internship experience by around 58 percentage points. The corresponding F-statistics of about 38 and 70 also point toward a strong first-stage relationship. In line with the summary statistics in Tables 4.1 and 4.2, the first-stage estimates show a negative relationship between studying at a university of applied sciences and having completed an apprenticeship before studying and the likelihood of doing an internship during the course of studies.[8]

4.5 Aspects of Identification

This section provides arguments and evidence that support the credibility of our results for causal interpretation. Four aspects are addressed: (4.5.1) differences in the quality of universities and study programs; (4.5.2) variation over time in requirements to complete an internship; (4.5.3) the impact of potential confounders, that is, simultaneity in the introduction or abolishment of mandatory internships with other changes at the level of university or the area of study; and (4.5.4) the possibility of self-selection into study programs with mandatory internships.

4.5.1 Differences in Quality of Universities and Study Programs

One potential concern may be that the quality and reputation of the university and/or the study program are correlated with the availability of mandatory university-organized internships, and with graduates' labor market outcomes later in life. If good universities offer, on average, more programs with mandatory internships and if their graduates are also more successful in finding high quality jobs, then instrumental variable estimates can be upward biased. To account for this, the regressions control for a maximum set of university and area of study fixed effects. As a result, differences between universities and differences between area of study at a given university are controlled for. To further

[8]Students at universities of applied sciences are less likely to complete an internship while being enrolled at university, but are much more likely to do an "practical semester" during the course of their studies than students at university. In the robustness section below, we will return to this issue in more detail.

mitigate this concern, one robustness check in Section 4.8 involves the inclusion of 1,149 dummy variables that represent unique combinations of university and area of study (i.e. departments). Another sensitivity analysis includes dummies for combinations of area of study and *type* of university (e.g. university or university of applied sciences). The estimates from both robustness exercises are not significantly different from the main results in Table 4.3. Differences in the quality of universities and their areas of study therefore do not pose a threat to our identification strategy.

4.5.2 Variation in Mandatory Internships over Time

A key premise for identification is that there is variation in the presence of mandatory internships that is exogenous to individual unobserved characteristics. In this section, we shed some light on the introduction and abolishment of mandatory internships across cohorts, universities, and areas of study, which is the main source of variation.

The data allow us to identify the existence of mandatory internships for individuals who report having chosen a certain subject in a certain area of study at a certain university. We also know the cohort to which they belong. However, single observations do not reveal whether there was a change in the occurrence of mandatory internships for earlier or later cohorts. In order to capture potential status changes, we therefore refer to departments as the smallest institutional units available, where departments are defined to be unique combinations of universities and areas of study. We then calculate the proportion of students in a department reporting a mandatory internship, separately for each cohort. If, from one cohort to the next, the majority of reports in one department change from the non-existence to the existence of mandatory internship, then we consider this department to have introduced mandatory internships. If the change occurs in reverse direction, then we think of the department as having abolished mandatory internships. In the same fashion, this procedure also allows us to detect departments that have not changed their status. Table 4.5 sorts department and observations from *Sample I* into distinct groups that result from the outlined procedure. As the reports within the combinations of department×cohort (*cells* hereafter) are rarely univocal, we have to define the (non-)existence of mandatory internships along the lines of thresholds. The 50/50 threshold defines cells as having mandatory internships if more than half of all graduates report that an internship was mandatory, and zero otherwise. Alternative thresholds are 60/40 and 70/30, which are more restrictive in the sense that they determine the status of cells only if the majority is more pronounced. That is, assignment is only established if the proportions exceed the 60 (70) percent level or stay below the 40 (30) percent level. When choosing the optimal threshold, one therefore faces a trade-off between maintaining a high number of observations (best 50/50) and precisely assigning departments into the different groups (best 70/30).[9]

Columns 1 and 2 of Table 4.5 define the different groups. Missing cells or cells which are ambiguous in the sense that they do not exceed the thresholds in either direction, are marked by a dash. Since not all departments are included in both surveys from 2001 and 2005, we have an unbalanced panel data set. *Sample I* comprises 262 different universities and 53 different areas of study, yielding a total of 1,149 departments. For the 50/50 threshold, column 3 shows that there are 64 departments that introduced mandatory internships from 2001 to 2005. Conversely, 53 departments abolished mandatory internships. The corresponding numbers of students in columns 4 and 5 suggest that around 11 percent (721 out of 6,424), of all observations belong to a department in which variation occurred over time. If we disregard the departments in rows 5-9, for which there is uncertainty about status changes, this share increases to 61 percent (232 + 140 + 122 + 227 = 721 out of 1,176) indicating that more than half of the departments might have changed the status of mandatory internships between the 2001 and 2005 cohorts. Hence, there is considerable variation in mandatory internships at the department level over time that contributes to the identification of our IV estimates.

4.5.3 Impact of Potential Confounders

If the introduction or abolishment of mandatory internships coincided with other changes at the area of study or university level that could in turn affect wages, this would pose a major threat to our identification strategy. For instance, if the introduction of mandatory internships coincided with improvements in career counseling, estimates of internship experience would likely be upward biased. In order to assess the influence of such potential confounders, we make use of items in the questionnaires that elicit the respondent's evaluation of various aspects of studying. More specifically, we examine twelve different quality indicators of the area of study and/or university that may have an independent effect on wages, thereby potentially biasing the main results.

The twelve indicators cover the following four areas: (1) overall quality of education, (2) educational media and infrastructure, (3) training, and (4) career counseling. Respondents can rate items in each of the categories on a five-point scale, from "very bad" (1)

[9] We are aware that this approach involves some measurement error as we only observe departments and not their actual study regulations, which would be more precise. However, we believe that this is the best we can do to evaluate the variation in mandatory internships over time, since no such information at the department level is available from external data sources. In the robustness section, we use all three thresholds to generate alternative instrumental variables to evaluate the robustness of the main findings. However, none of the alternative instruments captures the exposure to mandatory internships as precisely as students' own reports.

to "very good" (5).[10] In difference-in-difference regressions based on *Sample I*, we test whether changes in the quality indicators coincide with the introduction and abolishment of mandatory internships. We estimate regressions of the form

$$EduQual_j = \alpha_0 + \alpha_1 2005cohort + \alpha_2 Treat + \alpha_3 Treat * 2005cohort + X\gamma + \epsilon, \qquad (4.3)$$

where the outcome variable $EduQual_j$ measures the jth variable of educational quality with $j = 1, ..., 12$ and $Treat$ indicating the binary treatments of either introducing or abolishing mandatory internships in a person's department.[11] For the alternative treatments—introduction and abolishment—we run two separate regressions. The DiD estimate of interest is parameter $\hat{\alpha}_3$. For each variant of equation (4.3), two different specifications are estimated. The first specification estimates simple DiD regressions without controlling for additional explanatory variables, i.e. removing X from the equation. The second specification controls for a rich set of background variables that are identical to the full model (cf. Table 4.3). Furthermore, both specifications are estimated based on two different samples. In the first sample, the comparison group consists of graduates from departments that experienced no change in mandatory internship over time. In the second sample, the comparison group is restricted and depends on the treatment: for $Treat$ being the introduction (abolishment) of mandatory internships, the control group consists of graduates from departments that never (always) had mandatory internships.

Table 4.6 reports the estimates of α_3 from equation (4.3) for the treatment of *introducing* mandatory internships. Each estimated coefficient and standard error in parenthesis comes from a different regression. Positive coefficients imply that the introduction coincides with improvements in the quality indicators, and negative coefficients indicate a deterioration. None of the estimated DiD effects in Table 4.6 is statistically significant at the 5 percent level. The only estimates that are statistically significant at the 10 percent level are for the outcome variable *Writing skills training*. However, the estimated coefficients suggest that the introduction of mandatory internships coincides with a deterioration—rather than an improvement—in writing skills training. If writing skills training is a determinant of graduates' wages later in life, our results in Table 4.3 may be downward biased, not upward biased.

[10] Figure A.3.5 in the appendix displays the distribution of the twelve variables. The figure shows that there are considerable differences in how graduates evaluate the quality of their studies. For example, around 50 percent of the graduates rate the structure of the degree program and the modernity of methods taught as very good or good (panel A). In contrast, fewer than 15 percent of graduates assign this positive rating to the provision of career orientation (panel C).

[11] In line with the methodology described above, we use the 50/50 threshold for identifying changes in the occurrence of mandatory internships at the level of departments. In unreported regressions, we also use the 60/40 and 70/30 thresholds. The DiD estimates based on these alternative thresholds yield similar results and are available from the authors upon request.

Table 4.7 reports the DiD estimates from equation (4.3) for the treatment of *abolishing* mandatory internships. Consistent with the results in Table 4.6, there is no statistical evidence that the abolishment coincides with deteriorations in the quality indicators.[12] All in all, we interpret the findings of Tables 4.6 and 4.7 as evidence that the introduction and abolishment of mandatory internship does not coincide with other study related changes.

4.5.4 Self-Selection into Mandatory Internships

Our identification approach crucially hinges on the assumption that individuals do not select themselves systematically into study programs with mandatory internships based on unobservable characteristics. Put differently, the instrument must provide variation that is exogenous given the control variables. This assumption would be violated if, for example, more ambitious students were more likely to choose subjects with mandatory internships, and if they were also more successful in the labor market later in life. Moreover, ambition would have to be an omitted variable that is not sufficiently captured by observables such as high school degree, labor market orientation and parents' educational background, all of which are included in the full model specification. We believe that it is very unlikely that students choose their subjects and universities based on whether internships are mandatory. Instead, we believe the quality and reputation of the study programs and universities are the most important choice determinants (Parey and Waldinger, 2011). Proximity to the nearest university is also an important factor (Spiess and Wrohlich, 2010). Several German newspapers such as *Handelsblatt* and *Die Zeit* regularly publish university rankings by subjects and institutions, and this information is widely circulated. However, none of these published rankings includes information on internships. Moreover, gathering information from university websites as to whether or not internships are mandatory is rather difficult. We therefore believe that students' self-selection into mandatory internships is unlikely to bias the present estimates.

In order to support these arguments with tentative empirical evidence, we refer to the methodology of the DiD estimates above and investigate whether the composition of students' background characteristics changed as departments introduced and abolished mandatory internships. In the spirit of equation (4.3), we regress both treatments on the following predetermined variables: mother's and father's highest school degree (one if *Abitur*, zero otherwise), student's labor market orientation when starting university studies, and final high school grade. Table 4.8 presents the results based on the 50/50 threshold

[12] In unreported regressions, we also estimated the DiD models with dichotomous outcome variables equal to one if the graduates said that the particular aspect of study quality was very good or good, and zero otherwise. The results from these linear probability regressions are in line with the estimates in Tables 4.6 and 4.7, respectively.

for the introduction (panel A) and abolishment (panel B) of mandatory internships. The linear probability estimates in both panels show that these variables are not significantly correlated with the treatments.[13] This strengthens our argument that self-selection is unlikely to be a concern.

4.6 Heterogeneous Effects

This section studies the heterogeneity of treatment effects across subgroups of the population. We know from previous studies that, for example, college degree returns are higher for females than for males (Jacobson et al., 2005; Jepsen et al., 2014). In order to assess whether such differences also exist for internship experience, panel A of Table 4.9 reports the impact of internship experience separately for women and men. To investigate whether estimations differ significantly between groups, also the relevant p-values from interacted models are reported. Panel B investigates heterogeneity in treatment effects by parents' education. The sample is divided by whether or not one of the parents has an upper secondary school degree. Students with highly educated parents might benefit from their social networks, irrespective of their own labor market experience. Hence, student internship might be more rewarding for students without these intergenerational networks. In panel C in Table 4.9, separate effects are estimated for graduates by their final high school grade, since students with high grades are likely to have other unobservable characteristics (e.g., high motivation, intelligence, social skills) that might make them benefit more from an internship than students with lower grades. Further, due to their abilities, they might be more likely to participate in an internship of high quality and prestige, an aspect that we cannot observe. The estimates in panel D show heterogeneity of treatment effects across students' labor market orientation. Students for whom labor market aspects played a critical role in their choice of what to study might be more ambitious and motivated during their internships than students with lower levels of labor market orientation, potentially leading to higher returns. Alternatively, internships might be particularly beneficial for students who have not given much thought to labor market aspects. An internship experience might help them to gain a clearer self-concept and develop better career plans. Finally, panel E in Table 4.9 reports separate treatment effects according to whether the area of study has a strong or weak labor market orientation. Following Scarletti (2009), we sort graduates' areas of study into those with a *strong* labor market orientation when they lead to a particular profession. Examples are medicine and architecture, since nearly all medical students become doctors and most students of architecture work as architects

[13] When using the alternative thresholds 60/40 and 70/30 we find similar results. See Tables A.3.4 and A.3.5 in the appendix.

later in life. In contrast, study areas with a *weak* labor market orientation do not necessarily lead to a particular profession. They teach more general skills that qualify graduates for a wide range of different jobs. Examples are history, philosophy, and languages.[14]

The estimates in panels A, B and C in Table 4.9 do not point toward heterogeneous treatment effects of internship experience by gender, parental background, or high school performance. In contrast, the point estimates in panels D and E suggest that internships are particularly beneficial for students with lower levels of labor market orientation. For example, the IV estimates in Panel D, column 5 (*Sample II*) report returns of around 10 percent for students, for whom labor market aspects did not play an important role in their choice of what to study compared to only 1 percent for those who took labor market aspects strongly into consideration. The difference of 9 percentage points is statistically significant from zero at the 10 percent level, as indicated by the p-value of 0.066 from the interacted model. In line with this finding, the estimates in panel E in Table 4.9 also point toward higher returns of internship experience for graduates in areas of study with a weak labor market orientation. For *Sample II*, the difference is again significant at the 10 percent level. We conclude that those who benefit most from internship experience are individuals with a weaker labor market orientation. One explanation for this is that internships help students to develop a better understanding of their future occupation and a clearer concept of their own preferences. Moreover, for graduates in subjects with a weak labor market orientation, internships can help to establish contacts with potential employers, which may facilitate the screening of candidates when the subject of studies is not a strong signal.[15]

4.7 Transmission Mechanisms

In this section, we examine potential transmission mechanisms by which internships may affect wages. Table 4.10 presents OLS and IV estimates of the effect of internship experience on various measures of job matching, type of employment, occupation, job position, and doctoral studies, all of which are potential intermediary variables for positive effects on wages later in life.

Panel A in Table 4.10 sheds light on the match between a person's academic qualification and the job requirements. Respondents are asked: "Does your job match your

[14] Compare Table A.3.6 in the appendix for a complete classification of areas of study into weak and strong labor market orientation.

[15] In unreported regressions, we also distinguished between students who graduated from a university versus a university of applied sciences. Studies at universities of applied sciences are more practically oriented and the treatment effect of internship experience might therefore differ by the type of university degree. The regression results did not point toward heterogeneous effects.

academic qualifications in terms of: (1) your occupational status, (2) the level of tasks assigned to you, (3) your degree?" Answers can be given on a five-point scale, ranging from 1 "No, not at all" to 5 "Yes, definitely". We generate dichotomous outcome measures for job matching which are equal to one if graduates tick a four or five on the five-point scale, and zero otherwise.[16] The summary statistics in Table 4.1 reveal that—using these measures—73 percent of graduates say that their job matches their academic qualification in terms of the occupational status, 72 percent report a match in terms of the level of tasks assigned to them, and 66 percent in terms of academic degree. The match appears to be slightly better for former interns. Indeed, OLS estimates in Panel A in Table 4.10 suggest that a student internship experience during studies is associated with a 3-4 percentage point increase in the probability of reporting a good or very good job match in terms of occupational status and in terms of the level of tasks assigned to them. However, the corresponding IV estimates are considerably lower in magnitude and not statistically different from zero at conventional significance levels. This is also true for the third job matching outcome variable.[17] The IV estimates for employment outcomes in panel B in Table 4.10 suggest that internship experience increases the probability of being in full-time employment at the time of the interview by around four percentage points, but has no positive effect on the likelihood of having a permanent position. Further, panel C and D show that there are no positive effects on the likelihood of working as a civil servant, being employed, being self-employed, or being in middle or upper management position. However, the results in panel E suggest that part of the positive impact of internship experience may stem from a lower likelihood of continuing higher education with doctoral studies: the IV results indicate that internship experience decreases the probability of starting and completing doctoral studies by about 4 and 3 percentage points, respectively.

Another topic of interest is how internship experience affects these transmission variables over time, specifically during the first years after graduating from university. We use calendar information in the surveys to construct binary activity indicators for every month during the first five years after graduating from university. Monthly information is available for employment, unemployment, full-time employment, and doctoral studies. Figure 4.2 graphically displays the estimated coefficients of internship experience for these activities from OLS and IV regressions. The vertical bars represent the 95 percent confidence intervals. Panel A in Figure 4.2 displays the effects of internship experience on

[16] In unreported regressions, we also estimated the effects on the original five-point scale variables. The estimates were in line with those reported here.

[17] Graduates were also interviewed about how satisfied they are in various domains of their current job (the content of their work, the working conditions, and whether the job is in line with their qualifications). We also investigated the effects of internship experience on these items of satisfaction. Consistent with the estimates in Table 4.10, none of the estimated IV coefficients pointed toward an improvement in job match quality.

the probability to be employed. While there are no significant effects during the first two years, later years exhibit positive coefficients, though significant at the five percent level only during the third year. Panel B reports estimates on the likelihood to be unemployed. The graph reveals that internship experience decreases the risk of being unemployed during the first year. However, in later years, this effect levels off to nearly zero and becomes insignificant in most regressions. Panel C in Figure 4.2 shows the results for being in full-time employment. This indicator is only defined for employed individuals in the respective month. The coefficients are positive and mostly significant, confirming the findings from Table 4.10. Finally, panel D reports for every month whether the individual is currently enrolled in doctoral studies. In line with the above findings, internship experience decreases the likelihood of engaging in doctoral studies over the whole time span.

Overall, the findings in Table 4.10 and Figure 4.2 suggest that the positive effect of internship experience on wages likely stems from graduates' educational and employment choices. Indeed, when plugging the variables that we identified as being intermediary outcomes—full-time employment, started and completed PhD, months in employment and unemployment—as additional controls into the wage equation (4.1), the premium of internship experience shrinks considerably. Table 4.11 shows that estimating this over-specified model cuts the OLS coefficient roughly by half. Similarly, the IV coefficient is reduced to a value near 2 percent and no longer retains its significance. This confirms our belief that most of the wage effect is driven by these intermediary variables.[18]

4.8 Robustness Checks

In this section, we first discuss alternative instrumental variable estimations to evaluate the robustness of the main findings in Table 4.3. Thereafter, we present sensitivity checks with respect to sample attrition, clustering, time-trends, and additional explanatory variables.

Table 4.12 presents results from five alternative instrumental variable estimations, together with the corresponding first-stage estimates and F-statistics. The first alternative instrument IV_{50} is an indicator variable equal to one if the majority of students of a certain graduate cohort and department (i.e. area of study at a specific university) say that an internship was mandatory, and zero otherwise. This instrument measures the strength of students' exposure to mandatory internships at the departmental level.[19] Similarly, the in-

[18] Note that the variable "Ever started a PhD" has a negative effect on wages, because it captures all individuals who have dropped out of doctoral studies or who have not finished them by the time of the interview. In contrast, successful PhDs can expect wage gains as indicated by the variable "Completed PhD".

[19] For a similar approach, see Parey and Waldinger, 2011, who use exposure to scholarships of the ERASMUS program to instrument study stays abroad.

struments IV_{60} and IV_{70} are dichotomous variables equal to one if the majority of students (e.g., 60 or 70 percent, respectively) report that a student internship was mandatory, and zero if fewer than 40 or 30 percent, respectively, do so.[20] The fourth instrument IV_{Ratio_1} measures the proportion of graduates for whom an internship was mandatory. Similar to the first three instruments, it is defined for cells that are constructed from combinations of cohorts × departments. The fifth instrument IV_{Ratio_2} also measures the proportion of graduates with a mandatory internship, but it is based on cells that are constructed from combinations of university starting years × departments. Using the year when individuals entered university rather than the graduation year may improve the precision of the instrument in the sense that it is more likely to capture different study regulations. In most cases, study regulations are imposed on students at the beginning of their studies.

Panel A in Table 4.12 shows the results of alternative IV regressions based on *Sample I*. The point estimates suggest positive returns on internship experience of between 6-11 percent. However, only the estimates in columns 1 and 5 are precisely estimated at the 10 percent and 5 percent significance levels, respectively. Note that the first-stage relationships are less precisely estimated than in our main instrumental variable regression, with the F-statistics ranging between 15 and 24. The IV estimates for the larger *Sample II* are displayed in panel B. They suggest positive wage returns of internship experience of around 9-13 percent. With the exception of the coefficient in column 1, the point estimates are statistically significant at the 5 percent level. Taken together, the estimates in Table 4.12 strongly support the main findings in Table 4.3, suggesting that student internships have a positive causal impact on wages later in life.

Table 4.13 reports the results of further sensitivity analyses based on the full model specification similar to the regressions in Table 4.3. First, we consider the fact that certain departments might differ in educational quality, connections to firms, or degree of support provided to students in finding high-quality jobs. To control for these potential differences, panel A in Table 4.13 reports the estimates when controlling for a maximum set of department fixed effects. These fixed effects are added to the full model specification, which already comprises area of study and university fixed effects. Hence, there might be the risk that this model is overspecified. It turns out that the coefficients for internship experience decrease, suggesting positive returns of around 4-5 percent.

Second, the regressions always control for whether students studied at a university or a university of applied sciences. However, there might be differences in labor market returns for the same area of study across the two types of universities. For example, studying

[20]Note that areas of study in which 40-60 percent (or 30-70 percent) of graduates say that an internship was mandatory are excluded from the regressions, resulting in smaller sample sizes in columns 2 and 3 in Table 4.12.

economics might differ in terms of quality or labor market returns between universities and universities of applied sciences. To address this concern, the regressions in panel B in Table 4.13 additionally include fixed effects for combinations of area of study and type of university. Reassuringly, the estimates do not change notably.

Third, there is the risk that the returns on internship experience are confounded by other forms of practical work experience. For instance, 38 percent of graduates say that they completed a "practical semester", that is, a semester spent working and not completing coursework (86 percent of whom are graduates of a university of applied sciences), and 48 percent report paid employment during the course of their studies that was related to their degree. Moreover, the requirement to complete an internship might affect whether students pursue other forms of work experience, which might be substitutes or complements for internships. The regressions in panel C therefore control for whether graduates completed a "practical semester", and the estimates in panel D in Table 4.13 also include a dummy variable for whether graduates worked during the course of their studies. The point estimates for internship experience remain largely unaffected.

Fourth, panel E reports the results when clustering at the area of study level, rather than at the university level. The standard errors are nearly identical to those in Table 4.3 and the overall conclusions do not change. Finally, sample attrition might be a problem, as only 41 percent of individuals participating in the initial survey were also interviewed in the follow-up survey. To address this concern, panel F reports estimates of internship experiences on wages only measured at the time of the initial survey, i.e., around one year after graduation. The estimates also point toward positive effects of internship experience on wages of around six percent. We therefore argue that the main findings are unlikely to be biased by selected sample attrition.

4.9 Conclusions

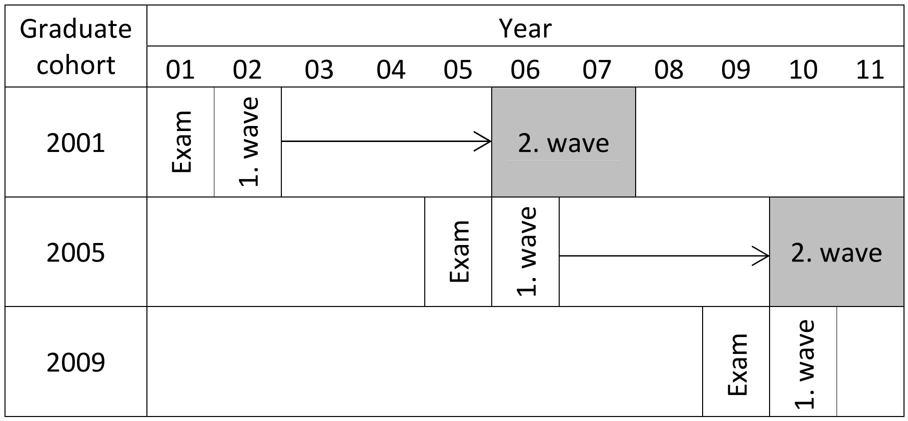
This study provides new evidence on the causal effects of student internships on wages for university graduates. It also investigates potential mechanisms, such as job matching and graduates' educational and occupational choices, which are likely to influence, or to be correlated with, wages later in life. The estimates from instrumental variable regressions suggest that work experience gained through student internships increases wages by around six percent five years after graduation. The positive returns are likely to be driven by a higher propensity to work full-time, and a lower likelihood to continue education by pursuing doctoral studies. Further, the empirical findings suggest that graduates who completed an internship face a lower risk of unemployment during the first years of their careers. However, there is little evidence that internships improve job matching, or impact on graduates' occupational choices. The positive returns are similar in magnitude for female and male graduates, and for students from universities and universities of applied science. There is also no empirical evidence of heterogeneous effects by students' socio-economic background and ability, proxied by their parents' educational attainments and students' average final high school grade, respectively. However, we do find significant differences in treatment effects with respect to the labor market orientation of students and areas of study. Highest returns are estimated for a weak labor market orientation, which is in line with the notion of internships serving as a means of vocational exploration and screening.

The present findings are of interest for university students, policy makers, and educators alike. Student internship experience can be regarded as a "door opener" to the labor market. In recent decades, much debate in higher education has centered on what are believed to be contradictory goals: on the one hand, the aim of incorporating labor market demands into the curricula of higher educational institutions and on the other hand, that of guaranteeing freedom and independence in academic research and teaching. Our study suggests that university education—combined with practical learning through internships—might be one way of bringing these two aspects together.

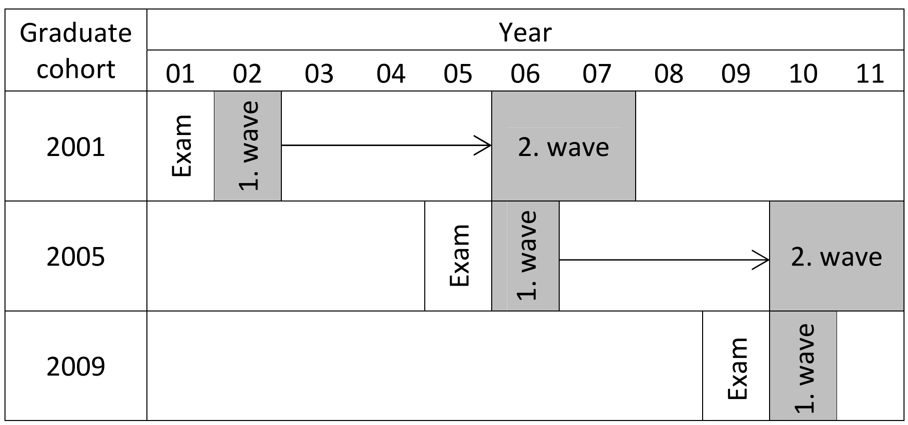
Figures and Tables

Figure 4.1: DZHW Panel Survey of Graduates

(a) Sample I

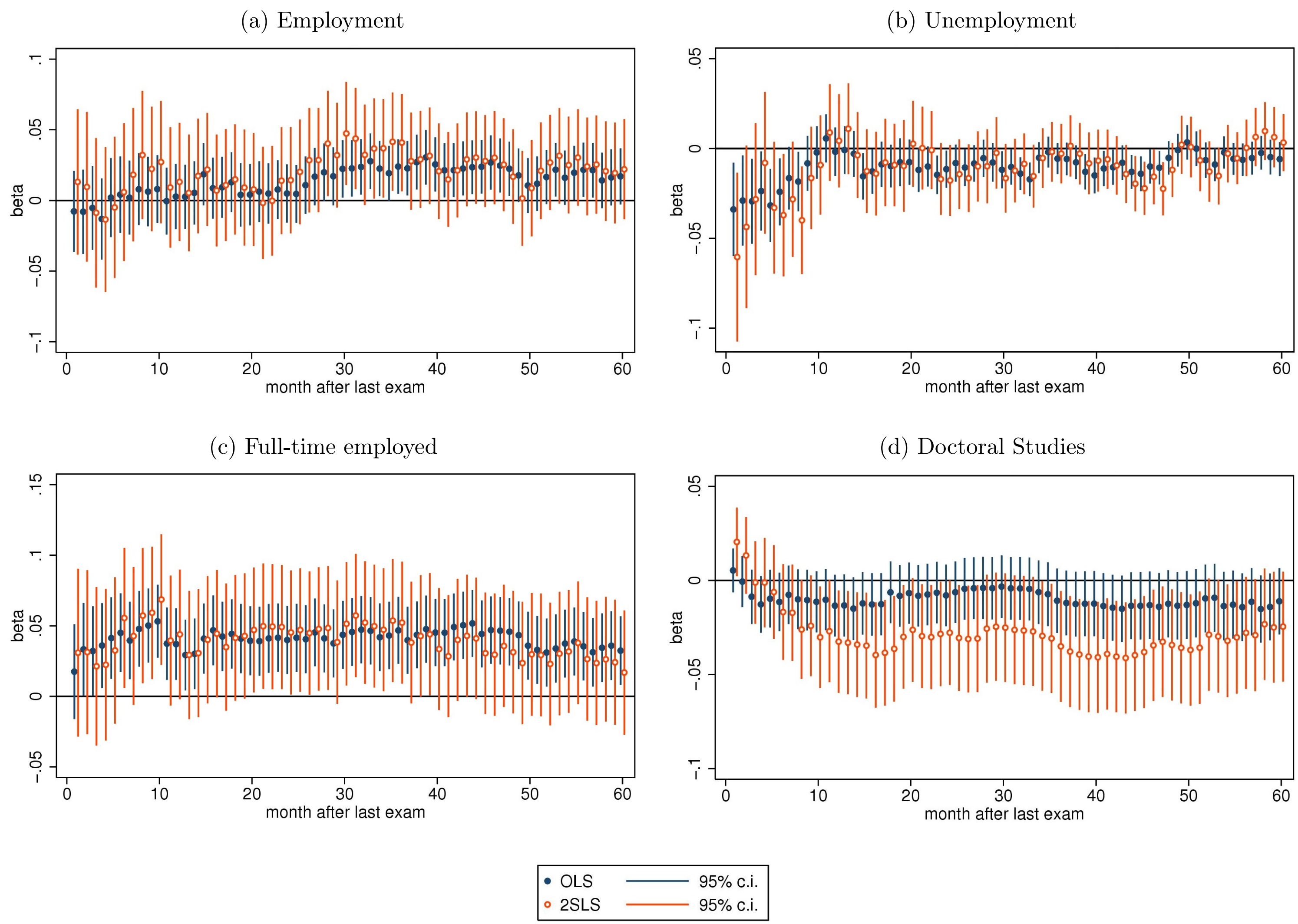


(b) Sample II



Note: Adopted from Rehn et al. (2011), p. 367. This study employs data from graduate surveys conducted by the Centre for Research on Higher Education and Science Studies. It includes random samples of university graduates who passed their last exam in 2001, 2005, and 2009. For the cohorts 2001 and 2005, we utilize an *initial survey* one year after graduation (first wave) and a *follow-up survey* about five years after graduation (second wave). For the cohort 2009, only the first wave is available. For the analysis, we use two different combinations of the data, as indicated by the shaded areas: *Sample I* comprises only the second wave observations of the graduate cohorts 2001 and 2005. *Sample II* is a pooled sample. It comprises the second-wave observations and all first-wave observations of the cohorts 2001, 2005, and 2009.

Figure 4.2: Transmission Variables over Time



Note: Estimates from OLS and 2SLS regressions for the effect of internship experience on binary variables indicating monthly status activity. Each circle represents the coefficient for one particular month. Vertical spikes stand for the 95% confidence intervals. All models control for gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation.

Table 4.1: Summary Statistics (*Sample I*)

		Cohort		Internship	
	All	2001	2005	No	Yes
Panel A. Explanatory variables					
Year of birth	1976	1974	1978	1976	1976
Female	0.54	0.56	0.52	0.50	0.56
Apprenticeship	0.30	0.31	0.30	0.42	0.25
High school grade	2.22	2.20	2.24	2.24	2.21
Labor market orientation[a]	2.88	2.69	3.04	2.77	2.93
Mother has upper secondary school degree	0.36	0.33	0.39	0.31	0.39
Father has upper secondary school degree	0.49	0.48	0.51	0.42	0.53
University of applied sciences	0.40	0.39	0.41	0.60	0.31
Panel B. Labor market variables					
Log wages	8.06	8.05	8.06	8.04	8.06
Job match: occupational status	0.73	0.72	0.74	0.71	0.73
Job match: level of tasks	0.72	0.72	0.73	0.71	0.73
Job match: degree	0.66	0.64	0.68	0.66	0.66
Employee	0.88	0.88	0.87	0.88	0.88
Civil servant	0.02	0.01	0.03	0.02	0.02
Self-employed	0.09	0.09	0.09	0.08	0.09
Upper management	0.08	0.07	0.09	0.07	0.09
Middle management	0.42	0.42	0.42	0.41	0.43
Full-time employed	0.85	0.85	0.85	0.85	0.85
Permanent position	0.70	0.70	0.70	0.71	0.69
Number of individuals	6,424	3,042	3,382	2,146	4,278

Note: DZHW graduate surveys 2001 and 2005. *Sample I* according to Figure 4.1a. [a] The variable "labor market orientation" measures how important labor market aspects were with respect to study choice, measured on a five-point scale with 5 indicating "very important" and 1 "unimportant".

Table 4.2: Summary Statistics (*Sample II*)

	All	Cohort 2001	2005	2009[a]	Internship No	Yes
Panel A. Explanatory variables						
Year of birth	1977	1974	1978	1982	1976	1977
Female	0.53	0.55	0.52	0.53	0.48	0.55
Apprenticeship	0.30	0.31	0.31	0.26	0.42	0.24
High school grade	2.23	2.21	2.25	2.24	2.26	2.22
Labor market orientation[b]	2.91	2.71	3.07	2.92	2.81	2.96
Mother has upper secondary school degree	0.37	0.34	0.39	0.42	0.32	0.40
Father has upper secondary school degree	0.50	0.49	0.51	0.52	0.44	0.54
University of applied sciences	0.41	0.40	0.43	0.35	0.59	0.31
Panel B. Labor market variables						
Log wages	7.72	7.83	7.69	7.48	7.72	7.71
Job match: occupational status	0.19	0.16	0.20	0.17	0.18	0.19
Job match: level of tasks	0.18	0.15	0.19	0.17	0.17	0.18
Job match: degree	0.20	0.19	0.20	0.18	0.19	0.20
Employee	0.84	0.91	0.79	0.85	0.85	0.84
Civil servant	0.03	0.01	0.04	0.05	0.02	0.03
Self-employed	0.09	0.06	0.11	0.08	0.08	0.09
Upper management	0.04	0.04	0.04	0.03	0.04	0.03
Middle management	0.34	0.33	0.34	0.33	0.35	0.33
Full-time employed	0.74	0.77	0.72	0.67	0.72	0.74
Permanent position	0.55	0.63	0.50	0.46	0.56	0.54
Number of individuals	13,630	4,874	6,117	2,639	4,618	9,012
Number of observations	19,218	7,590	8,989	2,639	6,486	12,732

Note: DZHW graduate surveys 2001, 2005 and 2009. *Sample II* according to Figure 4.1b. [a] Data only from the first wave. [b] The variable "labor market orientation" measures how important labor market aspects were with respect to study choice, measured on a five-point scale with 5 indicating "very important" and 1 "unimportant".

Table 4.3: The Effect of Student Internship Experience on Log Wages

	Sample I				*Sample II*			
	OLS		IV		OLS		IV	
	Parsim.	Full	Parsim.	Full	Parsim.	Full	Parsim.	Full
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Internship	0.063***	0.061***	0.056*	0.055*	0.061***	0.061***	0.064**	0.065***
	(0.014)	(0.015)	(0.025)	(0.025)	(0.011)	(0.011)	(0.019)	(0.020)
Female	-0.206***	-0.209***	-0.206***	-0.209***	-0.167***	-0.172***	-0.167***	-0.173***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.011)	(0.011)	(0.011)	(0.011)
University of applied sciences	-0.058	-0.057	-0.059	-0.058	-0.033	-0.043	-0.033	-0.042
	(0.047)	(0.047)	(0.047)	(0.047)	(0.036)	(0.035)	(0.036)	(0.035)
Apprenticeship		0.047**		0.047**		0.079***		0.079***
		(0.016)		(0.016)		(0.013)		(0.013)
High school grade		-0.003**		-0.003**		-0.003***		-0.003***
		(0.001)		(0.001)		(0.001)		(0.001)
Labor market orientation		0.031***		0.031***		0.037***		0.037***
		(0.005)		(0.005)		(0.004)		(0.004)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Area of study FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Degree type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
University FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parental schooling FE		Yes		Yes		Yes		Yes
Follow-up survey FE					Yes	Yes	Yes	Yes
Adjusted R^2	0.281	0.287	0.281	0.287	0.329	0.336	0.329	0.335
Number of observations	6,424	6,424	6,424	6,424	19,218	19,218	19,218	19,218

Note: The dependent variable is log(wage). For *Sample I* (columns 1-4), standard errors are clustered on the level of universities. For *Sample II* (columns 5-8), clustering is on the individual level as some individuals enter the data with more than one observation. The latter models additionally control for a dummy indicating whether the observation is from the second wave. $^+$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 4.4: First-Stage Results

	Sample I		*Sample II*	
	Parsim.	Full	Parsim.	Full
	(1)	(2)	(3)	(4)
Mandatory internship	0.581***	0.574***	0.567***	0.560***
	(0.015)	(0.015)	(0.008)	(0.008)
Female	0.016	0.022^{+}	0.020**	0.025**
	(0.012)	(0.012)	(0.008)	(0.008)
University of applied sciences	-0.133**	-0.119**	-0.089**	-0.077**
	(0.045)	(0.043)	(0.028)	(0.027)
Apprenticeship		-0.080***		-0.084***
		(0.016)		(0.010)
High school grade		-0.001		-0.001
		(0.001)		(0.001)
Labor market orientation		0.020***		0.019***
		(0.004)		(0.003)
Cohort FE	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes
Area of study FE	Yes	Yes	Yes	Yes
Degree type FE	Yes	Yes	Yes	Yes
University FE	Yes	Yes	Yes	Yes
Parental schooling FE		Yes		Yes
Follow-up survey FE			Yes	Yes
Adjusted R^2	0.465	0.471	0.461	0.467
F-statistic	38.244	37.806	70.412	69.420
Number of observations	6,424	6,424	19,218	19,218

Note: The dependent variable is equal to one if a graduate completed an internship during the course of studies, and zero otherwise. Standard errors in parentheses. $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 4.5: Variation in Mandatory Internships over Time, by Department (*Sample I*)

Row	Mandatory internship in cohort 2001	Mandatory internship in cohort 2005	Threshold I (50/50) Departments	Threshold I Cohort 2001	Threshold I Cohort 2005	Threshold II (60/40) Departments	Threshold II Cohort 2001	Threshold II Cohort 2005	Threshold III (70/30) Departments	Threshold III Cohort 2001	Threshold III Cohort 2005
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
1	0	1	64	232	140	49	194	96	36	153	53
2	1	0	53	122	227	32	64	125	14	7	54
3	0	0	101	407	508	90	357	407	72	308	321
4	1	1	116	549	517	89	461	437	80	434	399
5	-	0	203	0	1,013	196	57	935	174	92	758
6	-	1	191	0	977	169	72	850	147	80	730
7	0	-	231	955	0	225	862	88	208	719	154
8	1	-	190	777	0	175	682	87	167	638	132
9	-	-	0	0	0	124	193	357	251	611	781
			1,149	3,042	3,382	1,149	3,043	3,382	1,149	3,042	3,382

Note: Departments are constructed as unique combinations of areas of study and universities. Columns (1) and (2) indicate whether a majority of students (defined by thresholds I, II and II) report internships to be obligatory or not at the level of graduation cohort and department. Departments in rows 1 and 2 are defined as introducing or abolishing mandatory internships, respectively. In rows 3 and 4, departments did not introduce or abolish mandatory internships. For the remaining rows a dash indicates that the department is not observed in one cohort or that the share of reported mandatory internships lies out of the thresholds (i.e., between 40-60 for Threshold II and between 30-70 for Threshold III).

Table 4.6: DiD Estimates of Introducing Mandatory Internships on Quality Indicators (*Sample I*)

Comparison group:	Departments without a change in mandatory internship		Departments without a mandatory internship	
Controls:	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Overall quality of education:				
Structure of the study program	-0.059	0.001	-0.086	0.002
	(0.111)	(0.130)	(0.120)	(0.149)
State-of-the-art methods taught	-0.044	0.174	-0.033	0.220
	(0.108)	(0.135)	(0.116)	(0.158)
Up-to-date education[a]	-0.067	0.030	-0.146	-0.007
	(0.123)	(0.149)	(0.133)	(0.172)
Educational media and infrastructure:				
Availability of literature in the library	-0.001	0.093	-0.081	0.062
	(0.129)	(0.155)	(0.144)	(0.186)
Access to IT services (internet, databases)	-0.032	0.046	0.023	0.198
	(0.112)	(0.136)	(0.122)	(0.161)
Laboratory facilities	-0.640[c]	0.051[c]	0.024[d]	0.234[d]
	(0.137)	(0.169)	(0.151)	(0.203)
Training:				
Oral presentation training	-0.134	-0.016	-0.124	0.101
	(0.146)	(0.180)	(0.154)	(0.210)
Writing skills training	-0.238^{+}	-0.323^{+}	-0.288^{+}	-0.398^{+}
	(0.141)	(0.174)	(0.152)	(0.203)
Training in foreign languages[b]	-0.055	0.038	0.024	0.192
	(0.141)	(0.166)	(0.153)	(0.200)
Career Counseling:				
Help in finding a job and starting a career	0.050	0.081	-0.022	0.114
	(0.136)	(0.167)	(0.146)	(0.194)
Availability of career counseling	0.036	0.188	-0.035	0.177
	(0.126)	(0.155)	(0.137)	(0.179)
Provision of career orientation events	0.063	0.067	-0.026	0.011
	(0.125)	(0.155)	(0.133)	(0.180)
Number of observations	2,159	2,159	1,171	1,171

Note: Estimates from DiD regressions, based on threshold I (50/50) definition of treatment. Standard errors in parentheses. No control variables in columns (1) and (3). In columns (2) and (4), the set of control variables comprises gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. The models in columns (1) and (2) comprise all individuals from rows 1, 3 and 4 of Table 4.5, for whom the outcome variable is not missing. The models in columns (3) and (4) comprise individuals only from rows 1 and 3. [a] The variable measures the actuality of education with respect to current job requirements. [b] The variable measures subject- or job-specific training in foreign languages. [c] Sample size $N = 1,723$. [d] Sample size $N = 1,108$. $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 4.7: DiD Estimates of Abolishing Mandatory Internships on Quality Indicators (*Sample I*)

Comparison group:	Departments without a change in mandatory internship		Departments with a mandatory internship	
Controls:	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Overall quality of education:				
Structure of the study program	-0.055	0.139	-0.017	0.127
	(0.111)	(0.121)	(0.116)	(0.133)
State-of-the-art methods taught	-0.011	-0.164	-0.014	-0.193
	(0.110)	(0.126)	(0.117)	(0.139)
Up-to-date education[a]	-0.232^{+}	-0.181	-0.148	-0.179
	(0.125)	(0.139)	(0.132)	(0.158)
Educational media and infrastructure:				
Availability of literature in the library	-0.072	-0.043	-0.021	0.037
	(0.129)	(0.144)	(0.133)	(0.155)
Access to IT services (internet, databases)	0.054	0.189	0.018	0.056
	(0.112)	(0.124)	(0.120)	(0.136)
Laboratory facilities	-0.034[c]	-0.030[c]	-0.070[d]	-0.142[d]
	(0.130)	(0.140)	(0.134)	(0.151)
Training:				
Oral presentation training	0.007	0.111	0.018	0.005
	(0.149)	(0.169)	(0.159)	(0.187)
Writing skills training	-0.160	-0.261	-0.119	-0.172
	(0.142)	(0.162)	(0.149)	(0.178)
Training in foreign languages[b]	0.087	0.133	0.045	0.070
	(0.145)	(0.156)	(0.150)	(0.168)
Career Counseling:				
Help in finding a job and starting a career	-0.151	-0.055	-0.080	-0.006
	(0.140)	(0.159)	(0.148)	(0.178)
Availability of career counseling	-0.200	0.012	-0.129	0.036
	(0.129)	(0.147)	(0.135)	(0.163)
Provision of career orientation events	0.007	0.146	0.089	0.269^{+}
	(0.126)	(0.145)	(0.133)	(0.161)
Number of observations	2,155	2,155	1,314	1,314

Note: Estimates from DiD regressions, based on threshold I (50/50) definition of treatment. Standard errors in parentheses. No control variables in columns (1) and (3). In columns (2) and (4), the set of control variables comprises gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. The models in columns (1) and (2) comprise all individuals from rows 2, 3 and 4 of Table 4.5, for whom the outcome variable is not missing. The models in columns (3) and (4) comprise individuals only from rows 2 and 4. [a] The variable measures the actuality of education with respect to current job requirements. [b] The variable measures subject- or job-specific training in foreign languages. [c] Sample size $N = 1,723$. [d] Sample size $N = 1,108$. $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 4.8: DiD Estimates of Introducing/Abolishing Mandatory Internships on Individual Characteristics (Threshold I: 50/50, *Sample I*)

Comparison group:	Departments without a change in mandatory internship		Departments with/without a mandatory internship	
Controls:	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Panel A. Treatment: Introduction of a Mandatory Internship				
Mother has an upper sec. high school degree	0.002	-0.002	-0.020	-0.020
	(0.056)	(0.069)	(0.060)	(0.081)
Father has an upper sec. high school degree	0.019	0.079	0.027	0.086
	(0.058)	(0.072)	(0.063)	(0.085)
Labor market orientation	0.077	-0.044	0.037	-0.097
	(0.141)	(0.177)	(0.154)	(0.210)
High school grade	0.199	-0.559	-0.122	-1.428
	(0.713)	(0.798)	(0.779)	(0.946)
Number of observations	2,353	2,353	1,287	1,287
Panel B. Treatment: Abolishment of a Mandatory Internship				
Mother has an upper sec. high school degree	0.031	0.047	0.041	0.106
	(0.058)	(0.067)	(0.062)	(0.074)
Father has an upper sec. high school degree	-0.063	-0.040	-0.078	-0.024
	(0.060)	(0.069)	(0.064)	(0.077)
Labor market orientation	0.000	0.008	0.032	0.052
	(0.147)	(0.171)	(0.155)	(0.189)
High school grade	0.868	0.087	1.053	-0.022
	(0.738)	(0.774)	(0.781)	(0.857)
Number of observations	2,330	2,330	1,415	1,415

Note: Estimates from DiD regressions, based on threshold I (50/50) definition of treatment. Standard errors in parentheses. No control variables in columns (1) and (3). In columns (2) and (4), the set of control variables comprises gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. Each row's dependent variable is omitted from the set of controls variables. Panel A: The models in columns (1) and (2) comprise all individuals from rows 1, 3 and 4 of Table 4.5, for whom the outcome variable is not missing. The models in columns (3) and (4) comprise individuals only from rows 1 and 3. Panel B: The models in columns (1) and (2) comprise all individuals from rows 2, 3 and 4 of Table 4.5, for whom the outcome variable is not missing. The models in columns (3) and (4) comprise individuals only from rows 2 and 4. For alternative threshold definitions, see Tables A.3.4 and A.3.5. $^{+}$ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table 4.9: Heterogeneous Effects

	Sample I			*Sample II*		
	OLS	IV	Obs.	OLS	IV	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Gender						
Women	0.077**	0.083+	3,448	0.075***	0.060+	10,177
	(0.028)	(0.043)		(0.017)	(0.032)	
Men	0.043**	0.042	2,976	0.053***	0.079**	9,041
	(0.016)	(0.028)		(0.025)	(0.025)	
P-value of interaction	0.312	0.513	6,424	0.229	0.885	19,218
Panel B: Parental background						
Parents with ‘low’ levels of schooling	0.052*	0.038	2,840	0.047**	0.044	8,241
	(0.021)	(0.036)		(0.016)	(0.028)	
Parents with ‘high’ levels of schooling	0.052*	0.040	3,584	0.065***	0.069*	10,977
	(0.022)	(0.035)		(0.016)	(0.029)	
P-value of interaction	0.382	0.352	6,424	0.365	0.528	19,218
Panel C: High school performance						
High school grade $<$ median	0.050*	0.066	3,246	0.069***	0.101**	9,634
	(0.022)	(0.041)		(0.017)	(0.031)	
High school grade $\geq$ median	0.066**	0.070+	3,178	0.049**	0.034	9,854
	(0.020)	(0.036)		(0.015)	(0.027)	
P-value of interaction	0.579	0.898	6,424	0.400	0.274	19,218
Panel D: Labor market orientation of student						
LM orientation $<$ median	0.072**	0.062*	3,364	0.072***	0.098***	9,818
	(0.022)	(0.031)		(0.016)	(0.028)	
LM orientation $\geq$ median	0.043*	0.034	3,060	0.041**	0.008	9,400
	(0.019)	(0.041)		(0.016)	(0.029)	
P-value of interaction	0.452	0.536	6,424	0.581	0.066	19,218
Panel E: Labor market orientation of study subject[a]						
Strong LM orientation	0.053***	0.051+	4,815	0.049***	0.050**	14,440
	(0.015)	(0.026)		(0.011)	(0.019)	
Weak LM orientation	0.066	0.165+	1,609	0.104**	0.132+	4,778
	(0.053)	(0.087)		(0.035)	(0.076)	
P-value of interaction	0.679	0.499	6,424	0.097	0.076	19,218

Note: [a] See Table A.3.6 in the appendix for a classification of areas of studies into weak and strong labor market orientation. All models control for gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. + $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table 4.10: The Effect of Internship Experience on Intermediary Variables (*Sample I*)

	OLS	IV	Number of observations
Panel A: Matching			
Job match: occupational status	0.030*	-0.004	6,404
	(0.012)	(0.024)	
Job match: level of tasks	0.035**	0.013	6,394
	(0.012)	(0.025)	
Job match: degree	0.016	-0.021	6,389
	(0.016)	(0.025)	
Panel B: Employment			
Full-time employed	0.027*	0.037+	6,040
	(0.011)	(0.019)	
Permanent position	-0.012	-0.037	6,424
	(0.014)	(0.025)	
Panel C: Occupation			
Employee	0.005	0.020	6,424
	(0.009)	(0.017)	
Civil servant	-0.004	-0.002	6,424
	(0.005)	(0.009)	
Self-employed	0.007	-0.001	6,424
	(0.008)	(0.016)	
Panel D: Job position			
Upper management	0.004	0.015	6,424
	(0.008)	(0.016)	
Medium management	0.015	0.005	6,424
	(0.015)	(0.028)	
Panel E: Doctoral studies			
Currently PhD student	-0.013	-0.010	6,423
	(0.009)	(0.012)	
Ever started PhD	-0.012	-0.037*	6,406
	(0.010)	(0.015)	
Ever completed PhD	0.001	-0.033**	6,406
	(0.009)	(0.012)	

Note: All models control for gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 4.11: Wage regressions: The Impact of Intermediary Variables

	Over-specified OLS	Over-specified IV
Internship	0.036**	0.022
	(0.012)	(0.021)
Full-time employed	0.503***	0.503***
	(0.023)	(0.023)
Ever started PhD	-0.069***	-0.069***
	(0.017)	(0.017)
Completed PhD	0.062**	0.062**
	(0.023)	(0.023)
Number of months employed	0.008***	0.008***
	(0.001)	(0.001)
Number of months unemployed	-0.008***	-0.008***
	(0.002)	(0.002)
Adjusted R^2	0.491	0.491
Number of observations	6,424	6,424

Note: The dependent variable is log(wage). The variable *months (un)employed* can take values from zero to 60. Unreported explanatory variables include gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. $^{+}$ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table 4.12: Robustness Checks I : Alternative Instruments

	IV_{50} (1)	IV_{60} (2)	IV_{70} (3)	IV_{Ratio_1} (4)	IV_{Ratio_2} (5)
	Panel A: *Sample I*				
Internship	0.112^{+}	0.088	0.057	0.088	0.088^{*}
	(0.067)	(0.072)	(0.069)	(0.065)	(0.043)
First-stage estimate	0.293^{***}	0.354^{***}	0.413^{***}	0.500^{***}	0.521^{***}
	(0.02)	(0.022)	(0.027)	(0.027)	(0.022)
F-statistic	14.65	16.37	15.41	18.36	23.56
Number of observations	6,424	5,470	4,574	6,424	6,424
	Panel B: *Sample II*				
Internship	0.085	0.121^{*}	0.151^{**}	0.113^{*}	0.125^{***}
	(0.061)	(0.051)	(0.055)	(0.044)	(0.033)
First-stage estimate	0.270^{***}	0.341^{***}	0.392^{***}	0.477^{***}	0.509^{***}
	(0.012)	(0.014)	(0.017)	(0.016)	(0.012)
F-statistic	22.44	24.61	23.66	29.57	40.76
Number of observations	19,218	16,360	13,609	19,218	19,218

Note: All models control for gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. $^{+}$ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table 4.13: Robustness Checks II: Specification and Sample Selection

	Sample I		*Sample II*	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Panel A: Department fixed effects				
Internship	0.050**	0.047	0.057***	0.043*
	(0.017)	(0.031)	(0.012)	(0.021)
	6,424	6,424	19,218	19,218
Panel B: Area of study-university type fixed effects				
Internship	0.057***	0.057*	0.058***	0.053**
	(0.015)	(0.026)	(0.01)	(0.019)
	6,424	6,424	19,218	19,218
Panel C: Practical semester				
Internship	0.065***	0.066**	0.07***	0.093***
	(0.015)	(0.025)	(0.011)	(0.021)
	6,424	6,424	19,218	19,218
Panel D: Employed during studies				
Internship	0.054***	0.049+	0.055***	0.061**
	(0.015)	(0.025)	(0.01)	(0.019)
	6,411	6,411	19,186	19,186
Panel E: S.e. clustered on department level				
Internship	0.061***	0.055*	0.061***	0.065**
	(0.014)	(0.025)	(0.011)	(0.02)
	6,424	6,424	19,218	19,218
Panel F: Only initial wave				
Internship	0.058***	0.067**		
	(0.014)	(0.024)		
	12,428	12,428		

Note: All models control for gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. Exceptions: The regression in panel A omits area of study and university FE due to the newly introduced department FE. Panel B omits area of study FE and the dummy indicating the university type. $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

5 Concluding Remarks

5.1 Summary and Conclusions

This dissertation starts with two observations about individual investments in education: The first observation is that education tremendously matters for a person's success in the labor market. The introductory remarks of this dissertation reveal that formal education is a key determinant for labor market earnings. The empirical literature draws a clear and concise relationship: The more education one attains, the higher his or her earnings. Findings also show that education serves as a safeguard against the risk of unemployment. Moreover, education is positively associated with favorable outcomes of many other indicators of labor market performance. The second observation is that investments into education are costly and their returns are risky. Human capital theory and signaling theory both postulate that the principle "the more the better" does not hold for everybody. Given the fact that the direct costs and opportunity costs of education vary by idiosyncratic characteristics, like innate ability, optimal investment is different across individuals. For given characteristics theory may provide solutions to this personal optimization problem. In practice, however, deciding upon educational attainments can still be a difficult task. The reason is that oftentimes uncertainty exists about the future benefits gained from the extra education, i.e. the returns to education. Against the background of its potential impact, knowing the returns to education is of direct significance to individuals. Policymakers also have a fundamental interest in knowing the returns to education, as this typically constitutes the rationale for designing institutions or implementing reforms and programs in the field of education.

It is this side-by-side of education's outstanding significance and uncertainty about its consequences that has spurred a large empirical literature and that also motivates this dissertation. When investigating the returns to education, the major challenge lies in eliciting causal effects. Simple correlations do typically not represent causal links due to the presence of endogeneity, which is caused, for instance, by omitted variables or self-selection into treatment. For this reason, one approach of scrutinizing involves the exploitation of "natural experiments" in combination with various estimation techniques. This is the departing point of the study at hand.

This dissertation comprises three articles that constitute distinct contributions to the empirical literature about the causal returns to education. They deal with individual decisions about attainments in secondary and tertiary education, occupational choice and absence from university to gather practical work experience. Each article fills a gap in the literature with respect to research question or method, or a combination of the two.

The first paper, included in Chapter 2, is entitled "*Estimating Heterogeneous Returns to Education in Germany via Conditional Heteroskedasticity*". It is guided by the question of how much wages are affected by one extra year of education. The results are of interest not only to individuals who have to decide upon their optimal duration of studies but also to policymakers. When implementing school reforms that affect the number of years students stay in education, or when launching programs to fight early school dropout, knowing the returns of one extra year of schooling is a crucial piece of information. I circumvent the endogeneity problem inherent to returns-to-education evaluations by using an empirical approach that bases identification on the presence of heteroskedasticity. I construct a control variable that is nonlinear, where nonlinearity stems from heteroskedasticity in the system of equations. Identification requires two conditions to be fulfilled: the variable impact property and the constant correlation condition. I employ GSOEP data and separately calculate the return to education for the subsamples of upper school track graduates and lower/intermediate school track graduates. The results show that the former group receives a wage premium for one extra year of education of more that eight percent. In striking contrast, the latter group is rewarded by only one percent. The results are in line with evidence from instrument variables studies for Germany if one takes IV's local average treatment interpretation into account. The major contribution of this paper lies in the application of a relatively new approach to a research question that has received much attraction from researchers using IV methods. Given that the source of identification—heteroskedasticity—is present along the entire distribution of one or more characteristics, the employed control function approach yields the advantage over IV methods that its estimates allow for average treatment interpretation. That is, it sidesteps IV's parameter interpretation that is bounded only to the subsample of compliers. The investigation uncovers that graduates from the upper school tracks receive significantly higher returns from extra education than graduates from the lower school track. While counterintuitive, given the belief of decreasing marginal returns, the paper argues that causality could only be established within subgroups, not between them. In other words, the results allow to draw conclusions about the returns of an extra year of education given the graduation from a secondary school track. Hence, much explanatory power applies to years spent in post-secondary education like apprenticeships or higher education. Moreover, increasing returns to education are not necessarily implausible for Germany's educational system and labor market. Lower track choice may constitute a signal of such strength that employers reward extra years in education only marginally.

Chapter 3 presents the article "*The Effects of Occupational Knowledge: Job Information Centers, Educational Choices, and Labor Market Outcomes*". This study examines the link between occupational knowledge, educational choices and labor market outcomes. The subjects under investigation are young adults in school that are about to enter the labor market. The treatment is an intensification and improvement of occupational counseling in school realized by the introduction of job information centers in Germany. The centers provide thorough information on job types, local labor market conditions and educational pathways to occupations. The key question is whether more knowledge about occupations and labor market conditions leads to better decisions of young adults with respect to education and school-to-work transition. On a more specific level, we ask the question whether the public and free provision of job market related knowledge through job information centers improves the subsequent labor market situation of treated students. For identification, we make use of the fact that agreements between school authorities and the Federal Employment Agency made class trips to JICs mandatory for every student, wherever a JIC was available. To a large extent, this rules out self-selection into treatment. The introduction was spread across years and counties, which allows estimating the treatment effect in a difference-in-difference framework. Based on self-collected data on the timing and location of JIC openings, combined with ALWA survey data and linked administrative records from SIAB, we show that individuals who visited a JIC are more likely to experience educational upward mobility, less likely to be unemployed during the first five years in the labor market, less likely to loose their job involuntarily during this time, and they less often need to move to another county in order to find employment. However, we cannot detect an effect on earnings, neither when measured shortly after finishing education nor later in life. While often taken for granted that the provision of information is beneficial for its recipients, empirical proof is scarce. Only a few studies are dedicated to this topic and causal evidence is mainly based on small-scale field experiments. Ours is the first study to investigate an informational intervention as part of a nationwide reform that covers the majority of all school students. Our results allow policy conclusions on two levels: On a general level, they show that providing labor market related information to school students pays off in terms of educational attainment, smoother transition into jobs, and greater job stability. On a more specific level, the findings suggest that the introduction of job information centers in Germany was an effective tool in providing such information. Furthermore, the size of the effects prompts an interesting detail about theory: Human capital theory and signaling theory both assume rational expectations when individuals make decisions about investments in education. Being less informed about the returns of these investments increases uncertainty but leaves average decisions unchanged. In contrast, the results of this paper suggest that students update their expectations in light of new information. We observe that the treated individuals systematically make different decisions than their untreated counterparts do. We interpret the fact that visits to JICs lead to higher educational attainments as a consequence of the students having more precise ideas about the important role of education for obtaining the desired job and education's return for future outcomes. We therefore conclude that policy intervention can modify subjective expectations and, as a consequence, individual choices.

Finally, Chapter 4 investigates the returns to practical work experience in the form of internships for students in higher education. Entitled "*Door Opener or Waste of Time? The Effects of Internships on Labor Market Outcomes*", this paper draws upon DZHW graduate surveys that contain information on both internship experience during the course of studies and labor market outcomes up to five years after graduation. In a 2SLS setup we instrument internship experience with the occurrence of mandatory internships at universities and derive earnings returns of about six percent. Closer examination reveals that these returns are driven by intermediary outcomes that reflect the process of transiting into the labor market: Former interns are more likely to work in full-time positions, they have less unemployment experience shortly after leaving university, and they are less likely to pursue doctoral studies. Interestingly, the positive wage effects are most pronounced for students and areas of studies with low labor market orientation. This finding is plausible given the notion that internships can serve as devices for vocational exploration including job screening. That is, they help shape ideas about occupations and help develop clear concepts about own preferences. Our contribution is unique in the way that it is the first empirical study that elicits the causal effects of voluntary internships. Its findings are important for individuals and policymakers alike: For individuals, knowing the pros and cons of internships during the course of studies significantly matters as engaging in practical work incurs direct costs and opportunity costs by foregone time spent in education. Moreover, there is a vital debate about whether internships produce sufficient benefits for interns given the allegation that firms may exploit interns as cheap labor. For policymakers, knowing the returns to internships is important, because introducing mandatory internships is ultimately a policy question. Mandatory internships are implemented as one measure to increase the employability of graduates. Employability has become a central objective of higher education during the course of the Bologna reform. Much debate has centered on what are believed to be contradicting goals: incorporating labor marked demands in the study curricula and maintaining the independence and liberty of education. This study suggests that university education, combined with practical learning through internships, can be one way to combine both aspects with another.

Leaving behind the particularities of the three chapters, and moving on to more general conclusions, I would like to emphasize three main messages that can be taken away from this dissertation: First, the study indicates that a careful investigation of individual returns to education is necessary, because theory does not provide sufficient guidance when weighing the costs and benefits of investments in education. Second, policy measures that target individuals at the point of transition from school or university to work are particularly sensitive. The analyses show that interventions at this point in a person's life can have remarkable impacts on later performance in the labor market. Third, the comparison between associations and causal effects emphasizes the necessity to correct for endogeneity biases in empirical investigations, otherwise policy conclusions are drawn on empirical grounds that might be substantially biased.

5.2 Limitations and Future Research

There is no study without limitations. Specifying shortcomings of an empirical approach or the limits to parameter interpretation increases transparency and prevents inaccurate conclusions. At the same time, it also lays the foundation for further research that builds and improves upon the preceding work. For this reasons, this final section is dedicated to a critical discussion of those aspects that I consider to be the greatest weaknesses of the three contributions.

To my own judgment, the major limitation of Chapter 2 lies in the interpretation of the return-to-education parameters. The reason has been alluded to above and lies in the nature of the German school system, particularly in the system of early tracking. Calculating the effect of an extra year of education is difficult when the number of years in school is typically determined by the school track. Moreover, school types not only differ by the length of schooling but also by the quality of education, thus making comparisons across school types less meaningful. One way to alleviate this problem is to run separate regressions for different school types. Another option is to enhance the schooling measure by post-secondary educational activities, arriving at the utilized measure of years in education. Both strategies were realized in this study. Yet, the new measure "years in education" comprises both years in schooling and years in tertiary education. In a future paper, it would be worthwhile to disentangle the two by investigating the contribution of secondary and tertiary education separately and the role their reciprocity for overall returns.

The biggest shortcoming of Chapter 3 lies in the lack of knowledge about the transmission mechanisms between the input in form of the provision of job related information and the output in form of individual observable choices. Like a black box the data provides no direct information on how the informational intervention changed the mindset of students and how this translates into different actions. We study some aspects of the transmission phase from education to work, but leave other aspects aside. As a remedy, in a future paper I plan to more closely examine how visits to job information centers affect the actual decision in favor or against occupations. More specifically, I will investigate how job knowledge fosters the independence of occupational gender roles by estimating the effect of job information centers on the likelihood to choose an occupation that is atypical for one's own gender.

Finally, regarding Chapter 4, a major weak point lies in missing information on internships' number, quality, duration and timing within the course of studies. All of these aspects can influence the benefits of internships. For example, we expect that contact to employers through internships is most beneficial shortly before graduating from university and least beneficial at the beginning of one's studies. Similarly, unobserved differences in the quality of internships may constitute signals of different value to future employers. In our study we have to assume that these characteristics are uncorrelated with the occurrence of mandatory internships in order to maintain identification. In a future study, one could directly test this assumption by drawing upon an alternative data set, namely the Bavarian Graduate Panel. This panel data is very similar to the presently employed data, but contains information on the number, length and timing of internships. Using this data certainly involves the loss of generality, because it covers graduates only from a single state of Germany and has fewer observations, but it could help to make the analysis more convincing by controlling for unobservable characteristics of internships.

Appendices

A.1 Appendix of Chapter 2

A.1.1 The Klein and Vella (2010) Approach

Klein and Vella's 2010 approach starts with the controlled function

$$W = X\beta + \delta S + \lambda v + e. \tag{A.1.1}$$

Its endogeneity parameter λ comes from the errors equation $u = \lambda v + e$ and can be decomposed as following:

$$\lambda = \frac{\text{cov}(u,v)}{\text{var}(v)} = \frac{\text{cov}(u,v)}{\sigma_u \sigma_v}\frac{\sigma_u}{\sigma_v} = \rho\frac{\sigma_u}{\sigma_v} \tag{A.1.2}$$

where σ_j, $j = u, v$, denotes the standard deviations of the error terms u and v, and $\rho = \text{cov}(u,v)/\sigma_u\sigma_v$ is the correlation coefficient between them. Next assume that the errors are heteroskedastic: Their distributions—given by the standard deviations σ_j—are a function of X_j. Let $H_j(X_j)$ be such heteroskedasticity function. The control term impact now becomes $\lambda(X_u, X_v) = \rho_{uv}\left[H_u(X_u)/H_v(X_v)\right]$ and is no longer constant. Plugging this conditional variant of λ into (A.1.1) yields the final estimation equation:

$$W = X\beta + \delta S + \rho\frac{H_u(X_u)}{H_v(X_v)}\hat{v} + \varepsilon. \tag{A.1.3}$$

To support the plausibility of the *constant correlation condition* (CCC), let us decompose the errors to a multiplicative structure. The terms u^* and v^* represent the unscaled parts of the errors with constant variance. $H_j(X_j)$ represents the heteroskedastic parts of the errors

$$u = H_u(X_u)u^* \tag{A.1.4}$$

$$v = H_v(X_v)v^*. \tag{A.1.5}$$

The correlation coefficient ρ can now be written as

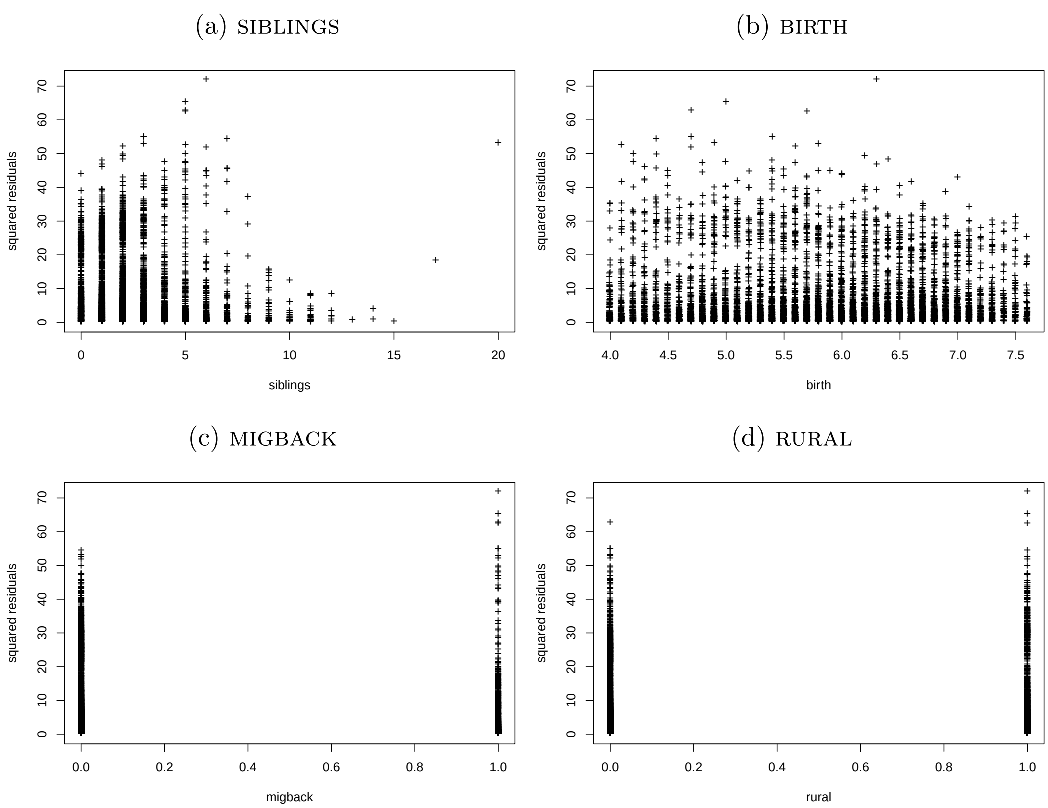
$$\rho = \frac{\mathrm{cov}(u, v)}{H_u(X_u)H_v(X_v)} = \frac{H_u(X_u)H_v(X_v)\mathrm{cov}(u^*, v^*)}{H_u(X_u)H_v(X_v)} = \mathrm{cov}(u^*, v^*) \tag{A.1.6}$$

This reformulation shows that the CCC depends on a constant degree of endogeneity across X *independent* of the presence of heteroskedasticity.

KV 2009b show that identification can be established also under a more general error structure than in A.1.4 and A.1.5 that includes an unobserved common error component ω, such that $u = H_u(X_u)\omega u^*$ and $v = H_v(X_v)\omega v^*$. In this setting ω can be interpreted as unobserved ability. It could also represent measurement error. Thus if the identifying assumptions are satisfied, the estimator is also consistent in the presence of measurement error of the dependent variable.

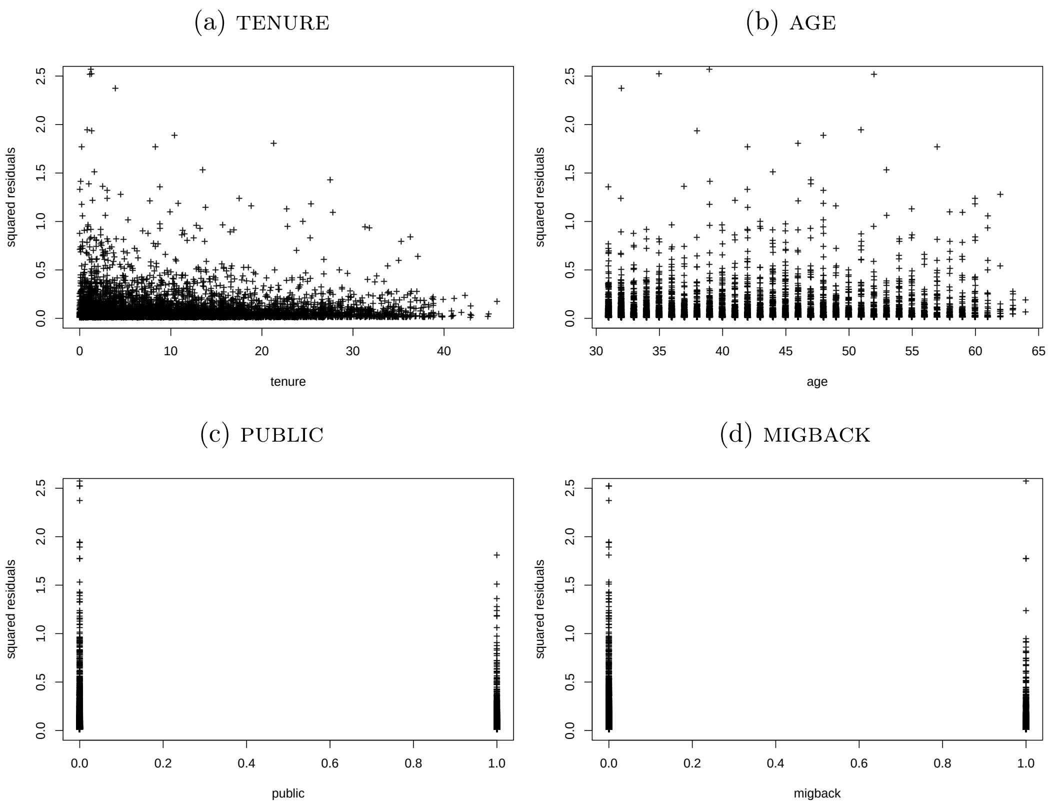
A.1.2 Graphical Analyses of Heteroskedasticity

Figure A.1.1: Graphical Analysis of Heteroskedasticity
in Education Equation



Note: Entire sample. Squared residuals v^2 from OLS of equation (2.2) on the vertical axes.

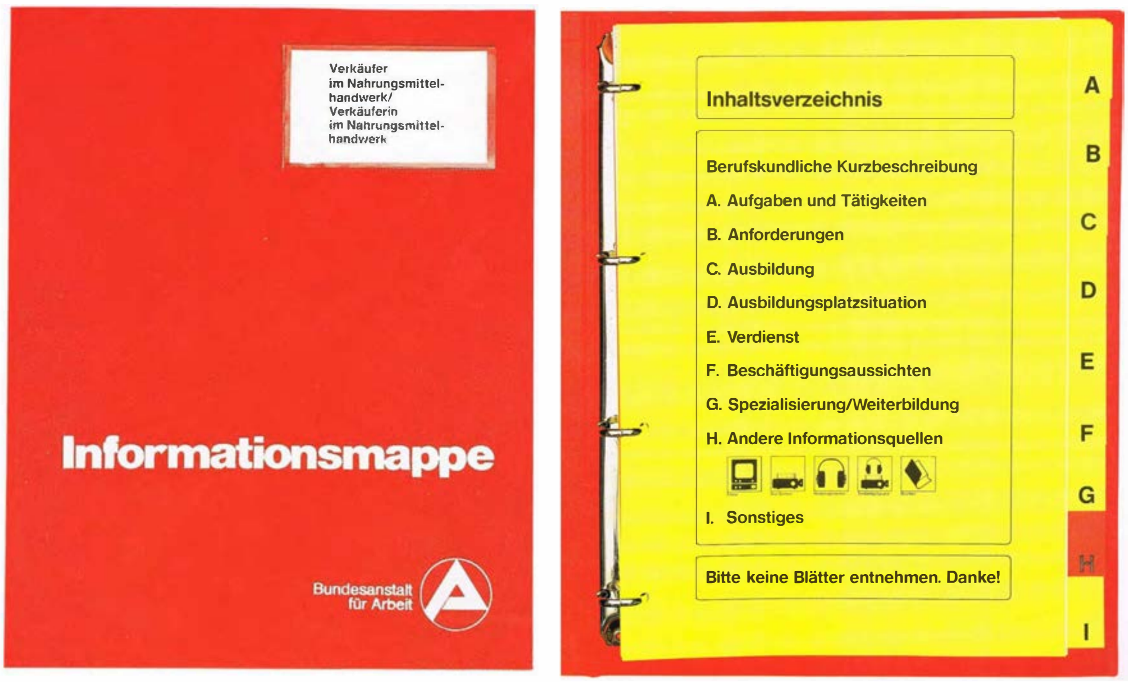
Figure A.1.2: Graphical Analysis of Heteroskedasticity in Wage Equation



Note: Entire sample. Squared residuals u^2 from last iteration of estimating equation (2.1) on the vertical axes.

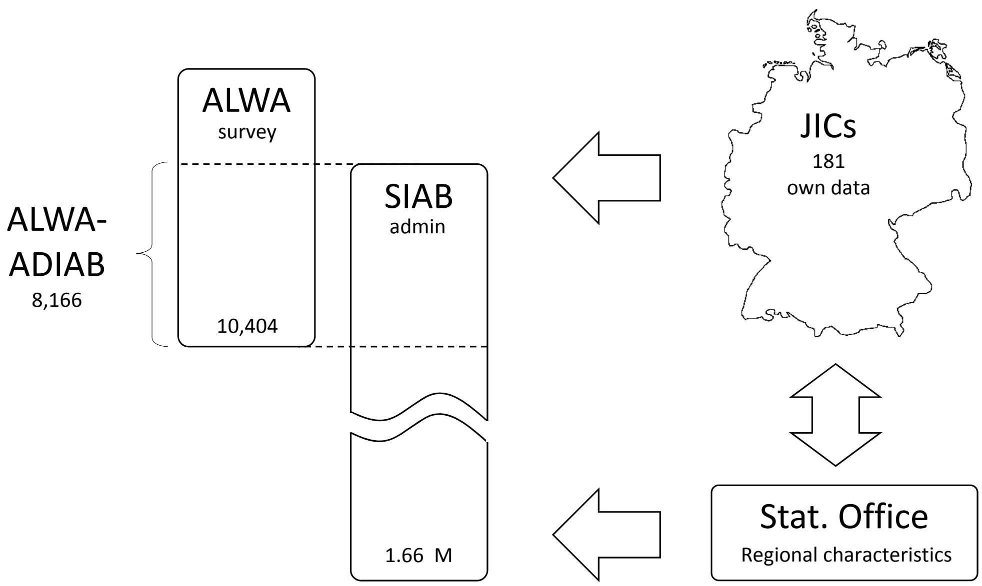
A.2 Appendix of Chapter 3

Figure A.2.3: Example of a Content Page from an Information Folder



Note: Weitzel (1987), p. 8. Right-hand page translated into English: Content Page. Short description of the occupation. A. Duties and Tasks; B. Requirements; C. Education; D. Availability of Traineeships; E. Income; F. Employment Prospects; G. Specialization and Advanced Training; H. Other Information Media; I. Miscellaneous. Please do not remove pages. Thank you.

Figure A.2.4: Data Sources



Note: The treatment variable is constructed using the ALWA data and own data about the location and opening time of job information centers. The outcome variables *daily wages first job* and *daily wage at age 35* use entries from SIAB that were matched with the ALWA at the individual level, forming the ALWA-ADIAB subsample of 8,166 observations. All regressions control for the population density of the district of potential treatment in the year 1995. This variable stems from administrative data from the Federal Statistical Office. In the survival analysis regressions, regional characteristics from this source are combined with the JIC data, and with further regional characteristics generated from SIAB. Finally, the SIAB provides the data base for the common trend graphs.

Table A.2.1: Description of Variables

Name	Description	Data source
Dependent variables		
	Education and Educational Mobility	
	Highest general school degree attained is a ...	
Low-track degree	low-track school track degree [1/0]	ALWA
Intermediate-track degree	intermediate-track school track degree (*Mittlere Reife*) [1/0]	ALWA
Upper-track degree	upper-track school degree (*Abitur*) [1/0]	ALWA
Upward mobility	Received a school degree from a track higher than the track attended at time of potential treatment [1/0]	ALWA
University degree	Received a degree from higher education [1/0]	ALWA
	Labor Market Attachment	
	During first five years after finishing formal education...	
Incidence part-time	... $\geq$ one month of part-time employment [1/0]	ALWA
Incidence full-time	... $\geq$ one month of full-time employment [1/0]	ALWA
Incidence unemployment	... $\geq$ one month of unemployment [1/0]	ALWA
Duration part-time	... number of months of part-time employment	ALWA
Duration full-time	... number of months of full-time employment	ALWA
Duration unemployment	... number of months of unemployment	ALWA
	Job Search and Job Matching	
Search duration	Number of months between last episode of formal education and first episode of regular employment	ALWA
Stayed in district	Lives in same district at time of survey as two years before graduating from school	ALWA
Stayed in state	Lives in same state at time of survey as two years before graduating from school	ALWA
Share invol. job changes	Share of employment spells that ended involuntarily during first five years after end of formal education	ALWA
	Daily Pay (Euros, logarithms)	
Pay for first job	Gross daily pay of first regular employment after finishing formal education (1995 prices)	ALWA-ADIAB
Pay at age 35	Gross daily pay at age 35 (1995 prices)	ALWA-ADIAB
Pay for last job	Monthly net pay at time of interview (2007 prices)	ALWA
Net income	Monthly total net income at time of interview (2007 prices)	ALWA
Treatment variable		
Job information center (JIC)	JIC existed in district in which individual lived in two years before graduating from school [1/0]	Own data & ALWA
Other explanatory variables		
Migrant background	Citizenship other than German [1/0]	ALWA
Female	Individual is female [1/0]	ALWA
FE school track	Dummies for school track at time of potential treatment (three groups)	ALWA
FE mother school degree	Dummies for mother's highest school degree (three groups)	ALWA
FE father school degree	Dummies for father's highest school degree (three groups)	ALWA
FE population density	Dummies for population density of the district lived in at time of potential treatment (ten groups)	Federal Stat. Office

Table A.2.2: Derivation of Potential Treatment Year

(1) Spell's school track	(2) Spell's school degree	(3) Definition of t
(a) School degree successfully obtained		
low	low, intermediate, high	graduation year - 2
intermediate	intermediate	graduation year - 2
intermediate	low	graduation year - 1
high	high	graduation year - 3
high	low, intermediate	graduation year - 1
(b) School dropouts or school track changers		
low	—	year at age 13
intermediate	—	year at age 14
high	—	year at age 16
(c) No potential treatment year derivable		
school outside Germany, unknown school type, unknown degree achieved, ongoing school spells, school not main activity, evening school		

Note: Own definition based on framework agreements between the BA and local school authorities. t = potential treatment year, low-track school=*Hauptschule*, intermediate-track school=*Realschule*, upper-track school=*Gymnasium*.

Table A.2.3: Detailed Regression Output (*Reduced sample I*)

	Low-track school degree (1)	Upper-track school degree (2)	Upward mobility (3)	University degree (4)	Unempl. incidence (5)	Unempl. duration (6)	Stayed in district (7)	Share invol. job change (8)	Wage 1st job (9)
Job information center (JIC)	-0.064+	0.069**	0.075**	0.054**	-0.101+	-0.767	0.082*	-0.079+	0.002
	(0.034)	(0.023)	(0.029)	(0.021)	(0.055)	(0.679)	(0.032)	(0.044)	(0.043)
Female	0.066**	-0.051**	-0.033*	-0.024*	0.018	0.751*	-0.031+	-0.038+	-0.204**
	(0.019)	(0.013)	(0.016)	(0.011)	(0.021)	(0.321)	(0.016)	(0.022)	(0.029)
Migrant background	-0.027	0.034	0.027	-0.022	0.085	-0.690	-0.034	0.019	-0.177+
	(0.065)	(0.041)	(0.058)	(0.014)	(0.099)	(0.933)	(0.061)	(0.098)	(0.094)
Low-track school	-0.592**	-0.089**	0.169**	-0.044**	0.073*	0.858*	0.021	0.097**	-0.036
	(0.022)	(0.014)	(0.021)	(0.009)	(0.029)	(0.417)	(0.020)	(0.029)	(0.030)
Father:									
Low-track school degree	-0.016	-0.004	-0.022	-0.004	-0.035	-0.535	0.115	0.021	0.057
	(0.070)	(0.037)	(0.072)	(0.025)	(0.042)	(0.754)	(0.074)	(0.039)	(0.058)
Intermediate-track school degree	-0.033	0.023	-0.002	-0.010	0.019	0.168	0.066	0.008	0.024
	(0.072)	(0.038)	(0.072)	(0.025)	(0.047)	(0.793)	(0.073)	(0.042)	(0.060)
Upper-track school degree	-0.067	0.109*	0.108	0.065*	(omit.)	(omit.)	0.037	(omit.)	(omit.)
	(0.078)	(0.045)	(0.076)	(0.029)			(0.076)		
Mother:									
Low-track school degree	0.257*	0.054	0.296**	0.162**	-0.397**	-1.550	-0.525**	0.120	-0.307**
	(0.113)	(0.125)	(0.087)	(0.048)	(0.059)	(3.491)	(0.064)	(0.265)	(0.061)
Intermediate-track school degree	0.271*	0.039	0.301**	0.181**	-0.403**	-1.664	-0.517**	0.156	-0.237**
	(0.113)	(0.125)	(0.088)	(0.051)	(0.067)	(3.499)	(0.066)	(0.268)	(0.071)
Upper-track school degree	0.255*	0.081	0.363**	0.192**	-0.375**	-1.698	-0.618**	0.058	-0.266**
	(0.116)	(0.128)	(0.095)	(0.048)	(0.072)	(3.596)	(0.075)	(0.266)	(0.100)
Age									0.358**
									(0.096)
Age^2									-0.007**
									(0.002)
Constant	0.837**	-0.132	-0.408**	-0.299**	0.215	0.532	0.585**	-0.096	0.193
	(0.172)	(0.153)	(0.156)	(0.068)	(0.185)	(3.958)	(0.140)	(0.289)	(0.966)
District treat FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pop density FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
adj. R2	0.362	0.055	0.093	0.065	0.080	0.052	0.244	0.007	0.258
n	2866	2866	2866	2737	2198	2198	2864		

Note: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. ALWA-ADIAB. OLS regressions. Each estimate represents the coefficient from a different regression. Standard errors (in parentheses) are clustered on the district level. All specifications control for a female and migration dummy, dummies for year of birth, dummies for district of residence and school track attended at time of potential treatment, dummies for mothers' and fathers' highest school qualification, and dummies for population density on the district level at time of potential treatment (10 groups). Based on *Reduced sample I* that only includes individuals in low- and intermediate-track schools (*Hauptschule* and *Realschule*) at the time of potential treatment.

A.3 Appendix of Chapter 4

Table A.3.4: DiD Estimates of Introducing/Abolishing Mandatory Internships on Individual Characteristics (Threshold II: 60/40, *Sample I*)

Comparison group:	Departments without a change in mandatory internship		Departments with/without a mandatory internship	
Controls:	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Panel A. Treatment: Introduction of a mandatory internship				
Mother has upper sec. high school degree	0.048	-0.061	0.036	-0.058
	(0.065)	(0.081)	(0.068)	(0.095)
Father has upper sec. high school degree	0.015	0.071	0.021	0.03
	(0.067)	(0.084)	(0.072)	(0.1)
Labor market orientation	0.023	-0.223	-0.026	-0.383
	(0.164)	(0.207)	(0.177)	(0.246)
Final high school grade	0.474	-0.503	0.415	-1.303
	(0.827)	(0.940)	(0.899)	(1.128)
N	1,952	1,952	1,054	1,054
Panel B. Treatment: Abolishment of a mandatory internship				
Mother has upper sec. high school degree	0.097	0.15	0.099	0.163
	(0.078)	(0.094)	(0.082)	(0.104)
Father has upper sec. high school degree	0.01	0.03	-0.002	0.051
	(0.081)	(0.098)	(0.083)	(0.107)
Labor market orientation	0.142	0.375	0.183	0.52*
	(0.196)	(0.24)	(0.203)	(0.262)
Final high school grade	-0.113	-0.081	-0.085	-0.551
	(0.986)	(1.09)	(1.022)	(1.193)
N	1,851	1,851	1,087	1,087

Note: Estimates from DiD, based on threshold II (60/40) definition of treatment. Standard errors in parentheses. No control variables in columns (1) and (3). In columns (2) and (4), the set of control variables comprises gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. Each row's dependent variable is omitted from the set of controls variables. Panel A: The models in columns (1) and (2) comprise all individuals from rows 1, 3 and 4 of Table 4.5, for whom the outcome variable is not missing. The models in columns (3) and (4) comprise individuals only from rows 1 and 3. Panel B: The models in columns (1) and (2) comprise all individuals from rows 2, 3 and 4 of Table 4.5, for whom the outcome variable is not missing. The models in columns (3) and (4) comprise individuals only from rows 2 and 4. For alternative threshold definitions, see Tables 4.8 and A.3.5. $^{+}$ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table A.3.5: DiD Estimates of Introducing/Abolishing Mandatory Internships on Individual Characteristics (Threshold III: 70/30, *Sample I*)

Comparison group:	Departments without a change in mandatory internship		Departments with/without a mandatory internship	
Controls:	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Panel A. Treatment: Introduction of a mandatory internship				
Mother has upper sec. high school degree	0.003	-0.093	0.017	-0.138
	(0.082)	(0.115)	(0.085)	(0.132)
Father has upper sec. high school degree	0.067	0.145	0.097	0.061
	(0.084)	(0.118)	(0.089)	(0.139)
Labor market orientation	0.1	-0.382	0.018	-0.607+
	(0.205)	(0.292)	(0.219)	(0.344)
Final high school grade	0.555	-0.293	0.606	0.008
	(1.041)	(1.339)	(1.117)	(1.592)
N	1,668	1,668	835	835
Panel B. Treatment: Abolishment of a mandatory internship				
Mother: high school degree	-0.008	0.016	-0.022	0.104
	(0.198)	(0.209)	(0.201)	(0.216)
Father: high school degree	-0.014	0.051	-0.041	0.105
	(0.202)	(0.213)	(0.201)	(0.218)
Labor market orientation	0.796	0.753	0.857+	0.903+
	(0.493)	(0.52)	(0.495)	(0.539)
High school grade	0.881	0.527	0.799	0.347
	(2.484)	(2.368)	(2.499)	(2.405)
N	1,523	1,523	894	894

Note: Estimates from DiD, based on threshold III (70/30) definition of treatment. Standard errors in parentheses. No control variables in columns (1) and (3). In columns (2) and (4), the set of control variables comprises gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. Each row's dependent variable is omitted from the set of controls variables. Panel A: The models in columns (1) and (2) comprise all individuals from rows 1, 3 and 4 of Table 4.5, for whom the outcome variable is not missing. The models in columns (3) and (4) comprise individuals only from rows 1 and 3. Panel B: The models in columns (1) and (2) comprise all individuals from rows 2, 3 and 4 of Table 4.5, for whom the outcome variable is not missing. The models in columns (3) and (4) comprise individuals only from rows 2 and 4. For alternative threshold definitions, see Tables 4.8 and A.3.4. $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.3.6: Classification of Areas of Study into Strong and Weak Labor Market Orientation

Strong LM orientation	Weak LM orientation
administrative studies	ancient/classic philology, modern Greek
agricultural sciences	area studies
architecture and interior design	arts, general art history
biology	catholic theology/religious education
chemical science	composition and design
civil engineering	cultural studies/cultural sciences
computer science	English studies, American studies
dentistry/dental medicine	extra-European linguistic and cultural studies
economics	film studies
electrical engineering	fine arts
engineering management	comparative literary and linguistic sciences
food and beverage technology	general cultural studies
forestry, forest and wood management	general economic and social science
general engineering	general linguistics and philology
geomatic/geospatial engineering	geography
geosciences (without geography)	German philology and studies
healthcare science	history
human medicine	library science, documentation, communication
jurisprudence/law	music, musicology
landscape conservation, - architecture	education
mathematics, natural sciences	performing arts, theater studies
mechanical engineering, process engineering	philosophy
mining and metallurgy	political sciences
nautical science / navigation	protestant theology/religious education
pharmacy	psychology
physics, astronomy	Romance philology and studies
social pedagogy	Slavonic, Baltic, Finno-Ugrian studies
spatial planning	social sciences
teletraffic engineering	special education
trophology, nutritional and domestic science	sport science
veterinary medicine	

Note: Based on Scarletti (2009).

Table A.3.7: DiD Estimates of Introducing Mandatory Internships on Quality Indicators (Threshold II: 60/40, *Sample I*)

Comparison group:	Departments without a change in mandatory internship		Departments without a mandatory internship	
Controls:	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Treatment: Introduction of a mandatory internship				
Overall quality of education:				
Structure of the study program	-0.153	-0.143	-0.204	-0.164
	(0.128)	(0.152)	(0.137)	(0.173)
State-of-the-art methods taught	-0.118	0.114	-0.105	0.084
	(0.125)	(0.155)	(0.133)	(0.182)
Up-to-date education[a]	-0.168	-0.039	-0.261+	-0.104
	(0.143)	(0.174)	(0.153)	(0.204)
Educational media and infrastructure:				
Availability of literature in the library	0.186	0.105	0.118	0.097
	(0.149)	(0.181)	(0.163)	(0.219)
Access to IT services (internet, databases)	0.028	0.04	0.094	0.229
	(0.13)	(0.159)	(0.142)	(0.191)
Laboratory facilities	-0.087	-0.08	-0.019	0.12
	(0.161)	(0.206)	(0.178)	(0.257)
Training:				
Oral presentation training	-0.316+	-0.078	-0.333+	-0.04
	(0.172)	(0.213)	(0.179)	(0.251)
Writing skills training	-0.228	-0.408*	-0.269	-0.408+
	(0.164)	(0.206)	(0.176)	(0.239)
Training in foreign languages[b]	-0.092	0.036	-0.016	0.174
	(0.165)	(0.195	(0.176)	(0.237)
Career Counseling:				
Help in finding a job and starting a career	0.049	0.076	-0.07	0.015
	(0.156)	(0.192)	(0.168)	(0.226)
Availability of career counseling	-0.007	0.142	-0.119	0.138
	(0.147)	(0.182)	(0.158)	(0.212)
Provision of career orientation events	0.107	0.161	-0.025	0.018
	(0.146)	(0.183)	(0.154)	(0.215)
N	1,788	1,788	954	954

Note: Estimates from DiD, based on threshold II (60/40) definition of treatment. Standard errors in parentheses. No control variables in columns (1) and (3). In columns (2) and (4), the set of control variables comprises gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. The models in columns (1) and (2) comprise all individuals from rows 1, 3 and 4 of Table 4.5, for whom the outcome variable is not missing. The models in columns (3) and (4) comprise individuals only from rows 1 and 3. [a] The variable measures the actuality of education with respect to current job requirements. [b] The variable measures subject- or job-specific training in foreign languages. [c] Sample size $N = 1,723$. [d] Sample size $N = 1,108$. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.3.8: DiD Estimates of Abolishing Mandatory Internships on Quality Indicators (Threshold II: 60/40, *Sample I*)

Comparison group:	Departments without a change in mandatory internship		Departments with a mandatory internship	
Controls:	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Treatment: Abolishment of a mandatory internship				
Overall quality of education:				
Structure of the study program	0.032	0.203	0.083	0.181
	(0.147)	(0.17)	(0.151)	(0.188)
State-of-the-art methods taught	0.209	0.038	0.2	0.04
	(0.143)	(0.174)	(0.15)	(0.19)
Up-to-date education[a]	-0.01	0.037	0.074	0.082
	(0.165)	(0.195)	(0.171)	(0.219)
Educational media and infrastructure:				
Availability of literature in the library	0.015	0.011	0.061	0.107
	(0.17)	(0.205)	(0.173)	(0.219)
Access to IT services (internet, databases)	-0.005	0.107	-0.057	0.024
	(0.148)	(0.174)	(0.155)	(0.188)
Laboratory facilities	0.096	-0.213	0.07	-0.343
	(0.173)	(0.203)	(0.175)	(0.217)
Training:				
Oral presentation training	0.125	0.11	0.148	-0.06
	(0.199)	(0.24)	(0.211)	(0.262)
Writing skills training	-0.049	-0.05	-0.019	-0.018
	(0.188)	(0.231)	(0.196)	(0.256)
Training in foreign languages[b]	0.248	0.122	0.197	0.015
	(0.194)	(0.221)	(0.199)	(0.233)
Career Counseling:				
Help in finding a job and starting a career	-0.175	-0.101	-0.066	0.024
	(0.183)	(0.22)	(0.19)	(0.243)
Availability of career counseling	-0.344*	-0.117	-0.244	0.016
	(0.171)	(0.208)	(0.177)	(0.228)
Provision of career orientation events	0.014	0.135	0.127	0.333
	(0.168)	(0.206)	(0.175)	(0.224)
N	1,715	1,715	1014	1014

Note: Estimates from DiD, based on threshold II (60/40) definition of treatment. Standard errors in parentheses. No control variables in columns (1) and (3). In columns (2) and (4), the set of control variables comprises gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. The models in columns (1) and (2) comprise all individuals from rows 2, 3 and 4 of Table 4.5, for whom the outcome variable is not missing. The models in columns (3) and (4) comprise individuals only from rows 2 and 4. [a] The variable measures the actuality of education with respect to current job requirements. [b] The variable measures subject- or job-specific training in foreign languages. [c] Sample size $N = 1,723$. [d] Sample size $N = 1,108$. $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.3.9: DiD Estimates of Introducing Mandatory Internships on Quality Indicators (Threshold III: 70/30, *Sample I*)

Comparison group:	Departments without a change in mandatory internship		Departments without a mandatory internship	
Controls:	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Treatment: Introduction of a mandatory internship				
Overall quality of education:				
Structure of the study program	0.059	-0.104	0.036	-0.156
	(0.16)	(0.216)	(0.168)	(0.245)
State-of-the-art methods taught	-0.159	0.204	-0.139	0.144
	(0.155)	(0.219)	(163)	(0.257)
Up-to-date education[a]	-0.187	0.149	-0.238	0.096
	(0.178)	(0.247)	(0.19)	(0.288)
Educational media and infrastructure:				
Availability of literature in the library	0.263	0.188	0.18	0.439
	(0.189)	(0.258)	(0.206)	(0.309)
Access to IT services (internet, databases)	0.093	0.106	0.163	0.323
	(0.163)	(0.223)	(0.174)	(0.265)
Laboratory facilities	-0.314	-0.147	-0.201	0.057
	(0.201)	(0.278)	(0.223)	(0.356)
Training:				
Oral presentation training	-0.409+	-0.203	-0.386+	-0.121
	(0.219)	(0.304)	(0.225)	(0.365)
Writing skills training	0.019	-0.313	-0.012	-0.067
	(0.208)	(0.295)	(0.22)	(0.342)
Training in foreign languages[b]	0.085	0.424	0.147	0.657+
	(0.21)	(0.276)	(0.222)	(0.34)
Career Counseling:				
Help in finding a job and starting a career	0.219	0.226	0.128	0.101
	(0.194)	(0.273)	(0.205)	(0.323)
Availability of career counseling	0.085	0.182	0.035	0.196
	(0.184)	(0.258)	(0.196)	(0.36
Provision of career orientation events	0.128	0.12	0.042	-0.013
	(0.182)	(0.257)	(0.19)	(0.302)
N	1,531	1,531	758	758

Note: Estimates from DiD, based on threshold III (70/30) definition of treatment. Standard errors in parentheses. No control variables in columns (1) and (3). In columns (2) and (4), the set of control variables comprises gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. The models in columns (1) and (2) comprise all individuals from rows 1, 3 and 4 of Table 4.5, for whom the outcome variable is not missing. The models in columns (3) and (4) comprise individuals only from rows 1 and 3. [a] The variable measures the actuality of education with respect to current job requirements. [b] The variable measures subject- or job-specific training in foreign languages. [c] Sample size $N = 1,723$. [d] Sample size $N = 1,108$. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

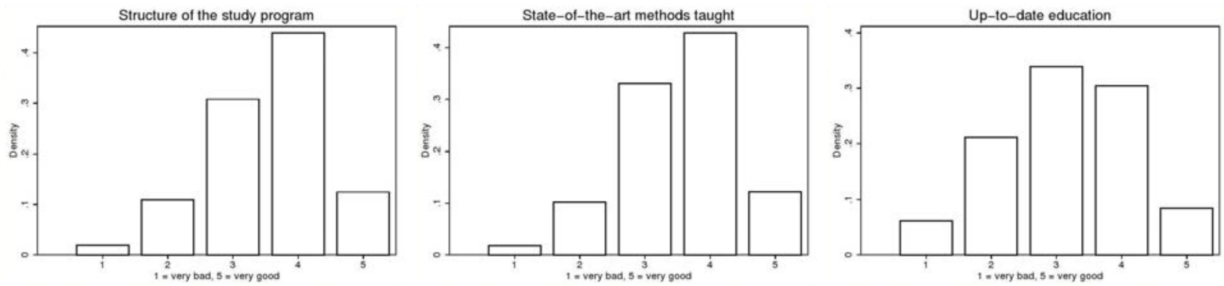
Table A.3.10: DiD Estimates of Abolishing Mandatory Internships on Quality Indicators (Threshold III: 70/30, *Sample I*)

Comparison group:	Departments without a change in mandatory internship		Departments with a mandatory internship	
Controls:	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Treatment: Abolishment of a mandatory internship				
Overall quality of education:				
Structure of the study program	0.382	0.600	0.402	0.516
	(0.371)	(0.371)	(0.371)	(0.384)
State-of-the-art methods taught	0.708*	0.813*	0.693+	0.852*
	(0.355)	(0.372)	(0.360)	(0.380)
Up-to-date education[a]	0.233	0.565	0.272	0.687
	(0.408)	(0.418)	(0.408)	(0.436)
Educational media and infrastructure:				
Availability of literature in the library	0.205	0.271	0.262	0.374
	(0.428)	(0.441)	(0.422)	(0.443)
Access to IT services (internet, databases)	0.106	0.424	0.054	0.503
	(0.366)	(0.372)	(0.373)	(0.38)
Laboratory facilities	-0.152	-0.308	-0.220	-0.219
	(0.403)	(0.398)	(0.393)	(0.398)
Training:				
Oral presentation training	0.517	0.464	0.503	0.460
	(0.499)	(0.514)	(0.511)	(0.522)
Writing skills training	0.354	0.025	0.376	0.019
	(0.74)	(0.504)	(0.474)	(0.525)
Training in foreign languages[b]	0.641	0.764	0.605	0.698
	(0.488)	(0.474)	(0.484)	(0.465)
Career Counseling:				
Help in finding a job and starting a career	0.09	0.207	0.160	0.523
	(0.442)	(0.468)	(0.447)	(0.486)
Availability of career counseling	-0.314	-0.021	-0.276	0.037
	(0.422)	(0.444)	(0.424)	(0.452)
Provision of career orientation events	0.244	0.482	0.309	0.516
	(0.415)	(0.439)	(0.421)	(0.450)
N	1,407	1,407	829	829

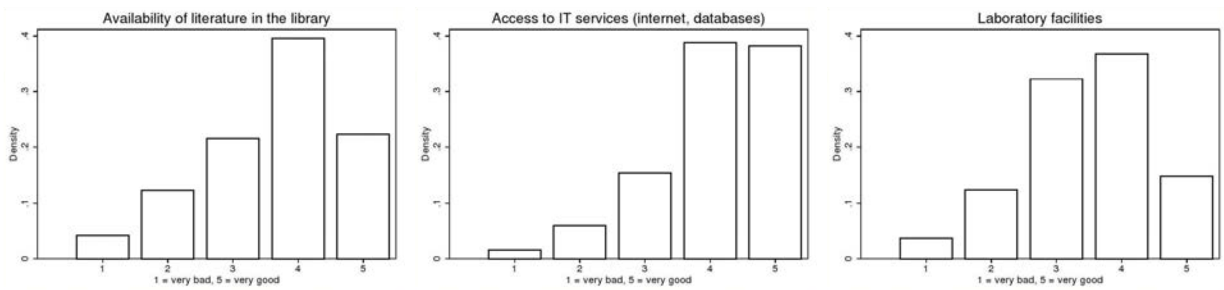
Note: Estimates from DiD, based on threshold III (70/30) definition of treatment. Standard errors in parentheses. No control variables in columns (1) and (3). In columns (2) and (4), the set of control variables comprises gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, and degree of labor market orientation. The models in columns (1) and (2) comprise all individuals from rows 2, 3 and 4 of Table 4.5, for whom the outcome variable is not missing. The models in columns (3) and (4) comprise individuals only from rows 2 and 4. [a] The variable measures the actuality of education with respect to current job requirements. [b] The variable measures subject- or job-specific training in foreign languages. [c] Sample size $N = 1,723$. [d] Sample size $N = 1,108$. $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Figure A.3.5: Students' Evaluation of Study Related Aspects

(a) Overall Quality of Education

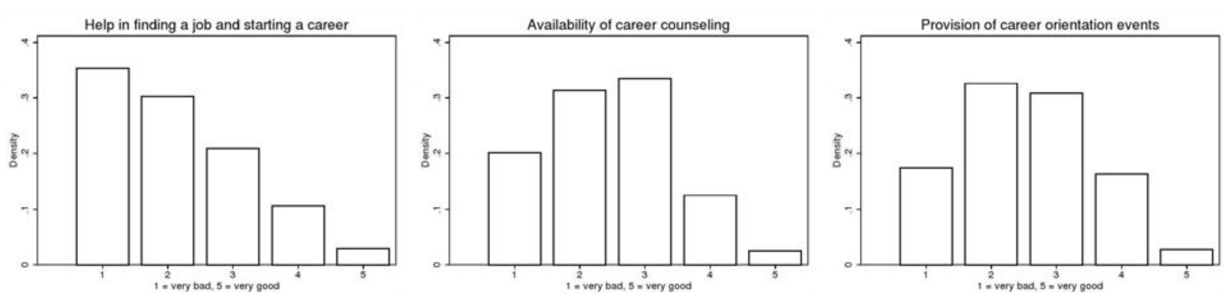


(b) Educational Media and Infrastructure



(c) Training

(d) Career Counseling



Note: The corresponding questionnaire item reads "How do you evaluate the following aspects of your completed studies?" Respondents are then asked to answer on a scale from 1 ("very bad") to 5 ("very good").

Bibliography

Acemoglu, D. and J. D. Angrist (2000). How Large Are Human-Capital Externalities? Evidence from Compulsory Schooling Laws. *NBER/Macroeconomics Annual 15*(1), 9–59.

Akerlof, G. and R. Kranton (2000). Economics and Identity. *The Quarterly Journal of Economics 115*(3), 715–753.

Akerlof, G. A. (1970). The Market for Lemons: Quality Uncertainty and the Market Mechanism. *The Quarterly Journal of Economics 84*(3), 488–500.

Angrist, J. D. and A. B. Krueger (1991). Does Compulsory School Attendance Affect Schooling and Earnings? *Quarterly Journal of Economics 106*(4), 979–1014.

Angrist, J. D. and A. B. Krueger (1999). Empirical Strategies in Labor Economics. In O. C. Ashenfelter and D. Card (Eds.), *Handbook of Labor Economics*, Volume 3, pp. 1277–1366. Elsevier.

Antoni, M., K. Drasch, C. Kleinert, B. Matthes, M. Ruhland, and A. Trahms (2011). Working and Learning in a Changing World: Part 1: Overview of the Study. *FDZ-Methodenreport* (05/2011).

Antoni, M. and S. Seth (2011). ALWA-ADIAB - Linked Individual Survey and Administrative Data for Substantive and Methodological Research. *FDZ-Methodenreport* (12/2011).

Beck, J. E. and H. Halim (2008). Undergraduate Internships in Accounting: What and How do Singapore Interns Learn from Experience? *Accounting Education 17*(2), 151–172.

Becker, G. S. (1991). *A Treatise on the Family* (2 ed.). Harvard University Press.

Becker, G. S. (1993). *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education* (3 ed.). Chicago: The University of Chicago Press.

Becker, S. and F. Siebern-Thomas (2007). Schooling Infrastructure, Educational Attainment and Earnings. *Updated version of: Returns to Education in Germany: A variable treatment intensity approach. EUI Working Paper ECO (9), 2001*.

Behrman, J. R. and M. R. Rosenzweig (2002). Does Increasing Women‘s Schooling Raise the Schooling of the Next Generation? *American Economic Review 92*(1), 323–334.

Beinke, L. (1988). Teil A, B und C. In U. Wiegand and U. Wascher (Eds.), *Berufswahl und Berufsinformation*, Volume 4 of *Beiträge zur Berufsorientierung, Arbeitslehre und Erwachsenenbildung*. Bad Honnef: Bock.

Ben-Porath, Y. (1967). The Production of Human Capital and the Life Cycle of Earnings. *Journal of Political Economy 75*(4), p 352–365.

Black, D. A., M. C. Berger, and F. A. Scott (2000). Bounding Parameter Estimates with Nonclassical Measurement Error. *Journal of the American Statistical Association 95*(451).

Black, S. E., P. J. Devereux, and K. G. Salvanes (2008). Staying in the Classroom and Out of the Maternity Ward? The Effect of Compulsory Schooling Laws on Teenage Births. *The Economic Journal 118*(530), 1025–1054.

Booij, A. S., E. Leuven, and H. Oosterbeek (2012). The Role of Information in the Take-Up of Students Loans. *Economics of Education Review*, 33–44.

Bordieu, P. (1986). Forms of Capital. In J. G. Richardson (Ed.), *Handbook of Theory and Research for the Sociology of Education*, pp. 241–260. New York: Greenwood Press.

Borghans, L., B. H. Golsteyn, and A. Stenberg (2013). Does Expert Advice Improve Educational Choice? *IZA Discussion Paper* (7649).

Bound, J., C. Brown, and N. Mathiowetz (2001). Measurement Error in Survey Data. In J. J. Heckman and E. Leamer (Eds.), *Handbook of Econometrics*, Volume 5, pp. 3705–3843. Amsterdam: Elsevier Science.

Bound, J. and G. Solon (1999). Double Trouble: On the Value of Twins-Based Estimation of the Return to Schooling. *Economics of Education Review 18*, 169–182.

Brooks, L., A. Cornelius, E. Greenfield, and R. Joseph (1995). The Relation of Career-Related Work or Internship Experiences to the Career Development of College Seniors. *Journal of Vocational Behavior 46*(3), 332–349.

Busby, G. (2003). Tourism Degree Internships: A Longitudinal Study. *Journal of Vocational Education & Training 55*(3), 319–334.

Büttner, B. and S. L. Thomsen (2010). Are we Spending Too Many Years in School? Causal Evidence of the Impact of Shortening Secondary School Duration. *ZEW Discussion Paper 10-011*.

Callanan, G. and C. Benzing (2004). Assessing the Role of Internships in the Career-Oriented Employment of Graduating College Students. *Education + Training 46*(2), 82–89.

Card, D. (1995). Using Geographic Variation in College Proximity to Estimate the Return to Schooling. In L. N. Christofides, E. K. Grant, and R. Swidinsky (Eds.), *Aspects of Labour Market Behaviour*, pp. 201–222. Toronto and Buffalo: University of Toronto Press.

Card, D. (1999). The Causal Effect of Education on Earnings. In O. C. Ashenfelter and D. Card (Eds.), *Handbook of Labor Economics*, Volume 3, pp. 1801–1863. Elsevier.

Card, D. and T. Lemieux (2001). Can Falling Supply Explain the Rising Return to College for Younger Men? A Cohort-Based Analysis. *The Quarterly Journal of Economics 116*(2), 705–746.

Coleman, J. S. (1988). Social Capital in the Creation of Human Capital. *American Journal of Sociology 94*, S95–S120.

Conneely, K. and R. Uusitalo (1998). *Estimating Heterogeneous Treatment Effects in the Becker Schooling Model: Mimeo*. Department of Economics, University of Helsinki.

Cook, S., R. S. Parker, and C. E. Pettijohn (2004). The Perceptions of Interns: A Longitudinal Case Study. *The Journal of Education for Business 79*, 179–185.

Crebert, G., M. Bates, B. Bell, C.-J. Patrick, and V. Cragnolini (2004). Developing Generic Skills at University, During Work Placement and in Employment: Graduates' Perceptions. *Higher Education Research & Development 23*(2), 147–165.

Cutler, D. M. and A. Lleras-Muney (2006). Education and Health: Evaluating Theories and Evidence. *NBER Working Paper* (12352).

Dee, T. (2004). Are There Civic Returns to Education? *Journal of Public Economics 88*(9-10), 1697–1720.

Devereux, P. J. and R. A. Hart (2010). Forced to be Rich? Returns to Compulsory Schooling in Britain. *The Economic Journal 120*(549), 1345–1364.

Dominitz, J. (1998). Earnings Expectations, Revisions, and Realizations. *Review of Economics and Statistics 80*(3), 374–388.

Dorner, M., J. Heining, P. Jacobebbinghaus, and S. Seth (2010). Sample of Integrated Labour Market Biographies (SIAB) 1975-2008. *FDZ-Datenreport* (1), 1–63.

Dustmann, C. (2004). Parental Background, Secondary School Track Choice, and Wages. *Oxford Economic Papers 56*(2), 209.

Emran, M. S. and Y. Sun (2011). Magical Transition? Intergenerational Educational and Occupational Mobility in Rural China: 1988-2002. *Working Paper, George Washington University*.

Farré, L., R. Klein, and F. Vella (2010). A Parametric Control Function Approach to Estimating the Returns to Schooling in the Absence of Exclusion Restrictions: An Application to the NLSY. *Empirical Economics 44*(1), 111–133.

Farré, L., R. Klein, and F. Vella (2012). Does Increasing Parents' Schooling Raise the Schooling of the Next Generation? Evidence Based on Conditional Second Moments. *Oxford Bulletin of Economics and Statistics 74*(5), 676–690.

Favara, M. (2012). The Cost of Acting Girly: Gender Stereotypes and Educational Choices. *IZA Discussion Paper* (7037).

Flossmann, A. and W. Pohlmeier (2006). Causal Returns to Education: A Survey on Empirical Evidence for Germany. *Journal of Economics and Statistics 226*(41), 6–23.

Francesconi, M., S. P. Jenkins, and T. Siedler (2010). Childhood Family Structure and Schooling Outcomes: Evidence from Germany. *Journal of Population Economics 23*(3), 1073–1103.

Gang, I. N. and K. F. Zimmermann (2000). Is Child Like Parent? Educational Attainment and Ethnic Origin. *Journal of Human Resources 35*(3), 550–569.

Garavan, T. N. and C. Murphy (2001). The Co-operative Education Process and Organisational Socialisation: A Qualitative Study of Student Perceptions of its Effectiveness. *Education + Training 43*(6), 281–302.

Gault, J., J. Redington, and T. Schlager (2000). Undergraduate Business Internships and Career Success: Are They Related? *Journal of Marketing Education 22*(1), 45–53.

Gödl, H. (1986). Berufswahl und Berufsinformationszentrum: Ergebnisse einer empirischen Untersuchung. *Berufsberatung und Berufsbildung 71*(3), 153–161.

Granovetter, M. S. (1995). *Getting a Job: A Study of Contacts and Careers* (2 ed.). University of Chicago Press.

Grave, B. S. and K. Goerlitz (2012). Wage differentials by field of study – the case of German university graduates. *Education Economics 20*(3), 284–302.

Griliches, Z. (1977). Estimating the Returns to Schooling: Some Econometric Problems. *Econometrica 45*(1), 1–22.

Hainmüller, J., B. Hofmann, G. Krug, and K. Wolf (2009). Do More Placement Officers Lead to Lower Unemployment? Evidence from Germany. *IAB Discussion Paper* (13).

Harmon, C. and I. Walker (1995). Estimates of the Economic Return to Schooling for the United Kingdom. *The American Economic Review 85*(5), 1278–1286.

Hastings, J. and J. Weinstein (2008). Information, School Choice, and Academic Achievement: Evidence from two Experiments. *The Quarterly Journal of Economics 123*(4), 1373–1414.

Heckman, J. J. (1976). A Life-Cycle Model of Earnings, Learning, and Consumption. *Journal of Political Economy 84*(4), S11–44.

Hermanns, K. (1989). BIZ '88. Ergebnisse einer Repräsentativbefragung von Benutzern der BIZ-Mediotheken. *Informationen für die Beratungs- und Vermittlungsdienste der Bundesanstalt für Arbeit - Nürnberg* (8), 281–286.

Hermanns, K. (1992). BIZ '91. Ergebnisse der repräsentativen Befragung 1991 von Nutzern der BIZ-Mediotheken in den alten Bundesländern. *Informationen für die Beratungs- und Vermittlungsdienste der Bundesanstalt für Arbeit - Nürnberg* (12), 841–846.

Hirsch, W. (1974). Planung und Vorbereitung von Berufsinformationszentren der Berufsberatung. *Arbeit, Beruf und Arbeitslosenhilfe 25*(8), 245–249.

Holland, P. W. (1986). Statistics and Causal Inference. *Journal of the American Statistical Association 81*(396), 945–960.

Ichimura, H. (1993). Semiparametric Least Squares (SLS) and Weighted SLS Estimation of Single-Index Models. *Journal of Econometrics 58*(1-2), 71–120.

Ichino, A. and R. Winter-Ebmer (1999). Lower and Upper Bounds of Returns to Schooling: An Exercise in IV Estimation with Different Instruments. *European Economic Review 43*, 889–901.

Ichino, A. and R. Winter-Ebmer (2004). The Long-Run Educational Cost of World War II. *Journal of Labor Economics 22*(1).

Imbens, G. W. and J. D. Angrist (1994). Identification and Estimation of Local Average Treatment Effects. *Econometrica*, 467–475.

Jacobson, L., R. LaLonde, and D. G. Sullivan (2005). Estimating the Returns to Community College Schooling for Displaced Workers. *Journal of Econometrics 125*(1-2), 271–304.

Jenschke, B. (1979a). *Berufsberatung und Schule: Aufgaben und Möglichkeiten der Zusammenarbeit.* Deutsches Institut für Fernstudien an der Universität Tübingen.

Jenschke, B. (1979b). Berufsinformationszentrum Berlin - Zielsetzung und Struktur: Ein Erfahrungsbericht. *Wirtschaft und Berufserziehung 31*(5), 133–139.

Jenschke, B. (1979c). Berufsinformationszentrum und Berufswahlunterricht. In U.-J. Kledzik and B. Jenschke (Eds.), *Berufswahlunterricht als Teil der Arbeitslehre*, pp. 154–162. Hannover u.a.: Schroedel.

Jensen, R. (2010). The (Perceived) Returns to Education and the Demand for Schooling. *The Quarterly Journal of Economics 125*(2), 515–548.

Jepsen, C., K. Troske, and P. Coomes (2014). The Labor-Market Returns to Community College Degrees, Diplomas, and Certificates. *Journal of Labor Economics 32*(1), 95–121.

Kane, T. J. and C. E. Rouse (1995). Labor-Market Returns to Two- and Four-Year College. *The American Economic Review 85*(3), 600–614.

Kane, T. J., C. E. Rouse, and D. Staiger (1999). Estimating Returns to Schooling when Schooling is Misreported. *NBER Working Paper* (7235).

Klein, M. and F. Weiss (2011). Is Forcing Them Worth the Effort? Benefits of Mandatory Internships for Graduates from Diverse Family Backgrounds at Labour Market Entry. *Studies in Higher Education 36*(8), 969–987.

Klein, R. and F. Vella (2009a). A Semiparametric Model for Binary Response and Continuous Outcomes under Index Heteroscedasticity. *Journal of Applied Econometrics 24*(5), 735–762.

Klein, R. and F. Vella (2009b). Estimating the Return to Endogenous Schooling Decisions via Conditional Second Moments. *Journal of Human Resources 44*(4), 1047–1065.

Klein, R. and F. Vella (2010). Estimating a Class of Triangular Simultaneous Equations Models Without Exclusion Restrictions. *Journal of Econometrics 154*(2), 154–164.

Kling, J. R. (2001). Interpreting Instrumental Variables Estimates of the Returns to Schooling. *Journal of Business and Economic Statistics 19*(3), 358–364.

Krawietz, M., P. Müßig-Trapp, and J. Willige (2006). *HISBUS Blitzbefragung: Praktika im Studium.* Kurzbericht Nr. 13. Hochschul-Informationssystem.

Kretschmer, G. and D. Perrey (1998). Ergebnisse der repräsentativen Befragung 1997 von Nutzern der Berufsinformationszentren (BIZ). *Informationen für die Beratungs- und Vermittlungsdienste der Bundesanstalt für Arbeit - Nürnberg* (9), 727–736.

Leuze, K. and S. Strauß (2009). Lohnungleichheiten zwischen Akademikerinnen und Akademikern: Der Einfluss von fachlicher Spezialisierung, frauendominierten Fächern und beruflicher Segregation: [Wage Inequality between Male and Female University Graduates: The Influence of Occupational Specialization, Female-Dominated Subjects and Occupational Segregation]. *Zeitschrift für Soziologie 38*(4), 262–281.

Lohmann, B. (1988). *Berufsinformationszentren Eine Idee setzt sich durch.* Düsseldorf.

Machin, S., O. Marie, and S. Vujić (2011). The Crime Reducing Effect of Education. *The Economic Journal 121*(552), 463–484.

Machin, S., P. Pelkonen, and K. Salvanes (2012). Education and Mobility. *Journal of the European Economic Association* (2), 417–450.

Machin, S. and P. A. Puhani (2003). Subject of Degree and the Gender Wage Differential: Evidence from the UK and Germany. *Economics Letters 79*(3), 393–400.

Martínez, A. and T. Dinkelman (2011). Investing in Schooling in Chile: The Role of Information about Financial Aid for Higher Education. *Discussion Paper*.

Massute, J. (1984). Berufsinformationszentren. Befragung von Besuchern. *Arbeit und Beruf 35*(7), 196–198.

Miller, R. (1984). Job Matching and Occupational Choice. *The Journal of Political Economy*, 1086–1120.

Mincer, J. (1974). *Schooling, Experience, and Earnings.* New York: Columbia University Press.

Nguyen, T. (2008). Information, Role Models and Perceived Returns to Education: Experimental Evidence from Madagascar. *mimeo*.

Nieder, H. (1980). Selbstinformationseinrichtungen der Berufsberatung werden ausgebaut: Ein Situationsbericht. *Arbeit und Beruf* (1), 4–6.

Nowak, G. (1996). *Die österreichischen Berufsinformationszentren.* Wien: Wissenschaftsverlag.

Nunley, J. M., A. Pugh, N. Romero, and R. A. Seals (2014). College Major, Internship Experience, and Employment Opportunities: Estimates from a Résumé Audit. *Auburn University Working Paper 2014–03*.

OECD (2004). *Career Guidance: A Handbook for Policy Makers.* Paris: OECD.

OECD (2013). *Education at a Glance 2013.* Paris: OECD.

Oreopoulos, P. (2006). Estimating Average and Local Average Treatment Effects of Education when Compulsory Schooling Laws Really Matter. *American Economic Review 96*(1), 152–175.

Oreopoulos, P. (2007). Do Dropouts Drop Out Too Soon? Wealth, Health and Happiness from Compulsory Schooling. *Journal of Public Economics 91*(11-12), 2213–2229.

Oreopoulos, P. and R. Dunn (2013). Information and College Access: Evidence from a Randomized Field Experiment. *The Scandinavian Journal of Economics 115*(1), 3–26.

Papay, J., R. Murnane, and J. Willett (2011). How Performance Information Affects Human-Capital Investment Decisions: The Impact of Test-Score Labels on Educational Outcomes. *NBER Working Paper* (17120).

Parey, M. and F. Waldinger (2011). Studying Abroad and the Effect on International Labour Market Mobility: Evidence from the Introduction of ERASMUS. *The Economic Journal 121*(551), 194–222.

Pedro, J. D. (1984). Induction into the Workplace: The Impact of Internships. *Journal of Vocational Behavior 25*(1), 80–95.

Peracchi, F. (2006). Educational Wage Premia and the Distribution of Earnings: An International Perspective. In E. Hanushek and F. Welch (Eds.), *Handbook of the Economics of Education*, Volume 1, pp. 189–254. Elsevier.

Perrey, D. (1995). Ergebnisse der repräsentativen Befragung 1994 von Nutzern der BIZ-Mediotheken in den alten und neuen Bundesländern. *Informationen für die Beratungs- und Vermittlungsdienste der Bundesanstalt für Arbeit - Nürnberg* (4), 333–340.

Pischke, J.-S. (2007). The Impact of Length of the School Year on Student Performance and Earnings: Evidence from the German Short School Year. *The Economic Journal 117*(523), 1216–1242.

Pischke, J.-S. and T. v. Wachter (2008). Zero Returns to Compulsory Schooling in Germany: Evidence and Interpretation. *Review of Economics and Statistics 85*(3), 1278–1598.

Psacharopoulos, G. and H. A. Patrinos (2004). Returns to Investment in Education: A Further Update. *Education Economics 12*(2), 111–134.

Rahmenvereinbarung (2004). Rahmenvereinbarung über die Zusammenarbeit von Schule und Berufsberatung zwischen der Kultusministerkonferenz und der Bundesagentur für Arbeit, 15 October 2004, State of Bremen.

Rehn, T., G. Brandt, G. Fabian, and K. Briedis (2011). *Hochschulabschlüsse im Umbruch: Studium und Übergang von Absolventinnen und Absolventen reformierter und traditioneller Studiengänge des Jahrgangs 2009*, Volume 17 of *HIS: Forum Hochschule.*

Reimer, D. and J. Schröder (2006). Tracing the Gender Wage Gap: Income Differences Between Male and Female University Graduates in Germany. *Zeitschrift für ArbeitsmarktForschung - Journal for Labour Market Research 39*(2), 235–253.

Richards, E. W. (1984). Undergraduate Preparation and Early Career Outcomes: A Study of Recent College Graduates. *Journal of Vocational Behavior 24*(3), 279–304.

Rodríguez-Planas, N. (2012). Longer-Term Impacts of Mentoring, Educational Services, and Learning Incentives: Evidence from a Randomized Trial in the United States. *American Economic Journal: Applied Economics 4*(4), 121–139.

Roy, A. D. (1951). Some Thoughts on the Distribution of Earnings. *Oxford Economic Papers 3*(2), 135–146.

Rubin, D. B. (1974). Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies. *Journal of Educational Psychology 66*(5), 688–701.

Scarletti, A. (2009). *Die Bedeutung von Praktika und studentischen Erwerbstätigkeiten für den Berufseinstieg: Dissertation*, Volume 77 of *Studien zur Hochschulforschung.* München.

Schmillen, A. and H. Stüber (2014). Bildung lohnt sich ein Leben lang: Lebensverdienste nach Qualifikation. *IAB Kurzbericht* (1).

Schnedler, W. (2004). *The Value of Signals in Hidden Action Models: Concepts, Application, and Empirical Evidence.* Contributions to Economics. Heidelberg: Physica-Verlag.

Schroeder, E. (2010). The Impact of Microcredit Borrowing on Household Consumption in Bangladesh. *Working Paper, Georgetown University*.

Schweikert, K. and V. Meissner (1984). *Berufwahl und Berufsinformation. Ergebnisse einer empirischen Untersuchung.*

Shoenfelt, E. L., N. J. Stone, and J. L. Kottke (2013). Internships: An Established Mechanism for Increasing Employability. *Industrial and Organizational Psychology 6*(1), 24–27.

Siebert, R. (1979). Das BIZ-Berlin. In Gemeinnützige Gesellschaft Gesamtschule (Ed.), *Berufsorientierung und Berufswahlvorbereitung*, pp. 80–84. Hamburg.

Siedler, T. (2010). Schooling and Citizenship in a Young Democracy: Evidence from Postwar Germany. *Scandinavian Journal of Economics 112*(2), 315–338.

Solon, G. (1992). Intergenerational Income in the United States. *American Economic Review 82*(3), 393–408.

Spence, M. (1973). Job Market Signaling. *The Quarterly Journal of Economics 87*(3), 355.

Spiess, K. C. and K. Wrohlich (2010). Does Distance Determine who Attends a University in Germany? *Economics of Education Review 29*(3), 470–479.

Statistisches Bundesamt (2012). Studierende an Hochschulen, Fächersystematik: Wintersemester 2006/207. *Fachserie 11 Reihe 4.1*.

Stiglitz, J. E. (1975). The Theory of Screening, Education, and the Distribution of Income. *The American Economic Review 65*(3), 283–300.

Stinebrickner, T. and R. Stinebrickner (2011). Math or Science? Using Longitudinal Expectations Data to Examine the Process of Choosing a College Major. *NBER Working Paper* (16869).

Taylor, M. S. (1988). Effects of College Internships on Individual Participants. *Journal of Applied Psychology 73*(3), 393–401.

Teichler, U. (2011). Bologna – Motor or Stumbling Block for the Mobility and Employability of Graduates? In H. Schomburg and U. Teichler (Eds.), *Employability and Mobility of Bachelor Graduates in Europe*, pp. 3–41. Rotterdam: SensePublishers.

Topel, R. H. and M. P. Ward (1992). Job Mobility and the Careers of Young Men. *Quarterly Journal of Economics 107*(2), 439–479.

Vikström, J., M. Rosholm, and M. Svarer (2011). The Relative Efficiency of Active Labour Market Policies: Evidence from a Social Experiment and Non-parametric Methods. *IZA Discussion Paper* (5596).

Wagner, G. G., J. R. Frick, and J. Schupp (2007). The German Socio-Economic Panel Study (SOEP) - Scope, Evolution and Enhancements. *Schmollers Jahrbuch 127*(1), 139–169.

Weber, B. and E. Weber (2013). Qualifikation und Arbeitsmarkt: Bildung ist der beste Schutz vor Arbeitslosigkeit. *IAB Kurzbericht* (4).

Weishaupt, H. (Ed.) (2012). *Bildung in Deutschland 2012: Ein indikatorengestützter Bericht mit einer Analyse zur kulturellen Bildung im Lebenslauf*. Bielefeld: Bertelsmann.

Weitzel, H.-J. (1987). *BIZ: Medien- und Kommunikationszentrum des Arbeitsamtes*. Landesarbeitsamt NRW.

Weitzel, H.-J. (1988). BIZ: Integrations- und Kooperationsort der Beratungs- und Vermittlungsdienste der BA. *Arbeit und Beruf 39*(10), 313–314.

Winkelmann, R. (1996). Employment Prospects and Skill Acquisition of Apprenticeship-Trained Workers in Germany. *Industrial and Labor Relations Review 49*(4), 658–672.

Wiswall, M. and B. Zafar (2011). Determinants of College Major Choice: Identification Using an Information Experiment. *Staff Report. Federal Reserve Bank of New York* (500).

Wolter, A. and U. Banscherus (2012). Praxisbezug und Beschäftigungsfähigkeit im Bologna-Prozess – A Never Ending Story? In W. Schubarth, K. Speck, A. Seidel, C. Gottmann, C. Kamm, and M. Krohn (Eds.), *Studium nach Bologna: Praxisbezüge stärken?!*, pp. 21–36. Springer Fachmedien Wiesbaden.

Zafar, B. (2011). How Do College Students Form Expectations? *Journal of Labor Economics 29*(2), 301–348.

Zimmermann, K. F., C. Biavaschi, W. Eichhorst, C. Giulietti, M. J. Kendzia, A. Muravyev, J. Pieters, N. Rodriguez Planas, and R. Schmidl (2013). Youth Unemployment and Vocational Training. *Foundations and Trends in Microeconomics 9*(1-2), 1–157.

List of Tables

List of Figures

English Summary (Abstracts)

Chapter 2: Estimating Heterogeneous Returns to Education in Germany via Conditional Heteroskedasticity

In this paper I investigate the causal returns to education for different educational groups in Germany by employing a method by Klein and Vella (2010) that bases identification on the presence of conditional heteroskedasticity. Compared to IV methods, key advantages of this approach are unbiased estimates in the absence of instruments and parameter interpretation that is not bounded to local average treatment effects. Using data from the German Socio-Economic Panel Study (GSOEP) I find that the causal return to education is 8 percent for the entire sample, 1.1 percent for graduates from the basic school track and 8.3 percent for graduates from a higher school track. Across these groups the endogeneity bias in simple OLS regressions varies significantly. This confirms recent evidence in the literature on Germany. Various robustness checks support the findings.

Chapter 3: The Effect of Occupational Knowledge: Job Information Centers and Labor Market Outcomes

This study examines the causal link between individuals' occupational knowledge, educational choices, and labor market outcomes. We proxy occupational knowledge with mandatory visits to job information centers (JICs) in Germany while still attending school. Exogenous variation in the location of JICs and timing of when they opened makes it possible to estimate causal effects in a difference-in-difference setup. Combining linked survey-administrative data with the data on JICs allows us to detect whether individuals benefited from the comprehensive information service when they were young. The results suggest that individuals, who went to school in administrative districts with a JIC, have higher educational attainments and a smoother transfer to the labor market than students who did not have access to these facilities. However, we find no positive effects on

individuals' earnings in their first job or later in life. Overall, our results tend to confirm the importance of policies that promote occupational knowledge among youths and young adults.

Chapter 4: Door Opener or Waste of Time? The Effects of Internships on Labor Market Outcomes

This paper studies the causal effect of student internship experience on labor market choices and wages later in life. We use variation in the introduction and abolishment of mandatory internships at German universities as an instrument for completing an internship while attending university. Employing longitudinal data from graduate surveys, we find positive and significant wage returns of about six percent in both OLS and IV regressions. This result is mainly driven by a higher propensity of working full-time and a lower propensity of being unemployed in the first five years after entering the labor market. Moreover, former interns pursue doctoral studies less frequently. The positive returns are particularly pronounced for individuals and areas of study that are characterized by a weak labor market orientation. Heterogeneous effects are not found across other subgroups of the population.

German Summary

Die vorliegende Dissertation ist ein Beitrag zur empirischen Arbeitsmarktökonomik. Sie beinhaltet drei Studien, die sich mit der Wirkung bildungspolitischer Interventionen auf den Arbeitsmarkterfolg von Individuen beschäftigen. Den gemeinsamen Ausgangspunkt dieser Studien bildet die Gegenüberstellung von zwei Beobachtungen: Erstens ist formale Bildung eine Schlüsseldeterminante von Löhnen und Arbeitslosigkeit. In einfachen Korrelationen gilt der statistische Zusammenhang: je höher der Bildungsabschluss, desto höher das individuelle Lohneinkommen und desto geringer die Wahrscheinlichkeit von Arbeitslosigkeit betroffen zu sein. Zweitens sind Investitionen in Bildung mit Kosten und die Erträge dieser Investitionen mit Risiken verbunden. Die Humankapitaltheorie und die Signaltheorie zeigen, dass die Maxime „Je mehr Bildung, desto besser" nicht für jeden gilt. Denn in seiner Eigenschaft als Investition werden Bildungsentscheidungen zum Ergebnis eines Optimierungsproblems, dessen Lösung für unterschiedliche Individuen unterschiedlich ausfällt. Der Grund hierfür liegt in persönlichen Eigenschaften, die sowohl die Kosten des Bildungserwerbs als auch die Erträge auf dem Arbeitsmarkt bestimmen. Für Individuen ist die Lösung dieses Optimierungsproblems oft schwierig, denn es kann erhebliche Unsicherheit über die zu erwartenden Erträge bestehen. Vor dem Hintergrund der starken Wirkungskraft von Bildung für den beruflichen Erfolg ist eine Kenntnis der Bildungsrenditen für individuelle Entscheidungsträger jedoch außerordentlich wichtig. Und auch politische Entscheidungsträger haben ein Interesse an der Kenntnis von Bildungsrenditen, da diese häufig zur Begründung von wirtschafts- und bildungspolitischen Maßnahmen herangezogen werden.

Diese Gleichzeitigkeit von der Bedeutungsstärke von Bildung einerseits und der Unsicherheit ihrer Erträge andererseits hat eine Vielzahl von empirischen Studien angestoßen, deren Ziel die Quantifizierung von Bildungsrenditen ist. Dieses Vorhaben ist jedoch von der grundlegenden Komplikation betroffen, dass Schätzungen unter statistischer Endogenität leiden können und unter diesen Umständen keine Aussagen über Kausalzusammenhänge ermöglichen. Einen Ausweg bieten natürliche Experimente, die dem Schätzmodell exogene Variation zuführen und unter Zuhilfenahme geeigneter statistischer Verfahren die Berechnung von Kausaleffekten ermöglichen. Dieser Technik bedienen sich auch die Studien der vorliegenden Dissertation. Die Dissertation besteht aus drei Studien in der Form von eigenständigen Artikeln. Sie behandeln individuelle Entscheidungen aus den Bereichen sekundäre und tertiäre Bildung, Berufswahl und Berufspraktika. Jeder Artikel schließt eine Lücke in der bestehenden Literatur hinsichtlich Forschungsfrage, methodischem Vorgehen oder einer Kombination aus beidem.

Der erste Artikel trägt die Überschrift „*Estimating Heterogeneous Returns to Education in Germany via Conditional Heteroskedasticity*". Er geht der Leitfrage nach, wie sehr Arbeitslöhne von zusätzlichen Bildungsjahren beeinflusst werden. Die Studie löst das Endogenitätsproblem, indem sie einen methodischen Ansatz verfolgt, der die Identifizierung der Parameter mithilfe eines nichtlinearen Kontrollterms erreicht. Die Varianz dieses Kontrollterms beruht wiederum auf der Präsenz von Heteroskedastizität. Unter zwei identifizierenden Annahmen – der variable impact property und der constant correlations condition – wird der Punktschätzer durch ein iteratives Verfahren ermittelt. Mit Daten des SOEP werden getrennte Berechnungen angestellt für Absolventen von Realschule und Gymnasium einerseits und Hauptschulen andererseits. Im Ergebnis zeigt sich, dass Realschüler und Gymnasiasten mit Bildungsrenditen von rund 8 % für ein zusätzliches Bildungsjahr rechnen können, währen die durchschnittlichen Bildungsrenditen für Hauptschüler nur rund 1 % betragen. Dieses Ergebnis bestätigt frühere, scheinbar widersprüchliche IV Studien für Deutschland unter Berücksichtigung der dem IV-Ansatz eigenen LATE Interpretation.

Der wichtigste Beitrag dieses ersten Artikels ist, dass er eine relativ neue Methode auf eine zentrale Fragestellung der Arbeitsökonomie anwendet. Unter der Annahme, dass Heteroskedastizität als Ursprung der identifizierenden Variation entlang aller Ausprägungen der Kontrollvariablen vorhanden ist, besitzt der verwendete Ansatz gegenüber IV Methoden den Vorteil, dass er nicht durch die LATE Interpretation eingeschränkt ist. Die Studie zeigt, dass sich zusätzliche Bildungsanstrengungen für Absolventen eine der beiden höheren Schulformen mit einer längeren Schuldauer mehr auszahlt als für Absolventen der Hauptschule mit einer kürzeren Schuldauer. Dieser Befund scheint den Vorhersagen des Humankapitalmodells von abnehmenden Grenzerträgen zu widersprechen. Er gewinnt jedoch an Plausibilität, wenn man erstens berücksichtigt, dass eine Kausalinterpretation nur innerhalb der beiden Untergruppen gültig ist und nicht zwischen ihnen. Dadurch sind es insbesondere die Jahre in der tertiären Bildung, denen im gewählten Ansatz eine besondere Erklärungskraft zukommt. Zweitens, sind zunehmende Bildungsrenditen nicht unwahrscheinlich für das Bildungssystem und den Arbeitsmarkt in Deutschland. Unter Umständen stellt ein Hauptschulabschluss ein solch starkes, negatives Signal an den Arbeitgeber dar, dass zusätzliche Bildungsjahre nur noch einen geringen Einfluss auf die Löhne haben.

Der Titel des zweiten Artikels lautet „*The Effects of Occupational Knowledge: Job*

Information Centers, Educational Choices, and Labor Market Outcomes". Diese Studie untersucht den Zusammenhang zwischen beruflichem Wissen, Bildungsentscheidungen und Arbeitsmarkterfolg. Im Fokus stehen junge Erwachsene im Übergang von der Schule in den Beruf, die von einer Intensivierung des berufsvorbereitenden Unterrichts in Schulen durch die Einführung von Berufsinformationszentren betroffen sind. Berufsinformationszentren wurden in Deutschland in den Achtziger- und Neunzigerjahren gegründet und stellen bis heute kostenfrei und öffentlich zugänglich Informationen über Berufe, Ausbildungswege und lokale Arbeitsmarktbedingungen zur Verfügung. Anliegen der Untersuchung ist es, herauszufinden, ob ein Mehr an berufsbezogenem Wissen zu besseren Entscheidungen der jungen Menschen hinsichtlich Bildung und Beruf führt, ferner ob die Berufseinmündung von Bildungsabgängern weniger reibungsvoll verläuft. Auf die Reform der Berufsberatung selbst bezogen, beantwortet die Studie auch die konkrete Frage, ob die Einführung von Berufsinformationszentren die von der Bildungspolitik beabsichtigten, positiven Effekte hatte.

Kausale Schätzungen können durchgeführt werden, weil es eine Regelung zwischen den Schulbehörden und Berufsinformationszentren gibt, die bestimmt, dass alle Schüler während ihrer Schulzeit mindestens ein Mal ein Berufsinformationszentrum besuchen. Der verzerrende Einfluss von Selbstselektion in das Treatment wird dadurch verringert. Um unbeobachtete regionale Einflüsse zu berücksichtigen, wird darüber hinaus die örtlich und zeitlich differenzierte Einführung von Berufsinformationszentren in einem Differenzen-in-Differenzen Ansatz ausgenutzt. Die Studie verwendet selbst erhobene Daten über Ort und Gründungsjahr von Berufsinformationszentren und verknüpft diese mit Umfragedaten der Studie ALWA, die detaillierte biografische Informationen zu Wohnort, Bildung und Erwerbsaktivität beinhalten. Um den Effekt von Berufsinformationszentren auf Arbeitslöhne für verschiedene Zeitpunkte nach Eintritt in das Erwerbsleben zu ermitteln, werden die Beobachtungen mit tagesgenauen Lohneinkommen aus den administrativen Daten des SIAB ergänzt. Die Schätzungen ergeben, dass Personen, die ein Berufsinformationszentrum während der Schulzeit besucht haben, eher von aufwärtsgerichteter Bildungsmobilität betroffen sind. Das heißt, Individuen der Versuchsgruppe schließen eher mit einem höheren Schulabschluss ab, als die Schulform vorgibt, die sie zum Zeitpunkt des Treatments besuchten, als Individuen aus der Kontrollgruppe. Darüber hinaus sind die von der Maßnahme betroffenen Individuen weniger häufig arbeitslos innerhalb der ersten fünf Jahre nach Eintritt in den Arbeitsmarkt, sie wechseln weniger häufig unfreiwillig ihren Arbeitsplatz und sie ziehen weniger häufig in einen anderen Landkreis. Die Studie findet keine statistisch signifikanten Lohneffekte, weder für die erste Erwerbstätigkeit, noch für Jobs später im Leben.

Oft wird als gesichert vorausgesetzt, dass die Bereitstellung von berufs- und arbeitsmarktbezogenen Informationen positive Effekte erzielt. Empirische Belege dafür gibt es jedoch kaum. Die wenigen Studien, die sich diesem Thema mit dem Ziel kausaler Inferenz widmen, führen meist Feldexperimente kleineren Maßstabs durch. Die vorliegende Studie ist die erste, die ein Informationsprogramm untersucht, das Teil einer landesweiten Reform ist und die Mehrheit aller Schüler betrifft. Die Ergebnisse gestatten Schlussfolgerungen allgemeiner und spezifischer Natur: Allgemein kann festgehalten werden, dass die Bereitstellung von berufs- und arbeitsmarktbezogenem Wissen zu höheren Bildungsabschlüssen führt, zu einem weniger reibungsvollen Übergang von der Schule in das Erwerbsleben und zu einer höheren Stabilität von Arbeitsverhältnissen. Die spezifische Schlussfolgerung richtet sich an die Reform der Einführung von Berufsinformationszentren selber. Sie hält fest, dass Berufsinformationszentren ein effektives Instrument der berufsvorbereitenden Bildung sind.

Im dritten Artikel „*Door Opener or Waste of Time? The Effects of Internships on Labor Market Outcomes*“ geht es um die Wirkungen von studienbegleitenden Praktika auf Arbeitsmarkteinkommen nach Abschluss des Studiums. Mit den Daten der DZHW-Absolventenbefragungen wird eine 2SLS Schätzstrategie verfolgt, bei der das Ableisten eines Praktikums mit der Präsenz von Pflichtpraktika an Universitäten instrumentiert wird. Der auf diese Weise berechnete Lohneffekt hat eine Größenordnung von 6 %. Dieser Effekt kann zu einem großen Teil mit intermediären Variablen erklärt werden, die den Übergang von Studium in den Beruf beschreiben. So arbeiten ehemalige Praktikanten eher in Vollzeitpositionen als solche ohne Praktikumserfahrung. Sie verbringen weniger Zeit in Arbeitslosigkeit und sie nehmen weniger oft ein Doktorandenstudium auf. Daneben zeigt die Analyse von heterogenen Effekten, dass der Lohneffekt am stärksten für jene Individuen ausgeprägt ist, denen Arbeitsmarktgesichtspunkte bei der Studienwahl weniger wichtig waren, und die Fächer mit einem diffusen Arbeitsmarktbezug studiert haben. Dies erscheint vor dem Hintergrund plausibel, dass ein wesentlicher Zweck von Praktika die Berufsorientierung ist.

Nach Kenntnis der Autoren ist dies die erste Studie, die kausale Arbeitsmarkteffekte von freiwilligen Praktika ermittelt. Die Ergebnisse sind für Einzelpersonen und wirtschaftspolitische Akteure gleichermaßen relevant: Für Einzelpersonen ist die Kenntnis der zu erwartenden Erträge von Berufspraktika wichtig, weil Praktika als Investitionen in Bildung und Arbeitsmarkterfahrung Kosten verursachen — zum Beispiel in Form von Opportunitätskosten — und ein Praktikum nur angetreten werden sollte, wenn der erwartete Nutzen die Kosten übersteigt. Für wirtschaftspolitische Akteure ist die Kenntnis der Lohnprämien von Praktika wichtig, weil Praktika zu einem bildungs- und arbeitsmarktpolitischen Instrument geworden sind. Im Rahmen der Bologna-Reformen etwa wurden an vielen europäischen Universitäten Pflichtpraktika mit dem Ziel eingeführt, die Beschäftigungsfähigkeit der Studierenden zu verbessern.

Aus einer Gesamtbetrachtung der vorliegenden Dissertation können drei Kernbotschaften abgeleitet werden: Erstens zeigen die einleitenden Worte zur inhaltlichen Verortung dieser Arbeit, dass die arbeitsökonomische Theorie für die Praxis nicht ausreichend Orientierung für die Abwägung der Vor- und Nachteile spezifischer Bildungsinterventionen bietet. Empirisch gewonnen Erkenntnissen kommt daher eine zentrale Bedeutung zu. Zweitens sind bildungspolitische Interventionen, die den Übergang vom Bildungssystem in den Arbeitsmarkt betreffen, mit besonderer Sensibilität zu gestalten, weil Einflüsse an diesem Punkt der Biografie nachhaltige Wirkungen auf den späteren Arbeitsmarkterfolg entwickeln können. Drittens deutet der Vergleich zwischen Korrelationen und kausalen Effekten auf die Notwendigkeit hin, dass empirische Studien das Problem von Endogenität berücksichtigen. Andernfalls können wirtschaftspolitische Schlussfolgerungen aus verzerrten Ergebnissen gezogen werden.