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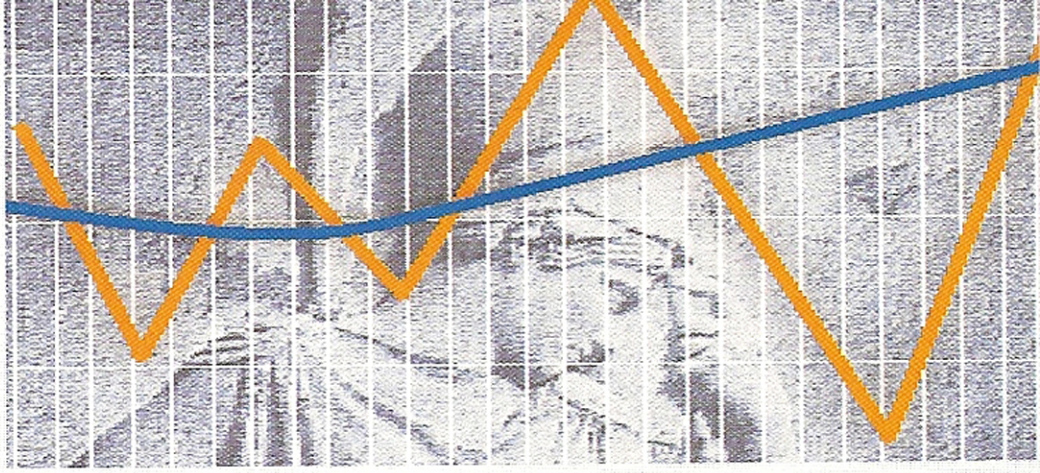

ifo Beiträge zur Wirtschaftsforschung

57

Microeconometric Evaluations of Education Policies

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Leibniz-Institut für Wirtschaftsforschung

an der Universität München e.V.

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Preface

This volume was prepared by Benedikt Siegler while working at the Center for the Economics of Education of the Ifo Institute. It was completed in June 2014 and accepted as a doctoral thesis by the Department of Economics at the University of Munich (LMU) in November 2014. The thesis includes three empirical studies, each of which evaluates one distinct education policy to improve educational outcomes of juveniles and young adults: the provision of private school vouchers in the context of a school accountability system to increase educational production at low-performing public schools (Chapter 2), opening of new universities in regions without local tertiary education supply to raise tertiary education attainment by the local population (Chapter 3), and the introduction of the Bachelor degree programs at German universities to foster student mobility and employability (Chapter 4).

Chapter 2 analyzes the effect of the private school voucher provision within the school accountability system in Florida on the educational production at low performing public schools. Until 2006, students of repeatedly failing public schools were entitled to obtain a state funded voucher to transfer to a private school. Applying a regression discontinuity approach to administrative micro data on all students in Florida public schools from 2003 to 2009, results do not indicate any significant effect from the voucher provision. Chapter 3 investigates the effect of the tertiary education expansion of the 1960s and 1970s in West Germany on tertiary education attainment by the local population. Using a difference-in-differences approach and data from the Socio-Economic Panel (SOEP), the chapter shows that the local availability of a tertiary education institution significantly increases the share of individuals with tertiary education attainment. The effect is found to be especially pronounced for universities of applied sciences. Chapter 4 estimates the effects of the Bologna Reform, which introduced the Bachelor degree programs at German universities, on student mobility and employability related outcomes. Using an instrumental variables approach and micro data on German high school graduates from 2006 (HIS-Studienberechtigtenpanel), the chapter finds that the reform did not have significant effects on national or international student mobility and participation in internships. However, positive effects are obtained for a student's satisfaction with the study atmosphere as well as a lower dropout probability for certain subgroups.

Keywords: Educational production, school accountability, private school vouchers, tertiary education attainment, education expansion, new university opening, student mobility, employability, Bologna Reform, Bachelor introduction.

JEL-No.: I20, I21, I22 I23, I28, H75.

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I gratefully acknowledge financial support from the Bavarian Graduate Program in Economics (BGPE) which gave me the opportunity to pursue the research presented in this thesis. I also thank the Ifo Institute for providing me with an office as well as for financial resources to purchase data and to present my research at international conferences. I also thank the Program on Education Policy (PEPG) at Harvard University for providing me with the necessary resources to conduct the analysis in Chapter 2. My special thanks go to Paul Peterson, Marty West, and Antonio Wendland for their support during my research visit.

Finally, with all my heart I thank my wife and family for their enduring love and encouragement throughout all those years.

Munich, June 2014

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Chapter 1

Introduction

1.1 Human Capital and Education Policy

A long literature documents the importance of education for individual well-being and for the society as a whole. Starting with the concept of human capital theory introduced by Mincer (1958), Schultz (1961) and Becker (1964), research on the economics of education has gained increasing interest of policy-makers and scientists around the world. Since those early days, a continuously growing body of research provides overwhelming evidence of the positive returns to education in both monetary and non-monetary regards.

At the individual level, a higher level of education is primarily associated with greater labor market success such as higher wages (e.g. Card, 1999; Harmon, Oosterbeek, and Walker, 2003) and lower unemployment risk (e.g. McIntosh and Vignoles, 2001). Education is also found to have significant effects on an individual's health status. For example, there is evidence that education significantly reduces the prevalence of smoking (e.g. De Walque, 2007, 2010; Currie and Moretti, 2003) and may lower the risk of weight problems (Kemptner et al., 2011).

At the social level, education is found to have important benefits in terms of 'good citizenship' and macroeconomic development. For example, several studies have analyzed the effect of education on crime and conclude that the social savings due to reduced crime and incarceration rates are substantial (e.g. Lochner and Moretti, 2004; Machin, Marie, and Vujic, 2011). Other studies have evaluated the effect of education on political participation and political interest (e.g. Dee, 2004; Milligan, Moretti, and Oreopoulos, 2004; Siedler, 2010).

Results show that, indeed, a higher level of education can have a positive impact on these outcomes which in turn are prerequisites for well-functioning institutions in a democracy.

Many studies have analyzed the relationship between education and economic growth and conclude that manifested education in the form of higher human capital is a significant determinant of long-run economic growth (e.g. Barro, 2001; Hanushek and Wößmann, 2008, 2012). In fact, higher levels of human capital may be the most relevant input factor for an economy close to the world technological frontier (Aghion and Howitt, 2009). In sum, the social returns to education may by far exceed the individual returns.

Given these findings, it is no surprise that education policy is a major area of political activity nowadays. The starting points for education policy are manifold. The education system of most countries consists of different stages. The earliest stage is often termed 'early childhood education' and includes education programs for children under the usual school entry age. This stage is followed by the classical schooling stage often divided into primary and secondary education. Universities and other institutions of higher education are part of the tertiary education stage. While early childhood and tertiary education are voluntary, primary and at least part of secondary education is usually compulsory.

One of the main policy goals at the primary and secondary level is to raise student achievement, in particular since international student assessment tests such as PISA (Programme for International Student Assessment) disclosed significant differences between countries. Some policies, such as grade retention or curricula regulations, focus on the individual itself. Others, such as teacher merit pay, concentrate on teachers as one of the main input factors in education production. Most policies, however, focus on the institutional features of the school system. Examples hereof are the tracking of students in different school types, class size policies, school autonomy, and school accountability. In particular, school accountability has gained much importance in recent years (Figlio and Loeb, 2011; Wößmann et al., 2009). Chapter 2 of this dissertation contributes to the literature on school accountability systems by analyzing the role of private school vouchers as an incentive for low performing public schools to raise educational production.

At the tertiary level, important areas of education policy are access to higher education, student mobility, and employability. A prominent education policy intended to facilitating the access to higher education for financially constrained individuals is student financial aid. However, if the policy goal is to increase tertiary education attainment of less mobile

individuals, it may be necessary to expand educational provision to distant areas. Therefore, in Chapter 3, I analyze the effect of a local university on tertiary education attainment by the local population.

Financial aid is also used to increase student mobility. For example, the ERASMUS Program provides financial assistance to students to go abroad for interim studies. A large-scale higher education reform of recent years which aimed at increasing a student's mobility and employability through the introduction of a homogeneous two-tier degree system in all European countries was the so-called Bologna Reform. Chapter 4 evaluates the impact of this higher education reform on student mobility and employability.

The challenge in education policy is to select the right strategy which a) has the desired effect and b) can be achieved with a minimum of resources. Theoretical analyses can give valuable suggestions in this respect and should be the basis for designing new policies. However, only an empirical evaluation can show whether the policy goal is achieved and whether resources are not being wasted. This thesis employs three distinct empirical research strategies to identify causal effects of education policies on education outcomes using non-experimental data.

1.2 Inference in Policy Evaluation

The common goal of policy analysis is to identify a causal relationship between the treatment and the outcome (Angrist and Pischke, 2009).[1] For example, we might be interested in the effect of student financial aid on tertiary education enrollment. The natural approach is to obtain a dataset of individuals who were offered financial aid and which contains information on whether a student actually received financial aid ('treatment') and whether he or she enrolled in tertiary education ('outcome'). An individual in our data either received the treatment or not so that we can only observe one potential outcome for each individual. This constitutes a fundamental problem: We cannot observe the counterfactual outcome for the same individual. We can only compare the outcomes of different individuals. However, if individuals not only differ with respect to their treatment status but also with respect to other characteristics, we might get biased estimates. This problem is commonly referred to as endogeneity bias in econometrics.

[1] Angrist and Pischke (2009) provide a thorough description of methods for identifying causal effects. For a non-technical guide of these methods see Schlotter et al. (2011).

Let $Y_1 i$ denote the potential enrollment decision of individual i in case he or she receives financial aid. And let $Y_0 i$ denote the potential enrollment decision of individual i in case he or she does not receive financial aid. D_i is a binary variable indicating the treatment status of an individual. The observed outcome is denoted Y_i. The following equation of conditional expectations helps understanding the problem:

$$\begin{aligned} E[Y_i|D_i = 1] - E[Y_i|D_i = 0] = E[Y_1 i|D_i = 1] - E[Y_0 i|D_i = 1] \\ + E[Y_0 i|D_i = 1] - E[Y_0 i|D_i = 0] \end{aligned} \tag{1.1}$$

The left side of the equation denotes the observed difference in average enrollment of individuals who received financial aid and those who did not. We can expand this difference by simultaneously adding and subtracting the conditional expectation of the counterfactual outcome ($Y_0 i$) of individuals who originally received financial aid. The first difference on the right hand side represents the average treatment effect of financial aid on the enrollment decision among individuals who received the treatment as if we could observe both potential outcomes for these individuals. Note, however, that $Y_0 i$ is not observed for these individuals. The second term on the right hand side of the equation symbolizes the endogeneity bias which is the difference in average $Y_0 i$ between individuals who received financial aid and those who did not.

The equation demonstrates that the observed difference in outcomes can be at least partially caused by selection of individuals into treatment. Intuitively, we can think of the endogeneity problem as follows: Suppose that individuals with high motivation are both more likely to enroll in tertiary education and to apply for financial aid. This means that motivation influences both our outcome as well as our explanatory variable. In a simple regression of enrollment on financial aid the estimator would therefore reflect at least in part the effect of motivation on enrollment. This phenomenon is called omitted variable bias in econometrics. One solution would be to apply a multivariate regression approach and control for motivation in the regression. However, as is the case with latent variables, motivation cannot easily be observed. In practice, it seems impossible to control for all potentially confounding effects.

A controlled experiment is generally considered the ideal econometric approach in which the selection problem is solved through random assignment of individuals into treatment and control group. The random assignment implies that treatment is orthogonal to other

variables and thus independent of potential outcomes. However, in many applications experiments are not feasible for financial or even more for ethical reasons. This concern is especially pronounced in the economics of education. In the above example, conducting an experiment would mean to exclude some individuals from access to financial aid. This would violate the equality of opportunity principle. But even in situations where economic experiments are feasible, identification of the true effect might not be an easy task. Individuals might change their behavior simply because they are aware of the treatment situation and not as a result of the treatment itself. This effect is described as the "Hawthorne effect" in the literature.

In some situations, however, it is possible to use so-called 'natural experiments' for identification. These are situations in which institutional rules or natural circumstances cause exogenous variation in the treatment variable. This means that at least part of the observed variation can be considered random with respect to the assignment of treatment. The analyses in Chapters 2 and 4 employ two distinct research designs that identify a causal effect based on exogenous variation in the treatment variable.

The regression discontinuity (RD) design used in Chapter 2 exploits the fact that assignment into treatment and control group is at least partly determined by an observable covariate with a fixed cutoff value. In the case of Chapter 2, a grading rule is used to assign grade points to schools based upon students' educational performance on a standardized test. There is a fixed value on the grade point scale which determines treatment status. For example, a school scoring below this cutoff value is exposed to the treatment whereas a school scoring above this value is not. The intuition is that scoring marginally below or above the cutoff is essentially random so that a comparison of these schools identifies the treatment effect. In practice, we use regression analysis to estimate a discontinuity at the cutoff while fitting local polynomials on either side of the same. The identifying assumption is that there are no confounding discontinuities of other covariates at the cutoff value.

Another approach that exploits exogenous variation in the treatment variable is instrumental variables (IV) estimation. This approach is used in the analysis of Chapter 4. The IV estimation strategy involves a variable that is not part of the structural model. This so-called 'instrumental variable' has an impact on the treatment variable, but is uncorrelated with any further observed or unobserved variables of the model which are not included in the regression. The intuition is that the instrumental variable can be used to isolate exogenous variation in the treatment variable and use this variation for identification of the treatment effect.

Exploiting natural experiments in empirical analyses is a powerful technique to identify causal effects with cross-sectional data. In case of longitudinal data (i.e. the same unit of observation is observed several times), we can use panel data methods to draw statistical inference on the causal effect of treatment. The difference-in-differences (DiD) approach - applied in Chapter 3 of this thesis - is such a panel data method.

In a cross-sectional dataset, we usually observe the outcome for the treatment and the control group after the policy intervention. In a panel dataset, we additionally observe the outcome for both groups in a pre-treatment state (e.g. before the policy intervention). The DiD approach lets us compare the *change* in outcomes between treatment and control group. Under the assumption of parallel trends in the outcome in the absence of treatment, i.e. without treatment the treatment group would have had the same development as the control group, this change in outcomes constitutes the causal effect.

The research designs are described in more detail in the respective chapters of this dissertation where they are applied to evaluate specific education policies.

1.3 Contributions of this Dissertation

This dissertation consists of three self-contained empirical essays that contribute to the literature of education policy evaluation. Each of the essays evaluates one distinct education policy. The first policy concerns the educational production of low performing public schools and intends to raise the educational production at these schools through the threat of private school vouchers. The second aims at increasing tertiary education attainment within the population by means of university openings in regions without local university access. The third education policy pursues the goals of increasing the mobility and employability of university students by introducing a homogeneous two-tier degree structure. The analyses presented here extend our understanding of suitable strategies to raise student achievement and human capital formation.

Chapter 2 investigates the sanction scheme of a school accountability system in Florida to answer the question how educational production at low performing public schools can be increased. School accountability has become increasingly popular in many countries. The rationale for school accountability arises out of a principal agent problem. If stakeholders such as parents and policy-makers have difficulties monitoring the educational production

process at the school, school officials (i.e. teachers and administrative staff) might not exert their entire effort to offer the best education (Figlio and Loeb, 2011). In this respect, standardized testing can be used to monitor a school's performance and reduce the risk of moral hazard.

A school accountability system allows defining minimum performance requirements which are to be met by all schools. Schools which perform below this minimum level of performance can be exposed to predefined sanctions such as school competition. Usually, students are assigned to public schools based on a school's catchment area. In Florida, students of repeatedly failing public schools had the choice to transfer to a better performing public school or even obtain a state-funded voucher to attend a private school. In 2006, however, the private school voucher option was terminated after being declared unconstitutional by the Florida Supreme Court. The option to choose an alternative public school remained in place.

I use administrative student-level data from Florida to analyze whether this reduction in sanction threats caused failing elementary schools to lower their educational performance. A school's educational performance is evaluated on the basis of a grade point scale. The number of grade points a school receives in turn depends on the performance of a school's students on a standardized test which is administered to all students in Florida public schools. There is a distinct cutoff value on the grade point scale which determines the minimum performance requirements and thus the treatment status. Exploiting this discontinuous jump along the point line, I estimate the effect of treatment (i.e. the threat of being exposed to sanctions) on a school's performance using a regression discontinuity (RD) design. Comparing the magnitude of the effect between voucher and no-voucher years, I find no indication that the termination of the voucher option reduced the incentive character of the accountability system for failing schools.

Chapter 3 investigates the effect of university openings on tertiary education attainment. Increasing tertiary education attainment is a policy goal in many countries (see e.g. European Commission, 2011). Between 1960 and 1979, 93 new institutions of higher education opened in Germany. Using this large tertiary education expansion, I analyze to what extend university openings in regions without prior local university access increased tertiary education attainment by the local population. In theory, this policy reduces the transaction costs of attending university for individuals who either cannot or do not want to leave their local area.

I exploit the geographical variation in local university supply in a difference-in-differences approach. In particular, I compare cohorts reaching the university entrance age before and after the openings in counties that were and were not affected by the opening. Results show that a new university increased the share of university graduates in a county by 7.2 percent. In addition, I find that the effect was mainly driven by openings of universities of applied sciences.[2] Furthermore, I supplement the analysis by investigating more recent university openings from the 1990s and 2000s. Results suggest that we may still expect a positive effect on local tertiary education attainment from university openings today.

Chapter 4 analyzes the effects of a university degree reform on student outcomes in Germany.[3] The so-called Bologna Reform was a large-scale European initiative which committed European universities to introduce a homogeneous degree system based on two main cycles, the Bachelor/Master system (see European Ministers of Education, 1999). The policy goal was to increase the mobility and employability of university students. In Germany, universities had to abandon their single-tier degree programs in favor of the new two-tier degree system.

We use a unique micro-level dataset on German high school leavers which allows us to analyze several student outcomes with respect to the policy goals. These outcomes are international student mobility, national student mobility (as measured by change of university within the same subject and degree program), dropout, and internship participation. In addition, we investigate whether the reform had a negative impact on a student's satisfaction with the study atmosphere to evaluate the concern of unintended side effects. While the first two outcomes are direct measures of the mobility goal, dropout and internship participation may be relevant determinants of a student's employability. Dropping out of university may signal low ability and decrease a student's employment options. Internship participation is generally perceived as increasing one's labor market chances. Data on direct measures of labor market success has not yet become available.

We apply an instrumental variables (IV) approach to account for potential endogeneity in a student's decision to enroll in either the new or the old degree program. In particular, we use the relative distance between a student's place of high school and the nearest department

[2] In comparison to the more research-oriented universities which also prepare students for an academic career, the education at universities of applied sciences is rather labor-market focused.

[3] This chapter was coauthored by Bernhard Enzi.

with a Bachelor's program in a student's subject. This strategy exploits variation in the timing of the reform implementation across university departments.

Overall, we find no significant effects of the reform on student mobility, dropout, and internship participation. However, we do find that the reform significantly increased a student's satisfaction with the study atmosphere. We also find a significantly negative effect of the reform on the dropout probability of high achieving students of about 10 percent and an almost statistically significant negative effect on dropout for females of about 9 percent.

1.4 Policy Conclusions

The findings from the analyses of the three education policies are of high interest to policy-makers. School accountability systems constitute a way to monitor educational production at schools and to reduce the risk of moral hazard of teachers and administrative staff. These systems are supposed to increase educational production at public schools, in particular low-performing/failing schools, by means of certain sanctions and rewards. The analysis contained in Chapter 2 reveals that private school vouchers are not necessarily required as a component of the incentive scheme of a school accountability system. This means that the implementation of a successful school accountability system may be possible even in places in which legal regulations prohibit the provision of state-funded private school vouchers.

The analysis in Chapter 3 shows that opening a new university in regions without prior local tertiary education supply can increase the tertiary education attainment by the local population. However, opening a new university may be a very costly and time-intensive process which requires a high degree of involvement by many different stakeholders. In fact, there may exist cheaper alternatives for raising tertiary education attainment of less mobile individuals, such as student financial aid which could be based on the distance to the next available university. However, it is important to remember that a new university also represents a large infrastructure investment which can stimulate the economy of underdeveloped regions. Therefore, the opening of a new university may be the preferred strategy when the goal of raising tertiary attainment levels is combined with other policy goals.

The Bologna Reform aimed at increasing student mobility and employability by introducing a homogeneous two-tier degree system within European countries. Our analysis in Chapter 4 does not provide clear evidence whether the policy goals were achieved or not due

to a lack of statistical power when analyzing the entire student population. However, point estimates, although rather small in magnitude, all point into the desired direction. Since we estimate short term effects, i.e. from a time when the new degree system was introduced, it may be that effects become more pronounced in the future. The Bologna Reform cannot be viewed as a one-shot policy change, but rather as a process which is continuously improving and developing. It changed the external framework of the degree structure, but left it to the universities' own responsibility to adjust the study content of the programs which can be a time-intensive process.

Chapter 2

The Role of Private School Vouchers in School Accountability Systems: Regression Discontinuity Evidence from Florida

2.1 Introduction

In recent years, school accountability systems have become more and more widespread around the world (Figlio and Loeb, 2011). The general idea behind school-based accountability is to increase educational production at schools by means of rewards and sanctions. To this end, standardized student testing is used to assess a school's educational output.

There are many ways to design school accountability systems. For policy-makers trying to increase a school's educational production by means of an accountability system it is important to know which incentives may work and which may not. This chapter analyzes the role of private school vouchers as a sanction threat for failing elementary schools. By exploiting a change in Florida's accountability legislation where a single regulation (the provision of private school vouchers) was removed while all others remained in place, we can shed some light on the question whether the threat of private school vouchers is a key driver of school improvement.

In the United States, school accountability was implemented nationwide with the federal No Child Left Behind (NCLB) act of 2001. Even prior to NCLB many states had their own accountability systems. However, it seems that some systems tend to work better than others. Previous research on Florida's accountability system, for instance, has found positive impacts on school improvement (e.g. Chiang, 2009; West and Peterson, 2006; Rouse, Hannaway, Goldhaber, and Figlio, 2007; Greene, 2001). Rockoff and Turner (2010) also report positive effects of school accountability in New York City. On the other hand, Smith and Mickelson (2000) analyze an accountability system in North Carolina and find no effects on student achievement. Among others, Dee and Jacob (2011) analyze the impact of NCLB and find positive effects on math but no effects on reading achievement.

Some studies have also tried to disentangle the effects from different accountability regulations. Most of these studies focus on the role of stigma, i.e. ranking schools based on their performance, and find positive effects of stigma on educational improvement (e.g. Ladd and Glennie, 2001; Figlio and Rouse, 2006). There is also evidence that school competition can have a positive impact on student achievement (e.g. Figlio and Hart, 2014; West and Wößmann, 2010). However, there is still much uncertainty with respect to the role of private school vouchers in accountability systems.

Florida's accountability system provides several incentives for failing schools to raise educational performance. When a school fails to meet the proficiency requirements for the first time, it is assigned a publicly announced letter grade F. Apart from this stigma, however, this does not have further consequences at that time. Only when the school fails a second time within the following three years, students of that school are given the opportunity to transfer to a better performing public school in either the same or an adjacent school district. The potential outflow of students ultimately results in lower public funding.

Until 2006, students of failing schools were also offered state-funded vouchers to attend a private school of their choice. This option led to a heated debate about its lawfulness. Proponents of the voucher option argued that the threat of losing students to the private school sector would unfold a particular incentive for failing schools to raise performance (Peterson, 2006, p. 225). Critics attacked the voucher option for using tax-payer money to fund private schools (Kahn, 2006). This conflict led to a court case in which the Florida Supreme Court eventually ruled the voucher option unconstitutional. In 2006, it was terminated. However, the option to choose an alternative public school remained in place. This

circumstance provides a unique opportunity to evaluate the importance of private school vouchers in an accountability system like the one in Florida.

I use a rich, administrative, student-level dataset from Florida which provides information on all public school students in grades 3 to 10 with their annual test score results in math and reading and numerous demographic characteristics for the academic years 2002-03 to 2008-09. The dataset also allows identifying the school of the students in each year which enables me to merge school-specific information.

To compare the effect of sanction threats prior and post the 2006 voucher option termination, I employ a difference-in-discontinuities estimation strategy. This approach exploits the discontinuous jump along the grade point scale from F to D using regression discontinuity (RD) analysis. In addition, it already provides a significance test of the difference in the effect between the two time periods. I restrict the analysis to elementary schools due to insufficient sample sizes for middle and high schools.

Results suggest that the termination of the voucher option did not reduce the positive effect of the accountability system on schools' performance. I conduct several robustness and sensitivity checks, such as using low-stakes test scores as an alternative outcome measure or investigating different subgroups of the student population. Results are robust to these modifications. Although I cannot rule out the possibility that private school vouchers had a positive incentive effect at the time when the accountability system was first introduced, the findings of this study indicate that an effective school accountability system does not require private school vouchers in order to increase educational production of low-performing public schools.

The remainder of this chapter is organized as follows. Section 2.2 describes the Florida accountability system and the changes it underwent between 2002 and 2008 in more detail. Section 2.3 presents the data and estimation strategy used in this analysis. Results are presented in Section 2.4, while Section 2.5 contains robustness checks. Section 2.6 concludes.

2.2 Florida's A+ Accountability System

The core element of Florida's accountability system (named A+) is the statewide annual testing of students in grades 3 through 10 in various subjects. This statewide test is called the Florida Comprehensive Assessment Test (FCAT). It is the basis for calculating a schools'

performance grade of A, B, C, D, or F (highest to lowest). Initially, accountability testing comprised three subjects: mathematics, reading, and writing. In 2007, science was added as the fourth subject to be tested.

Every school is assessed in several performance categories which measure both student achievement in the current year as well as student learning gains compared to last year's results. For every performance category the percentage of students that meet a pre-defined proficiency level is calculated from the FCAT scores. The sum of these percentages constitutes a school's grade points. These are translated into a letter grade depending on a distinct cutoff value on the grade point scale.[1] The grade point scale has a lower limit of zero and an upper limit of 800 points since 2007. The cutoff values are at 525 points (A/B), 495 points (B/C), 435 points (C/D) and 395 points (D/F). Before 2007, the upper limit was at 600 points as there were only 6 performance categories at that time. The cutoff values were at 410 points (A/B), 380 points (B/C), 320 points (C/D) and 280 points (D/F).

When a school receives the first F in a 4-year-period, this does not have immediate consequences other than the stigma of failing minimum achievement requirements. Only the second F in a 4-year-period, triggers ultimate sanctions in the sense that students are given the opportunity to transfer to a higher scoring public school ("public school choice option"). Until 2006, students could also obtain a state funded voucher to attend a private school of their choice ("opportunity scholarship program"). In January 2006, the Florida Supreme Court ended the voucher option by ruling it unconstitutional. However, the public school choice option remained unaffected.

Florida's A+ accountability system underwent several changes and revisions since its first introduction in 1999. Apart from the termination of the voucher option, these changes applied to adjustments of the grading rule with the purpose of rising accountability standards: The system started off with three performance categories measuring the percentage of students proficient in mathematics, reading, and writing. In 2002, three more categories were added, measuring the learning gains in mathematics and reading as well as the learning gains of the lowest 25 percent in reading. In 2005, the range of students who are included in accountability calculations was extended to all students. Prior to this year, students with limited English proficiency and students with certain disabilities were excluded from

[1]For further details on the calculation procedure confer the annual Technical Assistance Paper (TAP) which can be downloaded from the website of the Florida Department of Education under http://schoolgrades.fldoe.org/reports/index.asp [last accessed: 03/26/2014]

accountability calculations.[2] In 2007, two more categories were added to school grades calculations: performance in FCAT science and learning gains of students scoring in the lowest 25 percent in mathematics. For high schools an additional category was introduced: performance of FCAT retakes in grades 11 and 12.

These adjustments always led to an increase in the number of F-schools in that particular year. In 2002, the number of elementary schools that were graded F totaled 38 (2.4 percent of all Florida public elementary schools) and went down to 16 (1.0 percent) in 2003. In 2005, 18 (1.1 percent) elementary schools received an F, but only 7 (0.4 percent) did so in 2006. In 2007, 30 (1.8 percent) schools were rated as failing, while this number dropped again to 21 (1.2 percent) in 2008. These numbers show that, in fact, only a very small fraction of schools was rated as failing. Table 2.1 gives an overview of the distribution of public elementary schools graded D or F for years 2002 to 2008.

Table 2.1: Distribution of Florida public schools graded D and F by year

	2002*	2003	2004	2005*	2006	2007*	2008
Elementary Schools							
D schools	121	52	62	79	36	51	54
F schools	38	16	10	18	7	30	21
$\sum$	159	68	72	97	43	81	75
Elementary schools in Florida	1581	1592	1614	1651	1639	1691	1726
% graded F	2.4	1.0	0.6	1.1	0.4	1.8	1.2

Notes: An asterisk indicates a year in which the grading rule was adjusted. Data from Florida Department of Education school accountability reports.

In the following analysis, I focus on elementary schools due to low sample sizes for middle and high schools.[3] Note, that the actual number of schools that can be used in the analysis is further reduced due to schools that stop operating and schools that received a second F grade in a four year period and thus are already exposed to sanctions. I refrain from running pooled regressions including all school types, because middle and high schools may be different from elementary schools in various respects. For example, middle and high schools are usually three to five times larger than elementary schools. It could be the case that they responded differently to accountability pressure compared to elementary schools.

[2]Prior to 2005, students with the following disabilities were excluded from accountability calculations: educable mentally handicapped, profoundly mentally handicapped, trainable mentally handicapped, orthopedically impaired, language impaired, deaf or hard of hearing, visually impaired, emotionally handicapped, specific learning disabled, dual-sensory impaired, autistic, severely emotionally disturbed, traumatic brain injured, developmentally delayed, or "other health impaired".

[3]See Table A2.1 for a distribution of middle and high schools that were graded D or F.

2.3 Empirical Framework

This section first describes the data used in this study and presents descriptive statistics. It then describes the empirical strategy for identifying the impact of the voucher option termination on a school's response to an F grade receipt.

2.3.1 Data and Descriptives

I use administrative, student-level data from the state of Florida. This dataset is provided by the Florida Department of Education Data Warehouse and provides information on all Florida public school students in grades 3 to 10 for school years 2001-02 to 2008-09 with roughly 1.5 million student observations each year. The dataset contains a student's annual FCAT scores in math and reading, demographic characteristics such as race, gender, limited English proficiency status, special education information, and free or reduced-price lunch eligibility.

The data also provides information on which school a student attended in a particular year. This enables me to merge school specific information. I obtain information on each school's performance grade from the School Accountability Reports website of the Florida Department of Education. In addition, I add school specific information such as operating costs, the number of violent offenses at the school within a year and information on overall teacher quality (i.e. (a) the percentage of teachers with an advanced degree and (b) teachers average years of experience) which I obtain from the Florida School Indicators Reports. I also include the information whether a school is located in a large city ($\geq$ 250,000 inhabitants) from the National Center of Education Statistics (NCES) Common Core of Data.

During the voucher period (i.e. during the school years 2002-03, 2003-04, and 2004-05) 64 F grades and 235 D grades were assigned to elementary schools. In comparison, 58 F grades and 141 D grades were assigned to schools during the no-voucher period. To evaluate the *threat* of vouchers, I drop those schools that received an F grade in the previous three years and are therefore already *exposed* to vouchers. I also drop schools that were no longer operating in the following school year. This lowers the sample to 45 F and 192 D school observations in the voucher years and 42 F and 117 D school observations in the no-voucher

years.[4] Note, that I do not use observations from the 2005-06 school year for the main analysis. Since the court ruling took place in the middle of the school year (January 2006) and the private school voucher option was immediately terminated it is a priori unclear whether this year should be considered a voucher or a non-voucher year. I include this year in a robustness check.

From this sample of schools, I drop students who are new to their school, as they might bias the achievement calculations depending on what school they attended before.[5] In addition, I drop students that are exempt from accountability calculations. In particular, these are limited English proficiency students in ESOL (English for speakers of other languages) programs for less than two years, and students with certain disabilities as mentioned in the previous section. However, this applies only to the 2002-03 and 2003-04 school year, as accountability adjustments in the 2004-05 school year led to the inclusion of all students in school grade calculation since then. The final dataset includes only students from D and F elementary schools in a particular year. This leaves us with a total of 89,080 student-year observations.

Table 2.2 contains summary statistics for elementary schools graded F and D in the voucher and the no-voucher period. It is noteworthy that the student body in both F and D schools is composed mainly of African American an Hispanic students, with F-schools being attended by even more African Americans and Hispanics on average than D schools. The average share of African American students in F schools was more than 80 percent in the voucher period and more than 70 percent in the no-voucher period. Another striking characteristic of these schools is the high rate of students which are eligible for free or reduced price lunch. In both sample years, about 90 percent of the students were classified as eligible for free or reduced price lunch.

These facts are important when considering the need of these schools to improve educational performance. Since African American students and poor students are often thought of being disadvantaged in comparison to other social groups, it is even more important to improve the academic performance of these schools. F schools also experience more violent offenses on average compared to D schools. Note also, that there are large differences in previous year FCAT scores between individual and peer level variables in the voucher period.

[4]I include charter schools in my analysis. Charter schools are privately operated public schools and the same accountability requirements apply to charter schools. Dropping charter schools from my analysis does not change the results.

[5]Including new students and controlling for new student status does not alter the results.

Table 2.2: Summary statistics

	Voucher		No Voucher	
	F-Schools	D-Schools	F-Schools	D-Schools
	Mean	Mean	Mean	Mean
	(SD)	(SD)	(SD)	(SD)
Individual Characteristics				
FCAT math score	-0.403	-0.342	-0.549	-0.46
	(0.868)	(0.88)	(0.961)	(0.968)
FCAT reading score	-0.396	-0.298	-0.544	-0.487
	(0.833)	(0.839)	(0.939)	(0.945)
FCAT math score in t-1	-0.536	-0.387	-0.696	-0.481
	(0.894)	(0.883)	(0.907)	(0.939)
FCAT reading score t-1	-0.432	-0.287	-0.575	-0.446
	(0.841)	(0.833)	(0.915)	(0.907)
African American	0.819	0.615	0.713	0.604
	(0.385)	(0.487)	(0.452)	(0.489)
Hispanic	0.112	0.193	0.206	0.254
	(0.316)	(0.394)	(0.404)	(0.436)
Female	0.517	0.519	0.489	0.502
	(0.5)	(0.5)	(0.5)	(0.5)
Free/reduced price lunch	0.894	0.836	0.896	0.88
	(0.308)	(0.37)	(0.306)	(0.325)
Grade 4	0.481	0.477	0.488	0.481
	(0.5)	(0.499)	(0.5)	(0.5)
Grade 5	0.475	0.504	0.494	0.489
	(0.499)	(0.5)	(0.5)	(0.5)
Grade 6	0.039	0.019	0.018	0.03
	(0.194)	(0.136)	(0.131)	(0.171)
Peer Characteristics (Classmates)				
FCAT math score in t-1	-0.698	-0.514	-0.699	-0.502
	(0.248)	(0.244)	(0.232)	(0.243)
FCAT reading score in t-1	-0.612	-0.443	-0.59	-0.459
	(0.227)	(0.235)	(0.214)	(0.226)
Share African American	0.797	0.603	0.711	0.604
	(0.197)	(0.304)	(0.271)	(0.315)
Share Hispanic	0.134	0.209	0.205	0.254
	(0.175)	(0.228)	(0.262)	(0.255)
Share Female	0.481	0.496	0.484	0.497
	(0.071)	(0.057)	(0.076)	(0.065)
Share FR-lunch	0.902	0.849	0.903	0.885
	(0.121)	(0.152)	(0.064)	(0.098)
Observations	3636	20086	3186	11939
Schools	45	192	42	117
School Characteristics				
Pupil-teacher-ratio in t-1	15.455	16.484	14.562	15.023
	(1.361)	(2.18)	(1.458)	(1.975)
ln(operating costs per student) in t-1	8.749	8.711	9.092	8.984
	(0.126)	(0.191)	(0.149)	(0.2)
Teacher's years of experience in t-1	10.751	10.783	11.562	10.775
	(2.972)	(3.127)	(4.032)	(2.967)
Number (violent) offenses in t-1	53.894	44.554	37.952	28.265
	(48.09)	(55.815)	(39.705)	(31.512)
Large city	0.427	0.541	0.685	0.619
	(0.495)	(0.498)	(0.465)	(0.486)
Observations	3427	19566	2290	6628
Schools	41	183	27	67

Notes: The table presents mean sample statistics for F and D schools for the voucher and the post-voucher period, respectively. FCAT math and reading scores are standardized with mean zero and standard deviation of one. Standard deviation in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

These differences are due to the exclusion of certain students from accountability calculations in those years as describes above. The test scores of these students are included in the mean calculations of the peer level variables.

2.3.2 Estimation Strategy

To estimate the effect of the termination of the private school voucher option on a school's performance, I use a difference-in-discontinuities regression approach as described by Grembi et al. (2012).[6] Starting point for this analysis is the RD estimation of the effect of receiving an F grade in year t-1 on a school's performance (as measured by its student's FCAT scores) in year t. School grades in Florida are determined by fixed cutoff values on a continuous grade points scale. In this context, it is intuitive to use a sharp regression discontinuities (SRD) design (Chiang, 2009). The equation to be estimated reads as follows:

$$Y_{ist} = \alpha_0 + \alpha_1 F_{s,t-1} + \alpha_2 GP_{s,t-1} + \alpha_3 (F_{s,t-1} \times GP_{s,t-1}) + X'_{ist}\gamma + Z'_{s,t-1}\zeta + \eta_t + \epsilon_{ist} \quad (2.1)$$

Y denotes the standardized FCAT score in math (and separately for reading) of student i in school s and year t. F is an indicator for a school which received an F grade in the previous year t-1. GP is the running variable and denotes a school's grade points from accountability calculation minus the cutoff value between D and F schools.[7] The interaction term $F \times GP$ adds additional flexibility with respect to the slope of the local linear regressions to both sides of the cutoff. X is a vector of student-level covariates and Z is a vector of school-level covariates. η denotes grade fixed effects and ϵ is an error term. To increase statistical power, I pool over the years before and after the regime change in 2006. The coefficient α_1 can be interpreted as the effect of receiving an F on student performance. However, we have to keep in mind, that this interpretation only applies to schools marginally close to the cutoff. A generalization to schools farther away from the cutoff is not easily possible.

The RD design aims at comparing the outcome of observations just above and just below the cutoff value. However, in practice we include also observations farther away from the cutoff in order to fit local polynomials to either side of the same. To determine the optimal

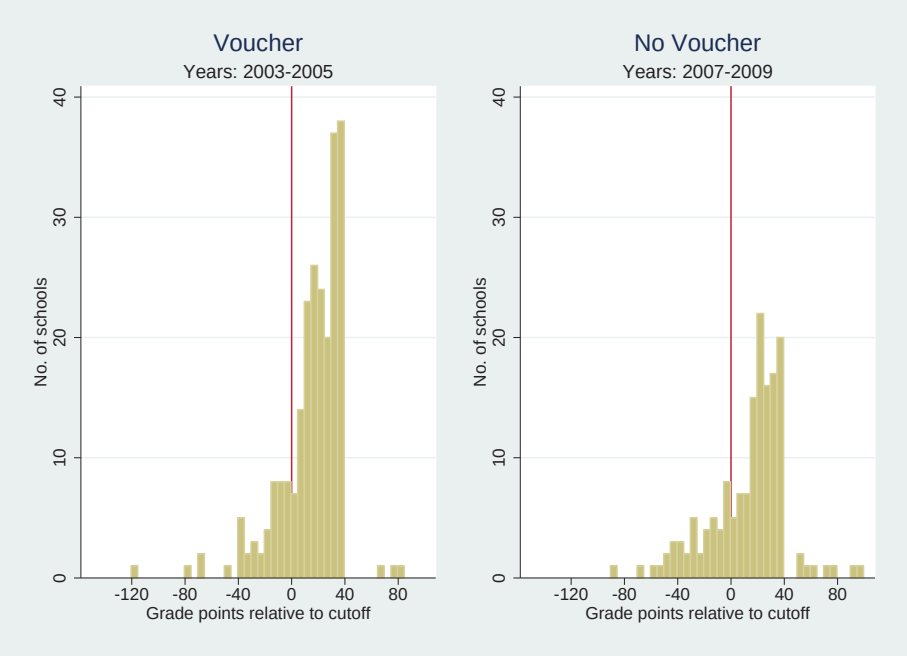
[6]The difference-in-discontinuities design combines difference-in-differences with RD estimation. In this study the RD estimates are compared across time periods. Other studies, such as Dickert-Conlin and Elder (2010) use a difference-in-discontinuities approach to compare RD estimates across spacial units.

[7]Until 2006, this cutoff value was at 280 grade points. From 2007 onwards, this cutoff value is at 395 grade points due to the introduction of additional performance categories.

bandwidth for the inclusion of observations in the regressions I use the cross-validation criterion presented e.g. in Chiang (2009, p. 1049).[8] The inclusion of observations farther away from the cutoff, however, may induce a bias which is one important reason why it is advisable to control for additional covariates (cf. Imbens and Lemieux, 2008, p. 626).[9] Therefore, I include individual and peer characteristics (as presented in Table 2.2) in the RD regression. I do not include school characteristics in the main analysis, as this would reduce the sample size considerably due to missing observations. I do, however, present a robustness check with the school level controls included.

The identifying assumption in RD analysis is that assignment into treatment (F schools) and control (D schools) around the cutoff was essentially random. Figure 2.1 shows the distribution of elementary schools graded F and D around the cutoff. There is no indication of heaping suggesting that schools were not able the manipulate their assignment into D and F grades.

Figure 2.1: Distribution of F and D elementary schools



Notes: The vertical line indicates the cutoff between F and D schools. *Voucher* refers to sample years 2003 - 2005, i.e. with private school voucher option. *No Voucher* refers to sample years 2007 - 2009, i.e. without private school voucher option.

[8] The cross-validation criterion method is an approach to find the optimal bandwidth for RD regressions. For every possible bandwidth the expected squared prediction error at the cutoff is calculated. The bandwidth that minimizes the squared prediction error at the cutoff is the optimal bandwidth.

[9] The inclusion of covariates can also help to increase the precision of the estimation.

An additional check of the random assignment assumption is to test for confounding discontinuities of observable covariates at the cutoff. Table 2.3 provides balancing tests of several observable characteristics by comparing the predicted values at the F/D cutoff for both sample periods. Although most of the estimates deviate in terms of their absolute value between F and D schools, the differences are not statistically significant. One exception, although small in magnitude, is the share of female students in the no-voucher period. The general picture that emerges supports our random assignment assumption.

The advantage of the difference-in-discontinuities approach is that it provides a significance test of the difference in the F effect prior and post the court ruling. The following equation represents the full setup:

$$\begin{aligned} Y_{ist} = \alpha_0 + \alpha_1 F_{s,t-1} + \alpha_2 GP_{s,t-1} + \alpha_3 (F_{s,t-1} \times GP_{s,t-1}) + X'_{ist}\gamma + \\ Z'_{s,t-1}\zeta + \eta_t + T_{t\geq 2007}[\beta_0 + \beta_1 F_{s,t-1} + \beta_2 GP_{s,t-1} + \beta_3 (F_{s,t-1} \times GP_{s,t-1}) + \\ X'_{ist}\delta + Z'_{s,t-1}\xi + \eta_t] + \epsilon_{ist} \end{aligned} \quad (2.2)$$

The indicator T is 1 for observations from 2007, 2008, and 2009. The coefficient of interest now is β_1 which measures the *change* in the F effect after the court ruling.[10]

Before turning to the results section, it is advisable to think about the expected outcomes. The accountability system incorporates three incentives (threats) for F schools to raise educational production. (A) The stigma effect, (B) the public school choice threat, and (C) the private school voucher threat. First we assume that A, B, C ≥ 0, i.e. none of them acts as a deterrent to raise performance. If $C > 0$ and A, B and C are constant over time, we should expect β_1 to be negative. If $C = 0$ and A, B and C are constant over time, we should expect β_1 to be zero. β_1 can also be positive, if A or B increase over time or if C decreases.[11] Another important factor are learning gains, i.e. over time schools learn from one another which strategies are most efficient to raise educational performance. In the presence of learning gains, we would expect a positive coefficient in all three above mentioned cases, unless C is very large.

[10] Adjustments to the grading rule should not induce a confounding effect, since both F and D schools were affected. Thus, they should already be accounted for when calculating the discontinuities.

[11] Ideally, one would like to test for these dynamics. For example, one could run year-by-year regressions and compare the F-effects across years. A positive or negative trend in the magnitude of the effect could indicate dynamics in the importance of the incentives over time. Unfortunately, year-by-year regressions are not feasible due to insufficient sample sizes.

Table 2.3: Predicted values at F/D cutoff

	Voucher			No Voucher		
	F-Schools	D-Schools	Diff. (D-F)	F-Schools	D-Schools	Diff. (D-F)
Individual characteristics						
FCAT math score in t-1	-0.463	-0.486	-0.023	-0.568	-0.643	-0.075
	(0.042)	(-0.486)	(0.065)	(0.037)	(-0.643)	(0.068)
FCAT reading score t-1	-0.354	-0.344	0.01	-0.497	-0.593	-0.096
	(0.036)	(-0.344)	(0.062)	(0.049)	(-0.593)	(0.059)
African American	0.855	0.71	-0.145	0.634	0.736	0.102
	(0.046)	(0.71)	(0.109)	(0.075)	(0.736)	(0.106)
Hispanic	0.079	0.116	0.038	0.293	0.247	-0.046
	(0.045)	(0.116)	(0.085)	(0.071)	(0.247)	(0.094)
Female	0.526	0.521	-0.006	0.471	0.509	0.038**
	(0.011)	(0.521)	(0.015)	(0.017)	(0.509)	(0.018)
Free/reduced price lunch	0.891	0.86	-0.031	0.894	0.944	0.05
	(0.019)	(0.86)	(0.054)	(0.016)	(0.944)	(0.032)
Grade 4	0.464	0.47	0.006	0.483	0.477	-0.006
	(0.029)	(0.47)	(0.032)	(0.02)	(0.477)	(0.024)
Grade 5	0.471	0.505	0.034	0.503	0.485	-0.018
	(0.027)	(0.505)	(0.033)	(0.02)	(0.485)	(0.024)
Grade 6	0.056	0.025	-0.031	0.014	0.038	0.024
	(0.025)	(0.025)	(0.028)	(0.015)	(0.038)	(0.029)
Peer characteristics (Classmates)						
FCAT math score in t-1	-0.644	-0.628	0.016	-0.579	-0.641	-0.062
	(0.037)	(-0.628)	(0.062)	(0.035)	(-0.641)	(0.066)
FCAT reading score in t-1	-0.54	-0.527	0.013	-0.496	-0.589	-0.093
	(0.041)	(-0.527)	(0.07)	(0.042)	(-0.589)	(0.058)

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	Voucher			No Voucher		
	F-Schools	D-Schools	Diff. (D-F)	F-Schools	D-Schools	Diff. (D-F)
Share African American	0.828	0.687	-0.141	0.634	0.732	0.098
	(0.048)	(0.687)	(0.108)	(0.075)	(0.732)	(0.104)
Share Hispanic	0.105	0.143	0.038	0.287	0.237	-0.05
	(0.047)	(0.143)	(0.085)	(0.072)	(0.237)	(0.093)
Share Female	0.496	0.496	0	0.481	0.501	0.02
	(0.008)	(0.496)	(0.013)	(0.015)	(0.501)	(0.015)
Share FR-lunch	0.904	0.871	-0.033	0.9	0.944	0.044
	(0.017)	(0.871)	(0.052)	(0.015)	(0.944)	(0.031)
Observations	3636	20086		3186	11939	
Schools	45	192		42	117	
School characteristics						
Pupil-teacher-ratio in t-1	15.464	16.153	0.689	14.623	14.96	0.337
	(0.342)	(16.153)	(0.834)	(0.403)	(14.96)	(0.605)
ln(operating costs per student) in t-1	8.712	8.669	-0.042	9.089	9.067	-0.022
	(0.025)	(8.669)	(0.064)	(0.039)	(9.067)	(0.069)
Teacher's years of experience in t-1	10.415	10.246	-0.169	11.558	9.556	-2.003
	(0.608)	(10.246)	(1.031)	(1.884)	(9.556)	(1.662)
Number (violent) offenses in t-1	60.331	50.764	-9.567	35.271	36.431	1.159
	(13)	(50.764)	(16.963)	(10.331)	(36.431)	(14.215)
Large city	0.498	0.487	-0.011	0.722	0.692	-0.031
	(0.127)	(0.487)	(0.185)	(0.15)	(0.692)	(0.199)
Observations	3427	19566		2290	6628	
Schools	41	183		27	67	

Notes: The table presents predicted values of covariates at the F/D cutoff for F and D schools for the years with and without vouchers, respectively. FCAT math and reading scores are standardized with mean zero and standard deviation of one. Standard errors in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

2.4 Results

2.4.1 Effect of F Grade on Educational Performance

Following Imbens and Lemieux (2008), I first inspect a graphical illustration of the RD-setup. Figure 2.2 shows scatter plots of the dependent variable (FCAT math and reading score, respectively) against school grade points. For clearness reasons, I combine schools within 5-point bins of school grade points, so that every dot in the figures represents the average of the dependent variable within a 5-point bin of school grade points.[12] As mentioned above, I also normalize the grade points variable by subtracting the cutoff value of the respective year, so that the F/D-cutoff is at zero grade points. From the figures, one can easily observe a jump in the dependent variable at the F/D-cutoff for both subjects. Schools located just to the left of the cutoff exhibit a higher average FCAT score in the following year than schools located just to the right of the cutoff.[13] This is in line with the hypothesis that the receipt of an F grade triggers actions at the school to improve educational performance. The fact that there is obviously also a positive treatment effect in the no-voucher years suggests that private school vouchers did not play a major role in this regard.[14]

Table 2.4 shows regression results of the effect of receiving an F grade on the academic performance of a school's students in the following year's FCAT math and reading test, respectively, using observations within the optimal bandwidth.[15] The table shows the average effects for the voucher years (Columns 1 and 4) and for the no-voucher years (Columns 2 and 5) as well as the difference between voucher and no-voucher years (Columns 3 and 6). Estimates shown in columns 1 - 3 do not include any controls, while estimates shown in columns 4 - 6 include individual- and peer-level controls as shown in Table 2.2 plus quadratic and cubic terms of previous year FCAT math (reading) scores and a dummy indicating a year in which the grading rule was adjusted.

The effects vary between the no-control and full-control specifications. Especially for the no-voucher years this difference is quite large (4.1 percentage points in math and 7.7 percentage points in reading), however not statistically significant. For the reasons mentioned

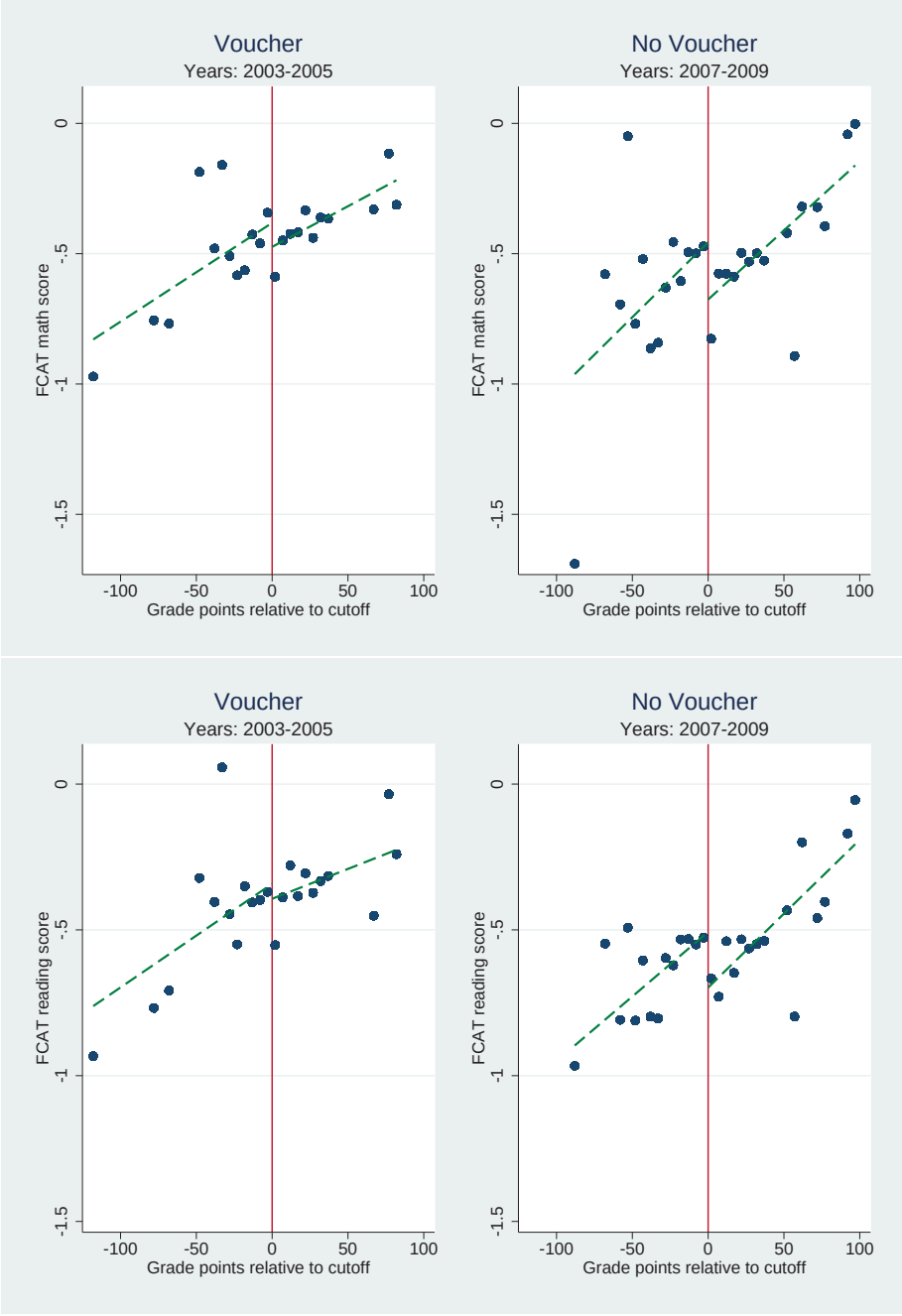
[12] This means that one dot might in fact represent the average of more than just one school.

[13] For middle and high schools there is no clear distinction at the cutoff for the voucher years and a positive effect for the no-voucher years (see Figure A2.1).

[14] This finding is also present in a year-by-year analysis (see Figure A2.2).

[15] See Table A2.2 for results using the maximum bandwidth, which are similar to the ones presented here.

Figure 2.2: The relationship between educational performance and previous year grade points



Notes: The figure shows scatter plots of average standardized FCAT scores (y-axis) within 5-point bins of previous year's school grade points (x-axis) and a linear fit on both sides of the cutoff. The vertical line marks the cutoff between F schools on the left and D schools on the right.

Table 2.4: Effect of F grade on FCAT scores (using optimal bandwidth)

	Optimal Bandwidth					
	No Controls			Full Controls		
	Voucher	No Voucher	Diff.	Voucher	No Voucher	Diff.
	(1)	(2)	(3)	(4)	(5)	(6)
Math						
'F'-Effect	0.0496	0.2051***	0.1555	0.0401	0.1643***	0.1242
	(0.0773)	(0.0678)	(0.1028)	(0.0558)	(0.0557)	(0.0789)
Observations	23241	15211	38452	23241	15211	38452
D-Schools	190	117	307	190	117	307
F-Schools	40	41	81	40	41	81
R^2	0.002	0.012	0.011	0.514	0.553	0.533
Reading						
'F'-Effect	-0.0317	0.1954***	0.2271**	0.0144	0.1185***	0.104
	(0.0719)	(0.0691)	(0.1034)	(0.0514)	(0.0387)	(0.0648)
Observations	23193	15058	38251	23193	15058	38251
D-Schools	189	116	305	189	116	305
F-Schools	40	41	81	40	41	81
R^2	0.002	0.007	0.016	0.553	0.56	0.561

Notes: Each coefficient in columns (1) - (6) is from a separate RD estimation. In the top panel, the dependent variable is the standardized FCAT math score and in the bottom panel the standardized FCAT reading score. *Voucher* refers to sample years 2003 to 2005, *No Voucher* to sample years 2007 to 2009. The optimal bandwidth for the math regressions from the voucher period is 43 grade points on the left side and 76 grade points on the right side of the F/D cutoff. For the reading regressions in the same period it is 42/62. Optimal bandwidth for the no-voucher period: 85/99 for the math regressions and 87/97 for the reading regressions. The full controls regressions include all individual and peer characteristics presented in Table 2.2 plus quadratic and cubic terms of previous year FCAT math (reading) scores and a dummy indicating a year in which the grading rule was adjusted. Robust standard errors (in parentheses) clustered at the school level. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

in the previous section, I refer to the full-controls specification (Columns 4 - 6) as the preferred specification. During the voucher years, an F grade triggered schools to improve educational performance by 4 percent of a standard deviation in math and 1.4 percent of a standard deviation in reading. During the no-voucher years, an F grade caused a performance increase of roughly 16.4 percent of a standard deviation in math and 11.8 percent of a standard deviation in reading. Although the differences are not statistically significant (Column 6), the point estimates are substantially higher in the no-voucher period.[16]

This result suggests that the threat of private school vouchers did not play a major role as an incentive to raise educational performance in elementary schools. If the private school voucher threat had been the key incentive for schools to raise performance in the voucher years, one would have expected a much lower - almost close to zero - effect in the no-voucher years. But what might have caused the larger positive effects in the no-voucher years?

[16]Table A2.3 shows results from pooled regressions for middle and high schools in the preferred specification. While there is no clear F grade effect in the voucher years, there is a large positive effect in the no-voucher years.

One obvious explanation is that over time schools could have developed more effective and sophisticated strategies to increase educational performance. Schools that received an F grade in the later years of our sample period likely profited from the experiences made by schools in the earlier years.

2.4.2 Heterogeneous Effects by Subgroups of the Student Population

An interesting question is, whether there are heterogeneous effects from accountability pressure within or across schools and how these effects change with the voucher option termination. Table 2.5 contains differences in the F effect (difference-in-discontinuity estimates) between voucher and no-voucher years for different subgroups of the student population. For every subgroup, the point estimates are positive. Although the size of the effect varies between groups, the differences are not statistically significant. Therefore, there is no indication that the results obtained in the previous analysis are restricted to special subgroups.

2.4.3 Heterogeneous Effects by Location of the School

From a theoretical point of view, it is also possible that schools which operate in a more competitive environment respond differently to accountability pressure than schools in less competitive environments. In particular, one could imagine that schools in large urban areas with potentially more private school competitors reduced their effort after the private school voucher option was terminated. To test this hypothesis, I ran separate regressions on two subsamples of schools depending on their location.[17] Results are presented in Table 2.6. The point estimates are positive for schools in large cities (i.e. $\geq$ 250000 inhabitants) and zero for schools in other regions (including small towns and rural areas). However, the effects are very imprecisely estimated so that the differences are not statistically significant. This makes it hard to draw a meaningful conclusion. If at all, one could argue that the private school voucher threat did not play a major role even in areas with potentially more private school competitors around.

[17]Due to otherwise small sample sizes I had to split the sample into schools in large cities (i.e. $\geq$ 250000 inhabitants) and a category comprising all schools in different areas (including small towns and rural areas).

Table 2.5: Difference in 'F'-Effect between voucher and no-voucher years by subgroups of the student population

	Optimal Bandwidth & Full Controls						
	Afr. Am.	Hispanics	Whites	Females	Males	F/R price lunch eligible	F/R price lunch ineligible
Math							
Δ 'F'-Effect	0.0724	0.2044	0.2465	0.1618**	0.0872	0.1359*	0.047
	(0.0857)	(0.1618)	(0.158)	(0.0782)	(0.0916)	(0.0826)	(0.0991)
Observations	24506	7874	4969	19665	18787	33060	5392
D-Schools	305	246	234	307	307	306	287
F-Schools	81	53	55	81	81	80	76
R^2	0.491	0.55	0.601	0.534	0.535	0.516	0.601
Reading							
Δ 'F'-Effect	0.069	0.2476**	0.039	0.0948	0.1105	0.1179*	0.0919
	(0.0723)	(0.0981)	(0.1281)	(0.0714)	(0.0732)	(0.0673)	(0.1009)
Observations	24535	7730	4894	19565	18686	32925	5326
D-Schools	303	244	232	305	305	304	286
F-Schools	81	54	55	81	81	80	76
R^2	0.515	0.576	0.629	0.563	0.557	0.539	0.622

Notes: The table shows the difference-in-discontinuities between voucher and no-voucher years by different subgroups of the student population. Robust standard errors (in parentheses) clustered at the school level. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table 2.6: Difference in 'F'-Effect between voucher and no-voucher years by location of the school

	Optimal Bandwidth & Full Controls	
	Large city	Other
Math		
Δ 'F'-Effect	0.1368	0.0065
	(0.1218)	(0.1089)
Observations	21747	16705
D-Schools	167	140
F-Schools	47	34
R^2	0.53	0.544
Reading		
Δ 'F'-Effect	0.1409*	-0.005
	(0.0755)	(0.093)
Observations	21809	16442
D-Schools	167	138
F-Schools	47	34
R^2	0.557	0.57

Notes: The table shows the difference-in-discontinuities between voucher and no-voucher years by different school location. *Large city* refers to an urban area with a population size $\geq$ 250,000. *Other* includes all areas with less than 250,000 inhabitants. Robust standard errors (in parentheses) clustered at the school level. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

2.5 Robustness

2.5.1 Dependent Variable

One concern is that the FCAT scores are not an objective measure for student achievement. Schools might have found ways to "game the system" (Figlio, 2006) so that students score high on the FCAT but the actual achievement increase is low or negligible. To investigate this concern, I use the test scores from a low-stakes exam (SAT-9/10) as an alternative outcome measure. The SAT is administered together with the FCAT, but is not used for accountability purposes. Table 2.7 shows the results from the respective regressions using the preferred specification.[18] Overall, the estimates are similar to the ones from Table 2.4 using the FCAT scores as dependent variable. The results support the conclusion that the private school voucher threat did not play a major role as an incentive to rise educational performance.

[18]Note, that in the no-voucher period not all schools participated in the low-stakes exam, which is why the sample size is considerably lower.

Table 2.7: Effect of F grade on low-stakes test scores

	Optimal Bandwidth & Full Controls		
	Voucher	No Voucher	Diff.
	(1)	(2)	(3)
Math			
'F'-Effect	0.0316	0.0953	0.0637
	(0.0495)	(0.063)	(0.0816)
Observations	22345	8740	31085
D-Schools	190	66	256
F-Schools	30	25	55
R^2	0.552	0.588	0.566
Reading			
'F'-Effect	-0.0067	0.1175**	0.1242*
	(0.0406)	(0.0476)	(0.064)
Observations	22879	8992	31871
D-Schools	189	66	255
F-Schools	40	29	69
R^2	0.578	0.549	0.569

Notes: Each coefficient in columns (1) - (3) is from a separate RD estimation. In the top panel, the dependent variable is the standardized math score on the SAT-9/10 (measured in national percentile ranks) and in the bottom panel the standardized reading score on the same test. *Voucher* refers to sample years 2003 to 2005, *No Voucher* to sample years 2007 to 2009. The optimal bandwidth for the math regressions from the voucher period is 43 grade points on the left side and 76 grade points on the right side of the F/D cutoff. For the reading regressions in the same period it is 42/62. Optimal bandwidth for the no-voucher period: 85/99 for the math regressions and 87/97 for the reading regressions. The full controls regressions include all individual and peer characteristics presented in Table 2.2 plus quadratic and cubic terms of previous year FCAT math (reading) scores and a dummy indicating a year in which the grading rule was adjusted. Robust standard errors (in parentheses) clustered at the school level. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

2.5.2 Sample

In all of the regressions presented above I dropped observations from 2006, since the court ruling took place in the middle of the academic year. As an additional robustness check, Table 2.8 presents regression results with observations from 2006 included (Columns 2 and 3). Column 2 presents regression results treating 2006 as a voucher year, while column 3 contains results treating 2006 as a no-voucher year. For comparison, columns 1 and 4 show the average F effect for the voucher and the no-voucher years as in the tables above (without observations from 2006). Treating 2006 as a voucher year increases the point estimates of the F effect for the voucher years. On the other hand, treating 2006 as a no-voucher year slightly reduced the point estimates of the F effect for the no-voucher years. This finding is in line with the hypothesis that over time schools learn more effective ways to increase educational performance. Again, the results support our conclusion that the private school voucher threat was not the most important incentive to raise educational production at failing schools.

Table 2.8: Effect of F grade including observations from 2006

	Optimal Bandwidth & Full Controls			
	Voucher years		No-voucher years	
	2003-2005	2003-2006	2006-2009	2007-2009
	(1)	(2)	(3)	(4)
Math				
'F'-Effect	0.0401	0.0753*	0.1551***	0.1643***
	(0.0558)	(0.0458)	(0.0493)	(0.0557)
Observations	23241	29471	22892	15211
D-Schools	190	241	179	117
F-Schools	40	50	53	41
R^2	0.514	0.524	0.555	0.553
Reading				
'F'-Effect	0.0144	0.0386	0.0982***	0.1185***
	(0.0514)	(0.0423)	(0.0325)	(0.0387)
Observations	23193	28984	22755	15058
D-Schools	189	236	177	116
F-Schools	40	50	53	41
R^2	0.553	0.548	0.554	0.56

Notes: This table presents RD estimates including observations from 2006. In the top panel, the dependent variable is the standardized FCAT math score and in the bottom panel the standardized FCAT reading score. The optimal bandwidth for the math regressions from the voucher period is 43 grade points on the left side and 76 grade points on the right side of the F/D cutoff. For the reading regressions in the same period it is 42/62. Optimal bandwidth for the no-voucher period: 85/99 for the math regressions and 87/97 for the reading regressions. Robust standard errors (in parentheses) clustered at the school level. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

2.5.3 School-level Controls

Table 2.9 presents regression results controlling for school characteristics such as pupil-teacher-ratio or operating costs per student (see Table 2.2). As stated earlier, this reduces the sample size due to missing observations in these variables. The overall finding, however, does not change. Point estimates in the no-voucher period are higher than in the voucher period.

2.6 Conclusion

This study evaluates the role of private school vouchers as a key to success in a school accountability system. In Florida, students of repeatedly failing public schools were allowed to either attend a better performing public school or to obtain a state funded voucher to transfer to a private school. However, in 2006, the Florida Supreme Court ruled the private school voucher option unconstitutional. Since then, students of repeatedly failing public schools are left with the option of transferring to a better performing public school.

Table 2.9: Effect of F grade including school level controls

	Optimal Bandwidth		
	Voucher	No Voucher	Diff.
	(1)	(2)	(3)
Math			
'F'-Effect	0.1301**	0.1652***	0.0351
	(0.0522)	(0.0626)	(0.0828)
Observations	21911	8297	30208
D-Schools	181	63	244
F-Schools	28	23	51
R^2	0.518	0.541	0.529
Reading			
'F'-Effect	0.0164	0.1292***	0.1128
	(0.0544)	(0.0462)	(0.0713)
Observations	22496	8491	30987
D-Schools	180	63	243
F-Schools	38	26	64
R^2	0.554	0.547	0.557

Notes: This table presents RD estimates including school level control variables. In the top panel, the dependent variable is the standardized FCAT math score and in the bottom panel the standardized FCAT reading score. *Voucher* refers to sample years 2003 to 2005, *No Voucher* to sample years 2007 to 2009. Optimal bandwidths used to either side of the cutoff respectively: Column (1) Math: 25/76, Reading: 42/62; Column (2) Math: 42/62, Reading: 57/68. Regressions include all individual, peer and school characteristics presented in Table 2.2 plus quadratic and cubic terms of previous year FCAT math (reading) scores and a dummy indicating a year in which the grading rule was adjusted. Robust standard errors (in parentheses) clustered at the school level. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

I apply a difference-in-discontinuities regression approach to evaluate the impact of the court's decision on the academic performance of elementary schools. Results show that in both states (with and without the private school voucher threat) schools increased educational performance once they were rated as failing. Interestingly, the positive effects are larger in the no-voucher period than in the voucher period. A plausible explanation for this finding are learning gains. Over time, schools learn which strategies work best to increase educational performance.

The findings of this study suggest that in a school accountability system like the one in Florida other incentives apart from private school vouchers are sufficient to make it work. It is still possible, however, that private school vouchers had an impact on school's performance when the accountability system was first introduced. Furthermore, it is also possible that the increase in educational production in the no-voucher years might have been even higher with the threat of private school vouchers. When evaluating the effectiveness of school accountability systems, one should not forget that school accountability might not necessarily improve student achievement even if the estimated effects are positive. Instead, it may increase strategic behavior at the schools. Examples hereof are the manipulation of exams

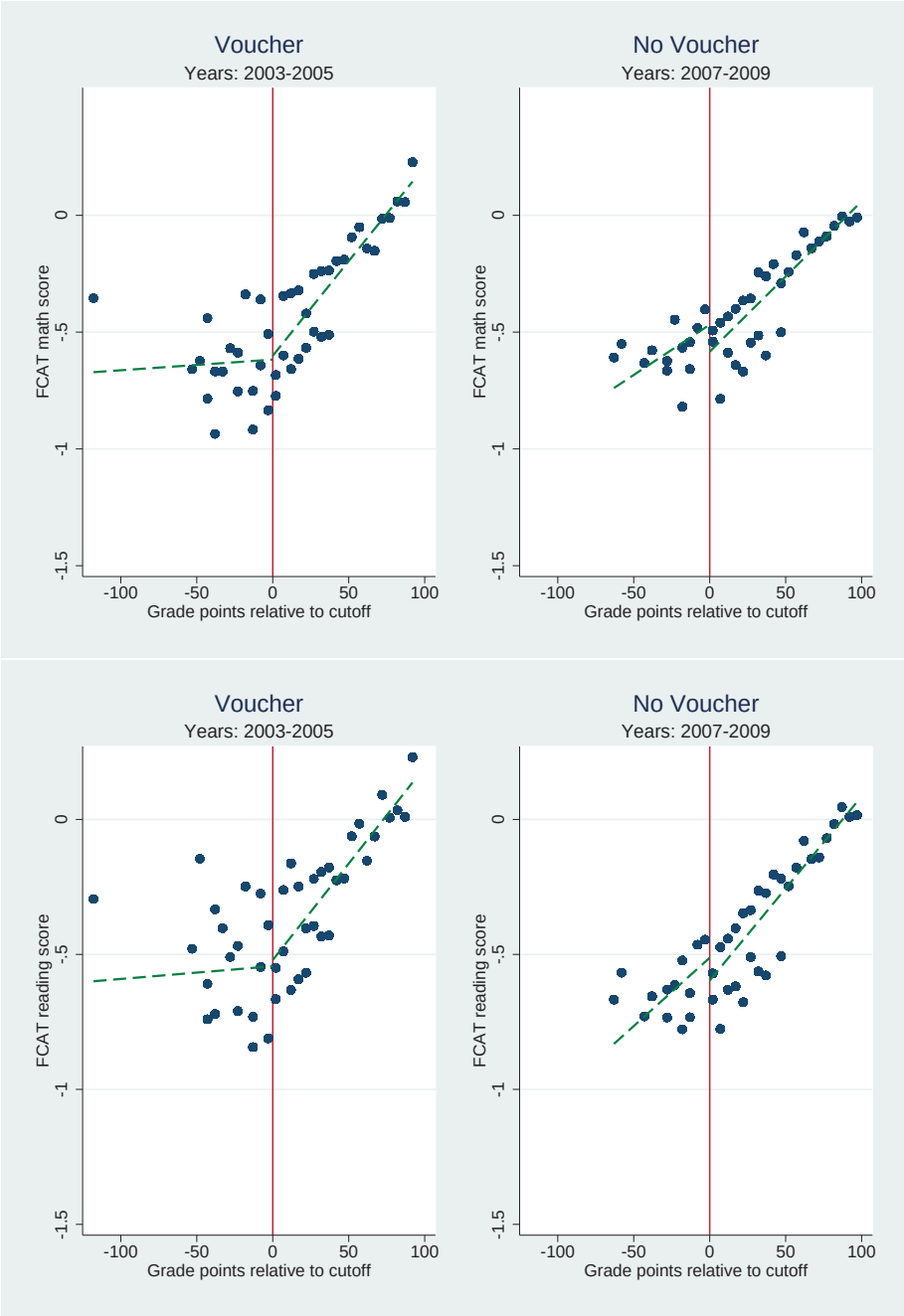
by teachers (Jacob and Levitt, 2003) or the suspension of bad students on testing days (Figlio, 2006).

It lies beyond the scope of this study to ultimately answer the question which component of the incentive scheme serves as the major incentive for school improvement. Is it the school choice threat (i.e. the threat of losing students and funding to other schools) or is it the stigma of being publicly labeled as a failing school? Despite much research on this question, the evidence is still mixed. Several studies report positive effects of school accountability systems that do not provide a school choice option and only rank schools based on their performance (Ladd and Glennie, 2001; Figlio and Rouse, 2006). On the other hand, there is also evidence that stigma alone might not be a sufficient incentive in the case of the Floridian school accountability system (Chakrabarti, 2013, 2008).

Nevertheless, the findings of this study are important for policy-makers around the world trying to set up effective school accountability systems. Knowing that a successful accountability system does not necessarily require private school vouchers might facilitate its implementation. On the one hand, a private school voucher option can be quite costly. On the other hand, there might exist legal issues as in Florida that prohibit its implementation. However, it is important to keep in mind that there is no "one size fits all" accountability system. Policy-makers are well advised to take into account local conditions and peculiarities when setting up a school accountability system. A valuable contribution of future research could be to investigate to what extend certain incentives work in different school environments and education systems.

Appendix

Figure A2.1: The relationship between educational performance and previous year grade points for middle and high schools



Notes: The figure shows scatter plots of average standardized FCAT scores (y-axis) within 5-point bins of previous year's school grade points (x-axis) for middle and high schools pooled. The vertical line marks the cutoff between F schools on the left and D schools on the right. The lines on both sides of the cutoff represent a linear fit of the data.

Figure A2.2: The relationship between educational performance and previous year grade points by year

Notes: The figure shows scatter plots of average standardized FCAT scores (y-axis) (by subject and year) within 5-point bins of previous year's school grade points (x-axis) and a linear fit on both sides of the cutoff. The vertical line marks the cutoff between F schools on the left and D schools on the right.

Table A2.1: Distribution of public middle and high schools in Florida graded D and F

	2002*	2003	2004	2005*	2006	2007*	2008
Middle Schools							
D schools	18	18	25	32	6	44	20
F schools	6	1	17	8	1	12	3
$\sum$	24	19	42	40	7	56	23
Elementary schools in Florida	476	487	502	530	531	553	558
% graded F	1.3	0.2	3.4	1.5	0.2	2.2	0.5
High Schools							
D schools	40	52	83	95	67	102	70
F schools	19	12	15	21	10	30	16
$\sum$	59	64	98	116	77	132	86
Elementary schools in Florida	340	356	364	391	400	411	394
% graded F	5.6	3.4	4.1	5.4	2.5	7.3	4.1

Notes: An asterisk indicates a year in which the grading rule was adjusted. Data from Florida Department of Education school accountability reports.

Table A2.2: Effect of F grade on FCAT scores (using maximum bandwidth)

	Maximum Bandwidth					
	No Controls			Full Controls		
	Voucher	No Voucher	Diff.	Voucher	No Voucher	Diff.
	(1)	(2)	(3)	(4)	(5)	(6)
Math						
'F'-Effect	0.0805	0.2068***	0.1263	0.0855*	0.165***	0.0796
	(0.0655)	(0.067)	(0.094)	(0.0487)	(0.0549)	(0.0743)
Observations	23857	15220	39077	23857	15220	39077
D-Schools	192	117	309	192	117	309
F-Schools	45	42	87	45	42	87
R^2	0.003	0.012	0.012	0.515	0.553	0.534
Reading						
'F'-Effect	0.0263	0.2007***	0.1743*	0.0678*	0.113***	0.0452
	(0.0593)	(0.0662)	(0.0923)	(0.0413)	(0.0366)	(0.0556)
Observations	23933	15253	39186	23933	15253	39186
D-Schools	192	117	309	192	117	309
F-Schools	45	42	87	45	42	87
R^2	0.004	0.009	0.017	0.553	0.562	0.562

Each coefficient in columns (1) - (6) is from a separate RD estimation. In the top panel, the dependent variable is the standardized FCAT math score and in the bottom panel the standardized FCAT reading score. *Voucher* refers to sample years 2003 to 2005, *No Voucher* to sample years 2007 to 2009. The full controls regressions include all individual and peer characteristics presented in Table 2.2 plus quadratic and cubic terms of previous year FCAT math (reading) scores and a dummy indicating a year in which the grading rule was adjusted. Robust standard errors (in parentheses) clustered at the school level. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table A2.3: Effect of F grade for middle and high schools

	Optimal Bandwidth & Full Controls		
	Voucher	No Voucher	Diff.
	(1)	(2)	(3)
Math			
'F'-Effect	-0.001	0.1397*	0.1407*
	(0.0224)	(0.0756)	(0.0784)
Observations	78186	76037	154223
D-Schools	198	264	462
F-Schools	43	21	64
R^2	0.607	0.638	0.624
Reading			
'F'-Effect	-0.0221	0.1976**	0.2197**
	(0.0303)	(0.0883)	(0.0922)
Observations	78905	61036	139941
D-Schools	198	248	446
F-Schools	43	23	66
R^2	0.598	0.606	0.603

Notes: This table presents RD estimates of the F effect for middle and high schools. In the top panel, the dependent variable is the standardized FCAT math score and in the bottom panel the standardized FCAT reading score. *Voucher* refers to sample years 2003 to 2005, *No Voucher* to sample years 2007 to 2009. Optimal bandwidths used to either side of the cutoff respectively: Column (1) Math: 118/84, Reading: 118/84; Column (2) Math: 15/99, Reading: 19/89. Regressions include all individual, peer and school characteristics presented in Table 2.2 plus quadratic and cubic terms of previous year FCAT math (reading) scores and a dummy indicating a year in which the grading rule was adjusted. Robust standard errors (in parentheses) clustered at the school level. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Chapter 3

The Effect of University Openings on Local Human Capital Formation: Difference-in-differences Evidence from Germany

3.1 Introduction

Increasing participation in tertiary education is a key policy objective in much of the world.[1] An obvious, albeit costly, way of increasing human capital investment at the tertiary level would be to open new universities in regions without prior local university access. By extending the regional coverage of tertiary education opportunities and thereby reducing the average costs of investment in tertiary education, policy-makers may hope to increase the demand for tertiary education. However, whether this strategy actually has the desired effect remains an empirical question.

This chapter investigates the importance of local university access to tertiary education participation by the local population. I collected information on new university openings during the large tertiary education expansion in Germany that occurred between 1960 and 1979, which I then linked to micro data on educational attainment from the German Socio-

[1]For example, the EU 2020 Agenda of the European Commission states the goal to raise tertiary education levels in member states to at least 40 percent among 30 to 34 year-olds (cf. http://ec.europa.eu/europe2020/europe-2020-in-a-nutshell/targets/index_en.htm).

Economic Panel (SOEP). Exploiting variation in local university availability over time and between counties, I estimate the effect of local university access on obtaining a tertiary degree in the local population in a difference-in-differences framework.

Results show that the effect of a new university opening on obtaining a tertiary degree is 7.2 percent for openings from the 1960s and 70s which is roughly equal to an increase in the tertiary attainment rate of 1 percentage point. This is the effect in the total local population including individuals from all secondary schooling tracks of which only high track leavers can immediately proceed to university. In addition, I find that the effect is more pronounced for females and individuals from low parental education background. Moreover, the effect seems to be driven by openings of universities of applied sciences which were first introduced during that time. I also follow the question whether we may expect similar effects for university openings nowadays by analyzing more recent university openings from the 1990s and 2000s. Results provide positive support for this claim.

To my knowledge, this study is the first to evaluate the effect of a new university opening on tertiary education attainment in the local population in Germany. Most existing research has either focused on the importance of universities as regional economic factors (e.g. Blume and Fromm, 2000; Stoetzer and Krähmer, 2007; Leusing, 2007; Pavel, 2008) or has evaluated determinants of investments in human capital at the tertiary level such as parental education background (e.g. Cameron and Heckman, 2001) or financial constraints (e.g. Dynarski, 2003). The study closest related to mine analyzes the effect of university openings on enrollment rates among the local population in Canada (Frenette, 2009). Results show that a local university increases enrollment rates by 1.3 percentage points.

The remainder of the chapter proceeds as follows. Section 3.2 contains an overview of related literature. In section 3.3, I briefly describe the process of the tertiary education expansion in Germany. A description of the data and the estimation strategy employed is provided in Section 3.4. Results are presented in Section 3.5. In Section 3.6, I present robustness checks and discuss potential biases that may arise due to data limitations. Section 3.7 concludes.

3.2 Related Literature

This study relates to two different areas of research. One investigates the determinants of tertiary education participation to derive policy recommendations on how to increase investment in human capital formation at the tertiary level. The other evaluates the regional economic effects of universities in the light of infrastructure investment.

Previous studies on the determinants of tertiary education participation identify parental education as one of the most relevant factors in the decision to enroll in tertiary education (e.g. Cameron and Heckman, 2001; Burnhill, Garner, and McPherson, 1990). In a recent study for Germany, however, Riphahn and Schieferdecker (2012) analyze intergenerational education mobility and find that in the transition to tertiary education, the effect of parental education, though important, is less significant than the effect of parental income.

It makes intuitive sense that financial constraints could inhibit the transition to tertiary education. As shown by Dynarski (2003), student financial aid has a strong positive effect on college attendance and completion rates. She finds that a 1,000 US-Dollar student benefit increases the share of high school graduates enrolled in higher education by 3.6 percentage points. Other studies that focus on the United States find similar results (e.g. Leslie and Brinkman, 1988; Kane, 1994; Dynarski, 2000). In a recent study for Germany, Steiner and Wrohlich (2012) find a positive but much lower effect (1.5 percentage points) of public student aid on enrollment rates.

Being located far from a university can pose considerable costs of obtaining a tertiary education. In this respect, the local availability of a university decreases the access costs and thus may act as a strong incentive for investment in tertiary education. Several studies relying on this distance-cost argument exploit the variation in local university access in order to obtain consistent estimates of the returns to education (e.g. Card, 1995; Currie and Moretti, 2003; Moretti, 2004).

The relationship between access costs and enrollment rates is further explored in two recent studies, both of which find large and robust effects (Frenette, 2009, for Canada; Spieß and Wrohlich, 2010, for Germany). Frenette (2009) examines the effect of a local university on tertiary enrollment rates in Canada in the 1980s and 90s. In his empirical approach, he uses a dummy variable indicating the presence of a local university in a census metropolitan area to estimate the effect of distance on the probability of attending tertiary education.

Results indicate an increase of 6.4 percentage points in the university attendance rate when a local university is available. However, this result is mainly due to a substitution effect from college to university, so that a net effect of 1.3 percentage points remains.

Spieß and Wrohlich (2010) analyze the enrollment decisions of German high track leavers conditional on the distance to the nearest university. To capture non-linearities in the distance effect, the authors use four dummy variables representing the four quartiles of the distance distribution. Their estimation results suggest that there is a threshold around 12.5 km. Below this threshold, distance does not seem to matter for the transition to university. Above this threshold, the probability of attending a university is reduced by 7 percentage points compared to the reference category (0 km - 6.5 km).

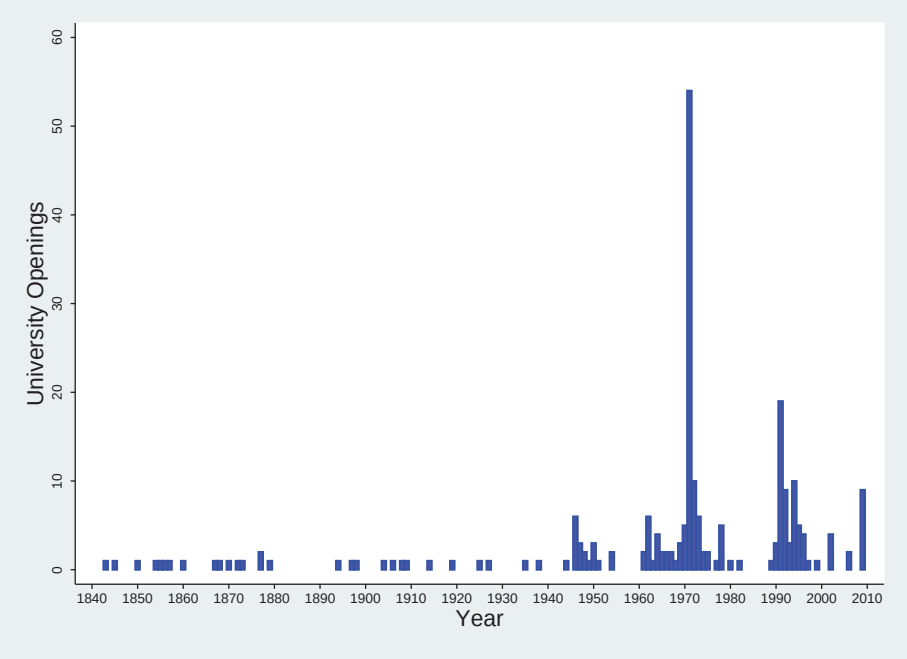
The role of universities as important economic factors and their impact on regional economic prosperity in Germany have been analyzed in a number of case studies. In particular, see Blume and Fromm (2000) for the analysis of the University of Kassel, Leusing (2007) for the analysis of the University of Flensburg and Pavel (2008) for the analysis of the Technical University of Berlin. In general, universities are found to have a strong and positive impact on the local economy due to direct and indirect demand effects. For example, the operation of a university requires personnel and other resources and attracts students who themselves increase demand for certain products and services. However, these effects may not necessarily be limited to universities but may also be achieved through other kinds of infrastructure investments. Also, the general equilibrium effects when considering the entire national economy are likely to be zero-sum. This increases the desirability to investigate to what extend a local university can have positive net effects on human capital formation.

3.3 Tertiary Education Expansion in Germany

The expansion of the tertiary education sector in Germany after World War II occurred in three main phases. The first increase in the number of universities occurred during the post-World War II era, roughly from 1946 to 1955. The second and largest increase took place during the 1960s and 70s. During this period a new type of university was introduced into the higher education system, the so-called universities of applied sciences ("Fachhochschulen"). The third large expansion of tertiary education institutions happened in the 1990s. However, a significant part of this last increase was due to the inclusion of

universities from East Germany after the reunification in 1990. Figure 3.1 shows the number of university openings per year and provides a clear illustration of the expansion activity during each of the three phases. The expansion peaked in 1971 with the foundation of 42 new universities.

Figure 3.1: University openings in Germany



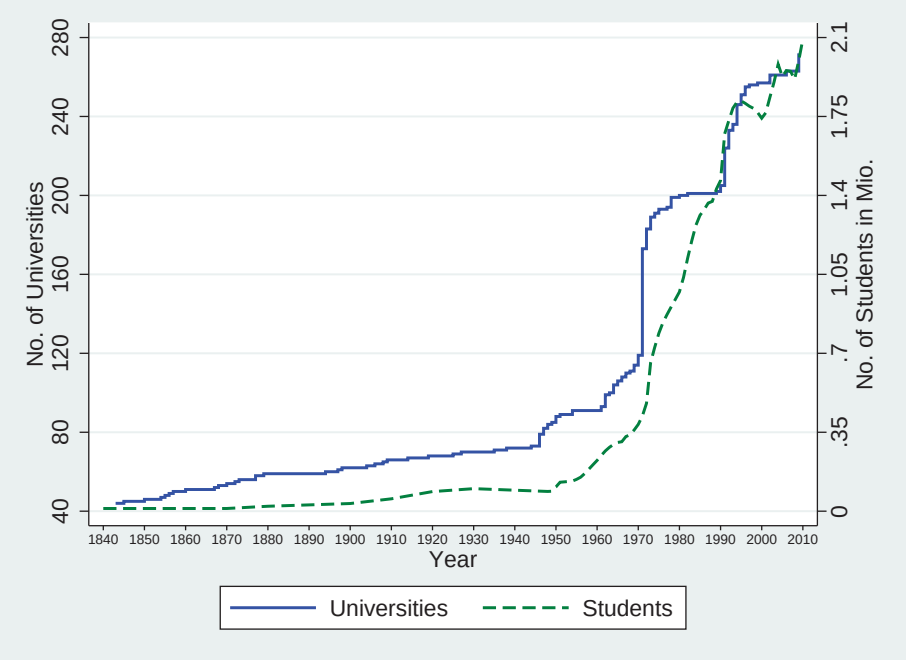
Notes: Based on data from the German Rector's Conference (2011). When a university opening comprised separate campuses in different counties, each of these campuses were counted separately.

Universities of applied sciences as a special type of university within the German tertiary education system were introduced by state law in 1968. They were intended to provide a higher education with a stronger focus on applied concepts. All study programs of universities of applied sciences usually include a mandatory internship at a firm. Indeed, a key characteristic of a university of applied sciences is its strong cooperation with local firms. Thus, the needs and requirements of the local labor market can be taken into account. The variety of course programs increased substantially since the early days, but the main subject areas are 'engineering' and 'business'.

The expansion of the tertiary education system allowed more students to enroll in university studies. Figure 3.2 shows the rapid increase in the number of students following the expansion of the tertiary education sector. There were many reasons behind expanding the tertiary education sector. At the beginning of the process in the 1950s, one major aspect was the Cold War. Supporters of the expansion argued that increased investment in human

capital was essential to effectively compete with countries of the Soviet Union (Picht, 1964). Over time, however, the founding of a new university was accompanied by less political ideology. Nowadays, it is primarily viewed as a public infrastructure investment which can stimulate the local economy of underdeveloped regions.

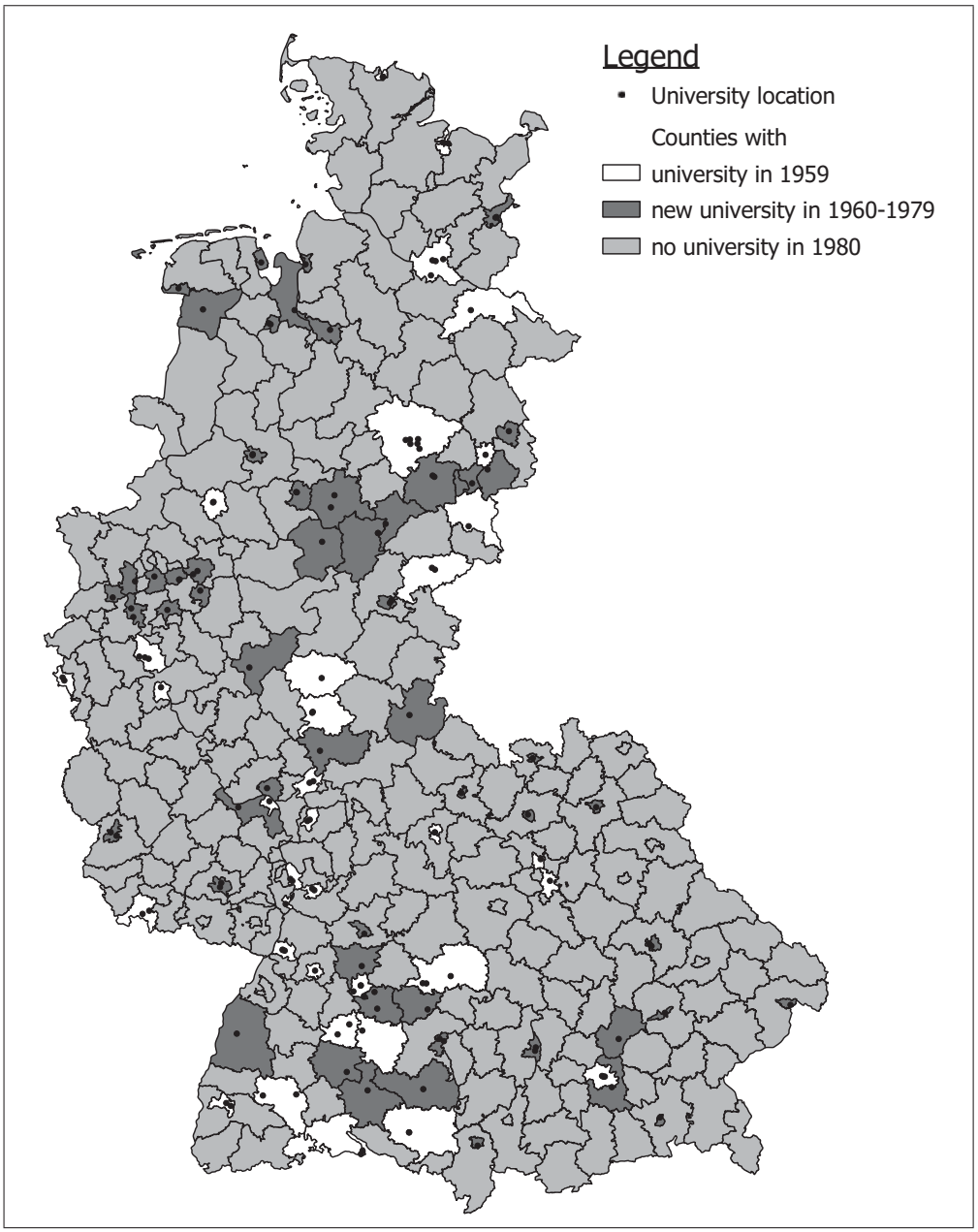
Figure 3.2: Development of tertiary education supply and demand in Germany



Notes: The solid line indicates the number of public universities in Germany over time (based on data from the German Rector's Conference, 2011). The dashed line indicates the total number of students in Germany over time (based on data from Rüegg (2004) for years prior to 1948 and from the Federal Ministry of Education and Research (2011) for years 1948 and after).

In the 1960s and 70s, policy-makers wanted to increase the tertiary education supply as quickly as possible and in some cases used the infrastructure of previously existing institutions such as schools of engineering. These institutions, however, were neither homogeneously structured nor part of the tertiary education system. Figure 3.3 shows the geographic distribution of universities in West Germany until 1980 which marks the end of the major expansion period. Dark shaded counties acquired a university between 1960 and 1979, light shaded counties did not have a university by 1980 and white colored counties already acquired a university prior to 1960.

Figure 3.3: Regional availability of universities in West Germany



Notes: The figure shows the geographical distribution of tertiary education supply (universities) across West German counties. White colored counties acquired a university prior to 1960. Dark shaded counties acquired a first university between 1960 and 1979. Light shaded counties had no university in 1980 or before.

3.4 Empirical Framework

3.4.1 Data and Descriptives

I use data from the German Socio-Economic Panel (SOEP), a large panel study that was initiated in 1984 with annual follow-up. In 2009, more than 20,000 individuals in roughly

12,000 households were interviewed. The SOEP is a representative survey of the population living in Germany and contains rich information on socio-economic variables. Despite all these advantages, however, I cannot analyze the majority of university openings using the panel structure of the SOEP.[2] Therefore, I use information from the latest available wave at the time of this study (i.e. the 2009 wave). The large time lag enables us to analyze tertiary degree completion rather than enrollment rates. For every individual, the data contain information on the highest degree achieved, father's and mother's education, gender and immigrant status and the county of residence in 2009.

In addition to the SOEP data, I obtained comprehensive information on all universities in Germany, including the date and place of their foundation from the HRK (German Rector's Conference).[3] Based on a university's location, I merged this information with the SOEP data at the county level. This means that all individuals in a particular county are assigned the same information regarding university openings.

Table 3.1 presents summary statistics of individuals in our sample based on whether they live in a county with a first university opening between 1960 and 1979 or not. It becomes evident that the rate of university graduates is 6.3 percentage points higher in counties with a first opening between 1960 and 1979 compared to counties with no university during this period. The other characteristics are balanced across the two groups except for parental education background. A plausible explanation is that higher educated parents - because they value education opportunities - moved with their children to counties with a newly opened university.

3.4.2 Estimation Strategy

The aim of this study is to estimate the effect of a new university opening on obtaining a tertiary degree in the local population.[4] During the period of the large tertiary education expansion in the 1960s and 70s in Germany, 93 new universities were opened, among them

[2]This is because the majority of new university openings occurred in the 1960s and 70s, prior to initiation of the SOEP survey. I do, however, exploit the panel structure to analyze university openings in the 1990s and 2000s in the robustness section.

[3]A list of universities in Germany, including detailed information, can be downloaded from the webpage of the German Rector's Conference at: http://www.hochschulkompass.de/index.html. However, the information supplied is not always accurate. To ensure the correct opening year, I went through each university's individual webpage to verify its date of opening.

[4]In contrast to many existing studies, I consider university completion rather than university enrollment as the outcome variable, for two reasons. First, a tertiary degree is an important job market certificate. Second, the difference between the number of students in the first year of university studies and those who

Table 3.1: Summary statistics

Mean sample characteristics of individuals

	Counties with green-field opening btw. 1960 and 1979			Counties with no university and no opening			
	Mean (SD)	Min Max	Obs	Mean (SD)	Min Max	Obs	Δ Mean [SE]
Tertiary degree	0.190	0	810	0.127	0	2398	0.063***
	(0.393)	1		(0.333)	1		[0.015]
Year of birth	1952.816	1912	819	1953.411	1914	2406	-0.595
	(14.443)	1981		(14.510)	1981		[0.585]
Female	0.518	0	819	0.509	0	2406	0.009
	(0.50)	1		(0.50)	1		[0.020]
High parental education	0.090	0	819	0.057	0	2406	0.033***
background	(0.287)	1		(0.232)	1		[0.011]
Immigrant	0.016	0	819	0.011	0	2406	0.005
	(0.125)	1		(0.103)	1		[0.005]
Family Status							
Married	0.692	0	819	0.726	0	2406	-0.033*
	(0.462)	1		(0.446)	1		[0.019]
Single	0.101	0	819	0.092	0	2406	0.009
	(0.302)	1		(0.289)	1		[0.012]
Divorced	0.038	0	819	0.037	0	2406	0.001
	(0.191)	1		(0.188)	1		[0.008]
Widowed	0.107	0	819	0.089	0	2406	0.018
	(0.310)	1		(0.285)	1		[0.012]
Secondary Education							
Hauptschule	0.444	0	819	0.523	0	2406	0.078***
(Basic Track)	(0.500)	1		(0.500)	1		[0.020]
Realschule	0.277	0	819	0.260	0	2406	0.017
(Middle Track)	(0.448)	1		(0.440)	1		[0.018]
Gymnasium	0.244	0	819	0.183	0	2406	0.061***
(High Track)	(0.430)	1		(0.387)	1		[0.017]

Notes: Based on data from the SOEP, 2009. Standard deviations in parentheses and standard errors in squared brackets. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

86 public universities. We focus on public universities, as private institutions play only a marginal role in the German tertiary education system. In the case of 45 openings, the university was established in counties which previously did not have any tertiary education

actually complete a degree can be substantial; based on administrative data for 2006, the student drop-out rate at German universities is about 30 percent (Heublein et al., 2008).

institution (henceforth called "green-field university openings").[5] This enables us to use a difference-in-differences estimation approach to discover the effect of a university opening on obtaining a tertiary degree in the local population. In particular, I estimate a fixed effects model represented by the following equation:

$$University\ degree_{ijk} = \alpha + \mu_j + \lambda_k + \beta Uni22_{jk} + X'_{ijk}\gamma + \epsilon_{ijk} \tag{3.1}$$

The dependent variable *University degree* is a dummy variable for whether individual i in county j from cohort k obtained a tertiary degree (=1) or not (=0). μ indicates county dummies and λ indicates cohort dummies. $Uni22$ is a dummy variable that denotes the presence of a local university at age 22 of an individual from cohort k in county j. X is a vector of individual level covariates such as gender, parental education background and immigrant status. ϵ denotes an idiosyncratic error term. To account for dependence of observations within county and cohort, standard errors are clustered at the county × cohort level.

The parameter of interest is β. It provides an estimate for the change in probability of obtaining a tertiary degree when a local university is present at age 22 of the individual compared to the case in which there is no university. There are two reasons for choosing age 22 as the age to determine university presence: First, the usual age of high track leavers in Germany is 19 or 20. For male students, the mandatory military service requirement in the 1960s and 70s ranged from 15 to 18 months which increases the university entrance accordingly. Some individuals also do vocational training before entering university which further increases the entrance age. Second, I used the data to empirically distinguish the most relevant age for university entrance. To this end, I ran the same regression using varying age cutoffs - from 18 to 26. Results, shown in Table A3.1, reveal that the effect is strongest when using age 22 and decreases almost symmetrically for ascending and descending age values.

I estimate several specifications of the model, including a specification with linear age trends instead of cohort fixed effects to uncover potential differences in the effect due to different modeling approaches. Note, however, that the cross-sectional approaches are prone to omitted variable bias. Therefore, the difference-in-differences (DiD) approach - as shown

[5] As explained above, some university openings had independent entities in distant regions. In particular, the 45 green-field openings affected 54 West German counties.

by the equation above - is our preferred specification. The intuition behind is as follows: Individuals in counties with a university opening constitute the treatment group, while individuals in counties without a university (and no opening) constitute the control group. Econometrically, these groups are represented by the county dummies in our model. A longitudinal before-after perspective is obtained by including cohort dummies in our model, with which we essentially compare individuals over different age cohorts.

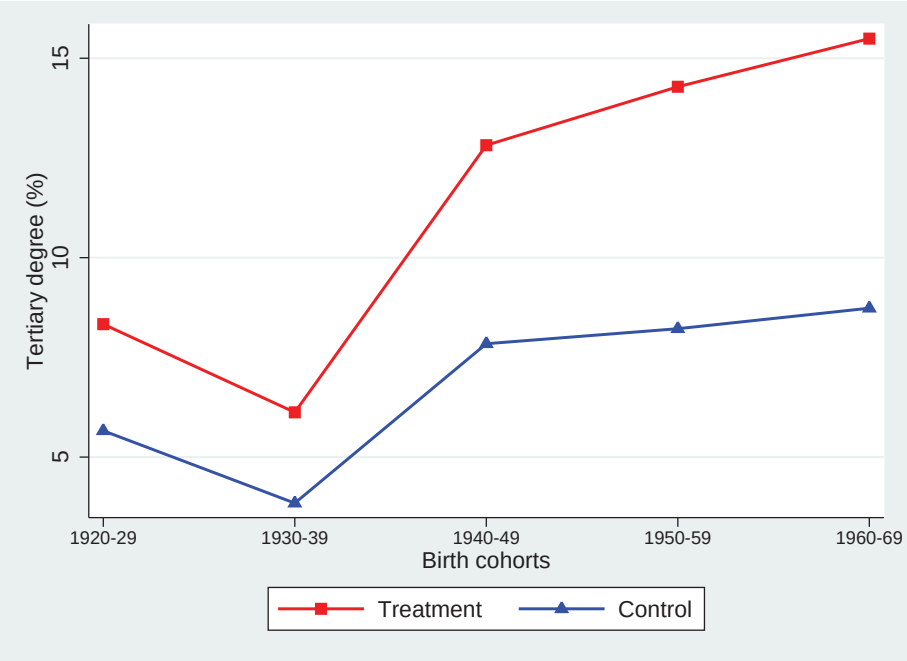
The identifying assumption in a DiD estimation approach is that both treatment and control groups follow a common time trend, meaning that there are no time effects which affect both groups in different ways. Suggestive evidence that this assumption is not violated can be obtained from inspecting the "pre-treatment" trends in tertiary degree completion of both groups which should be equal. To this end, Figure 3.4 depicts the share of university graduates for different birth cohorts in our data. Due to small sample sizes in the older cohorts, it was necessary to pool individuals over ten years respectively. The share of university graduates in treatment counties is always higher than in control counties. However, for birth cohorts until 1939, both lines follow a common trend. The two lines diverge for birth cohorts from 1940 onwards with a steeper trend in treatment counties than in control counties. Individuals of the 1940 birth cohort were among the first affected by the new university openings as they reached the usual university entrance age in the early 1960s.

In my analysis, I use an individual's residential information to relate him or her to a particular university opening. Unfortunately, the data does not contain information on an individual's place of residence at the time he finishes high school. Therefore, I had to use the residential information given at the time of the interview in 2009. This means that we might introduce measurement error in the treatment variable due to misclassification of treatment status for individuals who relocated to a different county. However, the data provides some indirect information on an individual's place of childhood residence, i.e. whether the place of childhood residence and the place of residence at the time of the interview are the same or not.[6]

To circumvent the problem of misclassification, I restrict the analysis to those individuals for whom we know the place of childhood residence (henceforth called "non-mover"). The

[6]When sampled for the first time, each individual completes a bibliography questionnaire. One question asks where a person lived until age 15. Another question asks whether the person still lives in that same place today. The possible options for answering are: 1. Yes, still; 2. Yes, again; 3. No. Since we know an individual's place of residence at the time of the interview, we also know the place of childhood residence for individuals who marked options 1 and 2.

Figure 3.4: Share of university graduates by treatment status



Notes: Based on data from the SOEP (2009). *Treatment* refers to birth cohorts from counties with first university between 1960 and 1979 (treatment counties). *Control* refers to birth cohorts from counties with no university in 1980 or before (control counties).

drawback is that, because migration is usually non-random, we estimate a treatment effect for a very distinct - namely less mobile - group of individuals. Therefore, the results need to be interpreted as local average treatment effects (LATE). However, from a policy-makers perspective, the non-mover might in fact constitute the relevant target group. Extending the tertiary education supply to regions with no prior universities should, in particular, lower the transaction costs of individuals for whom the costs associated with leaving the local area are very high. In Section 3.6, I present a bounding analysis where I estimate the effect using the full sample (including individuals who relocated) to gauge the potential range of the treatment effect.

I also restrict the sample to individuals with age 28 or older in 2009, i.e. individuals born in 1981 or earlier. Given a university entrance age of 22 and an average duration of study of about five years, the inclusion of younger cohorts might induce a bias due to not yet completed university studies. Our data, therefore, covers birth cohorts from 1912 to 1981, which is sufficient for our DiD analysis.

In our empirical framework, treatment status is defined at the county level. Due to the fact, that counties also represent natural and administrative environments such as settlement

structures or commuting zones, using these special units in the analysis is preferred over using arbitrarily defined concentric circles around an individual's place of residence.

Finally, some further considerations regarding the estimated effects are worthwhile mentioning. Consider two counties, A and B. County A acquires a new university, county B does not; thus, A is considered a treatment county and B a control county. Under the assumption that the university opening only affects the population of county A, we can calculate the exact treatment effect of this opening. However, the university opening in A likely affects individuals in B as well. If this effect is positive, individuals may move or commute to the university in county A. Econometrically, this would lead to an underestimation of the treatment effect. The result would be a lower-bound estimate. However, theoretically, there is also the possibility that the effect of a new university opening in county A can have a negative effect in county B, as a result of a general equilibrium effect. Due to increased tertiary education enrollment in county A, the return expectations from tertiary education might fall, so that in county B, where access costs are higher, fewer individuals than before obtain a tertiary degree. Consequently, the DiD estimator would overstate the true effect of a university opening in our framework.

3.5 Results

The following section presents the main results of our analysis for the non-mover population. All regressions are based on linear probability models.

3.5.1 Effect of University Opening on Tertiary Education Attainment

Table 3.2 presents estimation results for the effect of a university opening using different model specifications. The dependent variable is a dummy variable indicating whether or not an individual obtained a tertiary degree. In specification 1, the only explanatory variable is an indicator for the presence of a university at age 22 in the individual's county of residence. The result suggests that a university opening is associated with an increase in the probability of obtaining a tertiary degree of 9.1 percent. Specification 2 includes individual level control variables such as gender, parental education background and immigrant status. In addition,

specification 3 includes county fixed effects, while specification 4 also includes a linear age trend.

Table 3.2: Effect of university opening on tertiary education attainment

Dep. Var.: University degree (1=yes; 0=no)

	(1)	(2)	(3)	(4)	(5)
University at age 22	0.091***	0.073***	0.071***	0.076***	0.072**
	(0.019)	(0.018)	(0.025)	(0.027)	(0.029)
Female		-0.081***	-0.080***	-0.080***	-0.082***
		(0.011)	(0.011)	(0.011)	(0.012)
High parental educ.		0.417***	0.389***	0.390***	0.395***
		(0.035)	(0.036)	(0.036)	(0.036)
Immigrant		0.009	-0.003	-0.000	0.013
		(0.054)	(0.061)	(0.061)	(0.062)
Year of birth				-0.000	
				(0.000)	
Constant	0.129***	0.151***	0.192	0.640	0.104
	(0.007)	(0.010)	(0.130)	(0.828)	(0.168)
County FE	No	No	Yes	Yes	Yes
Cohort FE	No	No	No	No	Yes
Observations	3208	3208	3208	3208	3208
Cluster	1540	1540	1540	1540	1540
R^2	0.009	0.110	0.198	0.198	0.219

Notes: Based on data from the SOEP, 2009. The sample includes only non-movers as described in the text. Robust standard errors in parentheses clustered at the (county × cohort) level. Specifications (2) - (5) include dummy variables indicating missing observations in parental education and immigrant status, respectively. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

The estimated effect for the presence of a university at age 22 ranges from 7.1 percent in specification 3 to 9.1 percent in specification 1. Specifications 1 - 4 represent cross-sectional modeling approaches which are potentially prone to omitted variable bias. Specification 5 represents our DiD model with cohort and county fixed effects. The estimated effect of a university opening on obtaining a tertiary degree is 7.2 percent which is similar to specifications 2 and 3 but lower than in specifications 1 and 4. These differences might be due to omitted variable bias in the cross-sectional modeling approaches as mentioned earlier.

The effects of the individual characteristics on obtaining a tertiary degree are similar across all specifications. Females tend to have a lower probability of obtaining a tertiary degree compared to males of about 8 percent. Also, as one would expect, parental education background is a strong predictor for tertiary education attainment. An individual with at least one parent with a tertiary degree is about 40 percent more likely to obtain a tertiary degree than an individual where neither of the parents has a tertiary degree.

The rate of university graduates in counties with no university is 12.7 percent (see Table 3.1). Our results imply that a new university increases this rate by roughly 1 percentage

point to 13.6 percent (12.7∗1.072=13.61). This increase is similar to the 1.3 percentage points found by Frenette (2009) for Canada. However, Frenette estimates the effect on enrollment and not degree completion as in this analysis. Due to dropout, the hypothetical effect on degree completion in his study should be lower and even closer to the one we found.

3.5.2 Heterogeneous Effects by Gender, Attended School Track and Parents Education

To explore the effect of local university supply in further detail, I ran separate regressions on subgroups of the individuals in our sample. Results are reported in Table 3.3.

Table 3.3: Heterogeneous effects by gender, attended school track and parental education

Dep. Var.: University degree (1=yes; 0=no)

	Females	Males	High track leaver	Middle and low track leaver	No parent with tertiary degree
University at age 22	0.072**	0.066	0.167	0.024	0.081***
	(0.033)	(0.049)	(0.114)	(0.021)	(0.030)
Female			-0.147***	-0.029***	-0.083***
			(0.049)	(0.007)	(0.011)
Parent with tertiary	0.389***	0.389***	0.273***	0.065*	
degree	(0.049)	(0.055)	(0.061)	(0.038)	
Immigrant	0.017	0.012	-0.252	0.102	-0.026
	(0.059)	(0.100)	(0.173)	(0.080)	(0.065)
Constant	0.086	0.397	0.876***	-0.070	0.033
	(0.159)	(0.435)	(0.250)	(0.075)	(0.163)
Cohort FE	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Observations	1640	1568	640	2470	2998
Cluster	1084	1047	516	1323	1486
R^2	0.314	0.271	0.517	0.134	0.153

Notes: Based on data from the SOEP, 2009. Robust standard errors in parentheses clustered at the (county × cohort) level. The corresponding regressions in columns (1) and (2) include dummy variables indicating missing observations in parental education and immigrant status, respectively. The regressions in column (3) includes a dummy variable indicating missing observations for immigrant status. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

The treatment effect from a university opening among females is 7.2 percent and statistically significant on the 5 percent level. For males the effect is insignificant, but the point estimate is only slightly lower with 6.6 percent. This may suggests that females had higher costs of obtaining a tertiary degree compared to males. One explanation could be the differences in perceived societal role of males and females at that time. Until the 1960 and 70s, females were widely expected to becoming housewives and mothers, so that financial resources within a household were first allocated to the education of males. When access

costs decreased with the opening of a local university, it became easier for females to obtain tertiary education.

Attending a university in Germany requires one to have a university entrance diploma ("Abitur") or an equivalent certification. The Abitur is directly awarded to high track leavers (Gymnasium). However, special schooling opportunities such as evening classes exist for graduates from the middle and low track (Real-/Hauptschule) to make up for university entrance. Column 3 shows the effect of a university opening for high track leavers. Although not statistically significant, the point estimate is 0.167. In comparison, the point estimate of the effect for individuals from the two lower tracks is only 0.024.

The last column of Table 3.3 presents the effect for individuals from lower parental education background, i.e. neither parent obtained a tertiary degree. The estimated effect is 0.081 and highly statistically significant. Unfortunately, it was not possible to estimate a separate effect for individuals from higher parental education background, i.e. for individuals whose parents obtained tertiary education due to insufficient sample sizes.

3.5.3 Heterogeneous Effects by Type of New University

The treatment effect may also depend on the type of the opened university, i.e. rather research-oriented (university) or rather applied (university of applied sciences).[7] Results from the respective regressions are presented in Table 3.4. Using only openings of universities of applied sciences, the estimated effect on tertiary education attainment is 9.8 percent. The effect is only 5 percent and statistically insignificant when estimated using only openings of (research-oriented) universities. This implies that the results of our analysis are mainly driven by openings of universities of applied sciences. It may be that the course programs offered at this type of university were of greater relevance to our target group.

The results presented in this section indicate that the effect from a university opening in the 1960s and 70s is especially pronounced among females and individuals from low parental education background and that they were in particular responsive to an opening of a university of applied sciences. In general, these results provide additional support for the hypothesis that reducing access costs to tertiary education facilities can help increase the share of university graduates in a local population.

[7] See Section 3.3 for a description of differences of the two types of universities.

Table 3.4: Heterogeneous effects by type of university

Dep. Var.: University degree (1=yes; 0=no)

	University	University of applied sciences
University at age 22	0.050	0.098**
	(0.035)	(0.042)
Female	-0.079***	-0.078***
	(0.012)	(0.012)
High parental educ.	0.397***	0.381***
	(0.038)	(0.041)
Immigrant	0.011	0.021
	(0.063)	(0.068)
Constant	0.033	-0.084
	(0.157)	(0.111)
Cohort FE	Yes	Yes
County FE	Yes	Yes
Observations	2896	2710
Cluster	1363	1349
R^2	0.222	0.229

Notes: Based on data from the SOEP, 2009. Robust standard errors in parentheses clustered at the (county × cohort) level. Regressions include dummy variables indicating missing observations in parental education and immigrant status, respectively. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

3.6 Robustness

3.6.1 Dependent Variable

In much of the educational literature, 'years of schooling' is used to measure educational attainment. As a robustness check, I therefore employ 'years of schooling' as the dependent variable. Table 3.5 presents the respective results. Point estimates from our DiD estimation suggest that a university opening increased the average individual's educational attainment by 0.232 years of schooling, although the estimate is not statistically significant.

3.6.2 More Recent University Openings

The main results of this study are obtained by analyzing university openings from the 1960s and 70s. Without further evidence it is not clear that a university opening nowadays can be expected to induce effects of similar magnitude. This aspect, however, is crucial for policy recommendations. To shed some light on the question to what extend the regional expansion of tertiary education supply can be expected to yield similar effects today, I also evaluated

Table 3.5: Effect of university opening on years of schooling

Dep. Var.: Years of schooling

	(1)	(2)	(3)	(4)	(5)
University at age 22	0.937***	0.787***	0.955***	0.324*	0.232
	(0.125)	(0.118)	(0.160)	(0.174)	(0.184)
Female		-0.461***	-0.434***	-0.421***	-0.433***
		(0.075)	(0.077)	(0.076)	(0.077)
High parental educ.		3.329***	3.112***	2.971***	3.010***
		(0.200)	(0.212)	(0.215)	(0.214)
Immigrant		0.339	0.169	-0.170	-0.059
		(0.281)	(0.348)	(0.361)	(0.354)
Year of birth				0.026***	
				(0.003)	
Constant	11.592***	11.714***	11.610***	-39.139***	9.993***
	(0.049)	(0.066)	(0.827)	(5.638)	(1.010)
County FE	No	No	Yes	Yes	Yes
Cohort FE	No	No	No	No	Yes
Observations	3190	3190	3190	3190	3190
Cluster	1535	1535	1535	1535	1535
R^2	0.020	0.155	0.252	0.270	0.289

Notes: Based on data from the SOEP, 2009. Robust standard errors in parentheses clustered at the (county × cohort) level. Specifications (2) Ũ (5) include dummy variables indicating missing observations in parental education and immigrant status, respectively. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

more recent university openings. In the 1990s and 2000s, there were altogether 25 green-field university openings in West German counties.[8]

For the analysis we can exploit the panel structure of the data and directly assign the treatment (university presence at age 22) to all individuals based on the place of childhood residence. In particular, I refer to the place of residence as the county in which an individual lives at age 19, which is typically the age of most high track students while in their last year of high school. For the most recent openings, we are unable to observe a sufficiently large number of university graduates so that I use a combined measure of university enrollment and university completion as the dependent variable instead. I measure university enrollment or completion at age 22 of the individual. Apart from these adjustments, the same econometric approach is used as described above.

Results are shown in Table 3.6. Although not always statistically significant, point estimates suggest that a university opening has a positive effect on enrollment. In our DiD specification (Column 5) this effect is 0.078. One has to bear in mind, however, that not all students graduate from university so that the effect on university completion rates will be somewhat lower. On the other hand, we do not focus on less mobile individuals

[8]I focus on university openings in West Germany, due to the fact that information of the university foundation date of universities in East Germany are not reliable in the data.

as in the regressions above. It might very well be the case that the effect for less mobile individuals is higher.

Table 3.6: Effect of university opening for openings from 1990 - 2009

Dep. Var.: Enrolled in university or already completed

	(1)	(2)	(3)	(4)	(5)
University at age 22	0.046	0.051	0.142**	0.073	0.078
	(0.048)	(0.040)	(0.059)	(0.064)	(0.062)
Female		-0.002	0.002	0.001	0.001
		(0.015)	(0.016)	(0.016)	(0.016)
High parental educ.		0.371***	0.343***	0.329***	0.326***
		(0.027)	(0.029)	(0.029)	(0.030)
Immigrant		-0.058***	-0.069***	-0.054**	-0.053**
		(0.022)	(0.024)	(0.025)	(0.025)
Year of birth				0.005***	
				(0.001)	
Constant	0.186***	0.129***	0.199	-10.499***	0.191
	(0.010)	(0.012)	(0.191)	(2.623)	(0.181)
County FE	No	No	Yes	Yes	Yes
Cohort FE	No	No	No	No	Yes
Observations	2275	2275	2275	2275	2275
Cluster	827	827	827	827	827
R^2	0.001	0.145	0.273	0.280	0.296

Notes: Based on data from multiple SOEP waves (1988-2011). The sample includes individuals from West German counties which acquired a first university between 1990 and 2009 or which did not have a university in 2009 or before. Dependant variable is an indicator for university enrollment or university completion at age 22. Robust standard errors in parentheses clustered at the (county × cohort) level. Specifications (2) - (5) include dummy variables indicating missing observations in parental education and immigrant status, respectively. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Considering the other explanatory variables, parental education background has roughly the same magnitude and explanatory power as in the regressions above using the 1960s and 70s educational expansion. However, males and females are now equally likely to enroll in university. This finding reflects the strong increase in tertiary education participation by females within the last decades.

3.6.3 Bounding Analysis

As stated before, the data used to calculate the main results of this study does not provide information on the place of childhood residence for all individuals. In this section, I first discuss under which circumstances this lack of information might lead to biased estimates. I then carry out a bounding exercise by including individuals in the regressions who relocated since their childhood (henceforth called "mover") and for whom we only know their present place of residence.

Under the assumption that an individual's mobility is random with respect to treatment (i.e. the opening of a new university) our approach yields an unbiased effect for the target population. However, biases arise when an individual's mobility is correlated with a university opening. Consider the following example: County A is a treatment county, county B a control county. Before the university opening in county A, individuals of both counties have the same probability to move away to enroll in university studies and not return to their original county. Due to the university opening in county A, however, some individuals of that county may decide to attend the local university instead of moving to a faraway university. These individuals show up as additional university graduates in our treatment group. The estimated effect would overstate the true effect from a university opening. On the other hand, if individuals become more mobile after their tertiary education attainment this would lead to a greater outflow of university graduates from county A given that the local university has a positive effect on local tertiary education attainment. The estimated effect would, therefore, underestimate the true effect.

Malamud and Wozniak (2012) estimate a causal effect of tertiary education attainment on migration using US data and find that higher education indeed increases the likelihood to relocate outside one's birth state. Moreover, they find that later migration does not hinge on the distance to the attended university. Considering these findings, it seems likely that our estimates are not overestimated, but may in fact reflect a lower bound.

Table 3.7 shows descriptive statistics for individuals in our sample who relocated ("mover") and individuals who stayed at their place of childhood residence ("non-mover").[9] It becomes evident that the largest differences are in the educational attainment variables. On average, a mover is 50 percent more likely to have a tertiary degree and to have at least one parent with a tertiary degree compared to a non-mover. The difference with respect to secondary education is also significant: The average mover has a higher likelihood of being a high track leaver (13 percentage points) and a lower likelihood of being a low track leaver (14 percentage points).

To evaluate the possible range of the university opening effect, I run the same set of regressions as above on a sample including both movers and non-movers. Table 3.8 presents the respective results. In our DiD specification (Column 5), the estimated effect from a university opening is 0.045, i.e. 2.7 percentage points lower than in the estimation using

[9]The non-mover group also comprises individuals who relocated and moved back to their place of childhood. These are, however, only a few cases.

Table 3.7: Comparison of mover and non-mover

Mean sample characteristics of individuals

	Mover			Non-mover			
	Mean (SD)	Min Max	Obs	Mean (SD)	Min Max	Obs	Δ Mean [SE]
Tertiary degree	0.275	0	3306	0.143	0	3169	0.132***
	(0.446)	1		(0.350)	1		[0.010]
Year of birth	1951.421	1912	3358	1953.092	1912	3186	-1.670***
	(14.629)	1981		(14.424)	1981		[0.359]
Female	0.545	0	3358	0.513	0	3186	0.032***
	(0.498)	1		(0.50)	1		[0.012]
High parental education background	0.127	0	3358	0.065	0	3186	0.062***
	(0.334)	1		(0.247)	1		[0.007]
Family Status							
Married	0.711	0	3358	0.717	0	3186	-0.007
	(0.454)	1		(0.450)	1		[0.011]
Single	0.082	0	3358	0.094	0	3186	-0.012*
	(0.275)	1		(0.292)	1		[0.007]
Divorced	0.044	0	3358	0.037	0	3186	0.007
	(0.206)	1		(0.189)	1		[0.005]
Widowed	0.089	0	3358	0.094	0	3186	0.005
	(0.285)	1		(0.293)	1		[0.007]
Secondary Education							
Hauptschule (Basic Track)	0.368	0	3358	0.506	0	3186	-0.138***
	(0.482)	1		(0.500)	1		[0.012]
Realschule (Middle Track)	0.264	0	3358	0.264	0	3186	0.001
	(0.441)	1		(0.441)	1		[0.011]
Gymnasium (High Track)	0.328	0	3358	0.197	0	3186	0.131***
	(0.470)	1		(0.398)	1		[0.011]

Notes: Based on data from the SOEP, 2009. Standard deviations in parentheses and standard errors in squared brackets. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

only the non-mover population. Under the assumption, that all movers are observed in their original county of childhood residence, the true effect from a university opening would be 0.045. However, considering the findings of Malamud and Wozniak (2012) this scenario is rather unlikely to hold for university graduates. Therefore, let us assume for now that only individuals without a tertiary degree are still being observed in their original county and that movers with a tertiary degree relocated to a different county. We can now consider an extreme case, which is likely to present an upper bound estimate of our treatment effect.

Consider the case in which individuals who obtained a tertiary degree in a treatment county relocated to a control county. In the analysis using only the non-mover population, the share of university graduates in the treatment group would be too low, because these individuals are missing. In the analysis using both mover and non-mover populations, the share of university graduates in the control counties is confounded due to the movers and is therefore too high. We estimated an effect of 0.072 using only the non-movers and 0.045 using both groups. The difference in these effects is caused by the movers. Reallocating the movers back to the treatment counties and running a regression should therefore yield an estimate of 0.099. Note, that this bounding exercise relies on some strong assumptions and that other movement patterns are possible. It would go beyond the scope of this analysis, however, to consider all possible channels of migration.

Table 3.8: Effect of university opening on tertiary education attainment (including relocated individuals)

Dep. Var.: University degree (1=yes; 0=no)

	(1)	(2)	(3)	(4)	(5)
University at age 22	0.076***	0.061***	0.078***	0.069***	0.045**
	(0.013)	(0.013)	(0.017)	(0.018)	(0.019)
Female		-0.100***	-0.100***	-0.100***	-0.099***
		(0.008)	(0.008)	(0.008)	(0.008)
High parental educ.		0.390***	0.364***	0.363***	0.369***
		(0.017)	(0.017)	(0.017)	(0.017)
Immigrant		-0.040***	-0.044***	-0.045***	-0.046***
		(0.011)	(0.012)	(0.012)	(0.012)
Year of birth				0.000	
				(0.000)	
Constant	0.183***	0.219***	0.148**	-0.513	0.065
	(0.005)	(0.008)	(0.069)	(0.543)	(0.088)
County FE	No	No	Yes	Yes	Yes
Cohort FE	No	No	No	No	Yes
Observations	9627	9627	9627	9627	9627
Cluster	2547	2547	2547	2547	2547
R^2	0.005	0.112	0.162	0.162	0.171

Notes: Based on data from the SOEP, 2009. Robust standard errors in parentheses clustered at the (county × cohort) level. Specifications (2) - (6) include dummy variables indicating missing observations in parental education and immigrant status, respectively. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

3.7 Conclusion

This study estimates the effect of a new university opening on obtaining a tertiary degree in the local population by exploiting the large tertiary education expansion that occurred in Germany during the 1960s and 70s. A substantial number of new universities were built in

regions that did not previously have one. I use this exogenous variation in local university supply in a difference-in-differences regression approach to estimate the treatment effect from a university opening.

Results suggest that a new university opening increased the probability of obtaining a tertiary degree by 7.2 percent in the local population which corresponds to a 1 percentage point increase in tertiary attainment rates. I also find that the effect of a new university opening was more pronounced among females and individuals from low parental education backgrounds. Furthermore, the effect was mainly driven by openings of universities of applied sciences suggesting that this new type of tertiary education institutions offered learning opportunities which were more relevant to our target group of less mobile individuals. Conducting the same analysis using more recent university openings from the 1990s and 2000s, I still find positive effects from university openings on local human capital formation.

The results have important policy implications. Policy-makers may hope to promote investment in human capital at the tertiary level by opening new universities in regions without prior local university supply. Theoretically, this strategy may work as it reduces the average cost of human capital investment at the tertiary level in the presence of mobility costs. The findings of this study provide empirical support that opening a new university does indeed increase local human capital formation.

Future research should try to analyze the heterogeneous impact of different types of tertiary education institutions in more detail. This could help to understand which education opportunities are best suited for certain target groups. Also, a desirable advancement would be to redo the analysis with a dataset that contains full information on the place of childhood residence for all individuals.

Last but not least, one would like to investigate the labor market returns to tertiary education. The DiD approach of this study could serve as a compelling first stage in an instrumental variables framework. However, one has to bear in mind that statistical power has to be sufficient in order to avoid biases from weak instruments. In the analysis presented here, statistical power (as measured by the first-stage F-statistic) was not enough to indicate a strong instrument. A possible explanation is the relatively low sample size.

Appendix

Table A3.1: Effect of university opening on tertiary education attainment for different age cutoffs

Dep. Var.: University degree (1=yes; 0=no)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Female	-0.082***	-0.082***	-0.082***	-0.082***	-0.082***	-0.082***	-0.082***	-0.082***	-0.082***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
High parental educ.	0.395***	0.395***	0.395***	0.394***	0.395***	0.395***	0.395***	0.395***	0.395***
	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)
Immigrant	0.011	0.011	0.012	0.012	0.013	0.011	0.011	0.011	0.010
	(0.062)	(0.062)	(0.062)	(0.062)	(0.062)	(0.062)	(0.062)	(0.062)	(0.062)
Constant	0.131	0.123	0.121	0.110	0.104	0.115	0.118	0.122	0.136
	(0.170)	(0.169)	(0.169)	(0.169)	(0.168)	(0.168)	(0.168)	(0.170)	(0.170)
Uni18	0.036								
	(0.029)								
Uni19		0.047							
		(0.029)							
Uni20			0.050*						
			(0.029)						
Uni21				0.064**					
				(0.029)					
Uni22					0.072**				
					(0.029)				
Uni23						0.058*			
						(0.030)			
Uni24							0.054*		
							(0.029)		
Uni25								0.041	
								(0.029)	
Uni26									0.024
									(0.030)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3208	3208	3208	3208	3208	3208	3208	3208	3208
Cluster	1540	1540	1540	1540	1540	1540	1540	1540	1540
R^2	0.218	0.218	0.218	0.219	0.219	0.218	0.218	0.218	0.218

Notes: Based on data from the SOEP, 2009. Robust standard errors in parentheses clustered at the (county × cohort) level. Regressions include dummy variables indicating missing observations in parental education and immigrant status, respectively. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Chapter 4

The Impact of the Bologna Reform on Student Outcomes: Instrumental Variables Evidence from Germany*

4.1 Introduction

Higher education is generally perceived as becoming increasingly relevant in today's knowledge economies (Vandenbussche et al., 2006). In this regard, a country's future competitiveness relates to the productivity of its tertiary education system. The Bologna Reform was aimed at increasing the efficiency and attractiveness of higher education within European countries. In particular, policy-makers wanted to increase the mobility and employability of university students by introducing a homogeneous degree system based on two main cycles, the Bachelor/Master system (European Ministers of Education, 1999). In Germany, this led to the abandoning of the hitherto single degree system. Since the Bachelor degree (the first cycle degree) can be obtained in less time than a traditional degree, the new degree system reduces the costs of earning a first tertiary education degree. This reduction in costs should increase enrollment and reduce dropout rates. Policy-makers also hoped that the harmonization of the degree structure across European countries would increase in particular international student mobility.

*This chapter is based on joint work with Bernhard Enzi, Ifo Institute, Munich.

This chapter investigates to what extend the restructuring of the higher education degree system in Germany had the intended effects on students' mobility and employability. In particular, we analyze the effects of the reform on international and national student mobility as one of the major policy goals. We also analyze the effects on outcomes which are potentially related to employability, such as dropout and internship participation. While dropping out of university may reduce an individuals employment opportunities, participating in internships may increase them. Any direct measures of labor market outcomes are not available. In addition, we investigate whether the reform had a negative impact on the study atmosphere as perceived by students to evaluate the concern of unintended side effects.

Existing research has mainly focused on the impact of the reform on enrollment and dropout rates with different findings across countries (see Section 4.3). Positive enrollment effects have been reported for Italy and Portugal (e.g. Cappellari and Lucifora, 2009; Di Pietro, 2012; Cardoso, 2008), whereas no significant effect was found for Germany (Horstschräer and Sprietsma, 2013). The evidence for dropout appears to be mixed even within a country. We are not aware of any study that evaluates the effect of the Bologna Reform on student mobility, although this was one of the major policy goals. In a related study, Parey and Waldinger (2011) analyze the introduction of the ERASMUS program, which provides financial aid to students when going abroad, and find a significantly positive effect on international student mobility.

We exploit exogenous variation in the local availability of Bachelor programs to estimate causal effects of the reform on the student outcomes in Germany. Due to the decentralized implementation of Bachelor degree programs in Germany, both old and new degree programs coexisted for several years leading to potentially endogenous sorting of students into old and new degree programs. To solve this endogeneity problem, we employ an instrumental variables approach by instrumenting enrollment into a Bachelor's program with the distance differential between an individual's nearest university with a Bachelor's and the nearest university with a traditional degree program.

We use a unique micro-level dataset on German high track leavers who graduated in 2006 and whom we observe in 2009. This dataset contains information on the place of high school of the individual which enables us to link this data to administrative data on university study programs in 2006 to employ our instrumental variables approach. Our estimation results do not provide evidence that the reform had a significant effect on student mobility, dropout, and internship participation on average. However, we find a statistically significant negative

effect on dropout for higher achieving students of about 10 percent and an almost significant negative effect on dropout for females of about 9 percent. Furthermore, we find evidence that the reform had a positive impact on the study atmosphere as perceived by students.

The remainder of the chapter is structured as follows. Section 4.2 describes the Bologna Process and the changes it induced in the German higher education system in more detail. In Section 4.3, we discuss related literature. In Section 4.4, we describe the data and present our estimation strategy for the identification of causal effects. Section 4.5 contains our results. Section 4.6 concludes.

4.2 The Bologna Process

On June 19, 1999 the Ministers of Education of 29 European countries met in the Italian city of Bologna to discuss a common strategy to promote the European higher education area. Set forth in the Bologna Declaration, the main objectives of the so-called Bologna Reform are to improve international competitiveness of the European higher education area, foster (international) mobility of students, teachers and researchers, and to strengthen the employability of the European university graduates. In particular, the latter goal gained much momentum in Germany triggered by a broad discussion about the efficiency of the German higher education system in the late 1990s and early 2000s.[1] Many scientists as well as politicians and employers criticized that the average German university student took too long to finish a degree, dropped out too frequently and was lacking important soft skills.

The universities of each member state were requested to introduce a "system of easily readable and comparable degrees" based upon "two main cycles" (see European Ministers of Education, 1999, p. 3) together with a unitary credit point system. In Germany, this led to the abandoning of the single-tier study programs and the respective degrees (called "Diplom" in some subjects and "Magister" in others) and the introduction of the two-tier Bachelor/Master system. Theoretically, the new two-tier system may offer some important advantages compared to the old single-tier system, but there may also be some disadvantages.

The Bachelor degree was thought of as a first academic degree which qualifies for direct labor market entry whereas the consecutive Master degree should provide a profound academic education for a scientific career. Since the Bachelor degree can be earned in less time

[1] For example, see Kultusministerkonferenz (1997) and Wissenschaftsrat (2000) for suggestions on how to improve the German higher education system.

compared to one of the traditional degrees, this should lower the costs of investing in tertiary education for individuals interested in acquiring basic academic skills and quickly entering the labor market. On the other hand, the Master degree, which requires the successful completion of a Bachelor's degree, offers a more specialized education, but students typically have to commit themselves to an overall longer duration of study than before.[2]

A two-tier system also makes it possible to offer Master programs which do not require a Bachelor's degree in the same subject which increases the options for students within the new system and, therefore, its attractiveness.[3] However, it is not clear to what extend Bachelor and Master degrees qualify for distinct employment positions. In practice, both Bachelor and Master graduates might compete for the same job offer. This may reduce the value of the Bachelor degree, since Bachelor graduates obtained less human capital than Master graduates. In fact, there is evidence that more than 72 percent of the students choose to obtain a Master's degree upon successful completion of the Bachelor's degree (Heine, 2012).

The adoption process varied substantially across European countries: England, for instance, already had a two-tier Bachelor/Master system in place and had to carry out only minor adjustments. In Italy, the new system was introduced simultaneously at all universities in 2001. Portugal opted for a decentralized introduction of the new degrees and required its universities to switch to the new system at some point between 2006 and 2008. In Germany, universities were free to choose any point in time between 2000 and 2010 to introduce the new degree system. It was agreed upon that the introduction process should be completed by 2010. In Germany, this goal was widely achieved, with a few exceptions.[4] In 2003, less than 5 percent of all departments had adopted the new degree whereas by 2008 almost 90 percent had completely switched to offering Bachelor degrees (see Horstschräer and Sprietsma, 2013, p. 1).

The Bologna agreement did not provide any distinct implementation rules with regard to contents of the new degree programs. This led to a fairly heterogeneous adoption. Some departments tried to set up new programs that were specifically tailored to the shorter study period of the Bachelor cycle. Others continued to offer the same program and only replaced

[2]The usual duration of Bachelor programs is three years, that of Master programs two years. Traditional programs took four to five years.

[3]In fact, an explicit goal of the Bologna Reform also was the promotion of interdisciplinary study programs (European Ministers of Education, 2003).

[4]For example, neither of the medicine departments introduced the new degrees. Likewise, law departments were still offering traditional degree programs by 2010.

the old with the new degree which ultimately led to a tighter schedule of teaching (Winter et al., 2010).

4.3 Related Literature

The existing evidence on the effects of the Bologna Reform on student outcomes is rather scarce, although it induced large changes in the tertiary education systems of many European countries. This circumstance is most likely due to a lack of adequate data sources and compelling strategies to identify causal effects. Cappellari and Lucifora (2009), for instance, estimate the effect of the Bachelor introduction in Italy on enrollment and dropout rates using a simple before-after comparison, thereby ignoring any potential biases from time trends as well as confounding factors that may have occurred together with the implementation of the Bologna Reform and that may have had an effect on the enrollment decision. Di Pietro (2012) re-evaluates their analysis by employing a difference-in-differences approach. The author argues that the Bologna Reform was primarily targeted towards individuals from less advantaged social backgrounds, so that this subgroup constitutes the treatment group. The author identifies individuals as belonging to the treatment group when neither of their parents have a university degree. Individuals with at least one parent with a university degree constitute the control group.[5] In order to capture the effect from time trends in enrollment, the author uses four cohorts of high school leavers, two before and two after the Bachelor introduction in Italy in 2001.

While this approach is more refined than a simple before-after comparison, it hinges on the assumption that the Bachelor introduction did not affect individuals from the control group. In fact, it is plausible to assume that the Bologna Reform also influenced individuals from the control group in their decision to enroll in higher education as it introduced a considerable amount of flexibility as described in section 4.2. If this also motivated more individuals from the control group to enroll in higher education, the reform effect is underestimated. Cappellari and Lucifora (2009) conclude that the reform increased enrollment by 15 percent, whereas Di Pietro (2012) estimates a reform effect of 7 percent.

[5] Although not explicitly stated in the paper, the author most likely refers to the fact that in theory the Bachelor introduction reduced the cost of investing in higher education, because it takes less time to earn a first degree so that the investment becomes profitable for individuals at the margin of investing.

Two further studies attempt to gauge the effect on dropout rates in the Italian context based on mainly descriptive evidence: D'Hombres (2007) finds significant lower dropout rates among post-reform cohorts of university students, whereas Boero et al. (2005) find no evidence of reduced dropout. Finally, Bratti et al. (2006) analyze the extent to which the reform had an impact on study programs. They analyze data from a single Italian university department and conclude that it became easier for students to pass first-year courses. Cardoso (2008) and Portela et al. (2009) analyze students' demand for study programs in Portugal. They find that departments which introduced the Bachelor degree were more often chosen by first-year students than those which remained offering a traditional degree program.

In a recent study, Horstschräer and Sprietsma (2013) analyze the effect of the Bologna Reform on enrollment and dropout rates in Germany. They employ a fixed effects panel model to analyze administrative data on the department level from 1998 to 2008. Overall, they do not find any effect of the Bachelor introduction on neither enrollment nor dropout rates. However, results appear to differ by subjects. In English Language, German Language as well as Computer Sciences the Bachelor introduction seems to have had a positive enrollment effect, whereas in Mechanical Engineering and Electrical Engineering the effect is negative. Due to the decentralized introduction of Bachelor programs in Germany, i.e. old and new degree programs coexisted for several years, this result is likely to reflect students' selection into one or the other degree program. For the analysis of dropout rates a similar picture emerges. For Biology, the estimated effect is positive, whereas it is negative for Business Administration, English Language Studies, and German Language Studies. Unfortunately, the authors are not able to distinguish between students who quit studying and those who change subject or university.

Mühlenweg (2010) tries to answer the question whether studying in a Bachelor's program affected students' satisfaction. Controlling for observable student characteristics, she concludes that the satisfaction of students in Bachelor programs is slightly higher compared to their peers in traditional degree programs.

4.4 Empirical Framework

4.4.1 Data and Descriptives

For our analysis, we use a cross-section from a rich panel dataset on German high track leavers who graduated in 2006. We observe the individuals in December of 2009, i.e. three and a half years after graduating from high school.[6] The survey is conducted by the German Centre for Research on Higher Education and Science Studies (Deutsches Zentrum für Hochschul- und Wissenschaftsforschung (DZHW)) and offers some important advantages for analyzing the effects of the Bologna Reform on student outcomes. First, the dataset allows us to analyze several outcome variables related to the policy goals of the Bologna Reform. Second, it contains information on a student's place of high school (zip code) which enables us to merge information on German universities and their degree programs in 2006. This information is needed for our instrumental variable approach which is described in detail in the following section.

The dataset contains information on a student's international and national mobility, i.e. whether he/she went abroad for interim studies and whether he/she changed his/her university. It also contains information on whether a student dropped out or not and whether he/she did an internship while enrolled. The last two variables are likely to play a role for an individual's employability. Dropping out of tertiary education may signal a lower ability so that this outcome should be negatively correlated with labor market success. On the other hand, internship participation may increase an individual's chances on the labor market. Since most students were still enrolled at the time of the interview, we cannot observe any direct labor market outcomes yet.

To relate the student information to the tertiary education supply in 2006, we obtained an administrative dataset containing information on the universe of German higher education institutions and their degree programs in 2006 from the German Rectors' Conference (Hochschulrektorenkonferenz (HRK)).[7] For every institution of higher education, the dataset

[6]The individuals were originally sampled in 2005, when they were still in school. However, all our outcome variables are contained in the 2009 questionnaire.

[7]Some universities (especially universities of applied sciences) have departments that are located in different regions/towns, which is not accounted for in the original data. As our identification strategy is based on regional variation in the availability of degree programs, it was important to ensure that the location of the departments was exact. Therefore, in some cases, we had to manually check and add information on the exact location of a department.

includes information on its type and degree programs (including the subject and the degree) offered in the winter term of 2006/07.[8] Based on the awarded degree, we constructed a categorical variable on the university-subject level: 1 if only a Bachelor's program was offered, 2 if only a traditional degree program was offered, 3 if both a Bachelor and a traditional degree program were offered, and 0 if the subject was not offered at all.[9] Since correspondence courses are not bound to a specific location, we did not consider them in our analysis.

Based on the university's address, we geocoded all universities and used QGIS to calculate the air-line distance between an individual's place of high school and the universities. We merged the university data to our student dataset using the zip code of the high school location. This resulted in a student-university-level dataset, where each student was matched with 409 university observations. In addition, we obtained information on the GDP, population, and size of each county in Germany in 2006 from the regional statistics database of the Federal Statistical Office. From this, we calculated the population density as inhabitants per km^2 and the GDP per capita for each county and merged these variables at the high school county level to our individual data. This enables us to control for regional characteristics of a student's origin (place of high school). We consider students enrolled in the 20 most popular subjects as of the winter term 2006/07 (Statistisches Bundesamt, 2007, p. 46) which accounts for 68.3 percent of all students in the original dataset after dropping medical students.[10]

Our final dataset has a sample size of 1626 students, which enrolled in either a Bachelor's or a traditional degree program between the winter term 2006/07 and the winter term 2007/08. We have information on a student's demographic characteristics such as a student's gender, age, nationality, and father's and mother's education. Information is also provided on a student's grade point average in the high school exit exam and the type of the high school

[8]There are three basic types of higher education institutions in Germany. One is rather research oriented, called "university," the other is rather applied, called "university of applied sciences," and the third offers only art subjects, called "art college". The funding of these institutions can either be public, private, or clerical.

[9]The traditional degree category comprises all "old" degree types such as *Diplom*, *Magister*, and *Staatsexamen*. We also included teaching degrees if it was clear to which category (old or new) they belonged.

[10]The information on the degree programs was raw data, meaning that it indicated the specific title of the program. We were very cautious in categorizing them into subjects so as to avoid coding errors. As this was a time-intensive process, we focused our analysis on the 20 most popular subjects which are: business administration, law, German, medicine, mechanical engineering, computer sciences, economics, industrial engineering and management, electrical engineering, mathematics, biology, English, educational science, architecture, psychology, chemistry, physics, construction engineering, business informatics, political science. Since there were no Bachelor programs in medicine, we omitted this subject.

leaving certificate (general or subject specific university entrance diploma). We observe the subject in which a student enrolled and the semester of enrollment. For 1471 students we also observe the first university attended.

Table 4.1 provides summary statistics of our data. 56 percent of the students in our sample are enrolled in a Bachelor's degree program. The other 44 percent are enrolled in a traditional degree program. Students are 23 years old on average and have a high school GPA of 2.9 on a scale from 1 (lowest) to 4 (highest). 35 percent of the students enrolled in a subject within the area of social sciences, 26 percent within natural sciences, 21 percent within engineering, and 18 percent within language and culture studies. The nearest university is on average 23 km away from the high school location of the student. This distance varies considerably within a range from 0 to 115 km.[11] By the time we observe the individuals in 2009, 7.3 percent had gone abroad for interim studies, 2.3 percent had changed their university, 3.1 percent had dropped out, and 20.2 percent had done an internship. On average, a student's assessment of the study atmosphere is 3.9 on a scale from 1 (lowest) to 5 (highest).

For many variables there is a significant difference between students enrolled in a Bachelor's versus a traditional degree program. It is likely that a considerable fraction of these differences is due to student selection into old and new degree programs. The large differences in the fields of study also reflect variation in the timing of the introduction of the new degree system across departments. On average, programs in social sciences were changed earlier to the new degree system compared to programs in language and culture studies.

Most of our outcomes increase in probability with the time since enrollment. For example, students who enrolled earlier than others are more likely to have gone abroad by the time we observe the students in our data. In our sample, 68.5 percent of the students enrolled in the winter term 2006/07, 4.1 percent enrolled in the summer term 2007, and 27.4 percent enrolled in the winter term 2007/08. The later enrollment rates are mainly caused by male students due to the military/civilian service requirement at that time. 46 percent of the male students in our sample began their studies in the fall of 2007. To capture time effects from differential enrollment dates, we control for time of enrollment in all of our regressions.

[11] Due to data protection rules, we had to aggregate our distance measure in intervals of 5 km starting with zero.

Table 4.1: Summary statistics

	Full Sample			Traditional			Bachelor			
	Mean (SD)	Min/Max	Obs	Mean (SD)	Min/Max	Obs	Mean (SD)	Min/Max	Obs	Difference [SE]
Bachelor	0.561	0	1626							
	(0.496)	1								
Student Characteristics										
Female	0.601	0	1626	0.667	0	714	0.549	0	912	0.117***
	(0.490)	1		(0.472)	1		(0.498)	1		[0.024]
Year of birth	1986.274	1968	1624	1986.283	1968	714	1986.267	1969	910	0.016
	(1.305)	1989		(1.311)	1989		(1.302)	1988		[0.065]
German	0.974	0	1622	0.965	0	712	0.981	0	910	-0.016**
	(0.159)	1		(0.184)	1		(0.135)	1		[0.008]
High school GPA	2.917	1	1621	2.934	1	712	2.904	1	909	0.030
	(0.586)	4		(0.590)	4		(0.583)	4		[0.029]
Type of HS degree	0.087	0	1626	0.057	0	714	0.110	0	912	-0.052***
	(0.282)	1		(0.233)	1		(0.313)	1		[0.014]
Father's education	3.592	0	1626	3.634	0	714	3.559	0	912	0.075
	(1.396)	5		(1.360)	5		(1.422)	5		[0.069]
Mother's education	3.490	0	1626	3.517	0	714	3.469	0	912	0.048
	(1.280)	5		(1.259)	5		(1.296)	5		[0.064]
Enrollment WS 2006	0.685	0	1623	0.791	0	714	0.602	0	909	0.190***
	(0.465)	1		(0.407)	1		(0.490)	1		[0.022]
Enrollment SS 2007	0.041	0	1623	0.055	0	714	0.031	0	909	0.024**
	(0.199)	1		(0.227)	1		(0.173)	1		[0.010]
Enrollment WS 2007	0.274	0	1623	0.154	0	714	0.367	0	909	-0.213***
	(0.446)	1		(0.361)	1		(0.482)	1		[0.021]
Distance to next univ. in km	23.444	5	1626	23.018	5	714	23.777	5	912	-0.759
	(17.999)	115		(17.473)	115		(18.404)	105		[0.894]
Regional Characteristics										
GDP/C in Euro	29684.307	13542	1445	30770.451	13956	623	28861.110	13542	822	1909.341***
	(10285.564)	79039		(11534.295)	79039		(9149.057)	79039		[561.586]
Population density	818.197	39	1626	849.775	43	714	793.476	39	912	56.299
	(915.877)	4171		(925.772)	4171		(907.798)	4171		[45.869]

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	Full Sample			Traditional			Bachelor			
	Mean (SD)	Min/Max	Obs	Mean (SD)	Min/Max	Obs	Mean (SD)	Min/Max	Obs	Difference [SE]
Area of Study										
Language and Culture	0.179	0	1626	0.248	0	714	0.125	0	912	0.122***
	(0.383)	1		(0.432)	1		(0.331)	1		[0.0195]
Social Sciences	0.348	0	1626	0.280	0	714	0.401	0	912	-0.121***
	(0.477)	1		(0.449)	1		(0.490)	1		[0.0233]
Natural Sciences	0.261	0	1626	0.286	0	714	0.242	0	912	0.0433**
	(0.440)	1		(0.452)	1		(0.429)	1		[0.0220]
Engineering	0.212	0	1626	0.186	0	714	0.231	0	912	-0.0450**
	(0.409)	1		(0.390)	1		(0.422)	1		[0.0201]
Outcomes										
Going Abroad	0.073	0	1626	0.076	0	714	0.070	0	912	0.005
	(0.260)	1		(0.265)	1		(0.256)	1		[0.013]
Change of university	0.023	0	1626	0.031	0	714	0.018	0	912	0.013*
	(0.151)	1		(0.173)	1		(0.131)	1		[0.008]
Dropout	0.031	0	1536	0.033	0	675	0.030	0	861	0.002
	(0.174)	1		(0.178)	1		(0.171)	1		[0.009]
Internship	0.202	0	1626	0.210	0	714	0.196	0	912	0.014
	(0.402)	1		(0.408)	1		(0.397)	1		[0.020]
Satisfaction	3.881	1	1612	3.769	1	711	3.969	1	901	-0.200***
	(0.899)	5		(0.913)	5		(0.878)	5		[0.045]
Instruments										
IV1	-1.236	-240	1626	-14.545	-240	714	9.183	-170	912	-23.728***
	(44.703)	220		(42.389)	155		(43.713)	220		[2.147]
IV2	-7.301	-335	1471	-29.688	-335	705	13.303	-185	766	-42.991***
	(64.595)	220		(65.581)	155		(56.328)	220		[3.200]

Notes: The left part shows descriptive statistics on the whole student sample, the middle part on students enrolled in a traditional degree and the right part on students enrolled in a Bachelor's degree program. The last column shows the difference in means between both programs with standard errors [SE] in brackets. Standard deviations (SD) are shown in parentheses. To the right of each mean are the minima (Min) and maxima (Max) of each variable, as well as the number of observations (Obs). High school GPA ranges from 1 (worst) to 4 (best). Type of HS (high school) degree indicates subject specific *Fachabitur* versus general university entrance diploma *Abitur*. Father's and mother's education are categorical variables ranging from 0 (No degree) to 5 (University degree). Distance to next university is measured in kilometers and indicates the distance to the next university within a student's field of study. GDP/C indicates the gross domestic product per capita in Euro within a county. Population density is measured as inhabitants per km^2. All outcome variables are binary, with the exception of satisfaction which ranges from 1 (unhappy) to 5 (happy). Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

4.4.2 Estimation Strategy

To investigate the relationship between studying in a Bachelor's degree program and student outcomes of individual i from federal state m in subject l, we consider a model of the following form:

$$Y_{ilm} = \alpha_1 + \beta_1 Bachelor_i + X_i'\gamma_1 + \delta_l + \mu_m + \epsilon_{ilm} \tag{4.1}$$

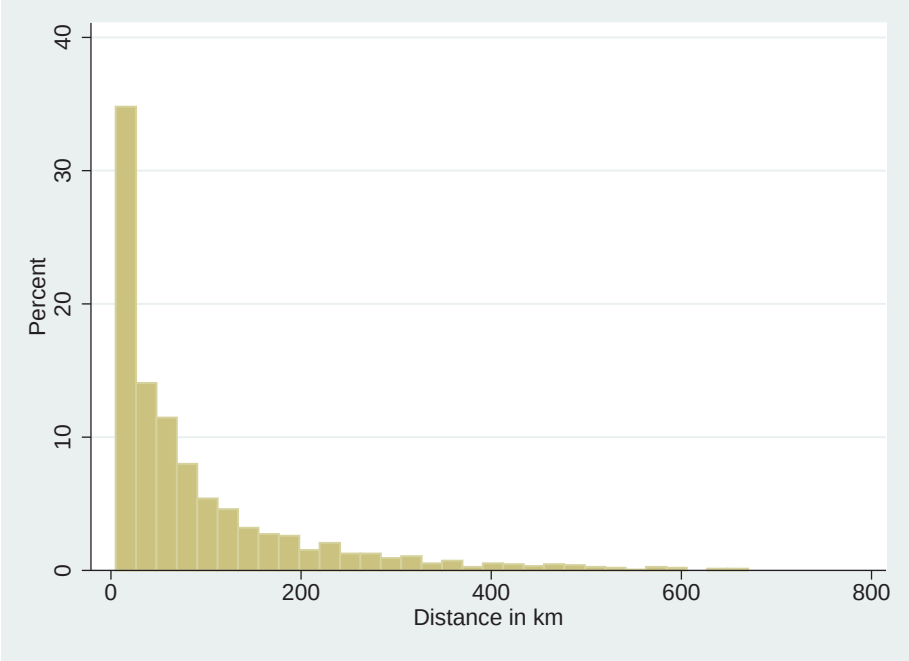
Y denotes our respective outcome of interest: going abroad, change of university, dropout, internship, and students' satisfaction with the study atmosphere. $Changeofuniversity$ includes only changes within a subject and degree program. This means that students who changed universities because they wanted to study a different subject or degree type are excluded. $Bachelor$ indicates studying in a Bachelor's degree program compared to in a traditional degree program and X is a vector of covariates that includes student demographic characteristics, information about parents education, and information about the location of the high school. We include subject dummies (δ) in order to account for unobserved heterogeneities between subjects. We also include state dummies with respect to the high school of an individual (μ). These are necessary because schooling policies, such as high school curricula, are set at the state level and can have a substantial impact on graduates' preparation for tertiary education. To account for interdependence of observations within a university, we cluster the standard errors on the attended university level.

The parameter of interest in the equation above is β_1 which is supposed to capture the effect of studying in a Bachelor's degree program on the respective student outcome. Estimating the equation by OLS, however, may yield biased estimates. Although controlling for potentially confounding influences can reduce the threat of biases, one can easily think of unobserved heterogeneities that can have influenced the selection of students into new or old degree programs. For example, since the new degree programs were intended to facilitate the transferability of course credits, it is possible that students with a higher taste for mobility choose to enroll in Bachelor's programs. In a regression with "going abroad" as our outcome variable, β_1 would be biased upwards, since the unobserved variable 'taste for mobility' is positively correlated with studying in a Bachelor's program.

To solve the problem of omitted variable bias we apply an instrumental variables (IV) approach that exploits regional variation in the supply of Bachelor and traditional degree programs. Due to the decentralized introduction of the Bachelor degree system in Germany under which university departments were free to choose when to implement the Bachelor,

both degree systems coexisted for many years. Our IV approach is based on the idea that most students choose to attend a local university so that it is the local education supply which matters to them. Figure 4.1 shows the distribution of the distance between a student's place of high school and his/her first attended university in our sample. The graph reveals that, indeed, most students decide to enroll at a university close to their place of origin.[12]

Figure 4.1: Distribution of distance to university attended



Notes: The figure shows the distribution of distances between a student's place of high school and the first university attended in our data.

We construct our instrument as the difference in distances between the nearest public university with a Bachelor's program and the nearest public university with a traditional program in a student's subject. We condition our instrument on a student's subject for two reasons: First, in 2006, almost all universities had introduced the Bachelor degree in at least one subject. Constructing the instrument on the university rather than the subject (department) level would result in almost no variation in the instrumental variable which is needed to identify a causal effect. Second, there is evidence that the personal interest in a particular subject is by far the most important determinant of the decision where and what to study (Heine et al., 2005, 2008).[13] We further restrict our university data

[12]This fact is also established in a number of other studies: For example, Spiess and Wrohlich (2010) investigate the relationship between the distance to the nearest university from a student's home and university attendance in Germany and find a negative correlation.

[13]Hachmeister et al. (2007, p. 58) provide suggestive evidence that almost 95 percent of German students choose their subject before their university location.

to public institutions since 95 percent of all students in the winter term 2006/07 enrolled in a public institution (Statistisches Bundesamt, 2007, p. 60). However, we do provide a robustness check using all universities (including private and clerical institutions) in the distance calculation.

Let $MinDist_trad_i$ denote the air-line distance between student i's place of high school and the nearest university with a traditional degree program in student i's subject.[14] Accordingly, let $MinDist_ba_i$ denote the air-line distance between student i's place of high school and the nearest university with a Bachelor's degree program in student i's subject. The difference of these two distance measures yields our instrumental variable:

$$IV \equiv Distance\ differential_i = MinDist_trad_i - MinDist_ba_i. \tag{4.2}$$

The distance differential can be thought of as a measure of the regional supply with a Bachelor's program relative to a traditional program.[15] Thus, our first stage is given by the following equation:

$$Bachelor_{ilm} = \alpha_0 + \beta_0 Distance\ differential_i + X_i'\gamma_0 + \delta_l + \mu_m + \epsilon_{ilm}. \tag{4.3}$$

The intuition is as follows: The nearer the university with a Bachelor's degree program relative to the university with a traditional degree program in student i's subject, the likelier it is that student i enrolled in a Bachelor's degree program.

Our IV approach identifies a local average treatment effect (Angrist and Imbens, 1994), i.e. the effect of the Bachelor introduction for individuals for whom distance matters. These individuals have higher transaction costs of moving to a faraway university than on average and thus prefer to attend an institution which is close to their home. In an attempt to reveal some basic traits of potential compliers in our sample, we divide the students into quartiles according to the distance between the place of high school and the first attended university. As can be observed from Table 4.2, students who stay rather close to their hometown (Column 1) have on average worse high school GPA scores compared to more mobile students and also are from lower educated families.

[14] We use the place of high school to calculate our distance measure, because we do not have exact information on a student's place of residence at the time he/she finishes school. In practice, this should not make a big difference since most students attend a school close to their home.

[15] A relative distance measure is also used in an instrumental variables approach in Oosterbeek et al. (2010) to estimate the effect of entrepreneurship education on entrepreneurship skills and motivation.

Table 4.2: Descriptive statistics by distance to university attended

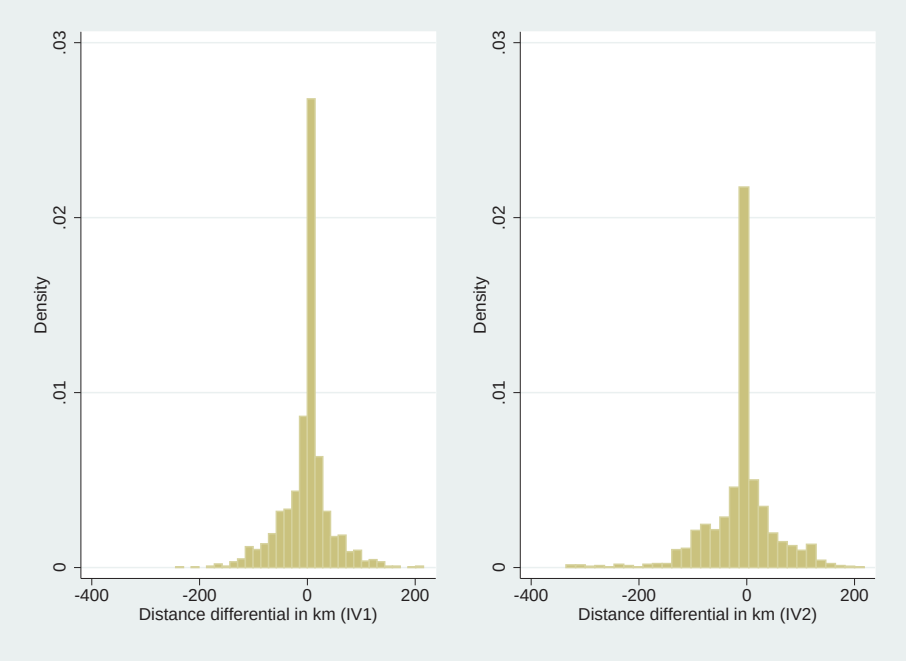
	1st quart. 0 - 20 km	2nd quart. 20 - 50 km	3rd quart. 50 - 120 km	4th quart. 120 - 670 km
	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Female	0.627	0.608	0.637	0.567
	(0.484)	(0.489)	(0.482)	(0.496)
Year of birth	1986.193	1986.222	1986.417	1986.289
	(1.602)	(1.488)	(0.871)	(1.174)
German	0.972	0.963	0.980	0.975
	(0.164)	(0.189)	(0.139)	(0.157)
High school GPA	2.829	2.855	2.901	3.030
	(0.594)	(0.574)	(0.598)	(0.563)
Type of HS degree	0.087	0.105	0.082	0.070
	(0.282)	(0.307)	(0.274)	(0.256)
Father's education	3.471	3.480	3.682	3.857
	(1.437)	(1.442)	(1.350)	(1.304)
Mother's education	3.398	3.392	3.470	3.803
	(1.317)	(1.267)	(1.283)	(1.204)
Enrollment WS 2006	0.725	0.715	0.699	0.612
	(0.447)	(0.452)	(0.460)	(0.488)
Enrollment SS 2007	0.048	0.037	0.042	0.039
	(0.214)	(0.189)	(0.201)	(0.195)
Enrollment WS 2007	0.227	0.248	0.259	0.348
	(0.419)	(0.432)	(0.439)	(0.477)
Distance to next univ. in km	12.437	25.298	31.451	27.444
	(9.812)	(13.014)	(21.311)	(20.971)
Observations	437	352	355	356

Notes: The table contains descriptive statistics by the distance between a student's place of high school and the first university attended. Each column shows means and standard deviations of student characteristics within quartiles of the distance distribution.

We also estimate the effects of the Bologna Reform using a modified version of the instrument described above. Because the German higher education system comprises two main types of higher education institutions (i.e. universities which are rather research oriented and universities which are rather applied), it might be the case that many students only consider studying at one specific type of university. Since our data provides information on a student's first attended university, we are able to calculate the distance differential based on the type of the university attended. Students who only consider studying at one type of university may constitute a different complier group, so that we do not expect the results to remain unchanged. Figure 4.2 shows density plots of our two instruments. There is substantial variation in both instruments, although for most students the nearest universities that offer new and old degree programs in the chosen subject are located rather close to each other. The last two rows in Table 4.1 contain summary statistics of our instruments. The average distance differential for IV1 is -1.24 km, for IV2 -7.3 km. IV2 denotes the instrument in which we account for the type of university attended. Students

who enrolled in a traditional degree program have a negative distance differential on average which means that the nearest Bachelor university is farther away than the nearest university with a traditional degree program. For students who enrolled in a Bachelor's degree program the distance differential is positive on average which means that the Bachelor university is closer.

Figure 4.2: Density distribution of distance differential



Notes: The figure shows the density distributions of our instruments. IV1 represents the distance differential between the nearest public university with a traditional degree program and the nearest public university with a Bachelor's program in a student's subject. IV2 represents the distance differential between the nearest public university with a traditional degree program and the nearest public university with a Bachelor's program in a student's subject while additionally accounting for the type of university a student enrolled at.

The identifying assumption of our estimation strategy is that the distance differential is uncorrelated with any observable or unobservable covariates which are not included in the regression. This requires that the Bachelor introduction was geographically random conditional on covariates included in the regressions. As stated earlier, the introduction of the Bachelor degree system occurred on rather heterogeneous grounds, because there was no common introduction plan. There is evidence that the variation in pace of introduction within a subject area was mainly caused by external, political pressure and not due to university or department specific factors like quality, finance or prestige (Krücken et al., 2005). However, individuals from rural areas are likely to have larger distance differentials than individuals from urban areas due to the lower density of universities in rural areas. To

account for this possibility, we control for regional characteristics of a student's place of high school which we believe to capture potentially spurious correlation between our instrument and geographic differences.

In Table 4.3, we provide suggestive evidence on the exogeneity of our instruments. The table shows results from regressions of the instruments on student characteristics and our regional controls. We do not find significant correlations between a student characteristic and the instruments, except for a weakly significant relationship between IV2 and the gender variable. Most notably, there is no correlation between a student's high school GPA and our instruments. Column 9 contains estimates from a regression of the instruments on all student characteristics. Their joint significance can be rejected as indicated by the p-values.

4.5 Results

Our headline results are presented in Table 4.6. All regressions are based on linear probability models with the exception of the categorical outcome variable 'satisfaction' which ranges from 1 (lowest) to 5 (highest). The standard errors in all estimated models are clustered on the attended university level. The first stage F-statistics in all IV specifications are sufficiently large to reject weak instrument concerns. We further divide the student population into subgroups to investigate heterogeneous effects on different subpopulations. In particular, we analyze heterogeneities by gender and high school GPA. Reduced-form estimation results are contained in Table A4.1.

4.5.1 First Stage Results

Table 4.4 provides first stage regression results for IV1 and IV2. The potentially endogenous variable *Bachelor* is regressed on the instrument and further explanatory variables. Each specification in columns 1 to 6 (IV1) includes additional covariates and fixed effects. Column 6 and 7 report estimates of IV1 and IV2, respectively, in our preferred specification. The F-statistic for IV1 is 18.86 and for IV2 22.42. Throughout all specifications, the estimated effect of the instrument on participating in a Bachelor's degree program is highly significant and fairly robust. The probability increases by 1.3 to 2.9 percent with every 10 km depending on the respective specification. This confirms our hypothesis that the nearer a department with a Bachelor's degree program relative to a department with a traditional degree program

Table 4.3: Correlations between the instruments and observed student characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var.: IV1										
Female	-3.0892									-3.0298
	(2.6251)									(2.8143)
Year of birth		-0.4467								-0.1298
		(0.9100)								(0.9188)
German			6.2182							7.0965
			(5.5634)							(5.5223)
High school GPA				-3.1721						-3.4519
				(2.2791)						(2.2261)
Type of HS degree					4.2643					2.7147
					(5.0773)					(5.2131)
Father's education						-0.4812				-0.9399
						(0.9387)				(0.9758)
Mother's education							0.7401			1.5774
							(1.0029)			(0.9977)
Enrollment SS 2007								-4.9525		-5.3893
								(5.0699)		(5.1991)
Enrollment WS 2007									-0.0001	-1.1676
									(0.0262)	(2.8270)
Regional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1434	1434	1434	1434	1434	1434	1434	1434	1434	1434
p-value	0.2405	0.6240	0.2649	0.1653	0.4019	0.6087	0.4613	0.3297	0.9981	0.3253
Dep. Var.: IV2										
Female	-7.5960*									-5.5751
	(4.2644)									(4.5275)
Year of birth		-0.8896								-0.1935
		(1.3827)								(1.2619)
German			6.0936							6.6868
			(10.0159)							(9.5799)
High school GPA				-2.6498						-2.1950
				(3.4218)						(3.2497)
Type of HS degree					14.8800					12.8274
					(13.6675)					(13.9312)
Father's education						-0.4358				-1.2285
						(1.3750)				(1.4786)
Mother's education							1.2284			2.4206*
							(1.3066)			(1.3332)
Enrollment SS 2007								-5.2798		-4.1309
								(8.2141)		(8.3264)
Enrollment WS 2007									0.0328	1.3906
									(0.0294)	(4.1515)
Regional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1292	1292	1292	1292	1292	1292	1292	1292	1292	1292
p-value	0.0764	0.5207	0.5436	0.4396	0.2776	0.7516	0.3483	0.5211	0.2645	0.5303

Notes: The table contains results from regressions of the instrumental variables (IV1 and IV2) on observed student characteristics. In Columns 1 to 9, the p-value indicates the level of significance of the explanatory variable. In Column 10, the p-value indicates the level of joint significance of all explanatory variables. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

the more likely it is that a student enrolled in a Bachelor's program. We find a highly significant effect of 0.0029 in a univariate regression of the Bachelor indicator variable on IV1 (Column 1). The inclusion of student controls, region controls, and state of high school fixed effects does not change the effect. Only the inclusion of subject fixed effects reduced the estimate to 0.0017 for IV1 and 0.0013 for IV2.

Results also show that the type of high school degree plays a crucial role whether a student enrolled in a Bachelor's or a traditional degree program. Students who obtained a subject specific or vocational university entrance diploma (i.e. study options are either limited to certain subjects or to the type of university) have a higher probability to enroll in a Bachelor's program compared to students with a general university entrance diploma. It may be that these students are attracted to the Bachelor degree due to the shorter duration of study. Results also show that the time of enrollment is a major determinant of enrolling in a Bachelor's degree program. Since the availability of Bachelor's programs increased over time whereas the availability of traditional programs decreased, the probability to enroll in a Bachelor's program increased by 26 to 29 percent for one year later enrollment.

4.5.2 OLS Results

Table 4.5 displays the results of OLS regressions for the respective outcome. Column 1 shows the effect of the Bologna Reform on international student mobility. Participation in a Bachelor's degree program has a small, positive, but insignificant effect of 0.02. Other explanatory variables have the expected signs. For example, better students, as measured by the high school GPA, have a higher probability of going abroad. A higher socio-economic background, as measured by the educational attainment of the parents, also increases the probability of going abroad. Time of enrollment is negatively correlated with going abroad reflecting the time effect of later enrollment.

Results for the effect on national student mobility (change of university) are reported in Column 2. Participation in a Bachelor's degree program has no effect on the probability of changing universities. Germans have a 3 percent higher probability of changing universities compared to immigrants.

OLS estimates further suggest that participating in a Bachelor's degree program has no effect on dropout (Column 3) or internship participation (Column 4). Better students have a significantly lower dropout probability (2.7 percent per 1 point better high school GPA)

Table 4.4: First stage results

Dep. Var.: Bachelor (1=yes; 0=no)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
IV1	0.0029***	0.0029***	0.0029***	0.0029***	0.0028***	0.0017***	
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	
IV2							0.0013***
							(0.0003)
Female		-0.0169	-0.0178	-0.0323	-0.0293	0.0265	0.0373
		(0.0362)	(0.0360)	(0.0364)	(0.0365)	(0.0316)	(0.0340)
Year of birth		0.0060	0.0055	0.0066	0.0105	0.0023	0.0018
		(0.0114)	(0.0114)	(0.0110)	(0.0110)	(0.0099)	(0.0108)
German		0.0909	0.0902	0.0810	0.0607	0.0355	0.0367
		(0.0746)	(0.0747)	(0.0775)	(0.0752)	(0.0632)	(0.0650)
High school GPA		0.0078	0.0071	0.0246	0.0338	0.0412**	0.0276
		(0.0250)	(0.0251)	(0.0238)	(0.0239)	(0.0200)	(0.0201)
Type of HS degree		0.1551***	0.1548***	0.1419***	0.1639***	0.0851	0.1165**
		(0.0461)	(0.0458)	(0.0466)	(0.0475)	(0.0530)	(0.0548)
Father's education		-0.0111	-0.0106	-0.0072	-0.0066	-0.0035	-0.0005
		(0.0088)	(0.0088)	(0.0095)	(0.0094)	(0.0085)	(0.0088)
Mother's education		-0.0014	-0.0009	-0.0104	-0.0152	-0.0068	-0.0060
		(0.0108)	(0.0108)	(0.0113)	(0.0114)	(0.0096)	(0.0104)
Enrollment SS 2007		-0.0547	-0.0532	-0.0127	-0.0137	0.0058	0.0313
		(0.0712)	(0.0709)	(0.0728)	(0.0738)	(0.0552)	(0.0592)
Enrollment WS 2007		0.2535***	0.2527***	0.2699***	0.2660***	0.2578***	0.2869***
		(0.0326)	(0.0325)	(0.0338)	(0.0343)	(0.0324)	(0.0338)
Distance to next university in km			0.0007	-0.0002	-0.0006	0.0019**	0.0016**
			(0.0007)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Region controls	No	No	No	Yes	Yes	Yes	Yes
State of high school FE	No	No	No	No	Yes	Yes	Yes
Subject FE	No	No	No	No	No	Yes	Yes
Observations	1625	1613	1613	1434	1434	1434	1292
Cluster	241	241	241	231	231	231	197
R^2	0.0691	0.1349	0.1355	0.1485	0.1705	0.3243	0.3259
F-Statistic	87.8161	88.1004	89.2101	75.6017	55.6276	18.8608	22.4182

Notes: Dependant variable indicates studying in a Bachelor's degree program (1) versus a traditional degree program (0). All specifications employ a linear probability model (LPM). Standard errors are clustered on the attended university level. All explanatory variables including the indicated regional controls are described in Table 4.1. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 4.5: OLS results for the effect of the Bologna Reform on student outcomes

Dep. Var.:	(1) Going Abroad	(2) Change of University	(3) Dropout	(4) Internship	(5) Satisfaction
Bachelor	0.0213	-0.0008	0.0011	0.0001	0.1050**
	(0.0160)	(0.0095)	(0.0104)	(0.0264)	(0.0491)
Female	0.0007	0.0053	0.0115	0.0383	-0.0985*
	(0.0194)	(0.0114)	(0.0117)	(0.0255)	(0.0557)
Year of birth	0.0062*	0.0013	-0.0116	0.0130	0.0454**
	(0.0033)	(0.0032)	(0.0087)	(0.0082)	(0.0216)
German	0.0251	0.0306***	-0.0179	0.0323	0.1266
	(0.0375)	(0.0079)	(0.0422)	(0.0498)	(0.1681)
High school GPA	0.0407***	-0.0120	-0.0266**	0.0263	0.0319
	(0.0111)	(0.0080)	(0.0126)	(0.0199)	(0.0501)
Type of HS degree	0.0048	0.0045	0.0494	-0.0726	-0.0296
	(0.0200)	(0.0178)	(0.0347)	(0.0445)	(0.0903)
Father's education	0.0062	0.0027	-0.0037	0.0100	0.0009
	(0.0045)	(0.0035)	(0.0042)	(0.0085)	(0.0182)
Mother's education	0.0106*	0.0006	0.0037	-0.0003	0.0213
	(0.0056)	(0.0033)	(0.0042)	(0.0087)	(0.0223)
Enrollment SS 2007	-0.0262	0.0110	-0.0356***	-0.0577	-0.0448
	(0.0320)	(0.0263)	(0.0104)	(0.0490)	(0.1457)
Enrollment WS 2007	-0.0214	0.0012	0.0030	-0.1146***	0.0236
	(0.0157)	(0.0117)	(0.0119)	(0.0232)	(0.0613)
Distance to next university	-0.0004	-0.0001	0.0003	0.0001	0.0003
	(0.0005)	(0.0003)	(0.0004)	(0.0008)	(0.0015)
Region controls	Yes	Yes	Yes	Yes	Yes
State of high school FE	Yes	Yes	Yes	Yes	Yes
Subject FE	Yes	Yes	Yes	Yes	Yes
Observations	1434	1434	1365	1434	1424
Cluster	231	231	228	231	229
R^2	0.0767	0.0278	0.0507	0.0893	0.0733

Notes: Dependent variable as indicated in the first row. 1 to 4 are binary outcomes, 5 is categorical ranging from 1 (lowest) to 5 (highest). Standard errors are clustered on the attended university level. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

and a higher, although insignificant, probability of doing an internship. Later enrollment significantly lowers the probability of having done an internship by the time the students are observed. A one year later enrollment is associated with an 11 percent lower probability of having done an internship.

Column 5 shows the effect of participating in a Bachelor's degree program on a student's satisfaction with the study atmosphere. Results suggest that students in a Bachelor's program are more content than students in a traditional degree program, although the effect is rather small. On a scale from 1 to 5, the effect is 0.11. Female students are on average less content than male students and younger students are on average more content than older students.

4.5.3 IV Results

As discussed above, OLS results are potentially biased by omitted variables. Table 4.6 presents our IV results using IV1 and IV2 in separate regressions for all outcomes. As mentioned above, we do not expect identical results from both IVs due to potentially different complier groups. Using IV1, we estimate a local average treatment effect (LATE) for students for whom the local tertiary education supply matters. Using IV2, we estimates a LATE for students who, in addition, make a more conscious decision about the type of university they want to enroll at. This group of students is likely to be better informed about their expected study conditions compared to the complier group of IV1.

Columns 1 and 2 contain our estimates of the effect of the Bologna Reform on international mobility. Results show no effect when using IV1 as an instrument for enrolling in a Bachelor's degree program. However, using IV2, we find a positive effect of 0.17 which is almost statistically significant at the 10 percent level. Since we most probably estimate different LATEs with IV1 and IV2, it may be the case that students who make a deliberate choice regarding the type of university are also more able to take advantage of the new homogeneous degree system which was intended to facilitated the transfer of course credits between universities. The estimates for high school GPA and parent education background have the expected sign in both IV regressions. A one point better high school GPA leads to a 4 to 5 percent higher probability of going abroad. Better educated parents also increase the probability of going abroad, although the effect is small.

IV point estimates for the impact of the reform on national mobility (change of university) indicate that there may be a small positive effect of roughly 2 percent in both IV specifications (Columns 3 and 4). However, standard errors increased substantially compared to the OLS estimations so that the effect is not statistically significant. Since IV is less efficient than OLS, the increase in the size of the standard errors is a common phenomenon in IV approaches. In addition, it is worth mentioning that our sample size is rather low with less than 1500 observations and about 200 cluster. It may be that the results show the true effect, however, we cannot make a definite statement. Intuitively, it makes sense that the new degree system may have increased the probability of changing universities because of the easier transferability of course credits.

The effect on dropout is shown in Column 5 and 6. Compared to the OLS result which indicates no effect of the reform on dropout, IV results suggest that the dropout probability

Table 4.6: IV results for the effect of the Bologna Reform on student outcomes

Dep. Var.:	(1) Going Abroad		(2) Change of University		(3) Dropout		(4) Internship		(5) Satisfaction	
	IV1	IV2	IV1	IV2	IV1	IV2	IV1	IV2	IV1	IV2
Bachelor	-0.0102	0.1713	0.0266	0.0172	-0.0148	-0.0377	0.0409	0.0755	0.3451	1.2487***
	(0.0914)	(0.1157)	(0.0560)	(0.0644)	(0.0609)	(0.0751)	(0.1592)	(0.1597)	(0.3633)	(0.3541)
Female	0.0014	-0.0016	0.0047	0.0019	0.0120	0.0146	0.0373	0.0354	-0.1035*	-0.1230
	(0.0190)	(0.0222)	(0.0111)	(0.0124)	(0.0118)	(0.0137)	(0.0257)	(0.0268)	(0.0572)	(0.0750)
Year of birth	0.0063*	0.0055	0.0012	0.0015	-0.0117	-0.0117	0.0129	0.0169**	0.0448**	0.0471**
	(0.0033)	(0.0037)	(0.0032)	(0.0035)	(0.0085)	(0.0089)	(0.0080)	(0.0075)	(0.0206)	(0.0219)
German	0.0266	0.0263	0.0293***	0.0316***	-0.0170	-0.0146	0.0304	0.0303	0.1156	0.1026
	(0.0373)	(0.0380)	(0.0082)	(0.0087)	(0.0414)	(0.0431)	(0.0503)	(0.0530)	(0.1658)	(0.1805)
High school GPA	0.0420***	0.0495***	-0.0131	-0.0142*	-0.0259**	-0.0273**	0.0246	0.0424**	0.0217	-0.0004
	(0.0115)	(0.0122)	(0.0084)	(0.0085)	(0.0130)	(0.0135)	(0.0205)	(0.0213)	(0.0492)	(0.0549)
Type of HS degree	0.0078	-0.0160	0.0019	0.0070	0.0509	0.0560	-0.0765*	-0.1033**	-0.0533	-0.1713
	(0.0210)	(0.0264)	(0.0174)	(0.0204)	(0.0335)	(0.0391)	(0.0455)	(0.0517)	(0.1009)	(0.1266)
Father's education	0.0060	0.0051	0.0028	0.0037	-0.0038	-0.0043	0.0102	0.0065	0.0020	0.0072
	(0.0045)	(0.0048)	(0.0034)	(0.0037)	(0.0042)	(0.0045)	(0.0085)	(0.0086)	(0.0184)	(0.0226)
Mother's education	0.0105*	0.0111*	0.0007	0.0017	0.0037	0.0044	-0.0001	-0.0026	0.0224	0.0338
	(0.0057)	(0.0059)	(0.0034)	(0.0034)	(0.0041)	(0.0045)	(0.0086)	(0.0089)	(0.0225)	(0.0265)
Enrollment SS 2007	-0.0261	-0.0361	0.0109	0.0108	-0.0353***	-0.0357***	-0.0579	-0.0776	-0.0484	-0.0772
	(0.0317)	(0.0342)	(0.0256)	(0.0281)	(0.0099)	(0.0113)	(0.0474)	(0.0504)	(0.1436)	(0.1602)
Enrollment WS 2007	-0.0135	-0.0678*	-0.0057	-0.0068	0.0072	0.0153	-0.1249**	-0.1434***	-0.0372	-0.3223**
	(0.0278)	(0.0376)	(0.0191)	(0.0221)	(0.0194)	(0.0248)	(0.0467)	(0.0529)	(0.1049)	(0.1252)
Distance to next university	-0.0003	-0.0006	-0.0002	-0.0003	0.0003	0.0004	0.0000	-0.0001	-0.0002	-0.0021
	(0.0005)	(0.0006)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0009)	(0.0009)	(0.0016)	(0.0019)
Region controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State of high school FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subject FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1434	1292	1434	1292	1365	1227	1434	1292	1424	1282
Cluster	231	197	231	197	228	194	231	197	229	195
1st stage F-Stat	18.8608	22.4182	18.8608	22.4182	16.5960	21.8354	18.8608	22.4182	18.6729	21.6952

Notes: Dependent variable as indicated in the first row. 1 to 4 are binary outcomes, 5 is categorical ranging from 1 (lowest) to 5 (highest). Standard errors are clustered on the attended university level. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

decreased by 1.5 to 3.8 percent. Again, standard errors are large for the reasons discussed above so that the effect is statistically insignificant. High school GPA has a negative impact on dropout which is in line with the common view that better students are more likely to finish their studies.

Columns 7 and 8 show our IV estimates of the effect of the reform on the probability of doing an internship. Whereas the OLS estimate is zero, the IV estimates are 0.04 and 0.07. Both estimates are not statistically significant due to large standard errors. Unfortunately, we do not have enough information to what extend the introduction of the new degree system caused changes in study conditions that might have facilitated doing an internship.

Columns 9 and 10 contain the results for the effect of the reform on students' satisfaction with the study atmosphere. The estimate is 0.35 in the IV1-regression and 1.25 in the IV2-regression. The latter is statistically significant at the 10 percent level. Both estimates are larger than the OLS estimate of 0.11. This suggests that the Bologna Reform had, in fact, a positive impact on the study atmosphere as perceived by students. The larger point estimate in our IV2-regression might again reflect the specific effect for students who deliberately chose one type of university.[16]

As our IV estimates do not provide clear evidence due to a lack of statistical significance, we cannot definitively state that the Bologna Reform had an impact on student mobility, dropout, and internship participation. However, IV point estimates slightly deviate from OLS point estimates. OLS estimates might be biased due to omitted variables, whereas IV estimates are unbiased but imprecisely estimated.

We also estimated the effects of the Bologna Reform on the outcomes using an unconditional distance differential as the instrumental variable. In particular, we included private and clerical institutions in the distance calculations. In comparison, IV1 is calculated using only public universities. Due to the fact that only 5 percent of all students enroll at private and clerical universities, the relevance of the unconditional instrument is lower compared to IV1. The first stage F-statistic is approximately 16 for this instrument, compared to 19 for IV1 and 22 for IV2. Nevertheless, we find very similar results to our IV1 specification.

[16]The differences in the estimates are not due to differences in sample size. Due to missing information in the variable indicating the university attended, IV2-regressions are based on a lower sample size than IV1-regressions. However, restricting the IV1-regressions to the sample used in the IV2-regressions yields almost identical results for IV1.

4.5.4 Heterogeneous Effects

It might be that certain subgroups of our student population were affected differently by the introduction of the new degree system. To explore the impact of the Bologna Reform on student outcomes in more detail, we estimate separate effects by gender and high school GPA. We do not find pronounced effect heterogeneities for our considered outcomes except for dropout (Table 4.7). For female students, we find that the reform reduced the dropout probability by about 9 percent. When IV1 is used as the instrument, the effect is almost statistically significant at the 10 percent level. In comparison to the IV results, OLS yields an estimated effect of zero as in the full sample. The instruments are highly relevant for females with first stage F-statistics of almost 24. For males, the F-statistics are insufficiently large so that we cannot make a statement for this subgroup.

Table 4.7: Heterogeneous effects by gender and high school GPA

Dep. Var.: Dropout

	OLS	IV1	IV2	OLS	IV1	IV2
	Females			Males		
Bachelor	0.0084	-0.0891	-0.0891	-0.0115	0.3473	0.1588
	(0.0123)	(0.0592)	(0.0902)	(0.0167)	(0.3151)	(0.1777)
Observations	818	818	752	547	547	475
F-Statistic		23.8050	23.5201		1.0927	3.5440
	Above median high school GPA			Below median high school GPA		
Bachelor	0.0054	-0.0865	-0.1004*	0.0003	0.1159	0.0154
	(0.0128)	(0.0635)	(0.0598)	(0.0180)	(0.1354)	(0.1002)
Observations	763	763	667	602	602	560
F-Statistic		12.1722	11.4985		7.2376	20.5845
Student controls	Yes	Yes	Yes	Yes	Yes	Yes
Region controls	Yes	Yes	Yes	Yes	Yes	Yes
State of high school FE	Yes	Yes	Yes	Yes	Yes	Yes
Subject FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Binary dependent variable for dropout (1=yes, 0=no). The upper panel shows estimation results of studying in a Bachelor's degree program for females and males, respectively. The lower panel shows estimation results of studying in a Bachelor's degree program for students with a high school GPA above and below the median of 2.9. Standard errors are clustered on the attended university level. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

We also find differential effects for students with a high school GPA above versus below the median of 2.9. For high achievers (GPA $>$ 2.9), we find that the reform significantly (IV2) reduced the dropout probability by 9 to 10 percent. For low achievers (GPA $<$ 2.9), point estimates are positive but not statistically significant.

4.6 Conclusion

This study investigates the impact of the Bologna Reform on student mobility, dropout, internship participation, and a student's satisfaction with the study atmosphere in Germany using survey data from 2009 on German high track leavers who graduated in 2006. To account for the potentially endogenous sorting of individuals into new and old degree programs at the time of enrollment, we use an instrumental variables approach based on the nearest universities that offer a Bachelor's and a traditional degree program in a student's subject. In particular, we use the distance differential between the nearest university with a Bachelor's and the nearest university with a traditional degree program in a student's subject as an instrument for participation in a Bachelor's degree program.

Overall, we do not find a significant effect from studying in a Bachelor's degree program on student mobility, dropout, and internship participation. However, we find a significantly negative effect on dropout for higher achieving students of about 10 percent and an almost significantly negative effect on dropout for females of about 9 percent. Results further indicate that the reform had a positive effect on a student's satisfaction with the study atmosphere.

It is important to emphasize that our results should be interpreted as short-term effects. Since we analyze students that were among the first cohorts to enroll in a Bachelor's program, our estimates are likely to reflect also the circumstances of the introduction of the new degree system. In many cases the new degree structure was applied to existing programs without much adjustments in study content. As the new study programs are gradually being improved and adjusted to the new two-tier degree structure, effects may differ for more recent cohorts. One should also keep in mind, that our IV approach identifies a local average treatment effect for individuals for whom distance matters. This means that the results are not easily transferable to more mobile students.

Future research should explore the mediating channels of the reform in more detail and try to disentangle the effects of the new, homogeneous, two-tier degree structure from effects related to adjustments in study content. To fully assess the implications of the reform, especially in light of further policy advice, it is crucial to also evaluate the reform effects on direct labor market outcomes, such as wages or unemployment probability. Once appropriate

data become available, one could use the IV strategy presented in this chapter to estimate causal effects of the reform on these outcomes.

Appendix

Table A4.1: Reduced forms

Dep. Var.:	(1) Going Abroad		(2) Change of University		(3) Dropout		(4) Internship		(5) Satisfaction	
IV1	-0.0000		0.0000		-0.0000		0.0001		0.0006	
	(0.0002)		(0.0001)		(0.0001)		(0.0003)		(0.0006)	
IV2		0.0002		0.0000		-0.0001		0.0001		0.0017***
		(0.0001)		(0.0001)		(0.0001)		(0.0002)		(0.0004)
Female	0.0011	0.0048	0.0054	0.0025	0.0115	0.0125	0.0384	0.0382	-0.0952*	-0.0801
	(0.0193)	(0.0213)	(0.0114)	(0.0127)	(0.0118)	(0.0132)	(0.0254)	(0.0269)	(0.0556)	(0.0581)
Year of birth	0.0063*	0.0058	0.0013	0.0015	-0.0116	-0.0116	0.0130	0.0170**	0.0456**	0.0494**
	(0.0034)	(0.0036)	(0.0032)	(0.0035)	(0.0086)	(0.0092)	(0.0082)	(0.0077)	(0.0218)	(0.0234)
German	0.0263	0.0326	0.0302***	0.0322***	-0.0177	-0.0162	0.0318	0.0331	0.1274	0.1455
	(0.0377)	(0.0393)	(0.0080)	(0.0083)	(0.0421)	(0.0434)	(0.0500)	(0.0528)	(0.1690)	(0.1723)
High school GPA	0.0416***	0.0542***	-0.0120	-0.0137	-0.0265**	-0.0285**	0.0263	0.0445**	0.0361	0.0338
	(0.0112)	(0.0116)	(0.0079)	(0.0086)	(0.0127)	(0.0136)	(0.0199)	(0.0211)	(0.0498)	(0.0530)
Type of HS degree	0.0069	0.0040	0.0041	0.0090	0.0496	0.0513	-0.0730*	-0.0945*	-0.0226	-0.0198
	(0.0201)	(0.0227)	(0.0173)	(0.0194)	(0.0349)	(0.0388)	(0.0441)	(0.0489)	(0.0910)	(0.0925)
Father's education	0.0061	0.0050	0.0028	0.0037	-0.0037	-0.0042	0.0100	0.0064	0.0007	0.0070
	(0.0045)	(0.0048)	(0.0035)	(0.0038)	(0.0042)	(0.0046)	(0.0085)	(0.0088)	(0.0181)	(0.0193)
Mother's education	0.0106*	0.0101*	0.0006	0.0016	0.0038	0.0046	-0.0004	-0.0030	0.0200	0.0262
	(0.0056)	(0.0060)	(0.0033)	(0.0034)	(0.0042)	(0.0047)	(0.0088)	(0.0091)	(0.0221)	(0.0233)
Enrollment SS 2007	-0.0261	-0.0307	0.0110	0.0113	-0.0356***	-0.0376***	-0.0576	-0.0753	-0.0439	-0.0281
	(0.0322)	(0.0351)	(0.0263)	(0.0288)	(0.0104)	(0.0117)	(0.0489)	(0.0523)	(0.1459)	(0.1473)
Enrollment WS 2007	-0.0161	-0.0187	0.0011	-0.0019	0.0032	0.0039	-0.1144***	-0.1217***	0.0518	0.0366
	(0.0155)	(0.0167)	(0.0109)	(0.0117)	(0.0119)	(0.0133)	(0.0215)	(0.0228)	(0.0583)	(0.0621)
Distance to next univ. in km	-0.0003	-0.0003	-0.0001	-0.0003	0.0003	0.0003	0.0001	-0.0000	0.0005	-0.0001
	(0.0005)	(0.0005)	(0.0003)	(0.0002)	(0.0004)	(0.0004)	(0.0008)	(0.0008)	(0.0015)	(0.0016)
Region controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State of high school FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subject FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1434	1292	1434	1292	1365	1227	1434	1292	1424	1282
Cluster	231	197	231	197	228	194	231	197	229	195
R^2	0.0756	0.0868	0.0279	0.0291	0.0507	0.0528	0.0894	0.1058	0.0716	0.0819

Notes: Dependent variable as indicated in the first row. 1 to 4 are binary outcomes, 5 is categorical ranging from 1 (lowest) to 5 (highest). Standard errors are clustered on the attended university level. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

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