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# Determinants of College Enrollment

## Three Empirical Essays in the Economics of Education

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# Rechtliche Erklärung

## Erklärung gem. §4 Abs. 2 (Promotionsordnung)

Hiermit erkläre ich, dass ich mich noch keinem Promotionsverfahren unterzogen oder um Zulassung zu einem solchen beworben habe, und die Dissertation in der gleichen oder einer anderen Fassung bzw. Überarbeitung einer anderen Fakultät, einem Prüfungsausschuss oder einem Fachvertreter an einer anderen Hochschule nicht bereits zur Überprüfung vorgelegen hat.

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# Abstract

This dissertation focuses on the decision to enroll in college and aims at better understanding the determinants of this choice. It consists of three self-contained research articles, each making an independent contribution to the higher education literature.

**Chapter 2** focuses on the persistent dependence of students' post-secondary educational choices on their socio-economic background. Despite increasing access to university education, students from disadvantaged or non-academic family backgrounds are still underrepresented in universities. In this regard, the economic literature mainly studies the effect of financial constraints on post-secondary educational decisions. However, in Germany where university education is free of charge and the government provides means-tested financial support to finance living expenses, financial constraints are less likely to explain the observed differences in enrollment rates. Another explanation for the differing decisions to enroll in college based on socio-economic background is a potential lack of information. Students from non-academic family backgrounds may be less informed about university education than their peers from academic family backgrounds because they have more difficulties in acquiring this type of information in their environment. Thus, Chapter 2 investigates the causal relationship between information and enrollment intentions based on a randomized field experiment. One year prior to their high school graduation exams, students in randomly selected high schools were provided with information about the benefits and funding possibilities of university education. During this in-class information intervention, labor market benefits of university education were compared to vocational education. Students were surveyed prior the information intervention, two to three months, and one year after the intervention *(Berliner Studienberechtigten Panel)*. Hence, it is possible to investigate short- and medium-term effects of the information intervention. The results of Chapter 2 show that the provision of information increases intended college enrollment for students from a non-academic family background, both two to three months and one year after the intervention. In contrast, it leads students from academic backgrounds to lower their enrollment intentions in the short run. However, this effect does not persist as no statistically significant treatment effect can be detected on their enrollment intentions one year later. The results of this chapter suggest that educational inequality can be reduced by providing students with relevant information.

**Chapter 3** examines the consequences of compressing secondary schooling on students' university enrollment. An education reform in Germany reduced the length of academic high school while simultaneously increasing the instruction hours in the remaining years *(G8 reform)*. Accordingly, students receive the same amount of schooling but over a shorter period of time, constituting an efficiency gain from an individual's perspective. This chapter exploits the differential timing of the reform implementation across states in a difference-in-differences setting. Relying on administrative data on the universe of students in Germany, the results of this analysis show that, due to the G8 reform, the share of students who enroll in university within one year after high school graduation decreases substantially. Further, as a consequence of the reform, students are more likely to delay their enrollment and less likely to make expected progress during their first year at university. The latter is explained by a higher probability to drop out of university and a higher probability to change majors. The main mechanism driving the results is not the age difference of students as the results do not change substantially when the analysis is focused – before and after the reform – on similar-aged graduates; this suggests that the higher workload experienced during high school is more likely to explain the results. Moreover, the negative reform effects seem to be general consequences of the reform as this chapter finds little evidence for effect heterogeneity between states, cohorts, or gender. This chapter includes a comprehensive set of robustness checks and falsification exercises that support the identifying assumption of common trends in the outcome variables in treatment and control states. Overall, the findings in this chapter suggest that due to unintended consequences of the reform, the achievement of the reform's main goal in bringing university graduates earlier to the labor market will not be fully realized.

**Chapter 4** investigates gender differences in earnings expectations. Several studies show that females start with lower earnings expectations than males, even before entering the labor market and that this partly translates into the actual gender wage gap through effects on educational choice and the formation of reservation wages. This chapter examines the gender gap in expected earnings and provides evidence for a novel explanation. Building on the theoretical reasoning of compensating differentials proposing that the labor market compensates higher earnings risk with higher average earnings, this chapter investigates whether the gender gap in expected earnings can be explained by individuals anticipating this form of risk compensation. Earnings risk is measured by a higher dispersion in earnings. Re-

lying on the same data set used in Chapter 2 *(Berliner Studienberechtigten Panel)* in which we elicited information on the entire distribution of expected earnings, Chapter 4 documents that females expect to earn considerably less than their male counterparts. At the same time, females expect lower earnings risk. In a decomposition exercise including a rich set of covariates capturing alternative explanations, this chapter shows that over three-quarters of the gender gap in expected earnings is attributable to differences in expected earnings risk. This suggests that females have lower earnings expectations because they expect to trade off higher earnings for lower earnings risk. The results of this study shed light on why women make different choices regarding education and careers, thereby enhancing our understanding of the observed gender wage gap.

# Zusammenfassung

Die vorliegende Dissertation befasst sich mit den Determinanten der Studienentscheidung und zeigt wie diese Entscheidung durch verschiedene Faktoren beeinflusst wird. Sie besteht aus drei eigenständigen empirischen Forschungsarbeiten, die im Folgenden kurz zusammengefasst werden.

**Kapitel 2** baut auf der Beobachtung auf, dass AbiturientInnen aus nicht-akademischen Elternhäusern an deutschen Hochschulen nach wie vor unterrepräsentiert sind. In der bildungsökonomischen Literatur wird der Unterschied beim Übergang ins Studium primär auf finanzielle Restriktionen zurückgeführt. Allerdings erscheint diese Erklärung in Deutschland nur in begrenzten Maße relevant, da zum einen keine Studiengebühren gezahlt werden müssen und zum anderen finanzielle Engpässe durch das Bundesausbildungsförderungsgesetz (BaföG) abgemildert werden. Neuere Studien nehmen zunehmend andere Erklärungsfaktoren, wie (fehlende) Informationen in den Blick. Informationsdefizite können ein zentraler Erklärungsansatz für die Herkunftsunterschiede beim Übergang in ein Studium sein, da AbiturientInnen aus nicht-akademischen Elternhäusern im Vergleich zu ihren Peers aus akademischen Elternhäusern weniger gut über ein Studium informiert sind. Auf Basis eines randomisierten Feldexperiments untersucht Kapitel 2 daher den kausalen Zusammenhang zwischen Informationen und Studienabsichten. Dabei wurden SchülerInnen ein Jahr vor dem Abitur an zufällig ausgewählten Berliner Schulen über den Nutzen eines Studiums im Vergleich zu einer beruflichen Ausbildung und über die Finanzierung eines Studiums informiert. Die SchülerInnen wurden sowohl vor dem Informationsworkshop als auch zwei bis drei Monate sowie ein Jahr später befragt *(Berliner Studienberechtigten Panel)*. Dies ermöglicht eine Analyse von kurz- und mittelfristigen Effekten der Informationsbereitstellung. Die Ergebnisse dieses Kapitels zeigen, dass die Bereitstellung von Informationen die Studienabsicht von SchülerInnen aus nicht-akademischen Familien erhöht. Bei dieser Gruppe zeigt sich die Erhöhung der Studienabsicht sowohl zwei bis drei Monate nach dem Informationsworkshop als auch ein Jahr später. Im Gegensatz dazu verringert sich die Studienabsicht von Schülerinnen aus akademischen Elternhäusern kurzfristig. Jedoch ist dieser Effekt ein Jahr später nicht mehr identifizierbar. Die Ergebnisse dieses Kapitels zeigen, dass die sozialen Unterschiede beim Übergang in post-sekundäre Bildungswege durch die Bereitstellung von relevanten Informationen verringert werden können.

**Kapitel 3** untersucht die Auswirkungen der Verkürzung der Gymnasialschulzeit um ein Jahr (G8 Reform) auf die Studienentscheidung. Bei dieser Verkürzung wurden die Mindestanforderungen für ein Abitur im Hinblick auf die notwendigen Jahreswochenstunden beibehalten, sodass die notwendigen Unterrichtsstunden auf acht - statt wie bisher auf neun - Schuljahre verteilt wurden. Diese Verdichtung von Unterrichtsstunden stellt aus der Perspektive der SchülerInnen einen Effizienzgewinn dar, da sie den gleichen Lernstoff innerhalb kürzerer Zeit abdecken. Kapitel 3 nutzt die zeitliche und regionale Variation der Reformeinführung über die Bundesländer um kausale Effekte der Reform zu schätzen. Dazu wird ein Differenzen-von-Differenzen Ansatz verwendet. Auf Basis von administrativen Daten *(Studierendenstatistik)*, welche eine Vollerhebung aller Studierenden darstellt, zeigt dieses Kapitel, dass die Verkürzung der Gymnasialschulzeit zu einer geringeren Studienaufnahme führt. Darüber hinaus verzögern AbiturientInnen ihre Einschreibung in Folge der Reform und weisen seltener einen regulären Studienverlauf auf. Der Anstieg in der Wahrscheinlichkeit eines nicht-regulären Studienverlaufs ist dabei auf eine Erhöhung der Studienabbrecherquote sowie einen erhöhten Studienfachwechsel zurückzuführen. Die Analysen hinsichtlich der Wirkungsmechanismen zeigen, dass sich die Ergebnisse nicht durch den Altersunterschied der AbiturientInnen erklären lassen, da die Effekte auch bei Betrachtung von etwa gleichaltrigen AbiturientInnen bestehen bleiben. Daraus lässt sich ableiten, dass die Befunde eher auf die erhöhte Lernintensität und damit die gestiegene Belastung während der Schulzeit zurückzuführen sind. Des Weiteren zeigt dieses Kapitel, dass es hinsichtlich der negativen Effekte keine signifikanten Unterschiede zwischen den Bundesländern oder den Geschlechtern gibt. Zudem nehmen die Effekte auch über die Zeit kaum ab. In diesem Kapitel werden umfangreiche Robustheitsprüfungen und Placebo-Tests durchgeführt, welche die Plausibilität der Identifikationsannahme, dass sich die Untersuchungsgrößen in der Treatment- und der Kontrollgruppe gleich entwickelt hätten, hätte es die Reform nicht gegeben, untermauern. Insgesamt weisen die Ergebnisse dieses Kapitels auf die unbeabsichtigten Folgen der Reform hin. Die Politik verfolgte mit dieser Reform primär das Ziel das Alter von AkademikerInnen beim Arbeitsmarkteintritt zu reduzieren. Jedoch legen die Ergebnisse dieses Kapitels nahe, dass das Potential der Reform in Bezug auf die Senkung des Alters beim Arbeitsmarkteintritt nicht voll ausgeschöpft wird.

**Kapitel 4** befasst sich mit geschlechtsspezifischen Unterschieden in Lohnerwartungen. Mehrere Studien zeigen, dass Frauen bereits vor dem Arbeitsmarkteintritt geringere Lohnerwartungen haben als Männer. Berücksichtigt man die Rolle von Lohnerwartungen im Hinblick auf Bildungsentscheidungen und der Formung des Reservationslohns, so können diese Unterschiede teilweise zur Entstehung der tatsächlichen Lohnlücke zwischen Männern und Frauen beitragen. Dieses Kapitel betrachtet eine neuartige Erklärung für die geschlechtsspezifischen Unterschiede in den Lohnerwartungen. Ausgehend von der Überlegung der kompensierenden Lohndifferentiale, wonach ein höheres Lohnrisiko am Arbeitsmarkt durch höhere durchschnittliche Löhne kompensiert wird, untersucht dieses Kapitel inwiefern sich die geschlechtsspezifischen Unterschiede in den Lohnerwartungen dadurch erklären lassen, dass Individuen diese Form von Risikokompensation antizipieren. Das Lohnrisiko wird dabei über die Messung der erwarteten Lohnschwankung operationalisiert. Für die Analysen wird der gleiche Datensatz wie in Kapitel 2 verwendet *(Berliner Studienberechtigten Panel)*, der Informationen zur gesamten Verteilung der Lohnerwartungen enthält. Die Ergebnisse dieses Kapitels zeigen, dass Frauen deutlich geringere Lohnerwartungen haben als Männer. Gleichzeitig erwarten sie jedoch auch ein geringeres Lohnrisiko. In einer Dekompositionsanalyse, unter Berücksichtigung alternativer Erklärungsansätze, zeigt sich, dass über dreiviertel der geschlechtsspezifischen Unterschiede in den Lohnerwartungen durch Unterschiede im erwarteten Lohnrisiko erklärt werden können. Dieser Befund legt nahe, dass Frauen geringere Lohnerwartungen haben, da sie bereit sind höhere Löhne gegen ein geringeres Lohnrisiko zu tauschen. Insgesamt trägt dieses Kapitel zu einem besseren Verständnis der geschlechtsspezifischen Unterschiede hinsichtlich Bildungs- und Karriereentscheidungen bei und damit auch zu einem besseren Verständnis des Zustandekommens der tatsächlichen Lohnlücke zwischen Männern und Frauen.

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Chapter 1

# Introduction

## 1.1 Motivation

Returns to education are multifaceted. Education not only benefits the individual but also leads to favorable outcomes for the entire society. Traditionally, economists focused on the monetary returns of education at the individual level in the form of higher earnings. It is well-established that higher levels of education result in higher average lifetime earnings (e.g. Peracchi, 2006; Hanushek, Schwerdt, Woessmann, and Zhang, 2016). In Germany, average life time earnings for individuals with a university degree are more than twice as high as for individuals with only a school degree (Schmillen and Stüber, 2014).

While these monetary returns at the individual level remain important, the characterization of returns broadened widely to acknowledge the many non-monetary returns that are associated with higher levels of education. Some of these returns are related to the labor market. Among others, there is plenty of evidence that the risk of unemployment decreases substantially with increasing levels of education (e.g. Hanushek, Schwerdt, Woessmann, and Zhang, 2016). For example, while the unemployment rate for university graduates is around 2.4% in Germany, it is about 5% for individuals with a vocational degree, and 20% for individuals without a vocational degree (Hausner, Söhnlein, Weber, and Weber, 2015). In addition, better educated individuals are more likely to find a job if unemployed, thus reducing the duration of unemployment (e.g. Nickell, 1979; Mincer, 1991). Similarly, it is shown that individuals with more education report higher levels of job satisfaction and more frequently benefit from other job characteristics, like fringe benefits or occupational prestige (e.g. Oreopoulos and Salvanes, 2011).

Apart from the non-monetary returns accruing in the labor market, non-monetary returns also arise outside the labor market affecting a variety of life outcomes. Several studies provide evidence that higher levels of education result in better health (e.g. Grossman and Kaestner, 1997; Lleras-Muney, 2005; Fletcher and Frisvold, 2009; Cutler and Lleras-Muney, 2010), higher civic engagement (e.g. Dee, 2004; Glaeser, Ponzetto, and Shleifer, 2007) and lower criminal activity (e.g. Lochner and Moretti, 2004), among others. Moreover, it is not only the individual himself who benefits from more education. The achieved level of education also has important intergenerational spillover effects by improving children's education and health (e.g. Currie and Moretti, 2003; Black, Devereux, and Salvanes, 2005; Black and Devereux, 2011; Kemptner and Marcus, 2012).

All these monetary and non-monetary returns generate positive externalities that induce additional benefits at the state level. From a fiscal perspective, for example, higher earnings yield higher tax revenues and lower unemployment rates reduce social welfare costs. Similarly, healthier individuals lower public health costs and lower crime rates reduce the costs of the criminal justice system (e.g. McMahon, 2010). In addition to these benefits, a better educated society positively affects the economy by driving economic growth (e.g. Moretti, 2004; Hanushek and Woessmann, 2015). A high level of education in a society is particularly crucial in order to maintain a competitive position in a global economy that rewards knowledge and skills.

It is this comprehensive role of education at both the individual and social levels that spurs the continuous investigation of how individuals make their educational choices. Individuals are confronted with a broad range of different educational choices throughout their life course. Generally, individuals not only have to decide on the level (or the quantity) of education, but also on the quality of education. While this dissertation acknowledges the diversity of educational choices occurring at different points over the life cycle, it focuses on the decision to enroll in college and aims at better understanding the determinants of this choice. Besides the decision whether or not to enroll in college, equally important aspects of this choice relate to questions when to enroll, where to enroll and, in particular, which major to choose. Although all these aspects are important, this dissertation primarily directs its attention toward the decision whether or not to enroll in college and provides empirical evidence on how this choice is affected by institutional regulations set by policy makers. In addition, it presents suitable policy perspectives for future action in order to support individuals in making this choice.

The decision to seek a college education is a complex choice affected by a range of different factors. Based on human capital theory, educational choices are modeled as an investment that yields a return (Mincer, 1958; Schultz, 1961; Becker, 1964). Individuals decide on whether to pursue college education by comparing expected (discounted) lifetime benefits to expected (discounted) costs in an attempt to maximize their lifetime utility.[1] Expected benefits can consist of either monetary returns, like higher earnings, or non-monetary returns, like a lower unemployment risk. Generally, expected benefits can include all returns that an individual associates with a higher level of education. The costs can be equally diverse, comprising direct (e.g. tuition fees, living expenses), indirect (e.g. foregone earnings), and psychic (e.g. study effort) costs. Individuals will only invest in college education if expected benefits are higher than expected costs.

Clearly, at the *individual level*, expected costs and benefits are not the same for every individual. Individuals who aspire to work in the social sector after completing college, for example, will have lower earnings expectations than individuals who intend to work as a doctor.[2] Moreover, labor market returns in terms of earnings may also depend on the performance during college education (Freier, Schumann, and Siedler, 2015) which is difficult to anticipate at the time of the decision making. The cost-benefit consideration will also differ between individuals depending on which aspects enter their utility function, how much individuals appreciate or depreciate these different aspects, and individuals' discount rate which reflects their time preference.

In addition, individuals do not contemplate to enroll in college in isolation. The enrollment choice is embedded in the individual environment and, on a broader level, the institutional framework in which individuals make their enrollment decision. Both the individual environment and the institutional context impact the cost-benefit consideration, thereby affecting the enrollment decision.

The *environmental level* includes, for example, neighborhood and peer effects on the one hand, and family background characteristics on the other hand. With

[1] Another prominent theory introduced by Spence (1973) proposes that individuals invest in education in order to signal their (innate) ability in the labor market. However, this theory is challenged as it implies that individuals do not acquire additional skills through education. Irrespectively, in this theory educational choices are similarly modeled as a cost-benefit consideration.

[2] See Glocker and Storck (2014) or Kugler, Piopiunik, and Wößmann (2017) for German evidence on the varying returns to different college majors. For a more general discussion on heterogeneous returns across individuals, see Carneiro, Heckman, and Vytlacil (2011).

respect to peer effects, for instance, being a high-achiever in a generally low-achieving class, can lead to an underestimation of effort costs needed to succeed in university and, thereby, to an increase in the probability to enroll in college (e.g. Elsner and Isphording, 2017). Similarly, it is shown that having peers who intend to enroll in college increases the likelihood of own college enrollment (e.g. Fletcher, 2015).

In contrast, originating from a low socio-economic background may increase the burden of bearing the costs during college education and consequently deter individuals to enroll. Relatedly, the literature documents that individuals from low socio-economic backgrounds are generally less well informed about the costs, benefits, and funding options of college education, as this type of information is not easily acquired in their environment (e.g. Scott-Clayton, 2012; Bettinger, Long, Oreopoulos, and Sanbonmatsu, 2012; Hoxby and Turner, 2015).[3]

At the *institutional level*, it is suitable to distinguish between the institutional framework that governs the school system, in which individuals are prepared for college education, and the institutional framework in which higher education institutions operate. Examples of institutional aspects at the school level are class size regulations (e.g. Angrist and Lavy, 1999; Chetty, Friedman, Hilger, Saez, Schanzenbach, and Yagan, 2011; Dynarski, Hyman, and Schanzenbach, 2013) or secondary school length and their impact on college enrollment choices (e.g. Morin, 2013; Krashinsky, 2014). At the college level, an extensively studied example of institutional features affecting the decision to enroll in college are student aid schemes that, depending on repayment regulations, considerably reduce the costs of college education (e.g. Dynarski, 2002, 2003). Additional examples include regulations regarding the formal length of a degree program that determines for how many years individuals can expect to reap the monetary returns of their educational investment on the labor market (e.g. Webbink, 2007; Morin, 2013; Krashinsky, 2014; Horstschräer and Sprietsma, 2015).

Differences occurring at these three levels – the individual, the environmental, and the institutional levels – may explain some of the observed heterogeneity in enrollment choices. In Germany, for example, even considering only individuals who earned an university entrance qualification, strong differences in the take up of

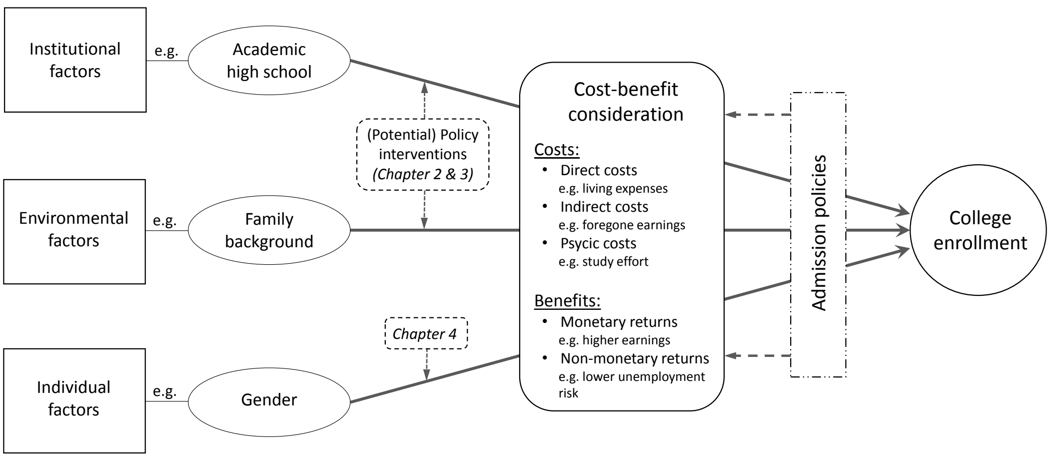
[3]The role of limited information is, however, not restricted to students from low socio-economic backgrounds. For Germany, the study by Saniter and Siedler (2014), for example, shows that visiting a job information center during school increases the probability to attain a higher school leaving as well as a college degree.

college education by gender, socio-economic background and type of entrance qualification are revealed (see Section 1.2). Given the comprehensive benefits of education at the individual and social levels, these systematic differences in the acquisition of education by observed characteristics are of particular interest. From an international perspective, the Organization for Economic Development and Cooperation frequently advises German policy makers to increase the number of college enrollees (OECD, 2016). Hence, in order for policy makers to design targeted and effective interventions, it is necessary to have a thorough understanding of heterogeneities in enrollment choices and a sound knowledge of how the enrollment choice can be supported.

The relevance of this matter not only stems from the perspective of equal opportunities but also from the perspective of an efficient use of human resources. While the persistent dependence of individuals' enrollment choice on family background, for example, is mostly discussed from the angle of inequality of opportunities, the loss of efficiency through an underutilization and miss-allocation of human resources is often neglected. However, the efficient use of these resources is crucial, especially in countries facing a shrinking labor force, like Germany.

This dissertation acknowledges the complexity of the enrollment choice by considering influencing factors at the individual, the environmental, and the institutional levels. Figure 1.1.1 summarizes the college enrollment choice and and depicts the different aspects that are considered in each chapter.

Figure 1.1.1: Enrollment decision and overview of chapters




*Source:* own illustration.

Factors occurring at the institutional, the environmental and the individual level can, as detailed in the previous paragraphs, influence the cost-benefit consideration of individuals, thereby impacting the decision to enroll in college. Among the many different aspects that can be considered within these levels, Figure 1.1.1 further indicates which particular aspects are considered in each chapter of this dissertation. At the individual level, Chapter 4 analyzes gender differences in earnings expectations – a key determinant shaping enrollment choices. At the environmental level, Chapter 2 investigates how the effect of students' family background on enrollment decisions might be reduced. This analysis sheds light on the effectiveness of a potential policy intervention that may help to reduce educational inequality at the transition to college. At the institutional level, Chapter 3 examines how an institutional change in the length of secondary schooling at academic high schools affect students' college enrollment choice, thereby evaluating how a recent policy intervention impact the decision to enroll in college. Although only implicitly addressed in this dissertation, the college enrollment choice is also influenced by the admission policies of higher education institutions, which determine whether individuals are able to attend the particular institution they want to enroll in. The anticipation of entry restrictions may also influence the cost-benefit consideration of individuals; yet, empirical evidence on this relationship is scarce.

## 1.2 College enrollment in Germany

In this section, I provide a brief overview of the institutional background of college enrollment choices in Germany in order to point out differences with respect to other countries. In addition, I provide some descriptive information on the population eligible for college enrollment as well as heterogeneities in enrollment rates.

*Legal framework:* Generally, education policy, including higher education, is the responsibility of individual federal states. Thus, higher education institutions operate under state legislation and receive basic funding from the state level. As a result, higher education systems differ across the 16 federal states. However, within the framework of the Standing Conference of the Ministers of Education and Cultural Affairs *(Kultusministerkonferenz)*, federal states agree on basic principles that federal state laws have to take into account. This ensures similar study conditions and enhances student mobility across federal states. In addition, there are certain aspects of higher education that are legislated at the federal level. These include regulations regarding access to higher education and academic degrees (although these can be adjusted by individual states). One core responsibility at the federal level is the provision of financial student aid according to the Federal Education and Training Assistance Act *(Bundesausbilungsförderungsgesetz, BAföG)*. In addition, based on article 91b(1) of the constitution, the federal government and the federal states are able to share certain responsibilities regarding the higher education system that are of national interest.[4]

*Higher education institutions:* The landscape of higher education institutions in Germany is rather diverse. In 2016, there were 445 higher education institutions in Germany (Destatis, 2016a). Approximately 37% of these higher education institutions are accredited private institutions that, in contrast to public institutions, usually charge tuition fees. However, the share of students enrolled in private institutions is only around 7.5% (Buschle and Haider, 2016) and consequently private institutions play only a minor role in Germany. Higher education institutions can be differentiated into three major types of institutions: (1) universities; (2) universities of applied sciences; and (3) colleges for arts and music, the latter offering study programs for artistic careers in different areas (fine arts, music, theater etc.). While universities typically follow a more theoretical orientation, universities of applied sciences aim to provide application oriented study programs that are often offered in close collaboration with companies. It is worthwhile noting that the formal length of degree programs does usually not differ between universities and universities of applied sciences. In this dissertation, I use the terms "university" and "college" interchangeably thereby referring to all type of higher education institutions.

*Access to higher education:* Access to higher education requires a university entrance qualification. Students typically complete four years of primary school[5] before being assigned to different tracks of secondary schooling based on their performance. Secondary school tracks can be differentiated into upper *(Gymnasium and gymnasiale Oberstufe)* and lower *(Haupt- and Realschule)* secondary school tracks. Only after completing the upper secondary school track students earn a university

[4]In the course of the adjustment of article 91b(1), which became effective in 2015, the possibilities for cooperation between the federal government and the federal states widened, in particular allowing for a stronger federal level engagement in the long-term funding of higher education institutions.

[5]In three federal states (Berlin, Brandenburg, and Mecklenburg-West Pomerania) the transition to secondary schooling occurs after six years.

entrance qualification, which in Germany is called *Abitur* and allows students to immediately start college education following graduation.

Depending on the secondary school attended, different university entrance qualifications are awarded. The general university entrance qualification *(allgemeine Hochschulreife)* entitles students to enroll in any higher education institution. In addition, there are more specialized university entrance qualifications, like the qualification that only allows enrollment in an university of applied sciences *(Fachhochschulreife)* or the qualification that only enables students to enroll in specific majors *(fachgebundende Hochschulreife)*.

The share of individuals eligible for college enrollment, i.e. students holding a university entrance qualification, has increased steadily from 28% of the respective age group in the whole population in 1985 to 37% in 2000, and 53% in 2014. Thus, today more than half of every cohort earns a university entrance qualification, implying that more than 453,000 students faced the decision of whether or not to enroll in college in 2016.

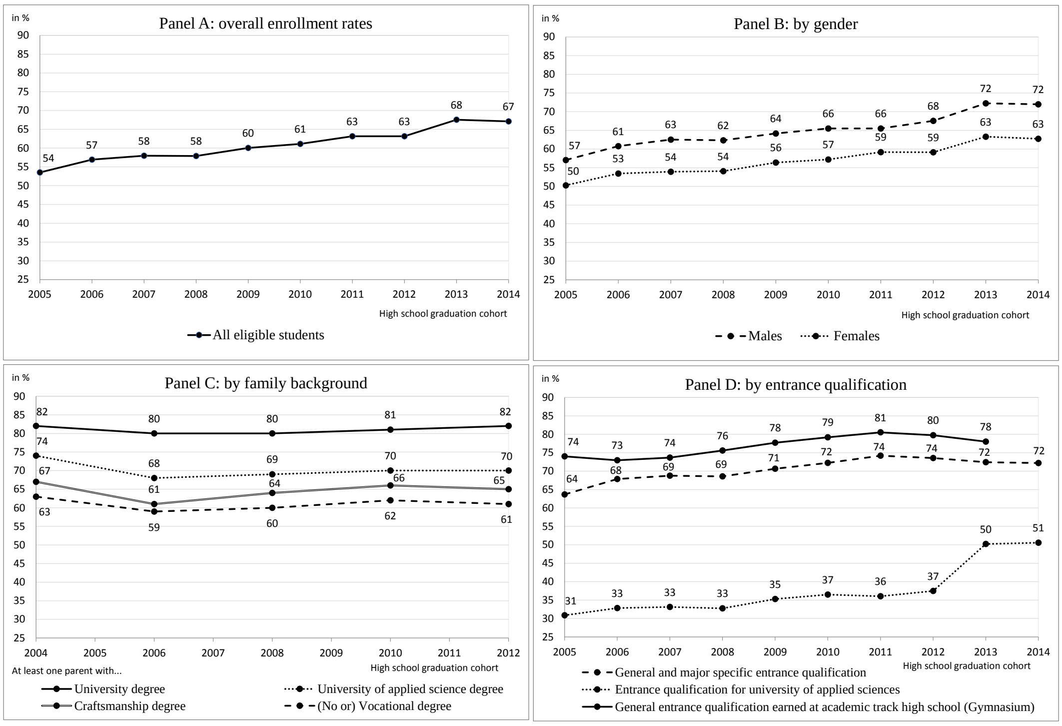
Over the past decade, an increasing number of eligible students have decided to seek college education, as depicted in Panel A of Figure 1.2.2. In 2005, around 54% of students holding a university entrance qualification enrolled in college in the year of high school graduation or the year after. This number increased to 67% in 2014. However, as outlined earlier, enrollment decisions are influenced by a variety of different factors occurring at the individual, the environmental, and the institutional levels. Thus, enrollment rates vary for different group of students.

*Heterogeneity in enrollment rates:* Considering differences in enrollment rates by gender, Panel B of Figure 1.2.2 illustrates that females are less likely to enroll than males. In 2014, for example, 72% of all eligible male students enrolled in college, while only 63% of female students decided in favor of college education.

Looking at heterogeneities with respect to students' family background, the differences in enrollment rates are even larger. Panel C of Figure 1.2.2 shows that 82% of eligible students with at least one parent holding a university degree take up college education. In contrast, this share is around 20 percentage points lower for students whose parents have no or a vocational degree. Moreover, the differences in enrollment rates by parental education did not change substantially over the last decade.

Finally, at the institutional level, Panel D of Figure 1.2.2 depicts heterogeneities of enrollment rates for students with different types of university entrance qualifications. For students earning only the qualification that allows for enrollment in universities of applied sciences, the probability to enroll in college is substantially lower than for students with the general or the major specific entrance qualification. In addition, it is not only the type of entrance qualification earned that affects enrollment rates but also the type of high school attended. As shown, students who graduated with a general entrance qualification from an academic high school *(Gymnasium)*, have the highest probability of enrolling in college among eligible students.

Figure 1.2.2: Heterogeneity in enrollment rates




*Notes:* This figure depicts heterogeneities in enrollment rates. The sharp increase in enrollment rates in 2013 in Panel D is due to a change in the definition regarding the entrance qualification for universities of applied sciences. Since 2013, the statistical office excludes students holding only the academic part of the entrance qualification. Source: Bildungsbericht 2016 (F2.5web) and own calculations based on Destatis (2016b) and Studierendenstatistik (2014).

Overall, Figure 1.2.2 illustrates that differences at the individual, environmental, and institutional levels result in systematic differences in enrollment rates.

## 1.3 Methodological approach

One of the many contributions economists can make in the field of empirical education research relates to their methodological tool set, in particular the identification of causal relationships. This dissertation makes such a contribution. Estimating causal effects is particularly important from a policy perspective. From an *ex ante* perspective, policy makers need to design measures that yield the desired effect. Empirical evidence on causal relationships helps policy makers to identify policy measures that are most likely to be effective. From an *ex post* perspective, it is equally important to analyze whether a specific policy indeed achieved its goal and to reveal potential trade-offs caused by unintended consequences of a reform.

Researchers aiming at estimating causal effects face the challenge of identifying the "missing counterfactual". This term describes the fundamental problem that we cannot observe the *same* individual with and without treatment (Rubin, 1974, 1978). In general, if we are interested in the causal effect of a treatment on a specific outcome, we are confronted with the well-known selection problem. That means, individuals who are more (less) likely to gain from the treatment are usually also more (less) likely to select into the treatment (Roy, 1951; Heckman and Honoré, 1990). Simply comparing outcomes of individuals with and without treatment would consequently lead to a biased estimate of the causal effect. The most reliable method to overcome this challenge is to conduct an experiment where individuals are randomly assigned to a treatment and a control group. Randomization ensures that the comparison between outcomes of the treatment and the control group does not suffer from selection into treatment, i.e. individuals in the treatment and the control group are similar in terms of observed and unobserved characteristics (Fisher, 1925). One example of this approach, typically referred to as the 'gold standard' in the program evaluation literature (e.g. Angrist, 2004), is analyzed in Chapter 2 in which the causal effect of information provision on students' intended college enrollment is investigated.

In many circumstances, however, conducting an experiment is not feasible either for financial, ethical, or practical reasons. Another possibility to deal with the "missing counterfactual" problem are so called natural or quasi-experiments. Under certain identifying assumptions it is possible to exploit the institutional context or a policy change – both outside of individuals' control – in such a way that exogenous variation in the treatment variable is generated; thereby establishing a treatment and a suitable control group (see e.g. Angrist and Pischke, 2009).[6] If the variation is truly exogenous, i.e. independent of individuals' potential outcomes, the allocation into treatment and control group can be regarded as good as random. If, in addition, policy changes are implemented at different points in time across federal states, this further allows to account for general time trends and time-constant differences between states. Focusing on enrollment choices, this is particularly important as, in Germany, we observe a positive time trend in the number of college enrollees and at the same time pronounced differences across federal states. In Chapter 3, the variation in the timing of a reform implementation across federal states is exploited to estimate causal effects of shortening the length of secondary schooling on college enrollment choices in a quasi-experimental setting.

However, not all research questions aim to identify causal effects. In many instances, the research question is not *"Does X cause Y?"* (x-centered) but rather *"What explains differences in Y?"* (y-centered). While the importance of identifying causal effects is undisputed, in some cases the extreme concentration on identification concerns diverts from the question how relevant this relationship is in explaining the variation in an outcome. In order to set different explanations in relation to each other and evaluate their relative importance in explaining observed heterogeneity, other estimation techniques are required (e.g. Fortin, Lemieux, and Firpo, 2011). In that vein, Chapter 4 performs a decomposition analysis and examines the role of a novel explanation for the gender gap in expected earnings while accounting for a large set of alternative explanations. Although this may not necessarily help us to understand the underlying relationship between the outcome and the explanatory factors, it can yield valuable insights by identifying hypotheses that should be investigated in more detail.

## 1.4 Overview and Summary

This dissertation consists of three self-contained research articles, each making an independent contribution to the higher education literature that is described in more detail in each chapter. They are connected through the unifying topic of empirically examining the determinants of the college enrollment choice. The chapters comple-

[6]For a non-technical overview of quasi-experimental approaches, see Schlotter, Schwerdt, and Woessmann (2011).

ment each other by considering heterogeneities at the three levels that influence the decision to enroll in college - the individual, the environmental and the institutional levels. In the following, I briefly summarize each chapter.

***Chapter 2*** focuses on the persistent dependence of students' post-secondary educational choices on their socio-economic background. Despite increasing access to university education, students from disadvantaged or non-academic family backgrounds are still underrepresented in German universities (Middendorff, Apolinarski, Poskowsky, Kandulla, and Netz, 2013). In the economic literature this underrepresentation is mainly studied as the result of financial constraints.[7] Our knowledge of potential effects of other constraints regarding university education is more limited. A relatively new explanation for the differing decisions to enroll in college based on socio-economic background is a potential lack of information (for an overview of the existing evidence, see Peter and Zambre, 2014). Heterogeneous information sets that differ by students' educational backgrounds may explain why students from different educational backgrounds arrive at different educational choices.

Thus, directly providing information may help students make a more informed and background independent decision. Chapter 2 sheds light on whether information deficits prevent students from non-academic family backgrounds to pursue university education by analyzing a randomized field experiment. In this field experiment, students in some randomly selected Berlin high schools were provided with information about the benefits and funding possibilities of university education one year prior to their graduation exams. This field experiment is embedded in a larger project called *Berliner Studienberechtigten Panel* in which students were surveyed prior to the information intervention, 2-3 months as well as one year after the intervention.

The results of this chapter indicate that students process the information provided and adjust their subjective beliefs on benefits of college education accordingly. Students in the treatment group are significantly more likely to expect their unemployment risk to be smaller and their prospects of finding a well-paid job to be higher with a university degree than with a vocational degree. It is further shown that the information workshop increases intended college enrollment for students from non-academic backgrounds, both two to three months and one year after the

[7] In Germany, however, university education is free of charge and the government provides means-tested financial support to finance living expenses. Thus, financial constraints are less likely to explain the observed differences in enrollment rates.

information treatment. For these students, the information treatment prevents a downward adjustment of their enrollment intentions, i.e. students are less likely to be discouraged from pursuing a college degree if peers and parents, based on their own preferences, support a differing educational trajectory. In contrast, the information treatment leads students from academic families to lower their enrollment intentions in the short term. However, this is only a temporary effect and family expectations seem to matter in the medium-run, since no statistically significant treatment effect can be detected on their enrollment intentions one year later. Thus, while the information provision is likely to increase college enrollment rates for students from non-academic family backgrounds, it seems unlikely that enrollment rates for students from academic family backgrounds will be affected. Overall, the findings of this chapter suggest that educational inequality can be reduced by providing students with relevant information.

***Chapter 3*** concentrates on how the institutional context, in particular the length of secondary schooling, affects the college enrollment choice. A policy reform in Germany, the so-called *G8 reform*, aimed at shortening the length of academic high schools without affecting students' human capital. To that end, the one-year reduction in the years of schooling was compensated by a simultaneous increase in instruction hours in the remaining years. Consequently, students received the same amount of schooling but over a shorter period of time. This chapter analyzes the effects of this reform on the decision to enroll in college, the timing of enrollment, and students' study progress during the first year of university studies. In order to identify causal effects of this reform, the differential timing of the reform implementation across states is exploited in a difference-in-differences setting. The analysis relies on administrative data covering the universe of students in Germany *(Studierendenstatistik)*. Our results show that, due to the G8 reform, the share of students who enroll in university within one year after high school graduation decreases by about 6 percentage points (pp), which corresponds to a decrease by about 8 percent. The impact on enrollment rates within two or three years after graduation is of similar magnitude, thus suggesting that enrollment rates do not catch-up. Further, Chapter 3 finds evidence that the achievement of the reform's main goal in bringing university graduates earlier to the labor market is mitigated: As a consequence of the reform, students are 6.8 pp more likely to delay their enrollment and 2.6 pp less likely to make expected progress during their first year at university. The latter is explained by a higher probability to drop out of university and a higher probability to change majors. The main mechanism driving the results is not the age difference of students, as the results do not change substantially when the analysis is focused exclusively on similar-aged graduates; this suggests that the higher workload experienced during high school is more likely to explain the findings. Moreover, the negative reform effects seem to be general consequences of the reform as there is little evidence for effect heterogeneity between states, cohorts, or gender.[8] This chapter includes a comprehensive set of robustness checks and falsification exercises that support the identifying assumption of common trends in the outcome variables in treatment and control states. Overall, this chapter shows that increasing education efficiency by reducing the years of schooling and simultaneously increasing weekly instruction hours sounds like a tempting policy option. However, the results of this chapter show that this policy might not come without unintended consequences regarding students' higher education decisions.

***Chapter 4*** investigates gender differences in earnings expectations. Based on the human capital theory, earnings expectations play a key role in educational choices. Several studies show that females start out with lower earnings expectations even before entering the labor market and that this translates into the actual gender wage gap partly through the effect on educational choices.[9] Considering that in Germany females are, conditional on holding the university entrance qualification, not only less likely to enroll in college but also less likely to choose a high paying college major, the analysis of gender differences in earnings expectations is particularly interesting. Building on the theoretical reasoning of compensating differentials, this chapter examines whether the gender gap in expected earnings can partly be explained by differences in expected earnings risk as measured by the individual-specific dispersion in expected earnings. Thereby the study assesses to what extent educational choices are driven by anticipated compensation for earnings risk.

The analysis draws on the data collected in the context of the randomized field experiment analyzed in Chapter 2 *(Berliner Studienberechtigten Panel)*. This enabled me to include survey questions eliciting information on the entire distribution of students' expected earnings before they actually decide on their future educational path. Using the individual-specific variance in expected earnings as a measure of earnings

[8] The effect on the timing of enrollment, however, decreases over time suggesting that this effect may fade out over a longer time horizon.

[9] This is particularly true for countries like Germany where educational choices largely determine in which industry and occupation individuals will be employed (Dustmann, 2004).

risk, I focus on risk as it is perceived by students at the time of the decision making. I perform an Oaxaca-Blinder decomposition of the gender gap in expected average earnings including – apart from expected earnings risk – a rich set of standard and non-standard individual characteristics that represent different explanations for the observed gender gap. These cover individual background characteristics, measures of academic performance and cognitive skills, intended college major, career motifs, personality traits, preferences, and a measure for self-confidence.

The results of this study show that females expect to earn considerably less than their male counterparts. Differences in earnings expectations can account for around 20 percent of the gender gap in choosing a high-paying college major. At the same time females expect lower earnings risk. In fact, gender differences in expected earnings risk explain about three-quarters of the gender gap in expected earnings. This observation cannot be explained by females being better informed about actual labor market earnings. Given the extensive set of additional covariates included in the analysis, the importance of expected earnings risk in explaining the gender gap is emphasized. Overall, the findings in this chapter shed light on why women may self-select into lower paying occupations and suggest that females may deliberately trade off higher earnings for lower earnings risk.

Finally, **Chapter 5** concludes with a critical discussion of each chapter highlighting the policy implications and pointing toward directions for further research.

Chapter 2

# Intended College Enrollment and Educational Inequality: Do Students Lack Information?*

## 2.1 Introduction

Around the world, post-secondary educational decisions are consistently related to individuals' socio-economic background. In Germany, the odds of starting university education is 37 percent for students from non-academic backgrounds,[1] but the odds are 84 percent for students from academic backgrounds (Middendorff, Apolinarski, Poskowsky, Kandulla, and Netz, 2013). In the economic literature, these observed differences in educational choices are mainly examined as an effect of financial constraints. This focus stems partly from the fact that most studies are based on English-speaking countries where tuition fees present a high financial burden. In countries like Germany, however, university education is free of charge[2] and the government provides means-tested financial support to finance living expenses.

*This chapter is based on joint work with Frauke Peter. A slightly revised version of this chapter has been published as Peter, F. and V. Zambre (2017): "Intended college enrollment and educational inequality: Do students lack information?," *Economics of Education Review*, 60, 2017, 125-141, https://doi.org/10.1016/j.econedurev.2017.08.002. We are grateful to the editor and two anonymous referees for helpful feedback and suggestions to improve the manuscript. We especially thank our colleagues from the Best Up project team at DIW Berlin: C. Katharina Spieß, Johanna Storck, and Mathias Huebener; and at WZB: Heike Solga, Alessandra Rusconi, Claudia Finger, and Martin Ehlert. Moreover, we thank Susan Dynarski, Brian McCall and Astrid Würtz Rasmussen as well as participants of the 6th IWAEE conference, the 4th SOLE/EALE world conference, the 2015 EEA Annual Congress and the 2016 AEA Annual Meeting for valuable comments. We gratefully acknowledge funding from the Einstein Foundation Berlin (A-2010-025 (FU)). The usual disclaimer applies.

[1] Students are considered to come from a non-academic family background if none of their parents holds a university degree.

[2] In 2006, seven out of sixteen states in Germany introduced tuition fees (around EUR 1000 per year), which triggered a lively discussion about fairness in access to university education. However, by 2014 all states had abolished tuition fees.

Thus, financial constraints are less likely to explain the observed differences in enrollment rates. The results of Steiner and Wrohlich (2012) support this argument, as they find only a small elasticity of student aid (BAföG) on participation in tertiary education in Germany.[3]

A relatively understudied explanation for the differing decisions to enroll in college based on socio-economic background is a potential lack of information. Given that educational choices are usually modeled as the result of cost-benefit considerations, it is essential that students know about costs and benefits of university education and how they compare to the alternatives. Since the odds of success and the returns to education are uncertain, students must base their decisions on the expectations they form using the information available to them at the time. These expectations are, in turn, shaped by the socio-economic environment of students (Manski, 1993a,b; Oxoby, 2008; Bifulco, Fletcher, Oh, and Ross, 2014). Consequently, expectations and information sets may differ by students' educational backgrounds. Heterogeneous information sets at the time of the decision making may explain why students from different educational backgrounds arrive at different educational choices. Thus, directly providing information may help students to make a more informed and background independent decision.

This paper investigates how students' intended college enrollment changes as a result of expanding their information set. We use data from a randomized controlled trial in Germany in which high school students were provided with information about the benefits and funding possibilities of university education one year prior to their graduation exams. During this in-class information intervention, labor market benefits of university education were compared to vocational education. The presentation was given using a standardized script in order to ensure that information was consistently presented across the random sample of high schools.

A growing number of studies investigate the relationship between information and educational choices based on field experiments. Some studies provide information about costs and benefits of education (Oreopoulos and Dunn, 2013; McGuigan, McNally, and Wyness, 2016; Kerr, Pekkarinen, Sarvimäki, and Uusitalo, 2015), while other studies focus on specific information, i.e. provide students solely with information on financing possibilities (Booij, Leuven, and Oosterbeek, 2012; Herber,

[3] Even in the English-speaking world the effect of financial aid programs is mixed (for an overview see Dynarski (2002)).

2015) or examine the effect of information on the application process for college and financial aid (Bettinger, Long, Oreopoulos, and Sanbonmatsu, 2012; Hoxby and Turner, 2014) or the admissions process (Castleman, Page, and Schooley, 2014). Furthermore, there are studies exploring the influence of (general) information on educational decision making in developing countries (Nguyen, 2008; Loyalka, Song, Wei, Zhong, and Rozelle, 2013; Jensen, 2010; Dinkelman and Martínez, 2014), where the lack of information may be even more severe as obtaining information is more difficult.

This existing evidence shows that providing information improves students' knowledge. As we would expect, these improvements are larger for students from low socio-economic backgrounds indicating that *ex ante* students might underestimate the returns to post-secondary education or their probabilities of succeeding in higher education. Yet, it is still unclear under which circumstances and in which contexts the provision of information impacts educational choice. The type of information, the mode of presenting information, as well as the duration and the level of interaction varies greatly across studies. Correspondingly, results are mixed, allowing neither the conclusion that information impacts educational choices nor that it does not. Most existing studies, however, find a significant effect on students' knowledge, some find an effect on their educational aspirations, but few studies find an effect on actual behavior. In addition, most evidence refers to countries with comparatively high tuition fees. In these countries the extent to which information can affect educational decisions may be restricted as financial constraints might likely outweigh the lack of information.

Hence, looking at data from a German randomized controlled trial may shed further light on the effectiveness of information provision in a tuition free context. We analyze the differential effects of providing information on intended college enrollment for students' from different educational backgrounds. We estimate the treatment effect on intended college enrollment (1) two to three months after the information provision, i.e. one year prior high school graduation and (2) one year after the intervention, i.e. shortly after students graduated from high school.[4]

[4] Hereafter we refer to students' intended college enrollment one year prior high school graduation as short run since these enrollment intentions are measured shortly after the information provision (two to three months later); similarly, we refer to students' intended college enrollment shortly after high school graduation as enrollment intentions one year later as these are measured one year after the information intervention.

We argue that students' intended college enrollment is a valid indicator for their actual enrollment, especially the closer enrollment intentions are measured to students' actual post-graduation decision. By analyzing intended college enrollment shortly after high school graduation, i.e. closer to the actual decision making, we might get at the potential effect of providing information on actual college enrollment. In support of this argument the empirical correlation between stated enrollment intentions and actual enrollment is very strong. Based on data from a German panel of high school students, 95 percent of students who state an enrollment intention half a year before high school graduation do enroll within three and a half years after graduation (Heine, Quast, and Beuße, 2010; Spangenberg, Beuße, and Heine, 2011).[5]

Additionally examining intended college enrollment one year prior high school graduation, i.e. two to three months after the information intervention, can yield further insights on the effectiveness of providing information as it partly abstracts from supply side restrictions. This is because these enrollment intentions are more likely to reflect students' individual preferences for university education that are less dependent on the number of places available at universities or enrollment restrictions based on grade point averages. Thus, while intended college enrollment measured a year prior high school graduation may already give us an indication about actual choices, enrollment intentions measured shortly after high school graduation, i.e. at the time students make their post-secondary educational choices, are likely to be linked to actual enrollment.

Our results indicate that students process the information provided and adjust their subjective beliefs on benefits of college education accordingly. The information treatment also affects students' intended college enrollment. We show that the information intervention increases intended college enrollment for students from non-academic family backgrounds by 8 percentage points in the short run, i.e. two to three months after the intervention. This effect persists when measuring intended college enrollment one year later, suggesting that the provision of information might also increase their college enrollment. For students from academic family backgrounds, we find a marginally statistically significant decrease in intended college enrollment two to three months after the intervention. However, this negative effect

[5]Although this correlation is not necessarily informative about trajectories for treated students in this paper, it corroborates the predictive power of intentions for actual behavior.

disappears one year later, indicating that information provision is unlikely to play a role for these students' post-secondary educational choices.

Our study relates to the information treatments assessed by Oreopoulos and Dunn (2013); McGuigan, McNally, and Wyness (2016) and Kerr, Pekkarinen, Sarvimäki, and Uusitalo (2015). Yet, to the best of our knowledge, the study by Kerr, Pekkarinen, Sarvimäki, and Uusitalo (2015) is the only other study providing information on the costs and benefits of university education in a tuition free country. Kerr, Pekkarinen, Sarvimäki, and Uusitalo (2015) focus on students' choice of major in Finland and, thus, provide students close to graduation with major-specific information. They find no significant effect on major-specific applications or enrollment rates. The authors conclude that a potential lack of information on labor market success may not be important for educational choices. Complementing their analysis, our study adds to the existing literature by examining the effect of providing information on the decision about the level of education that students pursue after graduating from high school with a specific focus on educational inequality. Furthermore, the way in which the information was presented to students differs between the two studies. While in the study by Kerr, Pekkarinen, Sarvimäki, and Uusitalo (2015) student counselors were provided with information material, our paper looks at the effect of an information workshop that was given by a trained person with a precise script followed by a short summary video at the end. This ensures a consistent provision of information across schools without risking any potential biases that could occur from student counselors or teachers presenting the information material to students.[6]

The remainder of the paper is structured as follows: Section 2.2 describes the institutional context in Germany. Section 2.3 describes the randomized controlled trial, the intervention as well as the data, while Section 2.4 introduces the empirical strategy. In Section 2.5 we report our estimation results and briefly discuss some robustness tests. Section 2.7 concludes.

[6]Teachers and/or student counselors who are provided with information material, may present this material with their own interpretation and/or present only a selection of the material to students.

## 2.2 Institutional context

In Germany, education policy is the responsibility of each individual federal state (*Bundesland*). Thus, education systems differ across the sixteen states. The data used in this paper stem from a randomized controlled trial conducted in the federal state of Berlin, where students complete six years of primary school[7] before being assigned to different tracks of secondary schooling based on their performance. Secondary school tracks can be differentiated into a vocational and a university track.[8] Only at university track schools can students earn the general university entrance diploma, which in Germany is called *Abitur*, that allows students to immediately start university following graduation. This study uses data on students working toward the *Abitur* qualification;[9] excluding those striving for other specialized high school diplomas. In Berlin students can earn their *Abitur* at 137 schools. These 137 schools are divided into three school types: (1) general high schools *(Gymnasium)*; (2) comprehensive high schools *(integrierte Sekundarschule)*; and (3) vocational oriented high schools *(berufliches Gymnasium)*.

Post-secondary educational decisions in Germany differ from other countries. After earning the *Abitur* almost all students stay in post-secondary education, with a very small share deciding not to seek any further education. Given the tracking system, students studying for the *Abitur* are, in general, on track to pursue a subsequent education at university. However, approximately a quarter of students graduating with the *Abitur* choose a vocational education instead (Autorengruppe Bildungsberichterstattung (2016): 127). The German vocational education system constitutes an attractive alternative to university education, as it is a highly recognized dual system that offers good employment prospects. Although primarily designed for students with a lower or middle secondary schooling degree, a range of vocational apprenticeship programs now require the *Abitur*. In addition, the prob-

[7]The transition to secondary schooling after six years occurs in three federal states (Berlin, Brandenburg, and Mecklenburg-West Pomerania); in all other federal states children transit to secondary school following the completion of grade four.

[8]We subsume *Hauptschule* and *Realschule* as vocational track schools and *Gymnasium* and schools with upper secondary level (*gymnasiale Oberstufe*) as university track schools.

[9]Given the early school tracking in Germany after grade four (or six), students attending university track schools represent a selected group, who may already be better informed than students attending other school tracks. Hence, focusing only on university track schools may lead to an underestimation of the potential effect of information provision. Treating students with information on the benefits of university education earlier in the school career could also affect students' high school track choice, as in most federal states teachers' track recommendations are not binding.

ability of admission to white collar vocational programs is very low without the *Abitur*. As the number of students pursuing an apprenticeship after obtaining the *Abitur* has increased over the last years, students who would have left school with a (very) good middle secondary schooling degree might decide to pursue the university entrance qualification only to enter profitable vocational education programs. If policy makers aim to increase enrollment rates at universities, targeting this group may be most effective because these students are already equipped with the necessary academic performance.

Students from low educational backgrounds are more likely to pursue vocational education than peers from an academic family background. Conditional on earning the *Abitur*, the transition probability to university education is between 10 and 20 percentage points lower for students with lower educated parents, i.e. parents without university degree (Autorengruppe Bildungsberichterstattung (2016): 127). Given tracking after primary school and the associated selectivity of students who earn the *Abitur*, the observed differences in post-secondary decisions by students' educational background is an additional source of concern: If the inequalities at earlier stages are taken into account, the odds of starting university education are more than three times as high for students from academic compared to non-academic family backgrounds (77% vs. 23%) (Middendorff, Apolinarski, Poskowsky, Kandulla, and Netz, 2013). One immediate benefit of vocational education in the dual system is its remuneration, which renders students somewhat more independent of other financial sources to cover their living expenses than students attending college. Some authors argue that having a vocational education system that offers students an attractive alternative to university education may partly explain why students from low educational backgrounds are underrepresented at German universities (Becker and Hecken, 2008).

## 2.3 Randomized controlled trial

In this Section the setup of the randomized controlled trial (RCT hereafter) and the data used are described in more detail. The information intervention was conducted as part of a larger project called ***Be**rliner-**Stu**dienberechtigten-**P**anel* (Best Up).[10] In this project, randomly selected high schools in Berlin were treated with an in-

[10] The project is a co-operation between the German Institute for Economic Research (DIW Berlin) and the Berlin Social Science Center (WZB). The Best Up project is

class presentation providing information on benefits of university education as well as on potential financing strategies.

***"Best Up" project setup***. The project aimed to obtain a sample of 27 schools (20% of all upper secondary schools in Berlin) that have a large share of students from non-academic family backgrounds. High schools without intakes in fifth grade[11] were stratified using (1) school type; (2) share of population aged 25 and older with low education (ISCED 0-2) per district; (3) cohort size one year prior the *Abitur* exams; (4) share of students with a migration background; and (5) share of female students as stratifying variables. With the exception of the share of low educated individuals within a district, all variables are measured at the school level. The Best Up project aimed at oversampling students from lower educated backgrounds. Since there is no school-level information available on students' parental educational background, we included district-level information. This allowed us to identify schools in areas with a higher share of low educated individuals and subsequently increased the likelihood of sampling students from non-academic family backgrounds. Stratification was implemented using coarsened exact matching (CEM) as proposed by Iacus, King, and Porro (2009).[12]

Based on the results of the stratification, a set of potential schools – 30 preferred schools and 20 replacement schools – was identified that was similar in terms of the stratifying variables. Schools in the preferred set were subsequently contacted and asked whether they would like to participate in a survey aiming to gain knowledge on how students can be better supported in choosing their post-secondary educational path. During the recruitment process nine out of the 30 preferred schools had to be replaced with schools from the replacement sample.[13] Table A2.1 in the Appendix presents descriptive statistics for the different sets of schools, comparing all Berlin high schools without intakes in grade five to potential schools (showing the set of preferred and replacement schools separately), contacted schools, and schools that participate in the *Best Up* study. This comparison shows that, in line with the aim

funded by the Einstein Foundation Berlin. For further information on the project see: http://www.diw.de/en/diw_01.c.409542.en.

[11]Out of the 137 Berlin high schools, 33 schools which admit high performing students in grade five are excluded from the target population, since students with a non-academic background are likely to be underrepresented in these schools.

[12]Stratification was only used to draw the school sample and played no role in randomization.

[13]Six of the nine schools that had to be replaced in the "preferred set" were general high schools and three were comprehensive high schools.

to oversample students from non-academic families, the set of preferred schools are more frequently located in districts with a higher share of low educated individuals, comprise a larger share of vocational high schools and exhibit a higher share of students with a migration background than the average Berlin high school. Table A2.1 further shows that the set of contacted schools and that of participating schools are similar in terms of the stratifying variables.

After schools had agreed to participate, schools within school types were randomly assigned into treatment and control groups. In the sample, nine schools out of 27 are treatment schools. After allocating schools into treatment and control groups, headmasters were contacted again to schedule a date for the survey. Treatment schools were asked for an additional class session (45 minutes), to accommodate the information workshop. A few weeks before the survey, an invitation to participate in the survey was distributed among all students who were on track to take *Abitur exams* the following year.[14] Among the nine treatment schools, one school did not receive the information workshop due to a miscommunication between the headmaster and its teaching staff. Nevertheless, it was possible to survey some students in this school. We further address the non-compliance of this school in our empirical strategy in Section 2.4.

***Information intervention***. The information workshop was composed of a 20-minute in-class presentation on benefits of post-secondary education as well as on different funding possibilities of university education. The information on labor market returns comprised visualized information on earnings differences, career perspectives, unemployment risk and the gain in lifetime earnings. Students received "tailored information," meaning information relevant for students with *Abitur*. The general numbers available on differences in earnings do not differentiate by highest achieved schooling degree. While *Abitur* is a prerequisite for university enrollment, most vocational degrees can also be obtained with a lower schooling degree. Consequently, the returns to a vocational degree largely depend on the highest achieved

[14]As part of the setup of the randomized controlled trial, power analyses were conducted to judge the feasibility of the intervention. Taking the full cohorts of the 27 schools as our potential sample (2,500 students) and assuming a response rate of at least 60 percent, the minimum detectable treatment effect was equal to 6 percentage points (with $\alpha$ equal to 0.05 and $\beta$ equal to 0.20). Additionally accounting for a panel mortality of 20%, increased the minimum detectable treatment effect size to 7 percentage points in the overall sample.

schooling degree.[15] Thus, during the information workshop, labor market benefits of university education were compared to vocational education conditional on holding the *Abitur*. Through the comparison of labor market benefits between a university and a vocational degree, the information workshop also conveyed information on labor market benefits of vocational education. The presentation also pointed toward gender differences in earnings and differences across fields of study.

With respect to the possibilities to finance university studies, the main sources of funding in Germany – BAföG (student aid), scholarships and students jobs – were introduced. The information on student aid also included basic information about repayment conditions, stressing that only half of the amount received as student aid must be repaid and repayment obligations only start once earnings exceed a certain threshold. The information on direct costs of university education emphasized that no tuition fees need to be paid (anymore) and, consequently, monthly average costs equal living expenses, which have to be financed irrespective of the educational path taken. Hence, the costs of university education boil down to the opportunity costs, which correspond to the remuneration of vocational trainees. Most of the information was visualized in order to make the information more accessible to students. Figure A.1 in the Appendix shows some example slides of the material presented in the information intervention.

The information workshop was not designed to advertise university education but rather to provide students with information relevant to making a more informed decision. In addition, the presentation was given by a trained person with a precise script from the RCT team. This type of treatment ensures a more consistent provision of information compared to other studies that give out information materials to schools or student counselors (see for example the studies by McGuigan, McNally, and Wyness, 2016; Kerr, Pekkarinen, Sarvimäki, and Uusitalo, 2015, for this type of treatment), who might present this material with their own interpretation and/or present only a selection of the provided information material. Another component of the information treatment was a 3-minute video at the end of the intervention summarizing the provided information and thereby further guaranteeing standardization of treatment.

[15]Students holding a lower secondary schooling degree do not qualify for all vocational education programs.

***Data***. We use data from the *Berliner-Studienberechtigten-Panel* (Best Up) with pre- and post-treatment surveys. The pre-treatment survey was administered in schools one year prior to the *Abitur* exams using a paper-based questionnaire. It was executed in schools under exam conditions. Teachers were only present to provide their obligatory supervision. In treated schools, the survey directly preceded the information workshop. A total of 1,578 students participated in the first survey.[16] Approximately two to three months and one year later follow-up online surveys were carried out. The response rates for the post-treatment surveys, each compared to the baseline number of students (1578), were 70% and 67%, respectively, which is higher than in comparable studies (see e.g. Booij, Leuven, and Oosterbeek, 2012; Oreopoulos and Dunn, 2013). More importantly, the response rate is virtually identical between treatment (69.69%) and control (70.71%) groups.[17] Yet, to obtain an unbiased estimator of the treatment effect it is important that intended college enrollment and background characteristics do not influence drop out differently by treatment status. Based on a Chow-test, we do not find any evidence for differential attrition.[18]

***Analyzed sample***. We restrict our analysis to students participating in both pre- and post-treatment surveys. Further, we keep only students with information on pre- and post-treatment enrollment intention as well as information on parental educational background. Intended college enrollment one year prior high school graduation is measured by asking students what education they plan to pursue after earning their *Abitur*.[19] Students can choose between university education (at different types of universities[20]), vocational education, or no education. We define intended college enrollment as a binary variable, such that it equals one if the student

[16] Taking the full cohort of each school as a reference, this corresponds to an overall response rate of 60%.

[17] These numbers refer to the first post-treatment survey, i.e. students who participated in the survey two to three months after the information workshop. Due to the change in survey mode, attrition is highest between the pre-treatment and the first post-treatment survey. 96% of students, who participated in the first post-treatment survey also responded to the second post-treatment survey one year later.

[18] Tested covariates comprise age, gender, migration background, non-academic family background, school types, enrollment intention, math and German grades as well as two measures of cognitive skills and again refer to participation in the first post-treatment survey; $F_{(12,1545)} = 0.68, p-value = 0.7725$..

[19] The translated survey question reads: Think of everything you know today: Which type of education will you most likely pursue after graduating from high school?

[20] The institutions comprise universities, universities of applied sciences, field specific universities, and vocational oriented universities.

intends to go to college and zero otherwise. The vast majority of students who do not intend to enroll, plan to pursue a vocational education.[21] The final sample for the analysis focusing on short-run treatment effects comprises 988 observations. Out of these students, valid information on intended college enrollment shortly after high school graduation is available for 842 students.

Given the variety of post-secondary educational paths, intended college enrollment measured shortly after high school graduation is determined as follows: For students, who already applied to study programs at the time of the survey, i.e. directly in the summer after high school graduation, this enrollment intention reflects their applications.[22] For other students, it reflects either their plan to apply/enroll in the same year or their enrollment intention after taking a gap year in order to, for example, travel, do an internship or voluntary work. Further, we define parental educational background to be either academic or non-academic. Students are from a non-academic family background if no parent (genetic or social) holds a university degree, or from an academic family background if at least one parent holds a university degree. For students who did not answer the question addressing education of both parents, we made the following assumption to determine their educational background: Students either stating that they do not know their mother or father or students with missing information on the level of education of one parent were classified according to the valid information on the one (the other) parent.[23] In specifications where we control for additional covariates, we deal with missing information by setting these variables to a constant value and including a dichotomous variable indicating missing covariates.[24] Missing information on the key variables does not differ significantly between treatment and control groups.

***Covariate balance***. We test whether randomization was successful by comparing the balance of covariates between treatment and control groups. As is common in RCTs in the field of education, schools instead of individuals were randomized to best mimic a potential policy measure and avoid spillover effects within schools.

[21]Only around two percent of the students who participated in the pre-treatment survey plan to obtain no further education.
[22]In Germany, college applications are only required for some study programs.
[23]If information on parental education is completely missing, we use the education of older siblings (if available) to proxy educational background; otherwise we dropped the observation from the sample.
[24]Estimating the treatment effect using only students with non-missing information on all covariates, does not change our conclusions.

Table 2.3.1: Covariate balance by treatment status

| | All | | Non-academic background | | Academic background | |
|---|---|---|---|---|---|---|
| | Control Group Mean | Treatment Group Difference | Control Group Mean | Treatment Group Difference | Control Group Mean | Treatment Group Difference |
| **Intended college enrollment** | 0.792 | -0.028 | 0.749 | -0.050 | 0.865 | -0.002 |
| | (0.028) | (0.047) | (0.032) | (0.065) | (0.027) | (0.040) |
| **Individual characteristics** | | | | | | |
| Age | 18.591 | -0.128 | 18.739 | -0.237 | 18.340 | 0.064 |
| | (0.155) | (0.231) | (0.138) | (0.252) | (0.189) | (0.242) |
| Female | 0.588 | 0.012 | 0.599 | 0.054 | 0.570 | -0.051 |
| | (0.034) | (0.060) | (0.037) | (0.048) | (0.040) | (0.091) |
| Migration background | 0.465 | 0.055 | 0.501 | 0.065 | 0.402 | 0.047 |
| | (0.058) | (0.121) | (0.071) | (0.138) | (0.043) | (0.115) |
| Non-academic background | 0.629 | -0.026 | | | | |
| | (0.030) | (0.051) | | | | |
| **Performance and skills** | | | | | | |
| German Grade | 8.775 | -0.154 | 8.467 | 0.000 | 9.296 | -0.436 |
| | (0.211) | (0.348) | (0.217) | (0.379) | (0.251) | (0.376) |
| Math Grade | 8.034 | 0.344 | 7.845 | 0.368 | 8.353 | 0.280 |
| | (0.190) | (0.324) | (0.178) | (0.283) | (0.301) | (0.486) |
| Cognition test (verbal) | 9.796 | 0.295 | 9.413 | 0.145 | 10.447 | 0.454 |
| | (0.251) | (0.495) | (0.276) | (0.459) | (0.241) | (0.533) |
| Cognition test (figural) | 11.014 | 0.159 | 10.749 | 0.518 | 11.463 | -0.433 |
| | (0.186) | (0.301) | (0.213) | (0.407) | (0.172) | (0.314) |
| **School type** | | | | | | |
| School type I | 0.307 | -0.001 | 0.278 | 0.054 | 0.357 | -0.089 |
| *(Gymnasium)* | (0.126) | (0.204) | (0.116) | (0.211) | (0.148) | (0.214) |
| School type II | 0.368 | 0.008 | 0.377 | -0.010 | 0.352 | 0.037 |
| *(Integrierte Sekundarschule)* | (0.133) | (0.220) | (0.131) | (0.218) | (0.143) | (0.233) |
| School type III | 0.325 | -0.007 | 0.345 | -0.044 | 0.291 | 0.053 |
| *(berufliches Gymnasium)* | (0.127) | (0.214) | (0.127) | (0.209) | (0.134) | (0.231) |
| **Perceived returns** | | | | | | |
| Unemployment risk smaller | 0.402 | -0.004 | 0.382 | 0.010 | 0.438 | -0.028 |
| | (0.026) | (0.042) | (0.033) | (0.055) | (0.038) | (0.049) |
| Prospects for well paid job higher | 0.712 | 0.004 | 0.712 | 0.016 | 0.711 | -0.014 |
| | (0.020) | (0.026) | (0.026) | (0.035) | (0.034) | (0.049) |
| Relative income premium | 1.525 | -0.021 | 1.546 | -0.043 | 1.487 | 0.018 |
| (bachelor's/vocational degree) | (0.035) | (0.051) | (0.039) | (0.053) | (0.042) | (0.078) |
| Life time income higher | 0.644 | 0.008 | 0.66 | -0.048 | 0.607 | 0.096* |
| | (0.017) | (0.035) | (0.021) | (0.044) | (0.025) | (0.052) |
| N | 658 | 330 | 414 | 199 | 244 | 131 |
| N (total) | 988 | | 613 | | 375 | |

*Notes:* This table presents control group means and treatment-control differences for the analyzed samples used to investigate short-run treatment effects measured 2-3 months after the information treatment. Means and mean differences are derived by separately regressing each variable on the treatment group indicator, i.e. $X_i = \alpha + \beta Z_s + \varepsilon_i$, where $X_i$ represents the variable in the left most column and $Z_s$ is an indicator variable for treatment status as obtained from randomization. Standard errors are clustered at the school level and shown in parentheses. Source: Best Up, wave 1. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

As we are interested in the differential effect by parental educational background, we examine treatment effects at the individual level. However, the composition of students within schools is usually non-random, such that the probability of balancing covariates at the individual level is lower if entire schools instead of individuals are randomized. We assess randomization in the combined sample as well as for the subgroups by parental educational background. Table 2.3.1 displays the covariate balance by treatment status and indicates that randomization successfully balanced most of the covariates.[25] The only exception is detected in the subsample of students from academic backgrounds, where students in the treatment group are more likely to state that lifetime income is higher with a university degree than with a vocational degree. Conducting F-tests in a regression of individual characteristics and measures of performance and skills (as listed in Table 2.3.1) on treatment status, we cannot reject the null hypothesis that these variables are jointly equal to zero in all three samples.[26]

## 2.4 Empirical framework

When analyzing data from a RCT it is generally sufficient to compare the average post-treatment outcomes by treatment status in order to identify the causal effect of the treatment. Randomization ensures that the estimates do not suffer from selection into treatment. However, based on the information of the pre-treatment survey, we see that (conditional on the sample used for the analysis) pre-treatment intentions to enroll in college are almost three percentage points lower in the treatment group than in the control group. If we look at the subsample of students with a non-academic background this difference is even larger and amounts to five percentage points (see Table 2.3.1).

Although these differences are statistically insignificant, the size of the difference cannot be ignored. If, for example, the true effect of the information intervention for students from non-academic families is less than five percentage points, by only comparing post-treatment outcomes we were to conclude that the information inter-

[25]This also applies to the covariate balance in the baseline sample as well as for the sample used to analyze the treatment effects one year after the information intervention. See Table A2.2 and A2.3 in the Appendix.

[26]Corresponding p-values of the F-tests are: in the combined sample 0.5229; in the sample of non-academics 0.2968; and in the sample of academics 0.7488. F-test are based on regressions with standard errors clustered at the school level.

vention had no effect on intended college enrollment. Further, even if the true effect is larger than five percentage points, we would still underestimate the treatment effect for students from a non-academic family background – the group of major interest in our study.

In addition to the differences in pre-treatment enrollment intentions, one school that was randomly assigned to the treatment group did not receive the information workshop (see Section 2.3). It was, however, possible to survey at least some of the students in this school. Thus, to obtain a causal effect of providing information, we compare post-treatment intended college enrollment by treatment status controlling for students' pre-treatment intention combined with a two stage least squares approach. In the first stage, we use the original classification of schools into treatment and control groups (based on randomization) as an instrument to predict actual treatment status, which is whether a school actually received the information workshop. The first stage is given by:

$$T_{is} = \mu + \eta Z_s + \gamma y_i^{(t_0)} + \delta X_i + \tau_{is} \tag{2.1}$$

where $T_{is}$ indicates actual treatment status and $Z_s$ indicates the treatment status obtained from randomization prior field start. We account for differences in students' pre-treatment enrollment intentions by including $y_i^{(t_0)}$, a binary variable indicating student $i's$ pre-treatment intended college enrollment. In order to increase the precision of our estimates in the second stage, we further include a vector of additional pre-treatment individual level controls $X_i$. $X_i$ includes age, gender, migration background, school type, (standardized) pre-treatment math and German grades as well as cognitive skills measured by a verbal and a figural test.

After obtaining the predicted treatment status $\hat{T_{is}}$, we estimate Equation 2.2 for the whole sample as well as separately for students from a non-academic and academic family background:

$$y_{is}^{(t_\omega)} = \beta_0 + \beta_1 \hat{T_{is}} + \beta_2 y_i^{(t_0)} + X_i' \beta_3 + \epsilon_{is} \tag{2.2}$$

where $y_{is}^{(t_\omega)}$ equals 1 if student $i$ in school $s$ intends to enroll in college at time $t_\omega$ ($\omega = 1, 2$), and 0 otherwise. $\omega = 1$ indicates the first post-treatment survey, i.e. two to three months after the information provision, and $\omega = 2$ indicates the second post-treatment survey, i.e. one year after the treatment. $\hat{T_{is}}$ is the predicted treatment group indicator as estimated from Equation 2.1. $y_i^{(t_0)}$ and $X_i$ are defined as before in Equation 2.1. The error term $\epsilon_{is}$ captures the remaining variation. To account for potential dependence of observations within schools we cluster standard errors at the school level. For the mean comparison of post-treatment intentions, $\beta_1$ is the coefficient of interest and identifies the effect of the information treatment.

However, controlling for students' pre-treatment intended college enrollment (see Equation 2.2) cannot completely resolve the pre-treatment difference, as it only adjusts the estimates for a fraction of these differences (Allison, 1990). Therefore, in our main specification we compare the *change* in students' intended college enrollment by treatment status and examine the difference between pre- and post-treatment enrollment intentions. Our outcome variable is given by $\Delta y_{is}^{(t_\omega)} = y_{is}^{(t_\omega)} - y_{is}^{(t_0)}$, where again $\omega = 1$ represents post-treatment intended college enrollment two to three months after the intervention and $\omega = 2$ indicates enrollment intention one year later. We estimate the following Equation and use the predicted treatment status $\hat{T_{is}}$ from Equation 2.1 as our treatment indicator. Our preferred specification is given by:[27]

$$\Delta y_{is}^{(t_\omega)} = y_{is}^{(t_\omega)} - y_{is}^{(t_0)} = \gamma_0 + \gamma_1 \hat{T_{is}} + X_i'\gamma_2 + \upsilon_{is} \tag{2.3}$$

where $\Delta y_{is}^{(t_\omega)}$ depicts the change in intended college enrollment of student $i$ in school $s$ between time $t_\omega$ and $t_0$. We also add a vector of additional covariates, $X_i$ (defined as before), to this specification in order to account for the possibility that some students may be more or less likely to change their enrollment intentions.[28] The error term $\upsilon_{is}$ is clustered at the school level.[29] The effect of the information treatment is given by $\gamma_1$.

By using the change between pre- and post-treatment intended college enrollment in our main specification, we not only fully account for the pre-treatment imbalance

[27]Results based on estimating Equation 2.2 are reported in Table A2.5 in the Appendix. Note that Equation 2.3 is a version of Equation 2.2 where we restrict $\beta_2$ to be equal to one.

[28]We argue that male/female students, students attending different school types, or students located in different parts of the skill distribution might differ regarding a change in their intended college enrollment depending on what other information they acquire or experiences they gain in the meantime. For example, traditional academic track high school (Gymnasium) may be more likely to provide information about university education, while vocational oriented university track high schools may be more likely to inform students about traineeships in companies.

[29]Accounting for the small number of clusters does not change our conclusions (see Table 2.6.6 in Section 2.6).

in enrollment intentions but also for any time invariant observables and unobservables that might influence intended college enrollment and differ by treatment status.

## 2.5 Results

Before we present the effect of the information workshop on students' intended college enrollment, we first provide some descriptive evidence on the lack and relevance of information using pre-treatment data. We then show that the information workshop successfully conveyed information to students and subsequently turn to our main results.

### 2.5.1 Pre-treatment survey evidence

Intended college enrollment in our sample is (pre-treatment) around 13 percentage points lower for students from non-academic compared to students from academic backgrounds. In Table 2.5.2 we differentiate between students from different educational backgrounds with and without intentions to enroll and investigate whether information sets are related to their intended college enrollment.[30] Table 2.5.2 shows that students from an academic background who intend to enroll in college are five percentage points more likely to rely on their parents and perceive this information source as more helpful than students having no enrollment intentions. In contrast, this does not apply to students from non-academic backgrounds.

Comparing the information set by intended college enrollment for students from a non-academic family background (columns 1 and 2 of Table 2.5.2) reveals that students with an intention to enroll feel significantly better informed about university education than their peers without an enrollment intention.[31] These students are also more likely to have investigated the possibilities of financing university attendance and perceive the cost burden of university education as lower.[32] Note that for students from a non-academic family background the subjective income pre-

[30]Table A2.4 in the Appendix further shows that students' information sets differ by parental educational background.

[31]This is based on the question of whether students feel well-informed about the general rules and possibilities of university.

[32]The perception of "how difficult financing university education" is also varies significantly by parental educational background. Almost half of the students from a non-academic family background state that bearing the costs during university education is very difficult or mostly difficult (see Table A2.4 in the Appendix).

mium associated with a higher degree is not correlated with educational aspirations. However, perceiving the unemployment risk to be lower, the prospects of finding a well-paid job and lifetime earnings to be higher with a university degree compared to a vocational degree is highly correlated with students' intended college enrollment. Thus, a lack of information on returns to tertiary education could potentially affect college enrollment.

Table 2.5.2: Relevance of information by educational background

| | Non-academic background | | Academic background | |
|---|---|---|---|---|
| | No Intention | Intention to enroll (Difference) | No Intention | Intention to enroll (Difference) |
| **Information source** | | | | |
| Information source: Parents/Family | 0.870 | 0.003 | 0.902 | 0.054* |
| | (0.024) | (0.027) | (0.032) | (0.031) |
| Parents/Family helpful as | 3.674 | -0.175 | 3.609 | 0.375* |
| information source (1-5) | (0.124) | (0.128) | (0.182) | (0.201) |
| **Costs** | | | | |
| Feeling well informed about | 0.236 | 0.129*** | 0.200 | 0.204** |
| university education | (0.040) | (0.043) | (0.082) | (0.075) |
| Problem: obtaining info | 0.264 | 0.025 | 0.294 | -0.053 |
| | (0.045) | (0.052) | (0.054) | (0.052) |
| Not/hardly dealt with financing | 0.608 | -0.165*** | 0.636 | -0.203** |
| possibilities | (0.051) | (0.049) | (0.065) | (0.078) |
| Perceived cost burden high | 0.593 | -0.148*** | 0.314 | -0.040 |
| | (0.041) | (0.052) | (0.054) | (0.055) |
| **Perceived returns** | | | | |
| Unemployment risk smaller | 0.274 | 0.152*** | 0.353 | 0.087 |
| | (0.047) | (0.045) | (0.050) | (0.051) |
| Prospects for well paid job higher | 0.571 | 0.201*** | 0.580 | 0.147* |
| | (0.028) | (0.034) | (0.075) | (0.072) |
| Relative income premium | 1.531 | 0.002 | 1.392 | 0.117 |
| (bachelor's/vocational degree) | (0.036) | (0.044) | (0.067) | (0.092) |
| Life time income higher | 0.580 | 0.095** | 0.588 | 0.061 |
| | (0.033) | (0.038) | (0.084) | (0.085) |
| N | 613 | | 375 | |

*Notes:* This table depicts the relevance of information separately for students from different educational backgrounds. It presents mean and mean differences based on regressing each variable on an indicator variable for intended college enrollment, i.e. $X_i = \alpha + \beta y_i^{(t_0)} + \varepsilon_i$, where $X_i$ represents the variable in the left most column and $y_i^{(t_0)}$ is an indicator variable for pre-treatment intended college enrollment. Standard errors are clustered at the school level and shown in parentheses. The numbers reflect the share of students whose answers are in accordance with the statements listed in the left column. Source: Best Up, wave 1. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Students from academic backgrounds, who intend to enroll in college, likewise perceive the returns to university education as higher. However, only the prospect of finding a well-paid job is (marginally) statistically different between students with and without an enrollment intention in this subgroup. Looking at the "costs" of university education shows that feeling well informed about university education and having dealt with financing possibilities is also positively correlated with enrollment intentions for students from academic family backgrounds.

Overall, Table 2.5.2 suggests that information is relevant in forming an enrollment intention for students from both, non-academic and academic, family backgrounds. However, it seems even more important for students from a non-academic family background.

### 2.5.2 The effect of information provision on intended college enrollment

Before we turn to the treatment effects on intended college enrollment, we briefly discuss whether the information workshop successfully conveyed information that was adequately processed by students. We compare students' perceived labor market benefits of university education pre- and post-treatment. We consider the subjective unemployment risk, the subjective prospects of finding a well-paid job, and the subjective income premium of university education. We are only able to assess a small subset of subjective beliefs of labor market returns. The information treatment, however, consisted of a bundle of information on post-secondary education among which labor market returns were just one aspect. Unfortunately, the post-treatment surveys do not contain questions about funding possibilities, making it difficult to disentangle the effects of the information regarding returns from that regarding financing.

Table 2.5.3: Treatment effect on perceived labor market returns

| | Treatment effect | Control Group Mean | N |
|---|---|---|---|
| Unemployment risk is smaller | 0.096** | -0.087 | [966] |
| | (0.048) | | |
| Prospects for finding a well paid job are higher | 0.079*** | 0 | [952] |
| | (0.028) | | |
| Relative income premium | 0.050 | -0.027 | [752] |
| (bachelor's/vocational degree) | (0.052) | | |

*Notes:* This tables presents the effect of information provision on students' perceived labor market returns to university education. Each row represents a separate regression with the outcome specified in the most left column. Estimates are based on Equation 2.3, i.e. using changes in subjective labor market benefits as dependent variables. In all estimations we control for age, gender, migration background, school types, standardized math and German grades as well as two measures for cognitive skills measured by a verbal and a matrix test. The number of observations is shown in square brackets in the utmost right column and varies across estimations due to item non-response. Standard errors in parentheses are clustered at the school level. Source: Best Up, wave 1 and 2. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

The estimates in Table 2.5.3 are based on Equation 2.3 and suggest that students absorbed the provided information. Treated students updated their subjective beliefs in the expected way and all estimates have the expected sign and are, with one exception, statistically significant. Students in the treatment group are significantly more likely to expect their unemployment risk to be smaller and their prospects of finding a well-paid job to be higher with a university degree than with a vocational degree. As such, the information workshop seems to have provided students with relevant information that may influence their educational decision making.

After providing evidence that students process the information from the in-class presentation, we now turn to the main results. Table 2.5.4 presents the treatment effects of the information intervention on intended college enrollment (1) one year prior high school graduation, i.e. two to three months after the intervention and (2) shortly after high school graduation, i.e. one year later. We argue that this second outcome is likely to be linked to students' actual enrollment behavior. Table 2.5.4 further shows the treatment effect for the whole sample as well as separately by parental educational background. We report estimates of the treatment effect based on Equation 2.3, i.e. our preferred specification, in which we analyze the change in students' intended college enrollment.[33]

Table 2.5.4: Treatment effect on the change in students' intended college enrollment

| | All | | Non-academic background | | Academic background | |
|---|---|---|---|---|---|---|
| **2/3 months after the intervention** | | | | | | |
| Change in intended college enrollment | 0.031 | 0.030 | 0.080** | 0.082** | -0.047 | -0.056* |
| | (0.024) | (0.025) | (0.033) | (0.035) | (0.029) | (0.029) |
| *Control group mean* | *-0.026* | | *-0.034* | | *-0.012* | |
| N | 988 | | 613 | | 375 | |
| **1 year after the intervention** | | | | | | |
| Change in intended college enrollment | 0.059** | 0.058* | 0.080** | 0.078** | 0.041 | 0.033 |
| | (0.029) | (0.033) | (0.032) | (0.035) | (0.051) | (0.047) |
| *Control group mean* | *-0.045* | | *-0.023* | | *-0.082* | |
| N | 827 | | 510 | | 317 | |
| Controls | No | Yes | No | Yes | No | Yes |

*Notes:* This table presents the effect of information provision on students' change in intended college enrollment 2/3 months after the intervention as well as one year after the intervention based on Equation 2.3. In all estimations school types are included as control variables. In columns 2, 4, and 6 additional controls include age, gender, migration background, standardized math and German grades as well as two measures for cognitive skills measured by a verbal and a matrix test. Standard errors in parentheses are clustered at the school level. Source: Best Up, wave 1-3. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

[33]Estimates based on Equation 2.2 are reported in Table A2.5 in the Appendix. In this mean comparison, the estimates of the short-run effects decrease in size but are contained in the 95% confidence interval of the effects presented in Table 2.5.4. The estimates of the effect on intended college enrollment directly after high school graduation (one year after the treatment) are similar to Table 2.5.4, but the effect for students from non-academic backgrounds is not statistically significant.

Looking at the effects of the information workshop on intentions one year prior graduation (see upper panel of Table 2.5.4), we find a positive, but statistically insignificant effect of the information intervention on intended college enrollment in the whole sample. However, this result masks considerable effect heterogeneities by parental educational background. Considering students from non-academic and academic backgrounds separately shows that the information intervention increases intended college enrollment for students from non-academic backgrounds by around 8 percentage points in the short run (p-value $< 0.05$).

In contrast, students from an academic background decrease their enrollment intentions 2-3 months after the information workshop by 5.6 percentage points (p-value $< 0.10$). Although the negative effect for students from academic families is only marginally statistically significant, the sign of the effect might at first be a rather surprising finding. However, the fact that students from different educational backgrounds respond in opposite direction to the information treatment in the short term, suggests that information sets of students may indeed be biased towards the educational level that prevails in their environment. Where students from non-academic family backgrounds may lack information about university education, students from academic backgrounds may have an information deficit about options other than university education.[34]

Focusing on the change in intended college enrollment one year after the intervention, i.e. shortly after high school graduation (see bottom panel of Table 2.5.4), reveals that the marginally statistically significant negative treatment effect on students from academic backgrounds does not persist. The information intervention has no statistically significant impact on students' enrollment intentions one year later. Among treated students from academic family backgrounds, more than two thirds revert to their pre-treatment intention to enroll one year later. For this group of students family expectations are likely to outweigh the information treatment, since path dependency might be even stronger for this subgroup as downward mo-

[34] After the information workshop, some students from academic family backgrounds might have regarded vocational education to be more attractive than they originally assumed and, for the first time, considered vocational degree as a valid "outside" option. It might be that for these students raising the awareness for alternatives to university education and providing further information on vocational education may indeed induce them to choose this path.

bility in educational attainment rarely occurs in Germany (see for example Heineck and Riphahn, 2009; Mueller and Pollak, 2015; Schnitzlein, 2016).[35]

For students from non-academic backgrounds, Table 2.5.4 shows that the information intervention still affects students' intended college enrollment shortly after graduation, i.e. one year after the workshop. The estimates of the effect on these intentions that are likely linked to actual enrollment remain similar in size and statistical significance level compared to the findings on enrollment intentions in the short run. Adding control variables only marginally changes our estimates. The information workshop increases students' intended college enrollment measured shortly after high school graduation, i.e. one year after the information workshop, by 8 percentage points. Given students' baseline enrollment intention, this effect corresponds to an overall boost in the share of students with a non-academic family background intending to go to university of about 11 percent.[36] Within the control group, the share of students from non-academic families intending to enroll in college decreases by 2.3 percentage points.

Our results further imply that the information treatment successfully decreases the gap in students' intended college enrollment by parental educational background. Prior to the information treatment this "education gap" in enrollment intentions was 15 percentage points in the treatment group and 12 percentage points in the control group. By increasing intended college enrollment for students from non-academic family backgrounds, the information workshop reduces the gap measured shortly after high school graduation in the treatment group towards 4 percentage points (by 11 percentage points); while the gap in the control group only decreases by 6 percentage points.[37]

In sum, while our findings on intended college enrollment one year prior students high school graduation yield valuable insights on the effectiveness of providing information in the absence of supply side restrictions, we argue that by analyzing enrollment intentions shortly after high school graduation, i.e. closer to the actual decision making, we might get at the potential effect of providing information on

[35]Note that enrolling in the German vocational education system, especially in the dual system, might be more difficult as it requires more timely effort and initiative from students than enrolling in college. This may further explain why some of these students revert to their enrollment intentions shortly after graduating from high school.

[36]Pre-treatment intended college enrollment for students from non-academic family backgrounds in the treatment group is equal to 69.9 (see Table 2.3.1).

[37]These numbers are calculated without the one non-compliant school.

college enrollment. Our results show, that the information workshop increased intended college enrollment for students from non-academic backgrounds. Thus, we similarly expect the information provision to increase college enrollment rates for these students. In contrast, it seems unlikely that enrollment rates for students from academic family backgrounds will be affected.

### 2.5.3 Adjustments to pre-treatment educational plans

In order to better understand the effect of the information workshop, we disaggregate the change in intended college enrollment into three further outcomes. Between periods,[38] students can either adjust their educational expectations upward, downward or remain within their educational plan. We define *upward adjustment* as a binary variable equal to one if a student has no intention to enroll in college pre-treatment and changes her intention towards pursuing a college degree post-treatment, and zero otherwise. Similarly, *downward adjustment* indicates students who change from having an enrollment intention (pre-treatment) to having no intention anymore (post-treatment). Finally, if students maintain their educational intentions, either to enroll in college or to obtain a vocational degree, we refer to this as *stable intentions*. This disaggregation is particularly interesting for students from non-academic backgrounds as it is shown in the literature that these students have more difficulties in forming and maintaining high educational expectations (see e.g. the literature reviewed in Engle, 2007). Based on Equation 2.3, we estimate the effect of the information intervention on these three outcomes separately and present the results in Table 2.5.5.

Focusing on adjustments one year after the information intervention, Table 2.5.5 shows that for students from non-academic families the information treatment significantly decreases the probability to adjust enrollment intentions downward. Treated students from non-academic family backgrounds are 6.3 percentage points less likely to change from intended college enrollment to no intention (see column 3 of Table 2.5.5). The corresponding mean in the control group is equal to 13.6 percent, which implies that the information intervention cuts the share of students who adjust their enrollment intentions downward almost in half. Moreover, Table 2.5.5 shows

[38]This either compares the period between the pre-treatment survey and the survey two to three months after the treatment or between the pre-treatment survey and the survey one year after treatment.

Table 2.5.5: Adjustments to pre-treatment intended college enrollment

| | 2-3 months after the intervention | | 1 year after the intervention | |
|---|---|---|---|---|
| | Non-academic background | Academic background | Non-academic background | Academic background |
| **(1) Upward adjustment** | 0.053** | -0.041** | 0.015 | 0.019 |
| | (0.022) | (0.020) | (0.026) | (0.032) |
| *Control group mean* | *0.063* | *0.049* | *0.113* | *0.048* |
| **(2) Downward adjustment** | -0.029 | 0.015 | -0.063** | -0.015 |
| | (0.026) | (0.024) | (0.025) | (0.030) |
| *Control group mean* | *0.097* | *0.061* | *0.136* | *0.130* |
| **(3) Stable intention** | -0.024 | 0.026 | 0.048 | -0.004 |
| | (0.033) | (0.034) | (0.037) | (0.040) |
| *Control group mean* | *0.841* | *0.889* | *0.751* | *0.822* |
| N | 613 | 375 | 510 | 317 |

*Notes:* This table shows how students adjusted their pre-treatment intended college enrollment in response to the information provision. All estimations are based on Equation 2.3 and include the following control variables: school type, age, gender, migration background, standardized math and German grades as well as two measures for cognitive skills measured by a verbal and a matrix test. Standard errors in parentheses are clustered at the school level. Source: Best Up, wave 1-3. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

that the short-run effect of the information intervention on enrollment intentions of students from non-academic family backgrounds can be attributed to a statistically significant upward adjustment in intended college enrollment. The information treatment almost doubles the share of students who adjust their intentions upwards in the short run. In addition, among those students who moved from no intention to intended college enrollment in the short run, approximately 71 percent maintain their changed enrollment intentions one year later.

In contrast, the marginally statistically significant negative treatment effect for students from academic backgrounds is caused by averting an upward adjustment rather than by provoking a downward adjustment in intended college enrollment (see column 2 of Table 2.5.5). For students from academic families the information treatment decreases students' likelihood of an upward adjustment by 4.1 percentage points in the short run.

The results in Table 2.5.5 suggest that overall the information workshop mainly worked through fostering enrollment intentions for students from a non-academic family background.[39] These students are more likely to maintain their intended college enrollment due to the information provision.

## 2.6 Robustness

In this Section we perform various robustness tests and investigate how sensitive our estimates are to different specifications. None of the sensitivity tests changes our conclusion. A summary of the sensitivity analyses is shown in Table 2.6.6, where the first row shows again the main estimates as a reference point. Columns 1 and 2 of Table 2.6.6 report the robustness of the estimates with respect to short-run intended college enrollment (2-3 months after the intervention) and columns 3 and 4 for enrollment intentions one year after the information workshop.

***Accounting for attrition.*** Attrition is a common problem in RCTs that rely on survey data to measure the outcome of interest. Generally, attrition poses a threat to the estimation of the treatment effect only if there are non-random differences between treatment and control groups. This may result in biased estimates of the treatment effect. As outlined in Section 2.3 differential attrition is of no concern for our estimations. However, even if there is no differential attrition between treatment and control groups, we might still be worried if certain types of students are over- or underrepresented in the analyzed sample and treatment effects vary for these groups. For example, if the information intervention is more (less) effective for underrepresented groups, our estimates will be biased downward (upward). It is well known that individuals with certain characteristics are more likely to respond to surveys than others. Comparing attritors and non-attritors in our sample shows that students who are younger, female, have no migration background, and have higher math grades, German grades, or have higher scores on cognitive measures are more likely to participate in the post-treatment surveys.

In order to investigate how this affects our estimates, we predict the subgroup-specific probability to participate in each of the post-treatment surveys and rerun our estimation using the inverse of these probabilities as sampling weights. To predict post-treatment participation we use the same set of covariates as in our main specifications as well as pre-treatment intentions to enroll in college. Additionally,

[39] Further analyses on students *with* enrollment intentions support this finding (Ehlert, Finger, Rusconi, and Solga, 2017).

Table 2.6.6: Sensitivity Analyses

| | Change in students' intended college enrollment | | | |
|---|---|---|---|---|
| | 2-3 months after the intervention | | 1 year after the intervention | |
| | Non-academic background | Academic background | Non-academic background | Academic background |
| (1) Main | 0.082** | -0.056* | 0.078** | 0.033 |
| | (0.035) | (0.029) | (0.035) | (0.047) |
| | [613] | [375] | [510] | [317] |
| (2) Inverse probability weighting | 0.076** | -0.085** | 0.063* | 0.014 |
| | (0.038) | (0.039) | (0.036) | (0.047) |
| | [613] | [373] | [510] | [315] |
| (3) Entropy balancing | 0.076** | -0.059* | 0.080** | 0.024 |
| | (0.033) | (0.031) | (0.035) | (0.053) |
| | [613] | [375] | [510] | [317] |
| (4) Without non-compliant school | 0.084** | -0.057** | 0.068* | 0.023 |
| | (0.036) | (0.027) | (0.033) | (0.046) |
| | [606] | [358] | [503] | [303] |
| (5) Reassigning non-compliant school to control group | 0.085** | -0.057** | 0.062* | 0.021 |
| | (0.036) | (0.027) | (0.034) | (0.046) |
| | [613] | [375] | [510] | [317] |
| (6) Wild cluster t-procedure | 0.082* | -0.056 | 0.078** | 0.033 |
| *corrected p-value* | *0.068* | *0.104* | *0.046* | *0.508* |
| | [613] | [375] | [510] | [317] |
| (7) Including school fixed effects | 0.083** | -0.050* | 0.064 | 0.013 |
| | (0.036) | (0.028) | (0.043) | (0.056) |
| | [613] | [375] | [510] | [317] |
| (8) Without low response schools | 0.083** | -0.056* | 0.065* | 0.022 |
| | (0.036) | (0.029) | (0.034) | (0.050) |
| | [559] | [324] | [461] | [271] |
| (9) Without potential 'spill-over schools' | 0.077** | -0.068*** | 0.085** | 0.009 |
| | (0.030) | (0.026) | (0.037) | (0.045) |
| | [532] | [352] | [446] | [299] |
| (10) Strict definition on educational background | 0.095*** | -0.058** | 0.106*** | 0.033 |
| | (0.034) | (0.027) | (0.034) | (0.048) |
| | [567] | [367] | [474] | [311] |

*Notes:* This table shows how sensitive our estimates are to different specifications. All estimates are based on Equation 2.3 and include the following control variables: school type, age, gender, migration background, standardized math and German grades as well as two measures for cognitive skills measured by a verbal and a matrix test. The number of observations is shown in square brackets. Standard errors in parentheses are clustered at the school level. To address the issue of school level randomization we include school fixed effects in row 7 of this table; this specification estimates the following equation: $y_{ist} = \alpha + \beta_1(T_s * post_{it}) + \beta_2 post_{it} + X_i'\beta_3 + \kappa_s + \varepsilon_{ist}$, where $y_{ist}$ is the intended college enrollment of student $i$ in school $s$ at time $t$ (t=0,1,2, i.e. before, 2-3 months or one year after the treatment). $T_s$ is the treatment indicator and $post_{it}$ indicates whether it is the post-treatment period. $X_i$ is a vector of additional (pre-treatment) individual level controls (as defined before) and $kappa_s$ represents school fixed effects. For this specification we do not use the predicted treatment status but use the treatment indicator where the non-compliant school is assigned to the control group. Source: Best Up, wave 1-3. * $p$<0.10, ** $p$<0.05, *** $p$<0.01.

we include a binary variable indicating whether we have valid data on the contact information (email, address, phone) that was used to contact students for the post-treatment survey and collected in the pre-treatment survey.[40] Using inverse probability weights slightly decreases our point estimate for students from non-academic backgrounds, whereas it increases (in absolute values) for students from academic backgrounds in the short run. Nevertheless, effect size and statistical significance remain mostly comparable.

***Accounting for covariate imbalance***. In Table 2.3.1 we show the covariate balance for the sample that we use for our analysis as well as for the subgroups by parental educational background. Most of the covariate differences are statistically insignificant. However, irrespective of the statistical significance of these differences the size of some of the differences may trigger concerns about the comparability of treatment and control group students. In order to increase the similarity of treated and control group students we rerun our estimation using entropy balancing weights (Hainmueller, 2012; Hainmueller and Xu, 2013). Entropy balancing reweighs control group students such that a set of pre-specified moment conditions are equal across treatment status. This procedure selects the set of weights that satisfies the pre-specified moment conditions but remains as close as possible to uniform weights (Hainmueller, 2012). In our estimation we require the first moment of all variables included as controls to be the same in the control group as in the treatment group. As shown in row 3 of Table 2.6.6 the short-run result for students from academic backgrounds is unaffected. For students from non-academic backgrounds we find a similar treatment effect one year after the information intervention and a slightly smaller effect with similar significance level in the short run.

***Dealing with non-compliance***. As pointed out in Section 2.4 one school in the treatment group did not receive the information workshop. For our estimations, as presented in Section 2.5, we therefore follow a two stage least squares approach. Yet, in order to assess the sensitivity of the results to the non-compliant school, we run two further analyses. We first examine the treatment effect without the non-compliant school in the sample and then estimate a specification in which we assign the non-compliant school to the control group. Compared to our main specification, the changes in short-run point estimates are only marginal (see row 4 and 5 of Table

[40]This information was updated in the first post-treatment survey.

2.6.6), while the effect size and the significance level decrease slightly for students from non-academic backgrounds one year after the information intervention.

***Wild cluster bootstrap t-procedure***. In our main specification we cluster standard errors at the school level. To account for the small number of clusters (27 schools), we also apply the wild cluster bootstrap-t procedure to calculate alternative p-values as suggested by Cameron and Miller (2015).[41] The corrected p-values are depicted in row 6 of Table 2.6.6 and do not change our conclusions.[42]

***Including school fixed effects***. Given the design of the RCT in which entire schools were randomized, it is advisable to include school fixed effects to account for any time invariant school level omitted variables that might affect students' enrollment intentions. In order to strengthen our results, we estimate a difference-in-difference type of regression, which allows us to additionally include school fixed effects.[43] Other than in our main specification, we do not use predicted treatment status (as obtained from Equation 2.1) but use $T_s$ as the treatment group indicator instead, where the non-compliant school is assigned to the control group. Table 2.6.6 shows that with school fixed effects and further control variables the short-run effects remain very similar. However, the effect for students from non-academic backgrounds one year after the intervention decreases in size and is no longer statistically significant (p-value: 0.145).

***Discarding selected schools***. In Table 2.6.6 we further investigate how sensitive our results are to considering specifics of the project setup, i.e. student level participation and geographical proximity of schools.

[41]We use the Stata command *clustse* (provided by Andrew Menger) and specify the wild option (1000 replications), which implements the program *cgmwildboot* created by Judson Caskey (available from his website at: https://sites.google.com/site/judsoncaskey/data).

[42]Although we have not found any other studies implementing the wild cluster bootstrap t-procedure in a two stage least squares (2SLS) setting, we calculate the corrected p-values in the second stage of the 2SLS approach. Nonetheless, we are confident to report these values, since we also calculated the corrected p-values in the sample without the non-compliant school as well as in the sample with the reassigned non-compliant school; in all cases the statistical significance level in the sample of students from non-academic families decreases to 10%; for students from academic families the statistical significance level of 10% only holds in the case of reassigning or excluding the non-compliant school but not for the specification shown here.

[43]We estimate the following equation: $y_{ist} = \alpha + \beta_1(T_s * post_{it}) + \beta_2 post_{it} + X_i'\beta_3 + \kappa_s + \varepsilon_{ist}$, where $y_{ist}$ is the intended college enrollment of student $i$ in school $s$ at time $t$ (t=0,1,2, i.e. before, 2-3 months or one year after the treatment). $T_s$ is the treatment indicator and $post_{it}$ indicates whether it is the post-treatment period. $X_i$ is a vector of additional (pre-treatment) individual level controls (as defined before) and $kappa_s$ represents school fixed effects. As before, we cluster standard errors at the school level.

First, although the information workshop as well as the pre-treatment survey were conducted during school hours, participation for students was still on a voluntary basis due to strict data protection regulations in Germany. As a result, we observe school-level differences in response rates to the pre-treatment survey. If students' decision to participate is correlated with intended college enrollment, our results will be biased. Thus, to limit the possibility that our results are driven by selection into (student-level) survey participation, we drop those schools with the five lowest school-level response rates from our sample. As shown in row 8, this yields almost no changes regarding short-run estimates. The effect for students from non-academic backgrounds one year after the intervention, however, decreases in size but remains statistically significant at the 10% significance level.

And second, given that the project's focus was to conduct its RCT in districts with a high share of low educated individuals in Berlin, one concern may be that students of treatment schools potentially inform control school students of the information workshop leading to spillover effects. We rerun our estimations excluding all students from control schools that are close, i.e. within a two kilometer radius of a treatment school (see row 9 of Table 2.6.6). For students from non-academic backgrounds the change in the short-run point estimate is minimal and the effect one year after the intervention even increases; for students from academic family backgrounds the short-run estimate slightly increases in absolute values implying a downward bias (in absolute values) of our main estimate of 1.2 percentage points.

***Defining educational background***. To cope with missing information on students' educational background we made several assumptions in order to approximate students' background (described in Section 2.3), thereby minimizing the loss of observations. Therefore, as a last robustness check, we investigate whether a potential misclassification of students affects our estimates. We restrict our sample to students for whom we have *complete* information on parental education only. This approach slightly changes the estimated effect sizes, but increases the statistical significance level of our estimates for students from non-academic backgrounds (p-value $< 0.01$). For students from a non-academic background the effect increases by around one percentage point in the short run and by around two percentage points one year later; whereas the short-run estimate for students of parents with a college degree remains nearly identical to our main specification (see row 10 of table 2.6.6).

Overall, our sensitivity analysis confirms our results. The estimates do not differ significantly from our preferred estimation presented in Table 2.5.4. However, the short-run point estimates for students from academic backgrounds vary slightly more given the smaller sample size.

## 2.7 Conclusion

This paper contributes to the growing economic literature on the effect of information provision on educational decisions. We present results using data from a randomized controlled trial in Germany. Students in randomly selected schools were treated with information about labor market benefits of university education as well as about different funding possibilities. Students seem to comprehend the information they were given. Our results show that students in the treatment group are significantly more likely to expect their unemployment risk to be smaller and their prospects of finding a well-paid job to be higher with a university degree than with a vocational degree. We find that the provision of information increases intended college enrollment for students from non-academic backgrounds, both two to three months and one year after the information treatment. For these students, the information treatment prevents a downward adjustment of their enrollment intentions, i.e. it avoids that these students might be discouraged, if peers and parents based on their own preferences support a differing educational trajectory.

In contrast, the information treatment leads students from academic family backgrounds to lower their enrollment intentions in the short term (albeit this effect is only marginally statistically significant). The treatment may have led these students to re-consider their options after graduation instead of routinely following the expectations of their surroundings. However, our results show that for students from academic families the change in intended college enrollment is only temporary and family expectations seem to matter in the medium run, since we do not find a treatment effect on their enrollment intentions one year later. Thus, we argue that the information provision is likely to increase college enrollment rates for students from non-academic family backgrounds, while it seems unlikely that the information treatment will affect college enrollment of students from academic family backgrounds.

Given the evidence from the U.S. on the so called "summer melt" (e.g. Castleman, Page, and Schooley, 2014; Castleman and Page, 2015) it may, however, not suffice to foster higher educational expectations of students from disadvantaged or non-academic backgrounds to increase their enrollment. Castleman, Page, and Schooley (2014) show that given the complex admission process in the U.S., students from disadvantaged backgrounds need further assistance to follow through on their educational plans. However, in Germany the matriculation process is comparatively less complicated. In Germany, students, who intend to enroll in college, face fewer challenges in the summer following high school graduation than in the U.S., i.e. less forms to fill out, hardly any placement test amongst other things. Thus, we argue that they are more likely to translate their enrollment intentions into actual enrollment.

The fact that we find a statistically significant effect on intended college enrollment for students from non-academic family backgrounds shows that pre-treatment plans do not reflect optimal choices and that those students indeed lack information. If students' intentions were already optimal prior to treatment, receiving information should have no effect. However, although we find a causal effect of information provision, the question of which specific information triggered this result, is less clear. Further research is needed to obtain a better understanding of what particular type of information helps students from non-academic family backgrounds to make an informed decision and encourages them to pursue university education.

In contrast to the study by Kerr, Pekkarinen, Sarvimäki, and Uusitalo (2015), our results indicate that providing (general) information has the potential to impact educational choices, especially for students from non-academic families. One explanation for the differing results, despite the similar context, may be that the general educational decision, i.e. students' choice between university education and an alternative, may be more responsive to information than students' choice of college major. Another possibility may be that teachers/counselors in the RCT by Kerr, Pekkarinen, Sarvimäki, and Uusitalo (2015) differ in their presentation of the information materials and thus no significant treatment effect can be identified. In addition, the mere fact that information is provided in school by an external person, i.e. a person outside the school context, may further contribute to the effectiveness of the information workshop analyzed in this paper.

The gap in educational attainment by family background is mostly discussed from the angle of inequality of opportunities, whereas the loss of efficiency through an underutilization of human capital is often neglected. However, the efficient use of these resources is important, especially in countries with a shrinking labor force. The findings of this paper show that educational inequality – measured by the differences in students' intended college enrollment by parental educational – can be reduced by providing students with relevant information. A tailored information workshop may indeed be an appropriate and inexpensive policy tool to narrow the gap in take up of university education.

# Appendix: Additional tables and figures

Table A2.1: Comparison of schools in recruitment sample and *Best Up* sample (in %)

| School and district characteristics | All schools | Preferred schools | Replacement schools | Contacted schools | *Best Up* schools |
|---|---|---|---|---|---|
| **School type:** | | | | | |
| General high schools *(Gymnasium)* | 53.8 | 33.3 | 55.0 | 41.0 | 33.3 |
| Comprehensive high schools *(Integrierte Sekundarschule)* | 31.7 | 36.7 | 30.0 | 35.9 | 33.3 |
| Vocational high schools *(berufliches Gymnasium)* | 14.4 | 30.0 | 15.0 | 23.1 | 33.3 |
| **District information:** | | | | | |
| Share of low educated aged 25 and older | 17.1 | 23.0 | 20.3 | 22.3 | 21.2 |
| **School information:** | | | | | |
| Cohort size (number of students) | 104 | 109 | 94 | 108 | 102 |
| Share of students with migration background | 13.9 | 18.4 | 15.8 | 18.4 | 17.6 |
| Share of female students | 52.4 | 53.4 | 49.9 | 53.9 | 54.2 |
| Number of schools | 104 | 30 | 20 | 39 | 27 |

*Notes:* This table presents descriptive characteristics of university track high schools in Berlin from which the final *Best Up* sample of schools was drawn. The share of low educated individuals aged 25 and older ranges from 7.1% to 30.3% across Berlin and all 104 schools. For the sample of contacted schools this range goes from 9.1% to 30.3% and from 12.2% to 30.3% in the *Best Up* sample. Source: Federal statistical office of Berlin-Brandenburg (Amt für Statistik Berlin-Brandenburg 2011/12); and regional data from Amt für Statistik Berlin-Brandenburg (2011).

Table A2.2: Covariate balance by treatment status based on the baseline sample

| | Baseline sample | | | |
|---|---|---|---|---|
| | Control Group Mean | | Treatment Group Difference | |
| **Intended college enrollment** | 0.77 | (0.021) | -0.016 | (0.042) |
| **Individual characteristics** | | | | |
| Age | 18.704 | (0.152) | -0.131 | (0.248) |
| Female | 0.570 | (0.031) | -0.007 | (0.054) |
| Migration background | 0.518 | (0.063) | 0.035 | (0.120) |
| Non-academic background | 0.623 | (0.028) | -0.022 | (0.052) |
| **Performance and skills** | | | | |
| German Grade | 8.558 | (0.192) | -0.061 | (0.329) |
| Math Grade | 7.676 | (0.158) | 0.253 | (0.275) |
| Cognition test (verbal) | 9.464 | (0.274) | 0.350 | (0.500) |
| Cognition test (figural) | 10.681 | (0.205) | 0.212 | (0.284) |
| **School type** | | | | |
| School type I *(Gymnasium)* | 0.280 | (0.116) | 0.021 | (0.200) |
| School type II *(Integrierte Sekundarschule)* | 0.376 | (0.129) | 0.013 | (0.219) |
| School type III *(berufliches Gymnasium)* | 0.345 | (0.127) | -0.034 | (0.207) |
| **Perceived returns** | | | | |
| Unemployment risk smaller | 0.390 | (0.017) | -0.000 | (0.030) |
| Prospects for well paid job higher | 0.700 | (0.017) | 0.008 | (0.020) |
| Relative income premium (bachelor's/vocational degree) | 1.542 | (0.032) | 0.009 | (0.040) |
| Life time income higher | 0.622 | (0.017) | 0.002 | (0.028) |
| N | 1059 | | 519 | |
| N (total) | 1578 | | | |

*Notes:* This table presents control group means and treatment-control differences for the baseline sample. Means and mean differences are derived by separately regressing each variable on the treatment group indicator, i.e. $X_i = \alpha + \beta Z_s + \varepsilon_i$, where $X_i$ represents the variable in the left most column and $Z_s$ is an indicator variable for treatment status as obtained from randomization. Standard errors are clustered at the school level and shown in parentheses. Source: Best Up, wave 1. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A2.3: Covariate balance by treatment status based on sample one year after the intervention

| | All | | Non-academic background | | Academic background | |
|---|---|---|---|---|---|---|
| | Control Group Mean | Treatment Group Difference | Control Group Mean | Treatment Group Difference | Control Group Mean | Treatment Group Difference |
| **Intended college enrollment** | 0.796 | -0.037 | 0.751 | -0.054 | 0.870 | -0.017 |
| | (0.024) | (0.047) | (0.029) | (0.066) | (0.025) | (0.049) |
| **Individual characteristics** | | | | | | |
| Age | 18.553 | -0.086 | 18.704 | -0.195 | 18.303 | 0.101 |
| | (0.160) | (0.244) | (0.138) | (0.262) | (0.206) | (0.267) |
| Female | 0.588 | 0.029 | 0.588 | 0.072 | 0.587 | -0.036 |
| | (0.029) | (0.057) | (0.032) | (0.048) | (0.039) | (0.097) |
| Migration background | 0.459 | 0.075 | 0.494 | 0.058 | 0.400 | 0.105 |
| | (0.058) | (0.119) | (0.071) | (0.143) | (0.043) | (0.103) |
| Non-academic background | 0.624 | -0.022 | | | | |
| | (0.034) | (0.051) | | | | |
| **Performance and skills** | | | | | | |
| German Grade | 8.831 | -0.198 | 8.569 | -0.115 | 9.263 | -0.357 |
| | (0.226) | (0.380) | (0.244) | (0.414) | (0.244) | (0.402) |
| Math Grade | 8.057 | 0.410 | 7.959 | 0.317 | 8.218 | 0.539 |
| | (0.198) | (0.339) | (0.191) | (0.308) | (0.324) | (0.533) |
| Cognition test (verbal) | 9.937 | 0.165 | 9.554 | 0.113 | 10.572 | 0.189 |
| | (0.224) | (0.415) | (0.263) | (0.404) | (0.228) | (0.486) |
| Cognition test (figural) | 10.971 | 0.233 | 10.664 | 0.615* | 11.481 | -0.389 |
| | (0.197) | (0.256) | (0.229) | (0.339) | (0.190) | (0.258) |
| **School type** | | | | | | |
| School type I | 0.327 | -0.013 | 0.296 | 0.038 | 0.380 | -0.095 |
| *(Gymnasium)* | (0.131) | (0.208) | (0.121) | (0.212) | (0.155) | (0.220) |
| School type II | 0.358 | 0.054 | 0.380 | 0.026 | 0.322 | 0.100 |
| *(Integrierte Sekundarschule)* | (0.134) | (0.223) | (0.134) | (0.225) | (0.140) | (0.231) |
| School type III | 0.315 | -0.041 | 0.325 | -0.064 | 0.298 | -0.004 |
| *(berufliches Gymnasium)* | (0.125) | (0.196) | (0.123) | (0.193) | (0.138) | (0.213) |
| **Perceived returns** | | | | | | |
| Unemployment risk smaller | 0.409 | -0.036 | 0.38 | -0.021 | 0.449 | -0.062 |
| | (0.027) | (0.044) | (0.034) | (0.056) | (0.043) | (0.054) |
| Prospects for well paid job higher | 0.704 | -0.000 | 0.704 | -0.005 | 0.703 | 0.007 |
| | (0.022) | (0.029) | (0.029) | (0.041) | (0.035) | (0.053) |
| Relative income premium | 1.519 | -0.025 | 1.531 | -0.043 | 1.498 | 0.006 |
| (bachelor's/vocational degree) | (0.034) | (0.054) | (0.037) | (0.058) | (0.050) | (0.088) |
| Life time income higher | 0.634 | 0.014 | 0.653 | -0.040 | 0.602 | 0.099* |
| | (0.020) | (0.046) | (0.023) | (0.061) | (0.032) | (0.050) |
| N | 553 | 274 | 345 | 165 | 208 | 109 |
| N (total) | 827 | | 510 | | 317 | |

*Notes:* This table presents control group means and treatment-control differences for the analyzed samples used to investigate treatment effects on intended college enrollment one year after the information treatment. Means and mean differences are derived by separately regressing each variable on the treatment group indicator, i.e. $X_i = \alpha + \beta Z_s + \varepsilon_i$, where $X_i$ represents the variable in the left most column and $Z_s$ is an indicator variable for treatment status as obtained from randomization. In addition to the marginally statistically significant difference regarding the perception on lifetime income (see also Table 2.3.1), in this sample treated students from non-academic backgrounds score slightly higher on the figural cognition test. However, the absolute size of the difference corresponds to around a fifth of a standard deviation, which we consider negligible. Standard errors are clustered at the school level and shown in parentheses. Source: Best Up, wave 1. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A2.4: Descriptive statistics by students' parental educational background

| | Non-academic background | Academic background | Difference |
|---|---|---|---|
| **Intended college enrollment** | 0.732 | 0.864 | -0.132*** |
| **Individual characteristics** | | | |
| Age | 18.662 | 18.363 | 0.300*** |
| Female | 0.617 | 0.552 | 0.065* |
| Migration background | 0.522 | 0.419 | 0.103** |
| **Performance and Skills** | | | |
| German Grade | 8.467 | 9.144 | -0.677*** |
| Math Grade | 7.965 | 8.450 | -0.485* |
| Cognition test (verbal) | 9.460 | 10.605 | -1.145*** |
| Cognition test (figural) | 10.917 | 11.312 | -0.395* |
| **School types** | | | |
| School type I *(Gymnasium)* | 0.295 | 0.325 | -0.030 |
| School type II *(Integrierte Sekundarschule)* | 0.374 | 0.365 | 0.008 |
| School type IIII *(berufliches Gymnasium)* | 0.331 | 0.309 | 0.022 |
| **Information sources** | | | |
| Internet | 95.402 | 94.879 | 0.524 |
| Friends | 89.256 | 88.679 | 0.577 |
| Central study counseling | 36.913 | 38.859 | -1.946 |
| Job information center/Employment agency | 60.738 | 52.162 | 8.576** |
| Parents/Family | 87.273 | 94.879 | -7.606*** |
| Parents/Family helpful as information source (1-5) | 3.545 | 3.935 | -0.389*** |
| **Costs** | | | |
| Feeling well informed about university education | 0.331 | 0.377 | -0.046 |
| Problem: obtaining information | 0.282 | 0.248 | 0.034 |
| Not/hardly dealt with financing possibilities | 0.485 | 0.458 | 0.027 |
| No scholarships known | 0.367 | 0.281 | 0.085* |
| Perceived cost burden high | 0.484 | 0.280 | 0.205*** |
| **Perceived returns** | | | |
| Unemployment risk smaller | 0.385 | 0.428 | -0.043 |
| Prospects for well paid job higher | 0.717 | 0.706 | 0.011 |
| Relative income premium (bachelor's/vocational degree) | 1.532 | 1.493 | 0.040 |
| Life time income higher | 0.650 | 0.641 | 0.009 |
| N | 613 | 375 | |

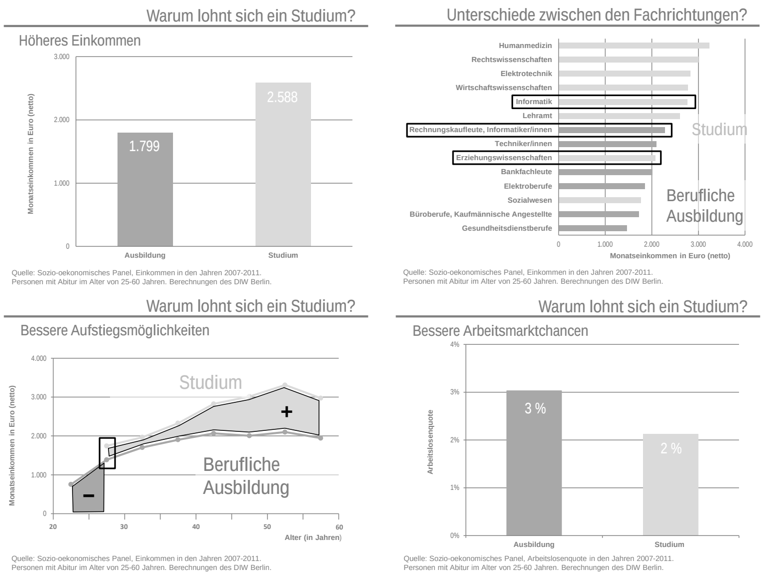
*Notes:* This tables documents differences of students by educational background with regard to various characteristics. Differences are based on a two-sided t-test. Source: Best Up, wave 1. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A2.5: Treatment effect on intended college enrollment: Mean comparison

| | All | | Non-academic background | | Academic background | |
|---|---|---|---|---|---|---|
| **2/3 months after the intervention** | | | | | | |
| Intended college enrollment | 0.019 | 0.015 | 0.055* | 0.055* | -0.046 | -0.052* |
| | (0.026) | (0.025) | (0.031) | (0.030) | (0.031) | (0.031) |
| *Control group mean* | *0.792* | | *0.749* | | *0.865* | |
| N | 988 | | 613 | | 375 | |
| **1 year after the intervention** | | | | | | |
| Intended college enrollment | 0.035 | 0.030 | 0.041 | 0.041 | 0.029 | 0.022 |
| | (0.032) | (0.034) | (0.036) | (0.037) | (0.045) | (0.043) |
| *Control group mean* | *0.796* | | *0.751* | | *0.870* | |
| N | 827 | | 510 | | 317 | |
| Controls | No | Yes | No | Yes | No | Yes |

*Notes:* This table presents the effect of information provision on students' intended college enrollment 2/3 months after the intervention as well as one year after the intervention based on Equation 2.2. In all estimations school types and pre-treatment intended college enrollment ($y_i^{(t_0)}$) are included as control variables. In columns 2, 4, and 6 additional controls include age, gender, migration background, standardized math and German grades as well as two measures for cognitive skills measured by a verbal and a matrix test. Standard errors in parentheses are clustered at the school level. Note that the treatment effect for students from non-academic backgrounds 2-3 months after the treatment is very similar in terms of magnitude, as the differences in pre-treatment intended college enrollment shown in Table 2.3.1. The difference in statistical significance stems from the decrease in residual variance in the treatment models, where we control for school types and students' pre-treatment intended college enrollment. Source: Best Up, wave 1-3. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Figure A2.1: Presentation slides used in the information treatment: Examples




Note: This figure provides examples of the presentation slides used in the information treatment, in which college and vocational education were compared conditional on having earned the *Abitur*. The slide in the upper left panel shows the difference in average earnings between individuals with a university degree *(Studium)* and a vocational degree *(Ausbildung)*. The upper right panel shows earnings differences across different university majors and occupations in vocational education. The slide in the lower left panel shows a comparison of lifetime earnings with a university degree and a vocational degree, while in the lower right panel the unemployment rate for individuals with a university degree and a vocational degree are depicted.

Chapter 3

# The Effect of Increasing Education Efficiency on University Enrollment

## Evidence from Administrative Data and an Unusual Schooling Reform in Germany*

## 3.1 Introduction

It is well-established that more education is beneficial in an array of different dimensions (see e.g. Card, 1999; Lochner, 2011). At the same time, the more years individuals spend in education, the later they enter the labor market. Hence, there is a trade-off between an earlier labor market entry and constant levels of education. In light of aging populations, this trade-off is particularly relevant for countries trying to increase the pool of active labor market participants by allowing for earlier labor market entries.

Several proposals have been made to reduce the age at labor market entry. Yet, the existing literature suggests that these have negative consequences: Lowering the general school starting age (Bedard and Dhuey, 2012), shortening the school year (Pischke, 2007), reducing the number of years required for specific degrees (Webbink, 2007; Morin, 2013; Krashinsky, 2014), and reducing the years of compulsory schooling (see e.g., Card, 1999) are found to have adverse effects on students' educational and labor market outcomes. An unusual education reform in Germany bears

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the potential to decrease the length of schooling without compromising other education outcomes. This so-called G8 reform reduced the number of years of schooling necessary to earn the university entrance qualification at academic high schools but simultaneously increased instruction hours in the remaining years in order to avoid detrimental effects on students' human capital. In this setting, students receive the same amount of schooling but over a shorter period of time. From an individual's perspective this clearly marks an efficiency gain.

The G8 reform did not just spark a lively discussion regarding the potential negative effects for affected students due to the higher workload and the younger age at graduation, but it also stimulated a growing literature on this topic (see Huebener and Marcus (2015) for an overview of existing studies). Most of these studies are confined to the short-term consequences of the reform, either examining students during or at the end of high school. We study medium-term outcomes of the reform relating to the goal of earlier labor market entries and to human capital acquisition: (i) enrollment rates in university, (ii) the timing of enrollment, and (iii) students' study progress at university.

Several arguments suggest that the compression of secondary schooling will affect higher education decisions – despite the intentions of policy-makers. First, the one year reduction in the length of academic high school implies a reduction in students' age at high school graduation. Younger students might be more likely to prefer present gains over higher future gains (Lavecchia, Liu, and Oreopoulos, 2016), thus making university education less attractive. Additionally, students at high school graduation have now one year less time for orientation, less time to discover their talents and less time to develop their preferences, which might increase uncertainty about post-secondary educational choices. If students are aware of their relative age advantage, this may further entice them to take things more slowly and to delay their enrollment decisions. Second, the compensating increase of instruction hours in the remaining years implies a higher weekly workload as measured by weekly instruction hours. Students able to meet these higher requirements may be even better prepared for the learning requirements at university. However, for students who are unable to cope with the higher workload this may result in worse performance. Indeed, existing evidence suggests that students' performance in school is negatively affected by the reform (Büttner and Thomsen, 2013; Trautwein, Hübner, Wagner, and Kramer, 2015; Huebener and Marcus, 2017). Additionally, affected students report increasing levels of stress and strain in school due to the reform (Meyer and Thomsen, 2015; Quis, 2015; Trautwein, Hübner, Wagner, and Kramer, 2015), which might also reduce the desire and motivation for further learning (Jürges and Schneider, 2010). Given that students' performance in school is one of the most important determinants for the enrollment decision as well as for success in university (Bowen and Bok, 1998), we expect adverse effects on higher education decisions.

We exploit the differential timing of the reform implementation across states in a difference-in-differences setting. Relying on administrative data on the universe of students in Germany, we find that, due to the G8 reform, the share of students who enroll in university within one year after high school graduation decreases by about 6 percentage points (pp), which corresponds to a decrease by about 8 percent. The impact on enrollment rates within two or three years after graduation is of similar magnitude, thus suggesting that enrollment rates do not catch-up. Further, we find evidence that the achievement of the reform's main goal in bringing university graduates earlier to the labor market is mitigated: As a consequence of the reform, students are 6.8 pp more likely to delay their enrollment (compared to a sample mean of 61%) and 2.6 pp less likely to make expected progress during their first year at university (sample mean: 81%). The latter is explained by a higher probability to drop out of university and a higher probability to change majors. The main mechanism driving our results is not the age difference of students as our results do not change substantially when we focus – before and after the reform – on similar-aged graduates; instead our analysis suggests that the higher workload experienced during high school explains our findings. The negative reform effects seem to be general consequences of the reform as we find little evidence for effect heterogeneity between states, cohorts, or gender. We perform a battery of robustness checks and falsification exercises to support the identifying assumption of common trends in the outcome variables in treatment and control states.

The results of our study are not only informative for the German context but also for policy-makers in other countries who are trying to increase the number of active labor market participants in order to address the challenges of an aging society. However, our study shows that it not easy to get around the trade-off between constant levels of education and an earlier labor market entry.[1]

[1] Note that as the reform was only implemented recently, it is not yet possible to directly examine outcomes at labor market entry. Only a small and highly selective group of affected students are already on the labor market.

The remainder of the paper is structured as follows. In Section 3.2 we provide details about the reform implementation before summarizing existing evidence on the reform effects. Section 3.3 introduces the data and describes the construction of our outcome variables, while Section 3.4 outlines the empirical approach. Section 3.5 presents the empirical evidence of the reform effects on higher education decisions. In Section 3.6 we show the robustness of these results to various model specifications. Section 3.7 examines effect heterogeneity, including gender and state-specific treatment effects as well as the development of the treatment effect over time. A discussion on potential channels is addressed in Section 3.8, while Section 3.9 concludes.

## 3.2 The G8 reform

In most German states students complete four years of primary school before being assigned to different tracks of secondary schooling based upon their ability. The G8 reform analyzed in this study affects only one of these tracks, the academic high school (*Gymnasium*), which is the high-ability track that prepares students for university. It is attended by about one-third of a cohort.

The idea of the G8 reform is to shorten the length of academic high school without affecting students' human capital. The intermediate aim of the reform is to allow for an earlier labor market entry of young people, thereby helping to achieve three further goals. First, to increase the number of contributors to the public pay-as-you-go pension system, which is under pressure due to an aging population. Second, to compensate for the skilled-worker shortage. Third, to make German university graduates more competitive on the international labor market by reducing their comparatively high age at graduation from university.

The G8 reform can be depicted as consisting of two parts. The first part is a reduction of the time until leaving the academic high school with the general university entrance qualification, the *Abitur*, from 13 to 12 years, making students one year younger at school graduation.[2] The second part is an increase in the weekly

[2]The reform derives its name G8 from the fact that – after usually four years of joint primary schooling – graduation requires now eight years of schooling at an academic high school instead of nine. Note that three states offer six years of joint primary schooling. Although the term G8 is not accurate for these states, the term G8 is widely used within Germany. Therefore, we stick to this term and use the term G9 to refer to the previous regime.

load of instruction hours in the remaining years as the number of instruction hours required for graduation was left unchanged.[3] On average the required number of weekly instruction hours at academic high schools increased from 29.4 to 33.1 hours per week (or 12.5%) and resulted in an increase in weekly workload. This second part was meant to compensate for the loss in instruction hours due to the omitted $13^{th}$ grade. Therefore, the G8 reform can be seen as a redistribution of instruction hours from the last grade to the previous grades. Due to the additional weekly instruction hours after the reform's implementation each grade covered also some material that was previously taught in higher grades. Note that by construction of the reform, the first G8 cohort and the very last cohort under the old G9 regime graduated in the same year. This cohort is referred to as the double graduation cohort. Figure 3.2.1 provides an overview of the timing of the G8 reform and shows that the first exclusive G8 cohorts graduated in different points in time in different states. The figure also shows that two states always had G8, while two other states did not switch to G8 during our observation period. Our empirical strategy exploits this regional and temporal variation.[4]

The introduction of the G8 reform sparked a lively discussion about potential negative effects for affected students due to the higher workload and the younger age at graduation. It has stimulated a growing number of research on this topic (see Huebener and Marcus (2015) for an overview of existing studies). Most of these studies examine short-term effects and analyze outcomes at the end of academic high school. There is evidence for slightly weaker performance at the end of school (Büttner and Thomsen, 2013; Trautwein, Hübner, Wagner, and Kramer, 2015), increased grade repetition rates (Huebener and Marcus, 2017), higher experienced levels of stress (Quis, 2015), and less time for working in a side job (Meyer and Thomsen, 2015). Further, these studies find no effects on graduation rates (Huebener and Marcus, 2017), but show that affected students feel more strained by learning (Meyer and Thomsen, 2015). The evidence with respect to the impact on personality traits is mixed. While Dahmann and Anger (2014) find that affected students are more extroverted and less emotionally stable, Thiel, Thomsen, and Büttner (2014) do not find an effect on personality traits of students.

[3] Unless explicitly stated *graduation* refers to graduation from academic high school.

[4] Some states have already decided to switch back to the G9 regime or leave the decision on track length to individual schools. However, these changes are outside of our observation period (see Huebener and Marcus (2015) for more details).

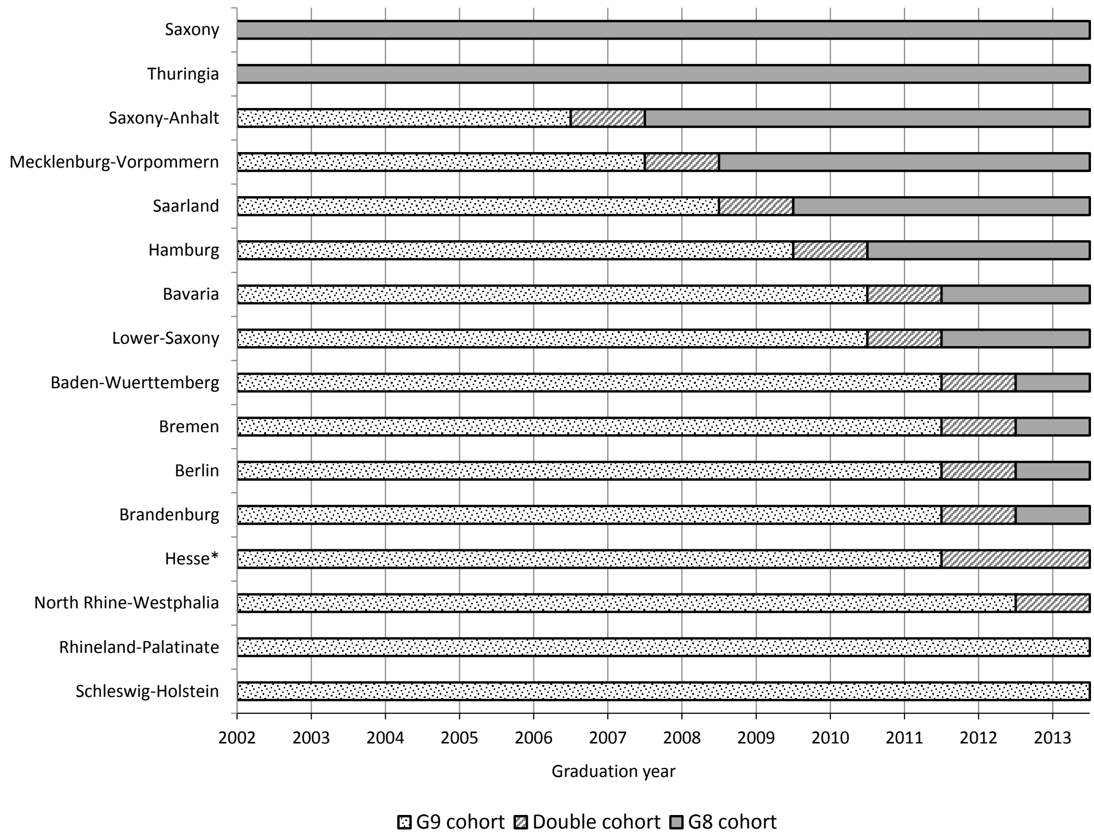


Figure 3.2.1: Timing of the G8 reform in the German states

*Notes:* The figure illustrates the treatment status of different graduation cohorts in the German states. * Hesse implemented the reform over various years and is not included in our main analysis sample.

Only two studies analyze medium-term consequences of the overall G8 reform. The first is Meyer and Thomsen (2014, 2016), who find that females delay their university enrollment, while there is no comparable effect for males. In contrast, they report no significant differences between G8 and G9 students with respect to dropping out, motivation and self-reported abilities. These findings do, however, rely exclusively on the double graduation cohort in two cities in a single federal state (Saxony-Anhalt) to identify the reform effects. Additionally, the double graduation cohort is a very particular cohort, as G8 students graduated together with the last cohort of the previous regime, which could affect their post-secondary education choices, due to the larger cohort size (Bound and Turner, 2007) and the increased competition for university resources. Morin (2015a), for example, shows that a large cohort size of high school graduates decreases labor market earnings for the affected graduates. If this earnings shock is anticipated by students, it may change their university enrollment decision. Further, Morin (2015b) provides evidence that males and females react differently to the increased competition resulting from larger cohort sizes at university. For these reasons, it is unclear to what extent the results based on one double cohort in a single state are also valid for later cohorts and other German states.[5]

The working paper by Meyer, Thomsen, and Schneider (2015) is the only other study looking at post-secondary education choices based on data covering all German states. Their findings suggest that students affected by the reform are less likely to enroll in university in the year of school graduation. If not only actual enrollment but also intended enrollment is considered, the effect disappears for females and decreases but persists for males. The study further finds an increase in the probability of spending a year abroad or performing voluntary services, which may partly explain the delayed enrollment effects. There is also some evidence that students are more likely to start vocational education. We complement and extend this working paper in several ways: First, by analyzing a time period up to three years after high school graduation, we can disentangle the effect on the timing of enrollment from the actual enrollment choice.[6] Second, we investigate further outcomes, revealing the effect on students' study progress, their dropout behavior as well as the likelihood of students to change their major. These outcomes strongly relate to the reform's major goal of reducing the age at labor market entry. Third, given the longer time horizon, we can investigate whether the effects are only of transitory nature or whether they persist across subsequent cohorts. Fourth, we make several methodological improvements, e.g. by accounting for the special incentives of the last pre-treatment cohort, by clustering standard errors at the level of the policy change, and by using the variation in the timing of the reform implementation more efficiently. And fifth, our analysis relies on a full population survey, such that attrition, item non-response, and non-representativeness are of little concern.[7]

[5]Furthermore, the first G8 cohort in Saxony-Anhalt was already in grade 9 when they were informed about the shortening of the school duration, making this cohort even more peculiar.

[6]Based on their dataset, Meyer, Thomsen, and Schneider (2015) can only look at actual choices six months after high school graduation.

[7]The survey data used by Meyer, Thomsen, and Schneider (2015) suffers from high attrition rates (over 50% during the course of a year).

## 3.3 Data

### 3.3.1 The German Student Register

Our empirical analysis is based on administrative data from the German Student Register *("Studentenstatistik")* that covers all students enrolled in any German university between 2002 and 2014 (Studierendenstatistik, 2014).[8] Each university in Germany is obligated to provide the Federal Statistical Office with information on each individual student. The dataset contains individual level information but, due to tight data protection regulations, information on individual students cannot be linked over time. In addition to information on the year of first-time enrollment, choice of study program and institution, the data also contains information on when and in which state the student graduated from high school. This is the crucial information for determining treatment status. It is further registered which type of university entrance qualification the student has earned and whether it was earned at an academic high school. Given this information, we can identify students who were affected by the reform and those who were not. As is common for administrative data in Germany, background information is limited to gender, nationality, and date of birth.

This data set comes with at least three main benefits. First, as it is a full population survey, the sample size is large, which allows precise estimates of the reform effects. Second, as it is administrative data panel attrition, non-representativeness, and item non-response are of little concern. Third, data quality can be regarded as high, as each institution is obligated to record the information by law. Despite these advantages, the data set is not used much at the individual level, with Görlitz and Gravert (2016) and Horstschräer and Sprietsma (2015) being the exceptions.[9]

In our analysis we exclude all students who earned their university entrance qualification in Hesse because this state gradually implemented the G8 reform over a period of three years. Thus, we are unable to distinguish treated from untreated students. Furthermore, we keep only students who earned their general university entrance qualification from an academic high school as the reform only affected this

[8] There are several types of higher education institutions in Germany: public universities, private universities, universities of applied science, as well as colleges specializing in theology, music, art, or education. We refer to all these institutions as "universities", unless explicitly stated otherwise.
[9] Hübner (2012) and Bruckmeier and Wigger (2014) use an aggregated version of the German Student Register that is publicly available and provided by the Destatis (2016c).

track. Alternative types of university entrance qualifications can be earned at other school types that were not affected by the G8 reform. In order to mitigate concerns about potential selection issues, the robustness section shows that the reform did not change the number of graduates from academic track schools and that there is no evidence that the reform entailed changes in the composition of students in academic track schools. We discuss potential selection issues in more detail in the robustness section.

### 3.3.2 Outcomes

In the following, we describe how we construct our three main outcome variables: *enrollment rate*, *timing of enrollment*, and *study progress*. For robustness purposes, we also work with alternative measures of our outcome variables. Generally, we use the individual level information and aggregate it at the state-cohort level as our treatment also varies at this level. Note that performing the analysis at the aggregate level yields the same results as performing the analysis at the individual level, if no individual control variables are included and the aggregate level analysis is appropriately weighted (Angrist and Pischke, 2009, 235). Furthermore, note that the construction of our outcome variables requires individual level data, as the aggregated data provided by the Destatis (2016c) does not include the relevant information to determine treatment status and construct our outcomes.

***Enrollment rate.*** A frequently stated policy goal in Germany, as well as in many other countries, is to increase the number of university students (OECD, 2016). The share of university educated individuals is often seen as a driver of economic growth (see e.g. Moretti, 2004) and associated with a range of non-monetary returns, like improved health (see e.g. Lochner, 2011) and participation in democratic activities (see e.g. Glaeser, Ponzetto, and Shleifer, 2007). Not surprisingly, a large number of studies investigate enrollment behavior. Each analysis of cohort enrollment rates must cope with right-censored data as not all students make their enrollment decisions immediately after high school graduation. In particular, until July 2011 males in Germany were obligated to complete military or civilian service, which most completed prior to entering post-secondary education. Additionally, some high school graduates take some time off before enrolling in university in order to stay abroad, do an internship or voluntary service, or just enjoy some free time. Further, some high school students complete a vocational degree before enrolling in university.

Many studies focus on immediate enrollment after high school graduation (Hübner, 2012; Bruckmeier and Wigger, 2014; Meyer and Thomsen, 2016), neglecting that a substantial share of students enrolls a year later. We extend this time window and focus on individuals who enroll in the year of high school graduation or the year after, thereby capturing the majority of students who eventually enroll in university (see also Table 3.3.1). Additionally, we will further alter this time window and analyze enrollment rates up to three years after high school graduation.

In order to analyze general enrollment rates, we combine the individual level dataset on all students enrolled in university with annual information on the number of graduates from academic high schools in each state (Destatis, 2015). From these two sources, we calculate aggregate enrollment rates for each state and graduation cohort. More specifically, the enrollment rate is given by the share of freshmen students who enrolled in university within one year after graduating from an academic high school.

$$Enrollment\ rate_{sc} = \frac{ENR_{sc}^{t} + ENR_{sc}^{t+1}}{GRAD_{sc}}\ , \tag{3.1}$$

where $ENR_{sc}$ refers to the number of freshman students, who graduated in state $s$ and graduation cohort $c$ and enrolled in university in the year of high school graduation ($t$) or the year after ($t+1$). Note that this measure is not affected by students' decisions to move to a different state in order to pursue university education, as the crucial information for our measure is the state of high school graduation and not the state in which students enroll in university. $GRAD_{sc}$ denotes the respective number of graduates from academic high schools.

***Timing of enrollment.*** A main goal of the G8 reform is to allow for an earlier labor market entry. The effectiveness of the reform in achieving this goal will be mitigated, if the reform induces students to delay their enrollment. Hence, we analyze the *timing of enrollment* as our second main outcome.

We construct a measure for the timing of enrollment ("speed of enrollment") by dividing the number of students who enroll in the year of high school graduation by the number of students enrolling within one year after high school graduation, i.e. in the same year or the year after high school graduation.

$$Speed\ of\ enrollment_{sc} = \frac{ENR_{sc}^{t}}{ENR_{sc}^{t} + ENR_{sc}^{t+1}} \tag{3.2}$$

This measure indicates how many students delay their enrollment decision and allows us to disentangle changes in the timing of enrollment from general enrollment decisions.[10] Students typically graduate from high school in June, such that enrollment in the same year means starting university in October, i.e. in the following winter term.

***Study progress.*** Similar to *timing of enrollment*, the outcome *study progress* relates to the reform's main goal in achieving an earlier labor market entry. Students not making regular study progress are unlikely to finish their university studies in the regular time. Unfortunately, our data does not include an individual panel identifier that would allow for following individuals over time. However, we can obtain a measure of study progress at the cohort level by exploiting the following particularity of the German higher education system: For administrative purposes, at the beginning of each winter term, the German higher education system not only counts the number of semesters students are enrolled in university (*Hochschulsemester*; semester at university), but also the number of semesters students are enrolled in the same major (*Fachsemester*; semester in same major). For students with regular study progress these two numbers do not differ. We focus on students' study progress within the first year and calculate the share of students with regular study progress out of all students who enrolled within one year after graduating from an academic high school.[11]

$$Regular\ study\ progress_{sc} = \frac{REG_{sc}}{ENR^{t}_{sc} + ENR^{t+1}_{sc}}\ , \tag{3.3}$$

where *REG* refers to the number of students with regular study progress one year after enrollment, i.e. students for whom the number of university semesters equals the number of semesters enrolled in the same major at the beginning of the third semester. Similar to the *timing of enrollment* the outcome *study progress* is only defined for students who enroll in university. Hence, both have a conditional-on-positives interpretation.

[10]Note that this measure only looks at the timing of enrollment for students that enroll in the year of high school graduation or the year after. In the robustness section, we use alternative measures for *timing of enrollment*.

[11]Note that the relevant information on study progress during the first year originates from the beginning of the third semester. Our dataset covers the full student population only in winter terms. Hence, unlike the other two outcomes, regular study progress is based on students who started university in the winter term; students who started in summer term are not included in this measure.

There are three main reasons for a non-regular study progress. First, students can drop out of university. Second, students may change their major.[12] Third, students may formally request a temporary interruption of their university studies (*Urlaubssemester*). In this case the number of interruption semesters is only added to the number of university semesters, while it does not increase the number of semesters in the same major. Among others, reasons for such temporary interruptions are maternity leaves, long-term illnesses, care responsibilities, and studying abroad - although the last is not very common within the first year of studies. We will also decompose *regular study progress* and differentiate between dropout, changing major, and temporary interruption. These three further outcomes are generated analogously to Equation 3.3, in which we successively substitute the numerator with the number of students who drop out, change their major or interrupt their studies, respectively.[13]

### 3.3.3 Descriptive statistics

Table 3.3.1 displays summary statistics related to our outcome variables. In our sample, 47% of high school graduates enroll in university in the same year they graduate from high school. One year later, three-quarters of the graduation cohort is enrolled, while this share increases only marginally to 82% two years after graduation, and to 86% three years after. These numbers indicate that the majority of a cohort enrolls in university in the year of graduation or the year after, i.e. within the first year after high school graduation. After this, only a small share of graduates enroll. Thus, our main analysis focuses on students who enroll in university within one year after high school graduation. Table 3.3.1 further shows that 61% of students who enrolled within one year did so in the year of graduation; this is our main measure for the timing of enrollment. Among students who enrolled within one year, 7% completely drop out of university within the first year of studies, while 11% change their major and 1% take a formal interruption; the remaining 81% of students show regular study progress.

[12]Unlike in the US, students in Germany have to decide on their major at the time they enroll in university. Changing one's major usually results in an increased duration of study.

[13]Note that students switching university are not counted as dropouts in our measure. However, for students who drop out, it might be possible that they enroll again after a break. These students are still counted as dropouts in our measure. Further note that changing major comprises changing major at the same university as well as changing major combined with switching to another university in Germany.

Table 3.3.1: Descriptive statistics

| | Share | N | Included grad. cohorts |
|---|---|---|---|
| Enrollment in the same year | 0.47 | 2,823,274 | 2002-2014 |
| **Enrollment within 1 year** | 0.76 | 2,601,880 | 2002-2013 |
| Enrollment within 2 years | 0.82 | 2,343,454 | 2002-2012 |
| Enrollment within 3 years | 0.86 | 2,091,000 | 2002-2011 |
| **Timing: Immediate enrollment** | 0.61 | 1,987,444 | 2002-2013 |
| **Regular study progress** | 0.81 | 1,656,629 | 2002-2012 |
| Drop out | 0.07 | 1,656,629 | 2002-2012 |
| Changing major | 0.11 | 1,656,629 | 2002-2012 |
| Interruption | 0.01 | 1,656,629 | 2002-2012 |

*Notes:* This table presents summary statistics related to our outcome variables. Our three main outcome variables are shown in bold. For all enrollment outcomes (see the first four lines) $N$ refers to the number of graduates from academic high schools, while for the other variables $N$ refers to university students, i.e. graduates from academic high schools who enrolled in university within one year. Further, for each graduation cohort, the time span after graduation that we can observe differs.

Note that due to the different timing of the reform implementation, the number of states already affected by the reform varies depending on the outcome under consideration. In the sample of our main analysis, we try to include as many observations as possible in order to fully exploit all available information. Therefore, sample sizes differ between the outcomes. Our conclusions, however, do not change when we apply more restrictive sample selection criteria (see Section 3.6).

## 3.4 Estimation strategy

In order to estimate the effect of the G8 reform on (i) the *enrollment rate*, (ii) the *timing of enrollment*, and (iii) *study progress*, we apply a difference-in-differences strategy of the following form:

$$y_{sc} = \beta_1 G8_{sc} + \beta_2 DC_{sc} + \beta_3 lastG9_{sc} + \kappa_s + \mu_c + \varepsilon_{sc}, \tag{3.4}$$

where $y_{sc}$ refers to one of the outcomes for graduation cohort $c$ in state $s$; $s$ denotes the individual's state of high school graduation not the state of the university enrollment. $\beta_1$ depicts the effect of the G8 reform and is the coefficient of interest.

$\kappa_s$ is a set of state fixed effects and captures general differences between states (like time constant differences in states' education systems). A set of time fixed effects ($\mu_c$) takes into account general time trends in the outcomes. This is an essential element of our identification strategy, as, for instance, the share of a birth cohort entering higher education is steadily increasing in Germany. Further, the time fixed effects also capture shocks that are common to all states, like the suspension of military service in 2011 (which is particularly relevant for *timing of enrollment*). The equation further includes an indicator variable, $DC_{sc}$, for the double graduation cohort; we thereby assign the double cohort neither to the treatment nor to the control group for two main reasons. First, the data only contains information on the year and state of high school graduation, not the individual G8 status. Thus, we cannot exactly determine treatment status for individuals in the double cohort. Second, students from the double graduation cohort may be affected rather differently by the reform, as students might have perceived the competition for available slots in university as well as vocational education as higher. We further augment this baseline model by adding a binary variable for the last cohort before the double cohort ($lastG9_{sc}$), which is the last exclusive G9 cohort. This is important because students in this cohort had a particularly strong incentive for speedy enrollment in order to avoid beginning to study with the double cohort. Hence, the G8 reform has spill-over effects on the graduation cohort directly preceding the double cohort. Finally, $\varepsilon_{sc}$ is the error term. As the error term is likely to be correlated within states, we follow the recommendation of Bertrand, Duflo, and Mullainathan (2004) and cluster the standard errors at the level of the policy change.[14] Note that all our aggregate-level regressions are weighted so that our results exactly equal individual level regressions (Angrist and Pischke, 2009, p. 235).[15]

## 3.5 Results

### 3.5.1 Main results

Table 3.5.2 presents our main results. While column (1) shows the results for the baseline difference-in-differences specification, which controls for state and time fixed effects as well as for the double graduation cohort, column (2) - our preferred specification - further controls for the last G9 cohort.

[14]Additionally, we apply wild cluster bootstrapping in the robustness section, which is recommended for situations with few clusters (Cameron, Gelbach, and Miller, 2008; Cameron and Miller, 2015).

[15]For each outcome the weights are given by the outcome's denominator, i.e. enrollment rates are weighted with the number of graduates and the other two outcomes with the number of freshmen students who enrolled in the year of graduation or the year after. Our results are very similar if we perform the analysis without weights (see Section 3.6).

The results in Panel A column (1) indicate that the enrollment rate declined by 5.1 percentage points due to the G8 reform. Controlling for the last cohort before the double graduation cohort (column 2) slightly increases this effect in absolute terms to 6 percentage points. The estimated reform effect amounts to a 8% decline in enrollment.[16]

Table 3.5.2: Effects of the G8 reform: Main results

| | Baseline (1) | + last G9 cohort (2) | % change (3) |
|---|---|---|---|
| **Panel A: Enrollment within one year** | | | |
| G8 reform | -0.051*** | -0.060*** | -7.9% |
| | (0.015) | (0.017) | |
| Double cohort | -0.078*** | -0.085*** | |
| | (0.008) | (0.011) | |
| Last G9 | | -0.016** | |
| | | (0.007) | |
| $N_{state*cohort}$ | 180 | 180 | |
| $N_{individuals}$ | 2,601,880 | 2,601,880 | |
| **Panel B: Speed of enrollment** | | | |
| G8 reform | -0.105*** | -0.068*** | -11.1% |
| | (0.024) | (0.014) | |
| Double cohort | -0.047*** | -0.015 | |
| | (0.014) | (0.009) | |
| Last G9 | | 0.069*** | |
| | | (0.007) | |
| $N_{state*cohort}$ | 180 | 180 | |
| $N_{individuals}$ | 1,987,444 | 1,987,444 | |
| **Panel C: Regular study progress** | | | |
| G8 reform | -0.022** | -0.026*** | -3.2% |
| | (0.008) | (0.009) | |
| Double cohort | -0.009 | -0.013* | |
| | (0.006) | (0.007) | |
| Last G9 | | -0.007* | |
| | | (0.003) | |
| $N_{state*cohort}$ | 165 | 165 | |
| $N_{individuals}$ | 1,656,629 | 1,656,629 | |

*Notes:* This table reports the G8 reform effects on different outcomes as indicated by Panel A-C. In all specifications we include fixed effects for federal states and graduation cohorts. Standard errors are clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

[16] For the calculation of the percentage change we use the average enrollment rate, as reported in Table 3.3.1, as a baseline.

The decline in the enrollment rate of 6 percentage points is quite large compared to other findings in the literature. For example, for the much debated introduction of tuition fees in Germany of 500 EUR per term, Hübner (2012) identifies a decrease in enrollment by 2.7 percentage points. Other studies even find smaller or insignificant reductions in enrollment rates (Helbig, Baier, and Kroth, 2012; Bruckmeier and Wigger, 2014). Comparing our result to findings for financial aid in Germany, Steiner and Wrohlich (2012) estimate that an annual increase in financial aid by 1000 Euro increases enrollment rates by 2 percentage points. Estimated effect sizes of financial aid are similar for Denmark (Nielsen, Sørensen, and Taber, 2010) and slightly larger for the U.S. (see e.g. Dynarski, 2002; Abraham and Clark, 2006). Compared to these effect sizes, our estimate suggests that the negative G8 reform effect on enrollment is substantial.

We further find that the timing of enrollment changes as a consequence of the reform (Panel B). Among those who enroll in the year of graduation or the year after, the probability to immediately enroll decreases by 6.8 percentage points (column 2), indicating that a non-trivial fraction of students delay their enrollment. The estimation results presented in Panel C show that the probability of regular study progress also decreases significantly. The share of students with regular study progress during the first year of studies decreases by 2.6 percentage points.

Table 3.5.2 also reports the coefficients for the double graduation cohort and the last exclusive G9 cohort, i.e. the cohort before the double cohort. Both cohorts are assigned neither to the treatment nor the control group in our main specification. It is worthwhile noting that the reduction in university enrollment in the double graduation cohort is even larger than the G8 effect. The probability to enroll within one year after graduation is reduced by more than 8 percentage points. The effect for this cohort is significantly different from the G8 effect and underlines that the double cohort is peculiar and findings for this cohort do not necessarily translate to later G8 cohorts. Further, this finding is in line with the argument that wages are lower in larger cohorts (Welch, 1979) and that rational students will take this into account in their enrollment decision (Bound and Turner, 2007). As we are unable to distinguish between the cohort's G8 and G9 students, the coefficient for the double cohort displays the joint effect for both G8 and G9 students.[17]

[17] In Table A3.2 in the Appendix, we approximate the treatment status for individuals in the double cohorts based on information on students' birthday and school entry regulations. Due to grade retention, this is only an imperfect approximation and, therefore, not our preferred specification.

It is also evident that the last cohort before the double cohort is affected by the reform. For this cohort, the probability to enroll in university within one year after graduation decreases by about 1.6 percentage points. Graduates of the last G9 cohort also strongly responded to the incentive to enroll in the year of their graduation, in order to avoid starting with the double graduation cohort (which is eligible to enter university one year after the last G9 cohort): The probability of enrolling immediately after graduation increases by 6.9 percentage points for this cohort. This further strengthens the argument that graduates take into account the cohort size in their decision to enroll in university (Bound and Turner, 2007). Thus, it seems advisable to control for these cohorts in our main specification and to assign neither to the treatment nor the control group.

Taken together, the results of this section suggest that fewer graduates enroll in university as a consequence of the G8 reform. On top of that, the reform's success in reducing the age of labor market entry may be mitigated: More students delay their enrollment and fewer students show regular study progress.

### 3.5.2 Outcome-specific supplementary results

In this section we provide further evidence on the reform's effects. These results complement our main analysis as presented in the previous section.

***Enrollment rates:*** The previous section focused on enrollment within one year after high school graduation as the majority of students who enroll in university do so within this time frame (see Table 3.3.1). However, from a human capital accumulation perspective it is important to analyze whether students refrain from enrolling entirely or just delay their enrollment beyond the first year. Thus, we redefine the numerator of Equation (3.1) and study the G8 effect on enrollment rates within 2 and 3 years after graduation. For completeness, and in order to compare our estimates with existing evidence on enrollment rates, we also report the effect on enrollment rates in the year of graduation. We compare these effects to the estimated reform effect on enrollment rates within one year after graduation - our main measure of enrollment.

Nevertheless, within the double cohort, G8 students are also less likely to enroll in university and more likely to delay their enrollment than G9 students. However, we find enrollment rates to decrease also for G9 students in this cohort. The negative effect for the double cohort with respect to study progress even seems to be driven by G9 students.

Table 3.5.3: Further results on enrollment rates

| | Enrollment ... | | | |
|---|---|---|---|---|
| | in the same year | within 1 year | within 2 years | within 3 years |
| | (1) | (2) | (3) | (4) |
| G8 reform | -0.084*** | -0.060*** | -0.058*** | -0.043** |
| | (0.015) | (0.017) | (0.019) | (0.016) |
| $N_{state*cohort}$ | 195 | 180 | 165 | 150 |
| $N_{individuals}$ | 2,823,274 | 2,601,880 | 2,343,454 | 2,091,000 |

*Notes:* This table presents the effect of the G8 reform for additional enrollment outcomes. All estimates are based on our main specification as outlined in Eq. (3.4). In line with controlling for the last G9 cohort in column 1 and 2, in column 3 (4) we additionally control for the cohorts two (and three) years before the double cohort in order to consider the disincentive for these cohorts to enroll together with the double cohort. Standard errors are clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.5.3 shows that the reform's effect is most pronounced for enrollment in the same year, which decreases by 8 percentage points (column 1). This is no surprise, given the evidence that students delay their enrollment. This is the only effect that we can directly compare to estimates in Meyer, Thomsen, and Schneider (2015), as they only observe students in the year of their graduation. Relying on survey data, the authors find an even higher decrease in enrollment in the same year (of about 15 percentage points). The effect on enrollment within two years after graduation (column 2) is as large as the effect on enrollment within one year after graduation. Similarly, the effect on enrollment within three years after graduation (column 3) is only marginally smaller in absolute terms. Note that enrollment within three years after high school graduation provides students starting a vocational education directly after high school graduation enough time to complete this degree (earning a vocational degree usually takes 2-3 years) and enroll in university afterwards. However, even considering these later enrollment decisions, three years after graduation still fewer students enroll in university in response to the G8 reform, providing no evidence for a quick catch-up of enrollment.

Due to the recency of the reform and the related right-censoring, we do not observe a cohort's lifetime enrollment rate (individuals could theoretically also enroll at the age of 25 or 80). However, it seems questionable whether lifetime enrollment for G8 students will catch up with those of G9 students for two reasons. First, three years after graduation the effect size is, with about 4.3 percentage points, still substantial. And second, in the past only few graduates enrolled in university later than three years after graduation.

***Timing of enrollment:*** Our measure for the timing of enrollment as defined in Equation (3.2) involves some degree of arbitrariness with respect to the student population that we look at (denominator) as well as the timing of enrollment (numerator). Hence, Table 3.5.4 shows the effect of the reform if we use alternative definitions of *timing of enrollment*.

Table 3.5.4: Alternative definitions for the timing of enrollment

| | Conditional on enrollment... | | |
|---|---|---|---|
| | within 1 year (1) | within 2 years (2) | within 3 years (3) |
| **Panel A: Share of students who enroll in the same year** | | | |
| G8 reform | -0.068*** | -0.065*** | -0.032* |
| | (0.014) | (0.017) | (0.015) |
| $N_{state*cohort}$ | 180 | 165 | 150 |
| $N_{individuals}$ | 1,987,444 | 1,921,285 | 1,797,470 |
| **Panel B: Share of students who enroll within one year** | | | |
| G8 reform | | -0.015*** | -0.012 |
| | | (0.004) | (0.008) |
| $N_{state*cohort}$ | | 165 | 150 |
| $N_{individuals}$ | | 1,921,285 | 1,797,470 |

*Notes:* This table reports estimates of the G8 reform for alternative definitions of the timing of enrollment. The column headers indicate the sample and, hence, refer to the denominator of Eq. (3.2), while the panels refer to the numerator of Eq. (3.2). The upper left coefficient, for instance, refers to the effect of the G8 reform on the timing of enrollment, measured as the share of students who enroll in the year of graduation among all students who enroll within one year after graduation. Similarly, the lower right coefficient refers to the share of students who enroll within one year among all those who enroll within three years after graduation. All estimates are based on our main specification as outlined in Eq. (3.4). In line with controlling for the last G9 cohort in column 1, in column 2 (3) we additionally control for the cohorts two (and three) years before the double cohort in order to consider the disincentive for these cohorts to enroll together with the double cohort. Standard errors are clustered at the state level. * $p$<0.1, ** $p$<0.05, *** $p$<0.01.

First, we only change the denominator and look at students who enroll within 2 and 3 years after graduation (instead of within 1 year, as in our main definition). Extending the time period between high school graduation and university enrollment, does not change our conclusion: The G8 reform significantly decreases the probability to enroll in the year of graduation (Panel A). Second, we additionally alter the numerator of our outcome measure and look at enrollment within one year (instead of immediate enrollment, as in our main definition). Panel B in Table 3.5.4 shows that the timing of enrollment changes also at other margins. Among those who enroll within two years after graduation, the probability of enrolling within one year after graduation is significantly lowered by the reform. Thus, using alternative definitions, we substantiate our finding that the G8 reform induces students to delay their enrollment.

***Regular study progress:*** The decrease in regular study progress found in the previous section can be explained by (i) more students dropping out of university, (ii) more students changing their major and (iii) more students formally requesting a temporary interruption of their studies. In order to separate these three reasons, we generate three new outcome variables. Similar to the main definition of *regular study progress*, these three outcomes refer to all students who enrolled within one year after graduation. Table 3.5.5 shows that the probability to drop out of university increases by about one percentage point (column 1). While this effect may appear rather small, it corresponds to an increase of 14%. We also find evidence that the reform increases the likelihood of students changing their major by 1.6 percentage points (or 15%). The effect on study interruptions is negligible and insignificant. These results suggest that affected students are less certain about their choices and consequently more likely to adjust their decisions than students before the reform. Table 3.5.5 also shows that the decrease in regular study progress is mainly driven by an increased probability of students changing their major; this effect accounts for about 62% of the overall decrease in regular progress, while 37% can be attributed to an increase in dropout rates.

Table 3.5.5: Decomposing the effect on regular study progress

| | Regular study progress (1) | Drop out (2) | Changing major (3) | Interruption (4) |
|---|---|---|---|---|
| G8 reform | -0.026*** | 0.010** | 0.016** | 0.000 |
| | (0.009) | (0.004) | (0.007) | (0.001) |
| $N_{state*cohort}$ | 165 | 165 | 165 | 165 |
| $N_{individuals}$ | 1,656,629 | 1,656,629 | 1,656,629 | 1,656,629 |

*Notes:* This table reports the G8 reform effects on different outcomes as indicated by the column headers. All estimates are based on our main specification as outlined in Eq. (3.4). Standard errors are clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

## 3.6 Robustness

This section conducts various robustness checks verifying a causal interpretation of our results and explores the sensitivity of our results to different specifications.

The key identifying assumption for a causal interpretation of our results is the existence of parallel outcome trends in G8 and G9 states in the absence of the G8 reform. As the common trend assumption cannot be tested directly, we examine whether the trends in outcomes differed before the treatment by means of placebo regressions. In the specifications of Table 3.6.6, we assume that the reform took place two, three or four years before its actual date, and include one additional regressor per column that picks up the effect of the respective placebo policy. The results of our placebo regressions strongly support a causal interpretation of the G8 reform effects; all placebo reform indicators are insignificant and close to zero.

Table 3.6.6: Placebo tests

| | Placebo reform in... | | |
|---|---|---|---|
| | t-2 | t-3 | t-4 |
| | (1) | (2) | (3) |
| **Panel A: Enrollment within one year** | | | |
| Placebo effect | -0.008 | -0.003 | 0.004 |
| | (0.007) | (0.007) | (0.010) |
| $N_{state*cohort}$ | 180 | 180 | 180 |
| $N_{individuals}$ | 2,601,880 | 2,601,880 | 2,601,880 |
| **Panel B: Speed of enrollment** | | | |
| Placebo effect | 0.007 | 0.006 | 0.003 |
| | (0.008) | (0.008) | (0.009) |
| $N_{state*cohort}$ | 180 | 180 | 180 |
| $N_{individuals}$ | 1,987,444 | 1,987,444 | 1,987,444 |
| **Panel C: Regular study progress** | | | |
| Placebo effect | -0.003 | -0.006 | -0.008 |
| | (0.005) | (0.004) | (0.005) |
| $N_{state*cohort}$ | 165 | 165 | 165 |
| $N_{individuals}$ | 1,656,629 | 1,656,629 | 1,656,629 |

*Notes:* This table reports various placebo tests for the G8 reform effects on different outcomes as indicated by Panel A-C. All estimates are based on our main specification as outlined in Eq. (3.4) and additionally include one further regressor per column that picks up the effect of the respective placebo policy. Standard errors are clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

The common trend assumption would also be violated, if the timing of the reform implementation was correlated with other factors that are related to the outcomes we investigate. States that implemented the reform early, thus contributing relatively more to our findings, may have been on different trajectories regarding our outcomes than those states implementing the reform later or those states that did not experience any changes. When researching states' decisions on when to implement the reform, we found no evidence that these decisions are related to the outcomes we investigate: Saxony-Anhalt and Mecklenburg-Vorpommern, two eastern German states, were the first to implement the reform. They were already familiar with G8 as they had a G8 system in the 1990s and before reunification. Saarland, the third to implement the reform, is a rather small state on the French border with close links to France. Here, policy-makers were eager to quickly implement the G8 reform as they saw their graduates at a disadvantage compared to the French graduates, who graduated one year earlier. While 2012 is the year with the most double graduation cohorts (4 states), it was reasonable for the most populous state, North Rhine-Westphalia, to have its double graduation cohort one year later. In order to refute any related concerns, in Table 3.6.7 we relax the common trend assumption and allow for state-specific linear time trends (column 2). All effects remain statistically significant and are of similar magnitude to those of our main specification.

Co-treatments, in the form of other policy reforms, are a related threat to the parallel trends assumption. Note that policy changes implemented at the federal level and common to all states (like the suspension of military service in 2011) are already taken into account by the time fixed effects. During our observation period, German states, however, implemented a set of other secondary schooling and university policies. These reforms were implemented at different points in time in different states and none of these policies are perfectly collinear to the G8 reform (for an overview of affected states and cohorts, see Table A3.1 in the Appendix). At the secondary school level, these policy reforms include the introduction of centralized school exit examinations, changes in the timing of secondary school tracking, as well as the reduction in subject choice during the last two years of academic high school. To account for these policy changes, in column (3) we include dummy variables for each of the three school policies. At the university level, policies that changed during the observation period include the introduction (and subsequent abolition) of tuition fees as well as the introduction of the two-tier degree system (introduction of

Bachelor's and Master's degrees) as part of the Bologna reforms. While the decisions about tuition fees were made at the state level, the decision when to switch to the two-tier degree system was left to universities, and even to departments within universities. We control for the existence of tuition fees with a dummy variable. For the expansion of the two-tier degree system we include a continuous variable capturing the share of students enrolled in a Bachelor program, as opposed to other degree programs (Diploma, Magister, state examination), among all newly enrolled students at university.[18] Controlling for all these school and university policies in column (3) does not change our conclusions.

Another threat to our identification strategy relates to compositional changes in treatment and control states. As the G8 reform only affected academic high schools, the composition of treated and untreated students might change as students try to evade the reform. This could happen in several ways. First, students could move to a different state that has not yet implemented the reform. Second, academic high school students might switch to a lower secondary school track that is unaffected by the G8 reform. Third, students might switch to alternative school types that offer university entrance qualifications. In all three cases, fewer individuals would graduate from academic high schools. However, in an analysis using full population data, Huebener and Marcus (2017) find no effect of the reform on the number of graduates from academic high schools and we can confirm this finding for an extended time window (see Table A3.3 in the Appendix). Further, Dahmann and Anger (2014) do not find evidence for increased mobility of academic high school students between states, and Huebener, Kuger, and Marcus (2017) show that - based on observable student characteristics - the composition of students did not change due to the reform. Moving to a different state and/or switching to a different school type in order to avoid the reform might be easiest in the city states of Berlin, Bremen, and Hamburg due to the regional proximity of other states and the availability of

[18]While it is straightforward to link the school reforms to high school graduation cohorts, the task is slightly more complicated for the university reforms for two reasons. On the one hand, the state of high school graduation can differ from the state of university enrollment. On the other hand, students can enroll in university in different years after graduation. We decided to link the graduation cohorts with the current status of university policies in the state of high school graduation. This is reasonable because the majority of students enrolls in universities in the state of high school graduation (about 65%). With respect to the time dimension, we chose the current situation at universities, i.e. the status of these policies in the year of students' high school graduation but verified that the results are robust to using the situation in the year before high school graduation.

further schools types. Column (4) in Table 3.6.7 shows that our results do not change when we exclude these three states.

As pointed out by several other studies, economic conditions cannot only influence enrollment decisions but also students' decision to stay in education and continue their studies (e.g. Betts and McFarland, 1995; Bedard and Herman, 2008; Sievertsen, 2016). Thus, in column (5) we control for GDP growth, unemployment rate, and youth unemployment in the state and year of students' high school graduation. Changes in the state's economic condition could also be seen as a potential co-treatment. All our estimates are robust to controlling for these potential co-treatments. Similarly, as argued by Bound and Turner (2007) and Bruckmeier and Wigger (2014), cohort size, specifically, the number of students earning a university entrance qualification, may affect students' enrollment decisions. It may further affect study progress if students are unwilling to continue their studies in crowded lectures and study classes. Therefore, in column (6) of Table 3.6.7 we control for the log of the number of high school graduates from all school types in each state and year; again, our estimates remain unchanged.

There are several specific details of the reform's implementation that could potentially affect our estimates. First, a double graduation cohort in one state might influence students' enrollment decisions in neighboring states. In column (7) we consider these potential spill-over effects by additionally controlling for the existence of double graduation cohorts in neighboring states. Second, in general, students in the first G8 cohort knew that they would graduate after 8 instead of 9 years when they entered academic high school. However, in Saxony-Anhalt and Mecklenburg-Vorpommern students in the first G8 cohort were only informed about the shortening of the school duration, when they were in grade 9. Thus, this and the following two cohorts were surprised by the G8 reform and exposed to an even higher increase in weekly workload, which makes these cohorts quite distinct. In column (8) of Table 3.6.7 we control for the two cohorts after the double graduation cohort in Saxony-Anhalt and Mecklenburg-Vorpommern in order to rule out that our effects are driven by these cohorts. Third, there are four states that did not experience any change in the length of schooling during our observation period (see Figure 3.2.1). We exclude these four states from our estimation sample to examine if these results depend on specific trends in states that did not change treatment status (see column 9). None of these alternative model specifications change our estimates significantly.

Table 3.6.7: Robustness tests

| | Identification issues | | | | | | Reform issues | | | Specification issues | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Main | + state trends | + other reforms | w/o city states | + econ. controls | + cohort size | + DC in neighb. states | + surpr. cohorts | w/o never chang. | Same cohorts | w/o weights | Wild boot-strap |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| **Panel A: Enrollment within one year** | | | | | | | | | | | | |
| G8 reform | -0.060*** | -0.067*** | -0.068*** | -0.064*** | -0.067*** | -0.055*** | -0.060*** | -0.063*** | -0.048** | -0.084*** | -0.059*** | -0.060*** |
| | (0.017) | (0.019) | (0.016) | (0.015) | (0.011) | (0.015) | (0.016) | (0.018) | (0.021) | (0.022) | (0.013) | [0.000] |
| $N_{state*cohort}$ | 180 | 180 | 180 | 144 | 180 | 180 | 180 | 180 | 132 | 165 | 180 | 180 |
| $N_{individuals}$ | 2,601,880 | 2,601,880 | 2,601,880 | 2,395,741 | 2,601,880 | 2,601,880 | 2,601,880 | 2,601,880 | 2,171,543 | 2,343,454 | | 2,601,880 |
| **Panel B: Speed of enrollment** | | | | | | | | | | | | |
| G8 reform | -0.068*** | -0.076*** | -0.067*** | -0.065*** | -0.072*** | -0.062*** | -0.067*** | -0.073*** | -0.073*** | -0.056*** | -0.050*** | -0.068*** |
| | (0.014) | (0.018) | (0.011) | (0.015) | (0.011) | (0.012) | (0.014) | (0.014) | (0.019) | (0.018) | (0.014) | [0.000] |
| $N_{state*cohort}$ | 180 | 180 | 180 | 144 | 180 | 180 | 180 | 180 | 132 | 165 | 180 | 180 |
| $N_{individuals}$ | 1,987,444 | 1,987,444 | 1,987,444 | 1,834,256 | 1,987,444 | 1,987,444 | 1,987,444 | 1,987,444 | 1,668,024 | 1,656,629 | | 1,987,444 |
| **Panel C: Regular study progress** | | | | | | | | | | | | |
| G8 reform | -0.026*** | -0.018* | -0.032*** | -0.029*** | -0.023** | -0.027*** | -0.026*** | -0.025** | -0.025** | -0.026*** | -0.033*** | -0.026*** |
| | (0.009) | (0.010) | (0.006) | (0.009) | (0.008) | (0.008) | (0.008) | (0.010) | (0.010) | (0.009) | (0.008) | [0.012] |
| $N_{state*cohort}$ | 165 | 165 | 165 | 132 | 165 | 165 | 165 | 165 | 121 | 165 | 165 | 165 |
| $N_{individuals}$ | 1,656,629 | 1,656,629 | 1,656,629 | 1,528,816 | 1,656,629 | 1,656,629 | 1,656,629 | 1,656,629 | 1,393,480 | 1,656,629 | | 1,656,629 |

*Notes:* This table reports various robustness tests of the G8 reform effects on different outcomes as indicated by Panel A-C. All estimates are based on our main specification as outlined in Eq. (3.4). Standard errors are clustered at the state level for columns (1)-(11) and presented in parentheses, the brackets in column (12) present $p$-values based on wild cluster bootstrapping (1000 replications, Mammen weights, testing under $H_0$). * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

The last three columns of Table 3.6.7 deal with various specification issues. Column (10) shows that our results for the first two outcomes are insensitive to using the same cohorts as for the last outcome. As there is a discussion about the appropriateness of weighting in difference-in-differences settings, we also estimate a specification without weighting (see column 11). Furthermore, column (12) shows that our conclusions do not change when applying wild-cluster bootstrapping (1000 replications, Mammen weights, testing under $H_0$) as an alternative method of inference.

Overall, the results of our robustness analysis as presented in Tables 3.6.6 and 3.6.7 support a causal interpretation of our effects.

## 3.7 Heterogeneity of the treatment effect

All results described in the previous sections represent average treatment effects. To investigate whether these average effects mask relevant differences, this section examines treatment effect heterogeneity across time, federal state, and gender.

### 3.7.1 Heterogeneity over time

It is important for researchers and policy-makers alike to analyze whether the estimated reform effects are only temporary or lasting. As the reform is relatively new, we cannot look at a long post-treatment horizon. Nevertheless, we can examine the size of the treatment effect for several cohorts after the reform implementation.[19]

Table 3.7.8 displays the results of our main specification, in which we substitute the single G8 indicator by a set of binary variables capturing the reform's effect for cohorts 1, 2, 3, and 4 or more years after the reform implementation (i.e. after the double graduation cohort). With respect to *enrollment rate*, there is no clear pattern of the treatment effect over time (see column 1). The effect for the first cohort after the implementation is of similar magnitude to the overall effect. The effect for the second cohort after the implementation is larger, while the effect for the third cohort is smaller. However, the effect for the cohorts four or more years after the double graduation cohort is similar to the effect after one year. Thus, there is little evidence that the reform's effect on *enrollment rate* is fading over time. Further,

[19]We choose to look at the effect up to four years after the double graduation cohort, so that there are always at least two states in the treatment group.

we demonstrate the validity of our approach by comparing these point estimates to effects in the cohorts before the double cohort. Column (2) shows that the effects for the cohorts 2-4 years before the reform are statistically insignificant, which is in line with the placebo regressions in Table 3.6.6. Further, the magnitude of these estimates is close to zero and clearly smaller than the effects for the G8 cohorts. The coefficients for the last cohort before the double cohort and the double cohort are significant, as before, indicating that both cohorts are affected by the G8 reform.

Table 3.7.8: Dynamics of the treatment effect

| | Enrollment | | Speed | | Study progress | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| 4 years prior | | 0.009 | | 0.001 | | -0.007 |
| | | (0.009) | | (0.008) | | (0.006) |
| 3 years prior | | -0.004 | | 0.005 | | -0.002 |
| | | (0.003) | | (0.005) | | (0.004) |
| 2 years prior | | -0.008 | | 0.005 | | -0.000 |
| | | (0.006) | | (0.007) | | (0.005) |
| Last G9 | -0.015* | -0.011** | 0.070*** | 0.066*** | -0.007* | -0.005* |
| | (0.007) | (0.005) | (0.007) | (0.006) | (0.003) | (0.003) |
| Double cohort | -0.085*** | -0.081*** | -0.013 | -0.017* | -0.013* | -0.012 |
| | (0.011) | (0.009) | (0.009) | (0.010) | (0.007) | (0.007) |
| 1 year after | -0.057*** | -0.053*** | -0.071*** | -0.075*** | -0.024** | -0.024** |
| | (0.016) | (0.013) | (0.013) | (0.013) | (0.010) | (0.010) |
| 2 years after | -0.071*** | -0.066*** | -0.067*** | -0.070*** | -0.029*** | -0.031*** |
| | (0.018) | (0.016) | (0.014) | (0.014) | (0.008) | (0.009) |
| 3 years after | -0.037 | -0.033 | -0.056** | -0.058** | -0.037*** | -0.039*** |
| | (0.022) | (0.021) | (0.025) | (0.024) | (0.006) | (0.007) |
| 4 or more years after | -0.051** | -0.047** | -0.022 | -0.023 | -0.039*** | -0.043*** |
| | (0.022) | (0.020) | (0.019) | (0.018) | (0.009) | (0.009) |
| $N_{state*cohort}$ | 180 | 180 | 180 | 180 | 165 | 165 |
| $N_{individuals}$ | 2,601,880 | 2,601,880 | 1,987,444 | 1,987,444 | 1,656,629 | 1,656,629 |

*Notes:* This table reports the G8 reform effects on different outcomes as indicated by the column header. All estimates are based on our main specification as outlined in Eq. (3.4), where we substitute the single G8 indicator by a set of binary variables capturing the reform's effect for cohorts before and after the reform implementation. Standard errors are clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

The development of the treatment effect appears to be different for *timing of enrollment* (see column 3). Here, each coefficient is smaller in absolute terms than the coefficient for the previous cohort indicating that the size of the reform's effect is declining over time. Again, there is no evidence that the outcome was trending before the last G9 cohort (column 4). With respect to *regular study progress* there is some evidence for an increase in the reform's effect over time (column 5) as the coefficients increase almost monotonously across cohorts (in absolute terms). Point estimates for the double cohort and the last G9 cohort are small but significant, while for earlier G9 cohorts coefficients are close to zero and insignificant (column 6).

Overall, Table 3.7.8 suggests that while the effect on the timing of enrollment may fade over a longer time period, the effects on enrollment rate and study progress seem to persist.[20]

### 3.7.2 Heterogeneity across states

In this subsection we differentiate the treatment effect by federal state in order to see whether specific states managed to implement the reform without negative consequences. For this purpose, we substitute the binary treatment indicator in Equation (3.4) with interactions between the treatment indicator and each treatment state.

Table 3.7.9 shows that the overall G8 effects are not driven by individual states. For *enrollment rate*, the treatment effect is negative and significant in the overwhelming majority of treatment states. These significant coefficients are close to the estimated overall reform effect of about 6 percentage points and vary between -3 and -8 percentage points. There seems to be no general pattern among the coefficients as these are similar for early and late adopters, for states in east and west Germany as well as for city states and other states.

A similar picture emerges for *timing of enrollment*. All coefficients are negative and nine are significantly different from zero. As for our first outcome, there is no general pattern across state characteristics. With respect to *study progress*, all coefficients but one are again negative and significant.[21] Also for *study progress* we find little evidence for substantial state differences.

[20]Note that due to the differential timing of the reform implementation in states, a varying subset of treatment states identify the point estimates for the different post-treatment cohorts. Table A3.4 in the Appendix presents estimates based on a constant set of treatment states and confirms the patterns regarding the dynamics of the treatment effect. The results for *enrollment rate* are even more stable across cohorts.

[21]Due to the nature of this outcome variable, we have to rely on fewer graduation cohorts (see also Table 3.3.1). Therefore, we cannot display coefficients for states that implemented the G8 reform in 2012 or later.

All in all, the results in Table 3.7.9 demonstrate that the effects are rather homogeneous across states. Lower enrollment rates, delayed enrollment, and decreased regular study progress appear to be general consequences of the G8 reform.

Table 3.7.9: Heterogeneity by federal state

| | Enrollment (1) | Speed (2) | Study progress (3) |
|---|---|---|---|
| Saxony-Anhalt | -0.055*** | -0.010 | -0.030*** |
| | (0.016) | (0.006) | (0.003) |
| Mecklenburg | -0.057*** | -0.045*** | -0.040*** |
| | (0.014) | (0.009) | (0.004) |
| Saarland | -0.032* | -0.028** | -0.049*** |
| | (0.015) | (0.010) | (0.004) |
| Hamburg | -0.017 | -0.088*** | -0.015** |
| | (0.015) | (0.012) | (0.006) |
| Bavaria | -0.078*** | -0.095*** | -0.029*** |
| | (0.015) | (0.012) | (0.007) |
| Lower-Saxony | -0.082*** | -0.070*** | -0.005 |
| | (0.016) | (0.012) | (0.007) |
| Baden-Wuerttemberg | -0.054*** | -0.088*** | |
| | (0.015) | (0.011) | |
| Bremen | -0.082*** | -0.088*** | |
| | (0.015) | (0.011) | |
| Berlin | 0.030* | -0.060*** | |
| | (0.015) | (0.011) | |
| Brandenburg | -0.047*** | -0.031** | |
| | (0.016) | (0.011) | |
| $N_{state*cohort}$ | 180 | 180 | 165 |
| $N_{individuals}$ | 2,601,880 | 1,987,444 | 1,656,629 |

*Notes:* This table reports the G8 reform effects by federal state on the outcomes indicated by the column header. All estimates are based on our main specification as outlined in Eq. (3.4) where we substitute the G8 indicator by interaction terms between this indicator and each treatment state. Standard errors are clustered at the state level. * $p$<0.1, ** $p$<0.05, *** $p$<0.01.

### 3.7.3 Heterogeneity by gender

Previous research on the G8 reform finds evidence of gender specific differences in the reform effects (see e.g. Dahmann and Anger, 2014; Büttner and Thomsen, 2013; Huebener and Marcus, 2017; Meyer and Thomsen, 2016). In light of this evidence we examine treatment effect heterogeneity by gender in Table 3.7.10.

We can neither establish differential reform effects for *enrollment rates* nor for the *timing of enrollment*. Our estimates do not confirm the finding for the double cohort in Saxony-Anhalt suggesting that only females delay their enrollment (Meyer and Thomsen, 2016). One explanation for the differing results could be that (Meyer and Thomsen, 2016) exclusively analyze the double cohort. The findings of Morin (2015b) support this argument as he shows that females react more strongly to increased competition resulting from a larger cohort size.[22]. This again highlights the importance of examining cohorts other than the double cohort, when evaluating the overall consequences of the G8 reform. For *regular study progress* the point estimate for males is higher (in absolute terms) than for females, although these two estimates do not differ significantly. Generally, the results do not suggest that males and females are differently affected by the G8 reform.

Table 3.7.10: Heterogeneity by gender

| | Female (1) | Male (2) |
|---|---|---|
| **Panel A: Enrollment within one year** | | |
| G8 reform | -0.061*** | -0.058** |
| | (0.015) | (0.020) |
| $N_{state*cohort}$ | 180 | 180 |
| $N_{individuals}$ | 1,452,630 | 1,149,250 |
| **Panel B: Speed of enrollment** | | |
| G8 reform | -0.063*** | -0.069** |
| | (0.013) | (0.026) |
| $N_{state*cohort}$ | 180 | 180 |
| $N_{individuals}$ | 1,069,225 | 918,219 |
| **Panel C: Regular study progress** | | |
| G8 reform | -0.020** | -0.034*** |
| | (0.009) | (0.011) |
| $N_{state*cohort}$ | 165 | 165 |
| $N_{individuals}$ | 894,127 | 762,502 |

*Notes:* This table reports the G8 reform effects on different outcomes as indicated by Panel A-C separately by gender. All estimates are based on our main specification as outlined in Eq. (3.4). Standard errors are clustered at the state level. * $p$<0.1, ** $p$<0.05, *** $p$<0.01.

Taken together, this section finds little evidence for differential treatment effects across time, state or gender. This underlines the general nature of our results. Regarding the external validity of our findings, it is likely that the G8 reform will

[22]Morin (2015b) looks at a double graduation cohort resulting from an education reform in Ontario, Canada, which shortened high school by one year (without compensating increases in instruction time in the remaining school years)

have similar consequences in the states that have implemented the reform outside of our sample period.

## 3.8 Channels

This section explores various mechanisms that may explain our results. We first discuss arguments concerning the supply of university slots, before we turn to demand-side arguments.

One mechanism that could explain the decrease in enrollment rates are supply-side restrictions, i.e. a shortage of university slots. However, this would mainly apply to students in the double graduation cohort, which is roughly double in size and which we excluded from the treatment group. Nevertheless, if universities are unable to provide sufficient places for the double graduation cohort this might have spillover effects on the enrollment decision of subsequent G8 cohorts. If resources were not adequately increased, subsequent G8 students may face more difficulties in being admitted to university since students from the double cohort still queue to gain access to universities. A decrease in enrollment rates may correspondingly only mirror higher competition for study places instead of students' actual choices. However, several arguments suggest that supply-side restrictions are not the key mechanism explaining our results: First, to cover the demand shock induced by the double graduation cohort, the governments of the treated states as well as the federal government continuously increased university funding under the Higher Education Pact *(Hochschulpakt)*. In part, this funding was explicitly directed toward increasing university slots to accommodate the double graduation cohort. Second, if there was a shortage of university slots, universities would have to tighten their admission policies. Consequently, the share of (locally) restricted study programs should increase, i.e. programs that use a cut-off based on the final high school grade points average to select students for admission (*numerus clausus*).[23] However, Table 3.8.11 shows that the share of restricted study programs does not significantly increase due to the reform, irrespective of whether we only look at Bachelor's programs (column 1)

[23]Unlike in other countries, admission is only *centrally* restricted for few programs. Generally, universities only set *local* admission restrictions if the number of applications exceeds available slots. This implies that cut-offs are determined retrospectively.

or also at other first degree programs like state examination (column 2).[24] Hence, there is no evidence that students affected by the reform faced higher competition with respect to being admitted to university. Third, if supply-side restrictions drive the results, we should see students circumventing these restrictions by studying in a different state, one that does not have a double cohort in the same year. Yet, our estimates in column (3) of Table 3.8.11 do not suggest a decline in the share of students who study in their home state; if at all, we even find G8 students to be slightly more likely to enroll in their home state. For these reasons, we conclude that supply-side restrictions are unlikely to be the main explanation for the decrease in enrollment rates.

Table 3.8.11: Supply-side restrictions

| | All locally restricted Bachelor programs (1) | All locally restricted first degree programs (2) | Enrollment in home state (3) |
|---|---|---|---|
| G8 reform | 0.027 | 0.044 | 0.019 |
| | (0.056) | (0.037) | (0.015) |
| $N_{state*cohort}$ | 144 | 144 | 180 |
| $N_{individuals}$ | | | 1,987,444 |

*Notes:* This table reports G8 reform effect on different outcomes as indicated by the column header. All estimates are based on our main specification as outlined in Eq. (3.4). Standard errors are clustered at the state level. Information on the share of locally restricted Bachelor programs as well as on the share of all first degree programs is only available from 2006 onwards and provided by the German Rectors' Conference (2006-2014) *(Hochschulrektorenkonferenz)*. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

We now turn to demand-side arguments. The G8 reform can be thought of as consisting of two parts: First, the reduction of the length of academic high school, which makes students one year younger at graduation (*age channel*). And second, the compensating increase in instruction hours in the remaining years, which resulted in a higher weekly workload (*workload channel*). Recall that while the *age channel* includes less time for orientation and the younger age at graduation, the *workload channel* comprises decreased performance at school, higher levels of stress and lower motivation for further learning.

It is difficult to determine whether our findings are driven by the *age channel* or by the *workload channel* as there is little independent variation between the two channels. Nevertheless, in the following we provide some suggestive evidence. We proceed by first estimating the reform's effect on students' age at university

[24]For this specification, we estimate a model in the style of Equation (3.4), in which we use the share of all restricted Bachelor's programs as well as the share of all restricted first degree programs as an outcome.

enrollment and then examine whether the reform effects persist when we try to keep students' age constant. In this specification, if the G8 effect is close to zero and insignificant, our findings can mostly be attributed to the age channel. If, on the other hand, we also find a significant effect of the G8 reform on similar aged students, this provides some evidence that the age channel seems to play a minor role.

Focusing on students who enroll within one year after graduation, as in our main specification, Table 3.8.12 shows that the reform successfully decreased students' age at enrollment (see column 1), although only by eight and a half months (0.73 years), compared to a potential reduction of a full year.[25] Having established the age effect of the reform, we try to hold students' age constant by looking at G8 and G9 students who graduated from academic high schools at the age of 19.[26] These students are of similar age, but experienced different amounts of weekly workload due to the reform. For all three outcome variables, holding students' age constant, the G8 reform indicator is still significant (columns 2-4). For *timing of enrollment* and *regular study progress* the effect is also similar in magnitude as in our main specification, while for *enrollment rate* the reform's effect is even larger. The results in Table 3.8.12 suggest that the reform's main mechanism does not run through the reduced age of students and that our findings are instead driven by higher workload.

The estimations in Table 3.8.12 may, however, be flawed by potential relative age effects: By analyzing similar aged G8 and G9 students, we compare G9 students who are relatively younger with respect to their graduation cohort with G8 students who are relatively older within their cohort. However, the literature on relative age effects in school suggests advantages for relatively older students (see e.g. Bedard and Dhuey, 2006; Mühlenweg and Puhani, 2010). If relatively older students perform better, we might also expect higher enrollment rates, faster enrollment and a higher probability of regular study progress for the relatively older G8 students

[25] Our results can be compared to findings in Huebener and Marcus (2017), who show that the G8 reform reduced the age at graduation by 0.86 years. This highlights that the difference between our point estimates and a reduction by a full year (i.e. a coefficient of "-1") results from two factors: Firstly, already at the time of graduation the reform did not achieve its full potential in terms of age reduction; secondly, graduates delayed their enrollment, as shown in the previous sections.

[26] According to the school entry regulations, posting the cut-off date for school entry at June 30th, we compare G9 students who are born between January and June with G8 students who are born between July and December. Note that for the denominator of enrollment rates (as defined in Equation 3.1) the information on the exact birth date is not available; thus, we have to assume that all 19-year-old high school graduates entered school according to school entry regulations.

Table 3.8.12: Examining the age channel

| | All | Only 19-year-olds | | |
|---|---|---|---|---|
| | Age at enrollment (1) | Enrollment (2) | Speed (3) | Study progress (4) |
| G8 reform | -0.725*** | -0.131*** | -0.063*** | -0.031* |
| | (0.070) | (0.019) | (0.017) | (0.010) |
| $N_{state*cohort}$ | 180 | 168 | 180 | 165 |
| $N_{individuals}$ | 1,987,444 | 1,027,614 | 617,703 | 519,762 |

*Notes:* This table displays the effect of the G8 reform on different outcomes as indicated by the column header. In column 1 we look at the age at enrollment for students who enrolled within one year after graduation. In columns 2-4 we only consider students who graduated from high school at the age of 19. All estimates are based on our main specification as outlined in Eq. (3.4). Note that for three states the age-specific number of academic high school graduates is not available for the cohorts between 2002-2005; thus, the number of observations differs in the 2nd column. Standard errors are clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

(as compared to the relatively younger G9 students). As we compare older G8 students with younger G9 students, the aforementioned arguments would rather bias our estimates toward zero. Yet, the G8 effects in Table 3.8.12 are not smaller than the estimates in our main specification, suggesting that relative age effects do not present a major concern for these estimates.

To sum up, we find little evidence that supply-side restrictions are the main channel that drives our results. We also find little support for the claim that the G8 reform primarily works through the reduced age of students. Hence, the obtained reform impact mainly operates through the higher workload during school.

## 3.9 Conclusion

We examine whether it is possible to reduce the length of secondary schooling – thereby allowing for an earlier labor market entry – without negatively affecting university enrollment. If it was possible to learn the same amount of material in a shorter period of time, this would mark an efficiency gain for the individual student. This efficiency gain would not only benefit the individual in terms of increased lifetime earnings but it would also come with benefits for the general public in terms of higher tax revenues and – most importantly – a longer working life, which could help in coping with challenges aging societies face. Against the backdrop of existing evidence on the negative effects of simple reductions in the years of schooling, a novel policy in Germany bears the potential to decrease the length of schooling without affecting students' human capital. This policy reduced the length of the academic high school by one year, but increased weekly instruction hours in the remaining school years in order to fully compensate for the omitted year.

We examine the medium-term consequences of this recent policy change for higher education decisions. We apply a difference-in-differences approach exploiting the variation in reform implementation over time and across states. Using administrative data from the German student register, which covers all students in Germany, we provide evidence for adverse consequences of this policy change: Students are less likely to enroll in university, more likely to delay their enrollment, and less likely to have regular study progress. For an illustration of the magnitude of the obtained effect sizes consider the following calculations: 213,000 students graduated from academic high schools in 2014 in the twelve treatment states. Taking our point estimates at face value and assuming effect homogeneity across states and cohorts our results suggest that due to the reform more than 12,000 students of the 2014 graduation cohort did not enroll in university; additionally, almost 11,000 students delayed their enrollment by one year, and about 4,000 students did not have regular study progress during their first year at university.

Further, we show that these reform impacts are quite general in nature. The effects are similar across states and gender, and they do not seem to be only short-lived implementation effects. An investigation of potential channels of the reform suggests that our findings are driven neither by supply-side restrictions nor by the reduction in students' age. Consequently, we argue that the main channel works through the higher workload at school.

Increasing education efficiency by reducing the years of schooling while increasing weekly instruction hours sounds like a tempting policy option. However, our empirical evidence shows that even this kind of policy might not come without unintended consequences regarding students' higher education decisions. Lower enrollment rates at universities and higher dropout rates may lower a country's human capital stock and, ultimately, economic prosperity. Additionally, by delaying the enrollment decision and by changing majors more often, students lose some of their initial age advantage, thereby counteracting the reform's main goal of earlier labor market entry. Overall, our study suggests that this reform cannot fully eliminate the trade-off between an earlier labor market entry and constant levels of human capital.

# Appendix: Additional tables

Table A3.1: Implementation of G8 and other education reforms in the federal states

| | School policies | | | | University policies |
|---|---|---|---|---|---|
| | G8 | Central exit examination | Tracking in grade 7 | Restricted upper-secondary subject choice | Tuition fees |
| | | **Change from G9 to G8** | | | |
| Saxony-Anhalt | from 2007 | all | 2006-2009 | from 2005 | none |
| Mecklenburg-Vorpommern | from 2008 | all | none | from 2008 | none |
| Saarland | from 2009 | all | none | from 2010 | 2007-2009 |
| Hamburg | from 2010 | all | none | from 2011 | 2007-2011 |
| Bavaria | from 2011 | all | none | from 2011 | 2007-2012 |
| Lower-Saxony | from 2011 | from 2006 | until 2011 | from 2008 | 2006-2013 |
| Baden-WÃ¼rttemberg | from 2012 | all | none | from 2004 | 2007-2011 |
| Bremen | from 2012 | from 2007 | until 2011 | all | none |
| Berlin | from 2012 | from 2007 | all | all | none |
| Brandenburg | from 2012 | from 2005 | all | none | none |
| North Rhine-Westphalia | from 2013 | from 2007 | none | all | 2007-2010 |
| | | **Always G8** | | | |
| Saxony | all | all | none | from 2010 | none |
| Thuringia | all | all | none | from 2011 | none |
| | | **Always G9 (in observation period)** | | | |
| Rhineland-Palatinate | none | all | none | from 2011 | none |
| Schleswig-Holstein | from 2016 | from 2008 | none | from 2011 | none |
| | | **Excluded from estimation sample** | | | |
| Hesse | from 2012 | from 2007 | none | from 2005 | 2007 |

*Notes:* This table informs about changes in education policies during our sample period. For school policies, numbers refer to the affected graduation cohort while for university policies numbers refer to years. *G8* indicates the year of the double graduation cohort. *Centralized school exit examinations* shift the design of exit exams from high schools to federal state institutions such that all students in the specific state sit the same exit exam. *Tracking in grade 7* indicates whether tracking takes place in grade 7 (or earlier). *Restricted upper secondary subject choice* indicates graduation cohorts for whom the set of subject choices for the final two years at academic high schools has been restricted. *Tuition fees* indicates the years in which tuition fees (about 500 Euro per semester) were charged. Sources for the reform dates are available from the authors upon request.

Table A3.2: Estimating the G8 effect within the double cohort

| | Enrollment | | Speed | | Study progress | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| G8 reform | -0.060*** | -0.060*** | -0.068*** | -0.068*** | -0.026*** | -0.026*** |
| | (0.017) | (0.017) | (0.014) | (0.014) | (0.009) | (0.009) |
| Last G9 | -0.016** | -0.016** | 0.069*** | 0.069*** | -0.007* | -0.007* |
| | (0.007) | (0.007) | (0.007) | (0.007) | (0.003) | (0.003) |
| DC | -0.085*** | | -0.015 | | -0.013* | |
| | (0.011) | | (0.009) | | (0.007) | |
| G8 in DC | | -0.103*** | | -0.051*** | | -0.001 |
| | | (0.017) | | (0.008) | | (0.006) |
| G9 in DC | | -0.071*** | | 0.015 | | -0.022** |
| | | (0.010) | | (0.011) | | (0.008) |
| $N_{state*cohort}$ | 180 | 191 | 180 | 191 | 165 | 175 |
| $N_{individuals}$ | 2,601,880 | 2,601,880 | 1,987,444 | 1,987,444 | 1,656,629 | 1,656,629 |

*Notes:* In this estimation we aim to disentangle the overall double cohort effect into an effect for the cohort's G8 and G9 students. As the exact treatment status is unknown for students in the double cohort, we assign it based on birth information and school entry regulations. Children who turn six before (after) June 30th of a given year usually start school in that (the following) year. Thus, double cohort graduates, who are older than 19, or 19 and born before June 30th, are assumed to be G9 students; likewise, graduates, who are younger than 19, or 19 and born after June 30th, are assumed to be G8 students. Note that the computation of separate enrollment rates within the double cohort requires separate graduation numbers for a cohorts' G8 and G9 students. Two states lack this information. For these two states we assume that the ratio of G8 and G9 students within the double cohort is the same as in the other eleven treatment states, which provide the relevant information. All estimates are based on our preferred specification and include state and time fixed effects. Standard errors are clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A3.3: Effect of the G8 reform on the number of graduates

| | no. of graduates normalized with no. of .... | | | log of |
|---|---|---|---|---|
| | 18-20 year olds<br>(1) | 18-19 year olds<br>(2) | 19 year olds<br>(3) | no. of graduates<br>(4) |
| G8 reform | -0.008<br>(0.014) | -0.000<br>(0.013) | -0.006<br>(0.014) | -0.072<br>(0.231) |
| $N_{state*cohort}$ | 195 | 195 | 195 | 195 |

*Notes:* The table reports the effect of the G8 reform on graduation rates with different normalisations. Columns (1)-(3) normalise the number of graduates from academic high schools by the average size of the populations of 18-20, 18-19 and 19 year olds, respectively. Column (4) takes the log of the number of graduates instead. All estimates are based on our main specification as outlined in Eq. (3.4) and rely on on the 2002-2014 graduation cohorts. Standard errors are clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A3.4: Dynamics of the treatment effect with different sample restrictions

| Only states that we observe for $X$ years after | X=1 (1) | X=2 (2) | X=3 (3) | X=4 (4) |
|---|---|---|---|---|
| **Panel A: Enrollment within one year** | | | | |
| G8 · 1 year after | -0.029*** | -0.053*** | -0.029* | -0.033* |
| | (0.008) | (0.014) | (0.013) | (0.017) |
| G8 · 2 years after | | -0.060*** | -0.033** | -0.048*** |
| | | (0.014) | (0.013) | (0.014) |
| G8 · 3 years after | | | -0.022 | -0.042** |
| | | | (0.015) | (0.018) |
| G8 · 4 years after | | | | -0.043** |
| | | | | (0.019) |
| $N_{state*cohort}$ | 179 | 171 | 165 | 161 |
| $N_{individuals}$ | 2,499,260 | 2,366,357 | 2,137,760 | 2,111,021 |
| **Panel B: Speed of enrollment** | | | | |
| G8 · 1 year after | -0.046*** | -0.073*** | -0.035 | -0.012 |
| | (0.007) | (0.017) | (0.023) | (0.011) |
| G8 · 2 years after | | -0.050*** | -0.040*** | -0.030** |
| | | (0.011) | (0.013) | (0.011) |
| G8 · 3 years after | | | -0.047** | -0.026* |
| | | | (0.021) | (0.012) |
| G8 · 4 years after | | | | -0.020 |
| | | | | (0.015) |
| $N_{state*cohort}$ | 179 | 171 | 165 | 161 |
| $N_{individuals}$ | 1,911,969 | 1,804,061 | 1,628,383 | 1,609,000 |
| **Panel C: Regular study progress** | | | | |
| G8 · 1 year after | -0.014* | -0.025*** | -0.034*** | -0.031*** |
| | (0.007) | (0.006) | (0.007) | (0.005) |
| G8 · 2 years after | | -0.021** | -0.034*** | -0.030*** |
| | | (0.008) | (0.007) | (0.004) |
| G8 · 3 years after | | | -0.032*** | -0.032*** |
| | | | (0.005) | (0.006) |
| G8 · 4 years after | | | | -0.032*** |
| | | | | (0.010) |
| $N_{state*cohort}$ | 165 | 161 | 157 | 154 |
| $N_{individuals}$ | 1,594,043 | 1,474,603 | 1,460,538 | 1,449,206 |

*Notes:* This table reports the G8 reform effects on different outcomes as indicated by Panel A-C. All estimates are based on our main specification as outlined in Eq. (3.4), where we substitute the single G8 indicator by a set of binary variables capturing the reform's effect for cohorts 1, 2, 3, and 4 or more years after the reform implementation. All treatment observations beyond the time frame indicated by the column header are excluded from the estimation. Standard errors are clustered at the state level. * $p$<0.1, ** $p$<0.05, *** $p$<0.01.

Chapter 4

# The Gender Gap in Wage Expectations: Do Females Trade Off Higher Earnings for Lower Earnings Risk?*

## 4.1 Introduction

The gender wage gap remains a highly disputed topic in labor economics as well as in politics. Numerous articles investigate the extent, development, and potential explanations of this gap (for a recent overview see Blau and Kahn, 2016). In Germany the raw gender wage gap amounts to 22%. Compared to other European countries, the extent of the gender gap is remarkably high (e.g. Finke, Dumpert, and Beck, 2017).

Several studies show that this gap is not only prevalent based on actual labor market earnings, but that females start with lower earnings expectations even before entering the labor market (Blau and Ferber, 1991; Filippin and Ichino, 2005; Zafar, 2013; Reuben, Wiswall, and Zafar, 2015). These early gender differences in earnings expectations can be particularly detrimental as they form the ground on which individuals base their decisions on. The gender gap in expected earnings may translate into the actual gender wage gap through at least two channels.[1] First, based on human capital theory, lower expected earnings reduce the incentives to invest in education. Existing evidence shows that expected earnings are a significant predictor for the choice which level of education to pursue (Hartog, Ding, and Liao, 2012; Schweri and Hartog, 2015; Belfield, Boneva, Rauh, and Shaw, 2016; Attanasio and Kaufmann, 2014, 2017) as well as for college major choice (Zafar, 2013;

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[1]Note that throughout this study, I use the terms "earnings" and "wages" interchangeably.

Arcidiacono, Hotz, Maurel, and Romano, 2014; Wiswall and Zafar, 2016a; Ruder and Noy, 2017). In addition, some studies show that changing students' perception of expected returns impacts their actual educational choices (Nguyen, 2008; Jensen, 2010; Wiswall and Zafar, 2015). Second, lower earnings expectations of females prior to labor market entry can become self-fulfilling. These expectations are likely to affect starting wages through the formation of reservation wages and lower starting wages have a persistent effect on future earnings trajectories.[2] More generally, females holding lower earnings expectations might be more likely to accept lower wage offers and less likely to negotiate for higher earnings because it matches their expectations. Hence, lower earnings expectations may directly result in lower actual earnings. In this regard, Caliendo, Lee, and Mahlstedt (2017) show that gender differences in reservation wages of unemployed individuals, can account for a large share of the subsequent gender gap in realized earnings.

Thus, analyzing the determinants of gender differences in earnings expectations early on may yield valuable insights on why women make different choices regarding education and careers, thereby enhancing our understanding of the gender gap in actual wages. An additional advantage of analyzing gender differences in earnings expectations instead of examining actual (realized) labor market earnings is that expectations abstract from labor market equilibrium effects and are not affected by unanticipated events that impact actual job choices.

While several studies document a gender gap in earnings expectations, the empirical evidence on the determinants of this gap is scarce. This is the focus of the current paper. Building on the theoretical reasoning of compensating differentials, the aim of this study is to examine whether the gender gap in expected earnings can partly be explained by differences in expected earnings risk, as measured by the dispersion in expected earnings. The basic idea is straightforward: First, females are, on average, more risk averse than males (see e.g. Croson and Gneezy, 2009). Second, it has been shown that individuals, who are more risk averse, are more likely to sort into occupations that exhibit lower earnings risk, as captured by the variation in earnings (Bonin, Dohmen, Falk, Huffman, and Sunde, 2007). And third, due to compensating differentials there is a positive relationship between higher average earnings and higher earnings risk (e.g. Hartog and Vijverberg, 2007).

[2] Evidence of the dampening effect of lower starting wages is provided by Oreopoulos, von Wachter, and Heisz (2012). The authors show that entering the labor market during a recession has a long-lasting and partly permanent negative effect on earnings.

Combining these empirical findings suggests that females sort into occupations with low earnings risk, which – due to compensating differentials – pay less. If individuals anticipate this form of compensation for earnings risk, this may explain why females expect to earn less than males even before entering the labor market.

In addition, it is precisely the concept of compensating differentials around which many explanations for the actual gender wage gap are built. Gender differences with respect to preferences make some jobs more, and others less, attractive to females employees (Bertrand, 2011). For example, females may value flexibility in working hours more than males, and since providing flexibility in working hours is costly to the firm, it is only offered in exchange for lower pay. In this regard, while not the primary focus of their analysis, Wiswall and Zafar (2016b) confront students from New York University with multiple hypothetical job choice scenarios that varied in expected earnings and other job characteristics. Their results show that, among others, females are willing to pay six times more in terms of expected earnings for a higher flexibility in working hours or more secure jobs than males. More generally, it is assumed that females are willing to trade off higher earnings for other appreciated job attributes. Earnings risk may constitute one such job attribute.

If females do indeed deliberately choose to trade off higher earnings for lower earnings risk, we should be able to uncover this relationship based on individuals *ex ante* earnings expectations, i.e. before they enter the labor market and self-select into different industries and occupations. Providing evidence that females not only have lower earnings expectations but also expect lower earnings risk, would strengthen the argument of conscious self-selection that is otherwise solely based on the idea of revealed preferences that assumes we can correctly identify preferences by only examining observed labor market outcomes.

My analysis draws on a unique survey of German high school graduates, in which we elicited information on the entire distribution of students' expected earnings. This allows me to construct a measure for earnings risk based on the individual-specific dispersion of expected earnings. As such, I focus on earnings risk, as perceived by students when they are making their post-secondary educational choice. I perform an Oaxaca-Blinder decomposition of the gender gap in expected earnings and account for various other explanations as captured by a large set of standard and non-standard individual characteristics. By including expected earnings risk in the decomposition of the gender gap, the study simultaneously assesses whether educational choices might be driven by anticipated compensation for earnings risk. Moreover, by having data on students' expected earnings as well as on subsequent actual educational choices for a sub-sample of students, I can further analyze to what extent gender differences in expected earnings are related to gender differences in college major choice.

The results of this study show that females expect to earn considerably less than their male counterparts and, at the same time, expect lower earnings risk. In fact, gender differences in expected earnings risk explain about three-quarters of the gender gap in expected earnings. Moreover, a supplementary analysis shows that gender differences in expected earnings can account for almost a fifth of the gender differences in choosing a high paying college major. Overall, this study sheds light on the self-selection process into different educations and careers and suggests that females expect to trade off higher earnings for lower earnings risk.

This study is most closely related to the study by Reuben, Wiswall, and Zafar (2015). Based on a sample of New York University undergraduate students, the authors analyze students' earnings expectations, specifically focusing on gender differences. Their analysis documents a large gender gap in expected earnings. While part of the gap is due to gender differences in college major choice, the gap in expected earnings within a college major still amounts to around 20 percent. The authors further show that gender differences in experimentally measured preferences, i.e. overconfidence, competitiveness and risk aversion, explain around 20 percent of the gender gap in expected earnings. Unfortunately, their data set does not allow for an analysis of the mechanisms through which these preferences are related to students' earning expectations.

Another interesting aspect on the gender gap in expected earnings can be drawn from a study conducted by Filippin and Ichino (2005), who collected data on expected earnings from students at Bocconi University in Milan as well as actual earnings for Bocconi graduates. The authors show that the gender gap implied by students' expected earnings is close to the gender gap based on actual earnings of Bocconi graduates. This evidence underlines the close relation between expected and actual earnings. Moreover, the analysis of Filippin and Ichino (2005) and Filippin (2003) suggest that part of the gender gap in expected earnings is likely due to females expecting to be discriminated in the labor market. When Filippin and Ichino (2005) ask students about the different explanations for the actual gender wage gap, females are more likely than males to attribute the gap to expected discrimination, while more males report differences in characteristics between gender as an explanation. Filippin (2003) derive a model, showing that this form of anticipated discrimination, even if it is entirely unsubstantiated, can result in actual earnings differences.

Finally, there are two studies examining whether students anticipate compensation for earnings risk, i.e. expect a positive correlation between higher average earnings and higher earnings risk (Mazza and Hartog, 2011; Schweri, Hartog, and Wolter, 2011); however, with mixed results. Schweri, Hartog, and Wolter (2011) gathered information on the expected wage distribution of first year economics students in Berne, finding that higher expected average earnings are indeed significantly positively related to expected earnings risk. By contrast, using the same survey questions as Schweri, Hartog, and Wolter (2011) on Dutch high school students in their last year, Mazza and Hartog (2011) cannot find a similar relationship between expected earnings and earnings risk. Further, the authors are unable to provide an explanation for these contrasting results. Thus, additional evidence on this relationship is required. The current study determines whether the results of Schweri, Hartog, and Wolter (2011) can be replicated using a different sample and an alternative measure for earnings risk.[3]

On the methodological side, this study contributes to the growing literature eliciting individuals' earnings expectations to shed light on their decision making (Dominitz and Manski, 1996; Manski, 2004). The main advantage of analyzing expectation data is that we do not have to make any assumptions about how individuals form their expectations and which information they consider in the formation process. In addition, by analyzing earnings expectations before entering the labor market, the common concern of *ex post* rationalization is mitigated. Early studies in this strand of the literature focused on comparing students' expected earnings to actual earnings of individuals currently in the labor market (Betts, 1996; Wolter, 2000; Wolter and Zbinden, 2002; Huntington-Klein, 2015; Frick and Maihaus, 2016). These studies examine whether the assumption of reference group based expectations, as generally

[3]Both studies use the median version (see Section 4.2) to elicit students' expected earnings distribution and use the probability mass assigned to the outer tails of the distribution as a measure for the variance. They further analyze the effect of skewness of the expected earnings distribution as an additional measure for earnings uncertainty. By contrast, this study elicited the distribution of expected earnings based on another approach that is detailed in Section 4.2.

made in educational choice models, is suitable.[4] Relatedly, other studies investigate how accurate students' expectations are by comparing their expectations to actual realized earnings for the same student, i.e. they follow students into the labor market (Webbink and Hartog, 2004; Jerrim, 2015). Additionally, some studies investigate the determinants of earnings expectations and examine whether differences in expected earnings reflect heterogeneity that is comparable to heterogeneity based on actual labor market earnings (see e.g. Brunello, Lucifora, and Winter-Ebmer, 2004; Bonnard, Giret, and Mener, 2014; Diaz-Serrano, Hartog, Nilsson, van Ophem, and Yang, 2016).

The majority of studies in this strand of the literature focuses on expected average earnings, i.e. ask individuals about a point estimate of their earnings expectations. By contrast, the entire distribution of expected earnings is rarely elicited. This study contributes to the existing literature by not only analyzing expected earnings but broadening the analysis toward a perspective on gender differences while simultaneously considering the role of expected earnings risk. It further provides corroborating evidence that compensation for earnings risk, as established based on labor market earnings, is similarly reflected in students' earnings expectations.

The remainder of the paper is structured as follows. Section 4.2 introduces the data and provides detailed information on the measurement of earnings expectations as well as other covariates. In addition, this section provides descriptive evidence for gender differences in expected earnings and other observed characteristics. In Section 4.3 the empirical approach is outlined, while Section 4.4 presents the empirical results. This Section also investigates the sensitivity of the key finding to different specifications and closes with a discussion of an alternative hypothesis for the empirical observations, showing that this explanation is unlikely to apply. Finally, Section 4.5 concludes.

## 4.2 Data

The empirical analysis is based on data from the *Berliner Studienberechtigten Panel (Best Up)* survey. This survey followed students in Berlin from the end of their penultimate year in high school through two years after graduating from high school.

[4]According to this assumption, individuals observe outcomes of current labor market participants with whom they share certain characteristics to infer their own future outcomes (Manski, 1993a).

Students were surveyed five times over that period. The survey aimed at obtaining a sample of students who were pre-dominantly from non-academic family backgrounds. Thus, the selected schools are located in districts with a high share of low educated individuals (ISCED 0-2) and cover twenty percent of all upper secondary schools in Berlin. As such the sample is neither representative of the German nor Berlin student population. Except for the first survey, which was a paper and pencil survey conducted in schools, the subsequent surveys were administered on-line. Of the 1578 students surveyed in the first wave, 1105 participated in the second, 1034 in the third, 1005 in the fourth, and 972 in the fifth wave. The data includes detailed information on student characteristics, their educational aspirations, and their actual educational choices.[5]

### 4.2.1 Measuring expectations about future earnings

In order to elicit information on students' expected earnings distribution, the literature offers two main approaches. The first asks students about their expected *median* earnings and some probabilities to earn more/less than X% of the median (Schweri and Hartog, 2015; Mazza and Hartog, 2011; Wolter, 2000). In contrast, the second approach asks about the minimum and maximum expected earnings as well as additional information on the probability distribution (Guiso, Jappelli, and Pistaferri, 2002; Attanasio and Kaufmann, 2014). As it is disputable whether the concept of the *median* and the difference of it to the mean is fully understood by our target group, we implemented the second approach that is described in more detail below.[6]

The module on labor market expectations was included in wave three of the Best Up panel survey, i.e. just after students graduated from high school and were about to make their decision about their post-secondary education.

[5]In the context of the data collection two interventions (one information and one financial intervention) were conducted. One concern might be that students may have adjusted their expected earnings in response to the information intervention. However, I do not find that the information intervention had a significant effect on students' expected earnings. Nevertheless, throughout this study I include an indicator variable accounting for the information intervention. For further details on the *Berliner Studienberechtigten Panel (Best Up)* see Ehlert, Peter, Finger, Rusconi, Solga, Spieß, and Zambre (2017); for the financial intervention see Peter, Rusconi, Solga, and Spieß (2016); and for the information intervention see Peter and Zambre (2016), Peter, Rusconi, Solga, Spieß, and Zambre (2016), and Ehlert, Finger, Rusconi, and Solga (2017).

[6]The results of a small pre-test of students of similar age as the target group suggested that most students do not perceive a difference between the mean and the median.

For three different education scenarios, students are asked about the range of their individual earnings distribution and the probability mass to the right of the midpoint of this range. More specifically, we asked students to state their net minimum ($y_m$) and net maximum ($y_M$) earnings they expect to earn at the age of 35 conditional on working full time and having earned a) a vocational degree, b) a Bachelor's degree, and c) a Master's degree. We then asked students about the probability to earn more than the midpoint of their range, $p = Pr(Y > \frac{y_m+y_M}{2})$, where the midpoint was calculated by the computer.

The literature on earnings risk typically measures risk by the dispersion around mean earnings within a specific education or occupation group. Following this approach, expected earnings risk is measured as the variance of individual earnings conditional on obtaining a particular educational degree.

Yet, in order to calculate moments of the individual earnings distribution, it is necessary to determine how expected earnings are distributed over the two intervals (from the minimum ($y_m$) to the midpoint ($y_{mid}$) and from the midpoint ($y_{mid}$) to the maximum ($y_M$)). In this study, I follow Guiso, Jappelli, and Pistaferri (2002) and Attanasio and Kaufmann (2014, 2017), assuming a triangular distribution, which gives expected earnings closer to the midpoint more weight than expected earnings further away from that point.[7]

Based on these three pieces of information on the individual earnings distribution ($y_m, y_M, p$) and the distributional assumption, average expected earnings and expected earnings risk for each student $i$ and education scenario $d = 1, 2, 3$ (i.e. vocational, Bachelor's or Master's degree) can be calculated as:[8]

$$\begin{aligned} E(y) &= \int_{y_m}^{y_M} y\ f(y)dy \\ &= \int_{y_m}^{y_{mid}} (1-p)\ y\ f(y)dy + \int_{y_{mid}}^{y_M} p\ y\ f(y)dy \end{aligned} \tag{4.1}$$

[7]In the robustness section, I investigate the sensitivity of this assumption by assuming that expected earnings are uniformly distributed over the two intervals.

[8]For further details see Guiso, Jappelli, and Pistaferri (2002), Appendix D.

$$
\begin{aligned}
Var(y) &= \int_{y_m}^{y_M} y^2 f(y)dy - E(y)^2 \\
&= \int_{y_m}^{y_{mid}} (1-p)\; y^2\; f(y)dy - E(y)^2 + \int_{y_{mid}}^{y_M} p\; y^2\; f(y)dy - E(y)^2 \qquad (4.2)
\end{aligned}
$$

where $y_m$ and $y_M$ are the expected minimum $(m)$ and maximum $(M)$ earnings of student $i$ at age 35 conditional on working full time with educational degree $d$ and $y_{mid}$ indicates the midpoint between these two points $(\frac{y_m+y_M}{2})$; $p$ represents the respective probability to earn more than $\frac{y_m+y_M}{2}$. Given that we elicited wage expectations conditional on full time employment, biases arising from different labor supply expectations are ruled out by construction.

### 4.2.2 Additional covariates

Including an extensive set of covariates in the analysis serves two main purposes. First, if expected earnings risk plays a role in explaining gender differences in earnings expectations, this correlation should be robust to accounting for other individual characteristics. These may be correlated with expected earnings as well as expected earnings risk and simultaneously vary by gender. Second, including a set of additional explanatory variables enables me to set the explanatory power of anticipated compensation for earnings risk in relation to the importance of other characteristics that differ by gender.

The literature offers a range of hypotheses that may explain gender differences in earnings expectations, some of which may also be correlated with expected earnings risk. In general, if we assume that students are able to correctly identify factors that are more or less rewarded in the labor market, then all explanations that are typically brought forward with regard to the actual gender wage gap may also help to explain the gender gap in expected earnings.[9] I include six sets of covariates that represent different explanations for the gender gap in expected earnings: (1) *Baseline characteristics* account for socio-demographic differences; (2) *Performance and cognitive skills* reflect standard human capital variables; (3) *Intended college major* accounts for the well-documented differences in college major choice by gender; (4) *Career motives* capture the importance of different job attributes that students

[9]For a comprehensive overview of existing explanations see Blau and Kahn (2016).

assign to their future job choice; (5) *Personality traits and preferences* account for differences in career path that students strive for; and (6) *Self-confidence* accounts for general differences in the assurance to succeed in the labor market that could result in higher expected earnings. The variables included in each of these categories, as well as how they are measured, is discussed below.

*Baseline characteristics.* Theses characteristics capture demographic information as well as high school type attended. With respect to demographic information, I include whether or not a student has a migration background and whether at least one of the parents earned a university degree. Attended high school types, ordered from most to least prestigious, include: academic high school *(Gymnasium)*, integrated high school, i.e. high schools offering different school leaving degrees *(Integrierte Sekundarschule)*, and occupation specific high schools *(berufliches Gymnasium)*.

*Performance and cognitive skills.* Students' academic performance during high school is measured by their final high school GPA, ranging from one to four. Note that in Germany a higher GPA corresponds to lower performance. The final high school GPA signals students their ability (which may or may not be correct) and we would expect higher performing students to anticipate that their higher ability (or signal thereof) is rewarded in the labor market. Cognitive skills are measured by the scores on two short tests that capture students' verbal and figural skills, as conducted in the first wave of the panel survey. Higher scores on the cognitive tests indicate higher skills.

*Intended college major.* As we asked students about their expected earnings for themselves, these expectations should at least partly be influenced by the type of occupation they aspire to or the college major they intend to enroll in (Montmarquette, Cannings, and Mahseredjian, 2002; Arcidiacono, Hotz, Maurel, and Romano, 2014). To minimize the possibility that the observed gender gap in expected earnings is entirely due to gender differences in (future) major choice, I control for students' intended college major. In that sense, I am focusing on gender differences within (intended) college major. The information on students intended major is derived from different waves of the survey. Firstly, if students already applied to university or reported to plan on applying in the third wave, we have information on which majors they applied for. If students apply for more than one major, I use the major that students rank as their first choice. Secondly, students who reported during high school that they intend to enroll in university, were also asked about the major that they would like to enroll in. Based on the classification of the German Statistical Office (Destatis, 2012), the different majors are grouped into ten fields of study, as listed in Table 4.2.2.

*Career motives.* Similar to the intended college major, expected earnings are likely to be affected by the career plans that students hold, which are likely to differ by gender (Humlum, Kleinjans, and Nielsen, 2012). In particular, one might expect that females anticipate future career breaks due to child bearing and rearing (Chevalier, 2007). If they incorporate family formation plans into their earnings expectations, this may explain their lower earnings expectations compared to males. Although I do not have direct information on these plans, the survey includes a battery of questions (11 items) capturing how important different career aspects are to students' future job choice. Among others, students report how important it is for their future job choice to have a job that leaves enough time for family commitments. The different items can be grouped into four main categories (see Weinhardt and Schupp, 2011): (1) Extrinsic motives; (2) Intrinsic motives; (3) Social motives; and (4) Work-life balance related motives. The grouping of the 11 items into these categories can be inferred from Table 4.2.2.[10]

*Personality traits and preferences.* A growing literature emphasizes the importance of psychological attributes, preferences, and personality traits in explaining educational choices (Koch, Nafziger, and Nielsen, 2014) as well as labor market outcomes (Heckman and Kautz, 2012) and document gender differences with respect to these non-cognitive skills (for an overview see Bertrand, 2011).[11] To capture differences in non-cognitive skills and preferences, I include personality traits, locus of control, patience, time preference (for present) and risk aversion.

Personality traits are measured by an adjusted version of the Five Factor Model that covers openness to experience, conscientiousness, extraversion, agreeableness, and neuroticism (Big Five) (McCrae and Costa, 1996).[12] Each dimension is repre-

[10]The grouping of items follows the recommendation of Weinhardt and Schupp (2011). However, one item was excluded because an exploratory factor analysis revealed that it did not load on the correct factor.

[11]For example, Grove, Hussey, and Jetter (2011) find that, based on U.S. data, the inclusion of measures on non-cognitive skills and work preferences significantly increase the explained part of the gender pay gap for a sample of individuals with a Master's degree.

[12]Definitions of the five dimensions as cited in Almlund, Duckworth, Heckman, and Kautz (2011) are: *openness* defines the tendency to be open to new aesthetic, cultural, or intellectual experiences; *conscientiousness* mirrors the tendency to be organized, responsible, and hard-working; *extraversion* describes an orientation of one's interests and energies toward the outer world of people and

sented by three statements that are answered on a 7-point Likert type scale ranging from 1 "does not apply at all" to 7 "fully applies" (Dehne and Schupp, 2007).[13] Based on this information, I generate summation scores for each personality dimension.

The locus of control indicates how strongly an individual believes that what happens is a consequence of her own actions (internal) or outside her control, i.e. due to luck and fate (external).[14]

Patience proxies inter-temporal choice behavior and is measured by asking students to rate their general patience on an 11-point scale from very impatient to very patient. This survey measure is shown to correlate well with individuals' choices in an incentive based choice experiment eliciting time preferences (Vischer, Dohmen, Falk, Huffman, Schupp, Sunde, and Wagner, 2013). Additionally, I include another measure for time preferences. On a 7-point scale students were asked about their degree of agreement with the following two statements. *"I pass on something today, in order to be able to afford more tomorrow"* (reversed item) and *"I rather have fun today and do not think about tomorrow"*. Time preference for the present is based on the average response to these two items.

Finally, risk aversion is measured by asking students to rate their willingness to take risks on a 11-point scale, a validated measure capturing the likelihood of risky behavior in different domains (Dohmen, Falk, Huffman, Sunde, Schupp, and Wagner, 2011).

*Self-confidence.* Self-confidence is approximated by the extent students agree with the following statement: *"I am a person who has a positive attitude toward herself."* The extent of agreement is measured on a 7-point scale. Additionally, students were asked how likely they think it is that they could successfully graduate from university. Answers are given on a 4-point scale ranging from very low to very high. I generate a binary variable that equals one if students rate their chances as very high and zero otherwise. This measure may yield some indication about students' confidence in their own abilities.

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things rather than the inner world of subjective experience and is characterized by positive affect and sociability; *agreeableness* shows the tendency to act in a cooperative, unselfish manner; and *neuroticism* is a chronic level of emotional instability and proneness to psychological distress.

[13] Unlike when measuring adults, measuring youth' openness to experience is based on four questions as defined in Weinhardt and Schupp (2011).

[14] These two measures are based on eight different items, capturing the extent to which individuals agree (on a 7-point Likert type scale) with statements such as *"The possibilities I have in life are dependent on social circumstances."*

For the empirical analysis in Section 4.4, all variables that are measured on a scale are standardized in order to facilitate interpretation and enhance comparison.

### 4.2.3 Sample selection

Out of the 1034 students who participated in the third wave of the survey, a large share of students did not answer the question on their expected earnings. Response rates are around 60% for each education scenario and slightly lower (56%) when considering only students with complete responses to all education scenarios. Existing studies on students' expected earnings primarily use data that was specifically collected to analyze earnings expectations. As such, item non-response in these studies are lower (although sample selection is still a major concern). In contrast, this study builds on data where questions on expected earnings were included in a larger survey and not the primary focus of the questionnaire. Hence, higher item non-response rates are not surprising.[15]

However, comparing individual characteristics across students who answered the complete module on expected earnings for all three education scenarios with those who did not, suggests that item non-response in our questionnaire does not occur randomly (see Table 4.2.1). Males, students with a better high school GPA, and students with higher scores on the verbal cognition test are statistically significantly more likely to provide complete information on their expected earnings. Differences regarding the intended college enrollment between respondents and non-respondents are particularly striking. While 80% among students who answered all questions on expected earnings intend to enroll, this number is only 67% among non-respondents. For the current analysis, however, it is more important whether response behavior differs by gender. As seen in Table 4.2.1, females who provided complete information are more likely to have a better high school GPA and higher scores on the cognition tests. Hence, there seems to be a positive selection in terms of performance and cognitive skills among females. Although these response rates seem selective, the pattern of selection would imply a downward bias of the gender gap in expected earnings as we observe expected earnings for more able females. Nevertheless, I

[15]The literature on reservation wages, for example, reports item non-response rates of similar magnitude if questions are included in surveys that do not exclusively focus on this issue (e.g. Caliendo, Gambaro, and Haan, 2009).

investigate how a potential non-random response rate affects the results and estimate a selection corrected model, as suggested by Heckman (1979), in Section 4.4.4.

Although we elicited earnings expectations conditional on different educational degrees, the empirical analysis focuses on expected earnings conditional on earning a Master's degree.[16] Hence, I restrict the sample to students with information on their gender and expected earnings with a Master's degree (N=607). I exclude students whose responses suggest that they did not entirely understood the question (N=25). These are students who assigned the entire probability mass either to the lower or the upper part of the support of their individual earnings distribution, i.e. to the left or the right of the midpoint. Further, to ensure that the analysis is not driven by a few outliers, I exclude students whose average expected earnings fall in either the highest or the lowest one percentile of the cross-sectional distribution of expected earnings (N=12). As mentioned above, response rates to the module on expected earnings is significantly higher among students who intend to enroll in college. Thus, to reduce unobserved individual heterogeneity, I further restrict the sample to students who state an enrollment intention (N=460). This additionally serves the analysis as information on intended college major is only available for students who at least once during the course of data collection stated an enrollment intention. Lastly, I only keep students who provided complete information on all covariates with the following exceptions.[17] For final high school GPA, personality traits, self-confidence, and intended college major, I deal with missing values by replacing missing values with sample means and including a binary variable indicating missing information. The final sample for the empirical analysis consists of 428 students, of whom 183 are males and 245 females.

[16]In Germany, the majority of Bachelor's students continue to earn a Master's degree (e.g. Neugebauer, Neumeyer, and Alesi, 2016). Thus, expected earnings with a Master's degree should be most relevant for students' higher education choice. In addition, the estimates in Table 4.4.3 reveal that the extent of anticipated risk compensation differs across the three education scenarios such that pooling all earnings expectations would mask heterogeneity across educational degrees.

[17]The results are qualitatively similar if estimations are performed including students without an enrollment intention or excluding students with any missing information (see Section 4.4.4). Given the extensive set of covariates, this approach prevents losing too many observation for the main analysis due to missing information.

Table 4.2.1: Comparing non-respondents and respondents

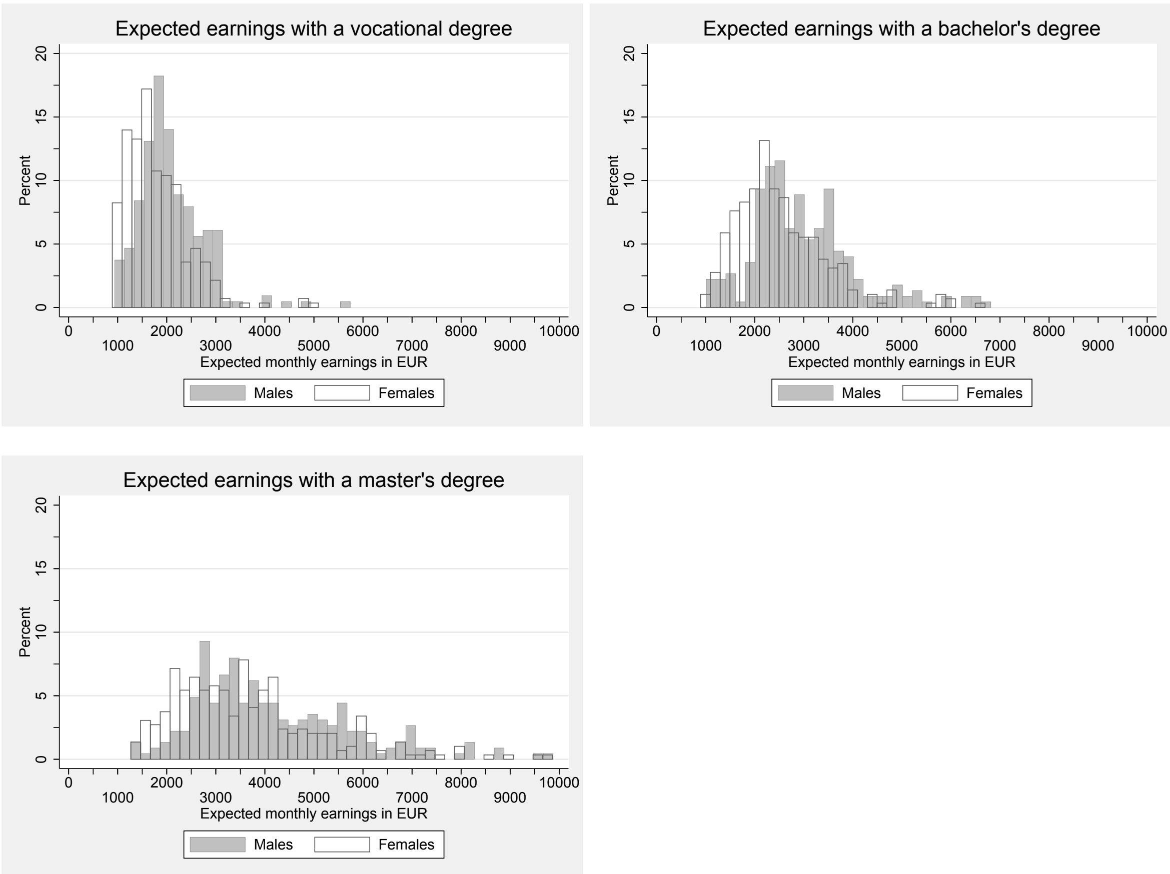
| | Full sample | | Females | | Males | |
|---|---|---|---|---|---|---|
| | Non-Response | Complete Response (Difference) | Non-Response | Complete Response (Difference) | Non-Response | Complete Response (Difference) |
| **Educational aspiration:** | | | | | | |
| Intended college enrollment | 0.673 | 0.126*** | 0.674 | 0.136*** | 0.673 | 0.113*** |
| **Covariates:** | | | | | | |
| **Baseline characteristics** | | | | | | |
| Female | 0.620 | -0.061** | | | | |
| Migration background | 0.507 | -0.016 | 0.524 | -0.021 | 0.479 | -0.003 |
| Non-academic fam.backgr. | 0.646 | -0.047 | 0.653 | -0.052 | 0.633 | -0.038 |
| Academic high school | 0.282 | 0.025 | 0.294 | 0.047 | 0.263 | 0.001 |
| Integrated high school | 0.378 | -0.028 | 0.384 | -0.055 | 0.368 | 0.009 |
| Vocational oriented high school | 0.340 | 0.003 | 0.323 | 0.009 | 0.368 | -0.010 |
| **Performance and cognitive skills** | | | | | | |
| Final high school GPA | 2.568 | -0.110*** | 2.553 | -0.124** | 2.595 | -0.097 |
| Figural cognitive skills | 10.817 | 0.219 | 10.685 | 0.606*** | 11.035 | -0.320 |
| Verbal cognitive skills | 9.479 | 0.681*** | 8.767 | 0.901*** | 10.647 | 0.138 |
| **Intended college major** | | | | | | |
| Language and Culture studies | 0.109 | -0.012 | 0.142 | 0.001 | 0.052 | -0.010 |
| Social Sciences | 0.077 | -0.021 | 0.108 | -0.049** | 0.022 | 0.028 |
| Business and Economics | 0.150 | -0.005 | 0.138 | 0.015 | 0.172 | -0.036 |
| STEM | 0.249 | 0.057* | 0.116 | 0.082** | 0.478 | -0.041 |
| Teaching | 0.041 | 0.014 | 0.056 | 0.010 | 0.015 | 0.027 |
| Law & Management Sciences | 0.079 | -0.010 | 0.099 | -0.040* | 0.045 | 0.036 |
| Medicine | 0.139 | -0.013 | 0.181 | -0.017 | 0.067 | 0.013 |
| Psychology | 0.082 | -0.025 | 0.091 | -0.021 | 0.067 | -0.025 |
| Arts and Sports | 0.044 | 0.014 | 0.034 | 0.018 | 0.060 | 0.004 |
| Others | 0.030 | 0.001 | 0.034 | 0.000 | 0.022 | 0.003 |
| **Career motives** | | | | | | |
| Extrinsically motivated | 3.064 | 0.014 | 3.091 | 0.002 | 3.021 | 0.038 |
| Intrinsically motivated | 3.255 | 0.036 | 3.281 | 0.080* | 3.213 | -0.010 |
| Socially motivated | 2.926 | -0.127*** | 3.016 | -0.087 | 2.782 | -0.148** |
| Work-Life-Balance motivated | 3.179 | -0.051 | 3.217 | -0.008 | 3.119 | -0.091 |
| **Personality traits** | | | | | | |
| Openness | 4.969 | 0.118* | 4.990 | 0.193** | 4.935 | 0.028 |
| Extraversion | 4.845 | 0.001 | 4.922 | -0.055 | 4.716 | 0.101 |
| Conscientiousness | 4.933 | -0.086 | 5.104 | -0.091 | 4.645 | -0.015 |
| Neuroticism | 4.334 | -0.118 | 4.618 | -0.003 | 3.854 | -0.158 |
| Agreeableness | 5.363 | -0.154** | 5.447 | -0.091 | 5.222 | -0.204* |
| External Locus of Control | 3.280 | 0.013 | 3.321 | 0.064 | 3.213 | -0.038 |
| Internal Locus of Control | 5.401 | -0.078 | 5.424 | -0.060 | 5.363 | -0.093 |
| **Preferences** | | | | | | |
| Riskaversion | 4.210 | 0.200 | 4.450 | 0.078 | 3.817 | 0.443* |
| Patience | 6.062 | -0.088 | 5.833 | 0.018 | 6.434 | -0.305 |
| Time preference for present | 3.352 | -0.009 | 3.306 | 0.016 | 3.425 | -0.056 |
| **Confidence** | | | | | | |
| Confidence in own ability | 0.197 | 0.059** | 0.178 | 0.034 | 0.228 | 0.086* |
| Self-confidence | 4.894 | -0.027 | 4.668 | 0.025 | 5.283 | -0.189 |
| N | 450 | 583 | 279 | 326 | 171 | 257 |
| N (total) | 1033 | | 605 | | 428 | |

*Notes:* This table presents differences in individual characteristics between students who answered the complete module on earnings expectations and those who did not. Means and mean differences are based on a two-sided t-test.* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

### 4.2.4 Gender differences in expected earnings and observed characteristics

This section examines gender differences in expected average earnings as well as earnings risk. Moreover, gender differences with respect to the set of covariates are explored.

Figure 4.2.1: Expected average earnings by education scenario and gender




*Notes:* This figure shows the cross-sectional distribution of expected average earnings with different educational degrees. Observations that fall in the upper or lower one percentile of the respective distributions are excluded. For illustration purposes earnings expectations exceeding 10,000 EUR per month are not depicted.

I start by looking at the distribution of expected average earnings across individuals. Figure 4.2.1 depicts the cross-sectional distributions of (individual) expected average earnings separately for males and females as well as for each education scenario. Several observations are noteworthy. First, average expected earnings increase with the level of the education scenario, indicating that students are aware of the monetary returns to higher levels of education. At the same time, the higher the educational degree, the more dispersed the distribution. This points toward increasing heterogeneity in expected average earnings with increasing levels of education. Second, the distribution for both groups, males and females, is skewed to the right, indicating that students are less likely to expect very high earnings. Third, and finally, in all education scenarios the distribution of males is shifted to the right and exhibits a thicker right tail, thus implying that males expect higher earnings than females on average and are more likely to expect exceptionally high earnings. The Kolmogorov-Smirnov test confirms that the distributions of cross-sectional average expected earnings differ significantly by gender in each education scenario.[18]

Table 4.2.2 presents gender differences in expected earnings and observed characteristics. In line with previous findings in the literature, this table confirms that even before entering the labor market females expect to earn considerably less than their male counterparts. While males expect to earn on average around 4,400 EUR per month with a Master's degree, females expect to earn 3,800 EUR. It also shows that students perceive a considerable amount of earnings risk as measured by the standard deviation of expected earnings, where females expect lower earnings risk than males.[19]

Males and females differ with respect to a range of different individual characteristics that could potentially influence the gender gap in expected earnings. The remainder of Table 4.2.2 presents averages in individual characteristics separately for males and females as well as the difference between them.

In regards to baseline characteristics, males and females do not differ significantly, with the exception of attended high school type. Females are around 7 percentage points (pp) more likely to have attended the most prestigious high school track (although this difference is not statistically significant at conventional levels). Looking at academic performance and cognitive skills, somewhat surprisingly, females score significantly lower on the verbal cognition test; however, the difference is, at one point, rather small and corresponds to only 42% of a standard deviation. Regarding intended college major, the well-known pattern is found. Females are more likely to intend to enroll in language and cultural as well as medical studies and significantly

[18] Corresponding D- and p-values are: vocational degree D=0.2639; p=0.000; Bachelor's degree D=0.2352; p=0.000; Master's degree D=0.1720; p=0.001.

[19] Females expect lower average earnings and lower earnings risk in all education scenario, not just conditional on earning a Master's degree. Expected earnings risk increases with the level of educational degrees for both genders, which is in line with the empirical findings on actual labor market data (Koerselman and Uusitalo, 2014).

Table 4.2.2: Gender differences in observed characteristics

| | Female | Male | Difference | p-value |
|---|---|---|---|---|
| **Earnings expectations (in EUR):** | | | | |
| Expected earnings with a Master's degree | 3865.23 | 4453.26 | -588.03*** | (0.007) |
| Expected earnings risk measured by the standard deviation | 484.40 | 648.86 | -164.46** | (0.022) |
| **Covariates:** | | | | |
| **Baseline characteristics** | | | | |
| Migration background | 0.514 | 0.470 | 0.044 | (0.365) |
| Non-academic fam.backgr. | 0.576 | 0.590 | -0.015 | (0.762) |
| Academic high school | 0.351 | 0.284 | 0.067 | (0.144) |
| Integrated high school | 0.335 | 0.350 | -0.015 | (0.746) |
| Vocational oriented high school | 0.314 | 0.366 | -0.052 | (0.263) |
| **Performance and cognitive skills** | | | | |
| Final high school GPA | 2.378 | 2.382 | -0.004 | (0.949) |
| Verbal cognitive skills | 9.735 | 10.929 | -1.194*** | (0.000) |
| Figural cognitive skills | 11.143 | 10.869 | 0.274 | (0.307) |
| **Intended college major** | | | | |
| Language and Culture studies | 0.127 | 0.039 | 0.088*** | (0.002) |
| Social Sciences | 0.055 | 0.050 | 0.005 | (0.809) |
| Business and Economics | 0.161 | 0.127 | 0.034 | (0.332) |
| STEM | 0.216 | 0.436 | -0.220*** | (0.000) |
| Teaching | 0.068 | 0.050 | 0.018 | (0.442) |
| Law & Management Sciences | 0.047 | 0.083 | -0.036 | (0.130) |
| Medicine | 0.174 | 0.072 | 0.102*** | (0.002) |
| Psychology | 0.081 | 0.055 | 0.025 | (0.316) |
| Arts and Sports | 0.038 | 0.061 | -0.023 | (0.285) |
| Others | 0.034 | 0.028 | 0.006 | (0.716) |
| **Career motives** | | | | |
| Extrinsically motivated | 3.129 | 3.061 | 0.068 | (0.249) |
| High income | 0.328 | 0.383 | -0.055 | (0.243) |
| Good promotion possibilities | 0.412 | 0.390 | 0.021 | (0.657) |
| Recognition | 0.298 | 0.240 | 0.058 | (0.187) |
| Intrinsically motivated | 3.361 | 3.232 | 0.129** | (0.018) |
| Interesting job | 0.657 | 0.617 | 0.040 | (0.399) |
| Independent working | 0.294 | 0.279 | 0.015 | (0.732) |
| Socially motivated | 2.931 | 2.628 | 0.303*** | (0.000) |
| Social interaction | 0.335 | 0.164 | 0.171*** | (0.000) |
| Important for society | 0.196 | 0.191 | 0.005 | (0.904) |
| Help Others | 0.328 | 0.181 | 0.147*** | (0.001) |
| Work-Life-Balance motivated | 3.205 | 2.991 | 0.215*** | (0.000) |
| Spare time | 0.155 | 0.148 | 0.008 | (0.830) |
| Good health/safety conditions | 0.600 | 0.436 | 0.164*** | (0.001) |
| Time for family commitments | 0.437 | 0.352 | 0.085* | (0.076) |
| **Personality traits** | | | | |
| Openness | 5.193 | 4.990 | 0.204** | (0.045) |
| Extraversion | 4.893 | 4.820 | 0.073 | (0.561) |
| Conscientiousness | 5.053 | 4.773 | 0.279*** | (0.007) |
| Neuroticism | 4.622 | 3.690 | 0.932*** | (0.000) |
| Agreeableness | 5.343 | 4.965 | 0.378*** | (0.000) |
| External Locus of Control | 3.361 | 3.143 | 0.218** | (0.023) |
| Internal Locus of Control | 5.365 | 5.341 | 0.024 | (0.756) |
| **Preferences** | | | | |
| Riskaversion | 4.510 | 4.230 | 0.281 | (0.197) |
| Patience | 5.943 | 6.038 | -0.095 | (0.660) |
| Time preference for present | 3.339 | 3.221 | 0.117 | (0.356) |
| **Confidence** | | | | |
| Confidence in own ability | 0.241 | 0.350 | -0.109** | (0.014) |
| Self-confidence | 4.784 | 5.154 | -0.369** | (0.019) |
| N | 245 | 183 | | |

*Notes:* This table presents differences in individual characteristics between males and females. Means and mean differences are based on a two-sided t-test.* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

less likely to intend to enroll in science, technology, engineering, and mathematics (STEM) majors. Likewise, career motives differ significantly between genders. Interestingly, males and females do not differ significantly with respect to their overall extrinsic motivation. Although females are around 6 pp less likely than males to state that a high income is very important for their job choice and 6 pp more likely to rate recognition as very important, these differences are not statistically significant. Moreover, females put more weight on intrinsic, social, and work-life balance oriented motives than males. For females' job choice it is more important to help others and have a job with frequent social interaction. Interestingly, having a job that is important for society is equally important to males and females. Females are also more likely to report that it is very important to have a job that offers good health as well as safety conditions and leaves sufficient time for family commitments. Comparing personality traits, females are more open, more conscientious, more neurotic, and more agreeable than males. The extent of extraversion is the only dimension of personality that does not differ significantly between gender. Females show a higher external locus of control, indicating that they perceive their life to be more affected by circumstances outside their control than males. Finally, females have less confidence in their abilities and a less positive perception of themselves when compared to males.

In Section 4.4, I investigate to what extent these differences are related to gender differences in expected average earnings.

## 4.3 Empirical Approach

### 4.3.1 Do students anticipate compensation for earnings risk?

The argument of this study rests on the assumption that students anticipate compensation for earnings risk. Thus, before focusing on whether anticipated compensation for earnings risk can explain the gender gap in expected earnings, I provide corroborating evidence on this relationship.

Labor economists typically uncover risk compensation by first estimating a standard Mincer wage equation and then use the variance of the residuals within an education-occupation group as a measure for earnings risk. In a second step, this risk measure in then included in the Mincer wage equation yielding the risk augmented Mincer wage equation (see Hartog, 2011). In line with this approach, and following the analysis of Schweri, Hartog, and Wolter (2011) and Mazza and Hartog (2011), I analyze whether students expect compensation for earnings risk by estimating the following model:[20]

$$ln(y_{id}) = \alpha + \beta_1 risk_{id} + X_i' \beta_2 + \varepsilon_{id}, \tag{4.3}$$

where $y_{id}$ represents expected earnings of student $i$ conditional on earning educational degree $d$ ($d = 1, 2, 3$, i.e. vocational, Bachelor's or Master's degree). $risk_{id}$ reflects expected earnings risk as measured by the dispersion in expected earnings and is the variable of main interest. More specifically, expected earnings risk is measured by the log of the individual-specific variance of expected earnings as described in Section 4.2, such that $\beta_1$ can be interpreted as an elasticity. If students expect compensation for earnings risk, the estimate of $\beta_1$ should be positive. Finally, $X_i$ is a vector of control variables including baseline characteristics, academic performance and cognitive skills as described in Section 4.2. $\varepsilon_{id}$ is an individual error term and captures the remaining variation. As the error terms of students who attended the same high school may be correlated, I cluster standard errors at the school level.

### 4.3.2 Examining the gender gap in expected earnings

In order to investigate how the gender gap in expected earnings can be explained, I start by estimating a sequence of regression models, where the full model is given by:

$$ln(y_i) = \alpha + \gamma_1 female_i + \gamma_2 risk_i + X_i' \gamma_3 + \varepsilon_i, \tag{4.4}$$

where $y_i$ is the expected average earnings with a Master's degree, $female_i$ is a female indicator variable, $risk_i$ is the measure of (log) expected earnings risk as defined in Section 4.2, and $X_i$ is a vector of explanatory variables containing a broad set of different student characteristics, cognitive and non-cognitive skills, economic preferences, intended college major as well as variables capturing different career

[20] In Section 4.4 I will not only report the results based on earnings expectations with a Master's degree but additionally present the results using expected earnings with a Bachelor's and a vocational degree as well as using pooled data, i.e. using expected earnings pooled over all three education scenarios. In the latter specification an indicator variable for the different educational scenarios is added to Equation 4.3.

motives (see Section 4.2 for more details). $\varepsilon_i$ is an individual error term, which is again clustered at the school level. The coefficient of major interest is $\gamma_1$. In particular, I am interested in how $\gamma_1$ changes in response to separately including different sets of covariates and the measure of expected earnings risk.

In general, I could add the covariates in Equation 4.4 sequentially and infer the role of each set of covariates by observing the change of the gender gap, i.e. the female indicator. However, the contribution of each set of variables will depend on the order in which they are added. Thus, in order to investigate the role of a particular set of variables in explaining the gender gap in expected average earnings, while holding the other covariates constant, I perform a standard Blinder-Oaxaca decomposition (Oaxaca, 1973; Blinder, 1973). This procedure circumvents the problem of choosing the order in which covariates should be added; i.e. the results of the decomposition do not suffer from path dependence.[21] I start by estimating separate regression for males and females:

$$ln(y_i) = \alpha_f + X_i'\beta_f + \varepsilon_i \tag{4.5}$$

$$ln(y_i) = \alpha_m + X_i'\beta_m + \varepsilon_i \tag{4.6}$$

where the measure of expected earnings risk is included in $X_i$, which is otherwise defined as in Equation 4.4. The average difference in expected earnings between males and females can then be expressed as:

$$(\bar{Y}_f - \bar{Y}_m) = (\bar{X}_f - \bar{X}_m)'\beta_m + \bar{X}_m'(\beta_f - \beta_m) - (\bar{X}_f - \bar{X}_m)'(\beta_f - \beta_m) \tag{4.7}$$

where the first part on the right-hand side captures the part of the difference that can be explained by mean differences in observed characteristics and is typically labeled the *explained component*; the second term measures the extent to which these characteristics affect males and females expected mean earnings differently; and the third term represents the interaction between differences in mean characteristics and differences in coefficients. The latter two make up the *unexplained component* of the

[21] This property of the decomposition stems from the fact that the regression coefficients used to weight the group differences (see below) are based on partial correlations, i.e. obtained from a regression including the full set of covariates.

difference. In this analysis I focus exclusively on the explained component. The result of the decomposition will depend on whether males or females are used as a reference group. Equation 4.7 is expressed from the viewpoint of males, i.e. it uses the male coefficient to weight the group differences in characteristics and likewise uses male characteristics to weight group differences in coefficients. However, Equation 4.7 could also be expressed from the viewpoint of females. Thus, in my preferred specification I follow the recommendation of Neumark (1988) and use the coefficient from a pooled regression, such that the average difference in expected earnings between males and females can be expressed as:

$$(\bar{Y}_f - \bar{Y}_m) = (\bar{X}_f - \bar{X}_m)'\beta^* + \bar{X}_f'(\beta_f - \beta^*) + \bar{X}_m'(\beta^* - \beta_f) \quad (4.8)$$

where $\beta^*$ represents the estimates from a pooled regression over males and females. In Section 4.4, I will report and compare the decomposition results from all three different weighting schemes, i.e. using males or females as a reference group respectively or using the coefficients from a pooled regression.

## 4.4 Results

The previous sections document a large gender gap in expected average earnings. However, the relevance of this gap depends on whether these differences in expected average earnings are associated with choices that may lead to the actual gender wage gap. Gender differences in college major choice are known to play an important role in explaining the actual gender wage gap (Machin and Puhani, 2003; Black, Haviland, Sanders, and Taylor, 2008). While not the focus of the current paper, the supplementary analysis in Appendix A shows that higher expected average earnings are positively related to choosing a high paying major and that gender differences in expected earnings can account for around 20 percent of the gender gap in choosing a high paying college majors.[22] This result provides supporting evidence for the relevance of students' earnings expectations and their role in explaining gender differences in educational choices.

[22] College majors are grouped into high, medium, and low paying fields according to their rank in terms of hourly wages based on actual labor market earnings as derived from the German Micro Census (see Appendix A for details).

The following analysis proceeds in three steps: First, I show that students of both genders anticipate compensation for earnings risk and that this relationship does not differ significantly between genders. Second, I investigate the role of different sets of covariates in explaining the gender gap in expected earnings without accounting for correlations between these sets. This exercise helps to understand the individual importance of different explanations. And third, I perform a Blinder-Oaxaca decomposition to quantify the role of expected earnings risk in explaining the gender gap in expected average earnings in relation to other explanations that are captured by the different sets of covariates.

### 4.4.1 Do students anticipate compensation for earnings risk?

This section shows that students anticipate compensation for earnings risk, thereby providing supporting evidence for the findings of Schweri, Hartog, and Wolter (2011). The results are presented in Table 4.4.3. While the first three columns focus on expected earnings with a Master's degree, column (4) and (5) show the estimates for expected earnings with a Bachelor's and vocational degree, respectively. In addition, column (6) is based on expectations pooled over the three different educational degrees.

Table 4.4.3: Expected compensation for earnings risk

| Dep.var.: log expected mean earnings | Master's degree | | | Bachelor's degree | Vocational degree | Pooled over degrees |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Expected earnings risk | 0.172*** | 0.173*** | 0.178*** | 0.161*** | 0.103*** | 0.153*** |
| | (0.009) | (0.009) | (0.013) | (0.014) | (0.020) | (0.012) |
| Expected earnings risk * Female | | | -0.009 | -0.020 | 0.017 | -0.003 |
| | | | (0.017) | (0.020) | (0.021) | (0.015) |
| Bachelor's degree | | | | | | 0.271*** |
| | | | | | | (0.017) |
| Master's degree | | | | | | 0.510*** |
| | | | | | | (0.019) |
| Controls | No | Yes | Yes | Yes | Yes | Yes |
| Adj. $R^2$ | 0.581 | 0.591 | 0.591 | 0.479 | 0.320 | 0.681 |
| N | 428 | 428 | 428 | 419 | 401 | 1248 |

*Notes:* Estimates are based on Equation 4.3 and include the following control variables: age, gender, migration, and academic family background, type of attended high school, final high school GPA, and two measures of verbal and figural cognitive ability. Expected earnings risk is measured by the log of the variance of expected earnings. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

As shown in column (1) the coefficient for expected earnings risk is positive and highly significant. An increase in expected earnings risk, as measured by the variance of the expected earnings, by one percent increases expected average earnings by

0.172 percent.[23] Adding baseline characteristics and controlling for academic performance and skills only affects this relationship marginally (see column 2). In column (3), I further test whether the relationship between expected average earnings and expected earnings risk differs by gender. This information is essential for evaluating the role of expected earnings risk in explaining gender differences in expected earnings. If, for example, females expect a significantly higher compensation for earnings risk than males, including expected earnings risk to explain gender differences in expected earnings would not only capture differences in expected earning risk but also the different extent of anticipated risk compensation. Hence, in column (3), I estimate Equation 4.3 and additionally include an interaction term between expected earnings risk and the female indicator. The coefficient on the interaction term is close to zero and not statistically significant, suggesting that males and females do not differ in their extent of anticipated compensation for earnings risk.

In order to investigate whether the relationship between expected average earnings and expected earnings risk is different for the three education scenarios, I repeat the estimation using earnings expectations for the two other education degrees (column(4)-(5)). The coefficient on earnings risk is positive and highly significant in all education scenarios. Interestingly, the coefficient on expected earnings risk increases with the level of the education scenario; it is lowest for expected earnings with a vocational degree (0.10), followed by expected earnings with a Bachelor's degree (0.16), and highest for expected earnings with a Master's degree (0.17). Anticipated compensation for earnings risk seem to increase with the education level, suggesting that students perceive higher levels of education to be more risky and thus expect an additional compensation for taking this risky decision. This argument fits well with the more theoretical rationale that students also expect compensation for postponing earnings while staying in education longer (Hartog, 2011). In addition, based on data covering 16 different countries, the study by Pereira and Martins (2002) similarly shows a positive relationship between average earnings and earnings risk with increasing levels of education.

The estimates reported in Table 4.4.3 are similar to the reported elasticity of 0.125 percent in the study by Schweri, Hartog, and Wolter (2011). Given the differences in eliciting information on the expected earnings distribution and measurement of expected earnings risk, this finding is remarkable. The elasticity estimates as shown

[23]For an in-depth discussion on why a positive relationship between the mean and the variance in earnings is not a mechanical relict, see Hartog (2011).

in Table 4.4.3 are also comparable to estimates on risk compensation using actual labor market data. In an overview Hartog (2011) reports that most estimates for risk compensation fall between 0.1 - 0.2;[24] clearly, the elasticity estimates in Table 4.4.3 are contained in this interval.

Overall, this analysis shows that students anticipate compensation for earnings risk at similar elasticities as found based on actual labor market earnings. Having established this relationship, I now turn to analyzing the gender gap in expected earnings.

### 4.4.2 The gender gap in earnings expectations: The role of different explanations

This section examines the role of different explanations in accounting for the gender gap in expected earnings. Table 4.4.4 displays how a particular set of covariates affects the gender gap in expected earnings with a Master's degree. Each row includes only the female indicator and one particular set of covariates as indicated by the column label.[25]

Row (1) indicates that the raw gap equals 0.154 log points, i.e. females expect to earn 14.3% less than males. Adding baseline characteristics increases the gender gap by 0.6 pp (row 2). Similarly, accounting for differences in performance at school and cognitive skills marginally increase the gender gap by 0.3 pp (row 3).

Assuming that students are aware of the earnings differences across college majors, differences in expected earnings should reflect gender differences in intended field of study. As shown in row (4) including information on students' intended college major reduces the gender gap by 1.5 pp. This implies that gender differences in intended college major explain 10% of the gender gap in expected average earnings. While the role of intended college major is large, the remaining gender gap of 12.8% indicates that most of the gender differences in expected average earnings occur within (intended) college major. This finding is line with the results reported in Reuben, Wiswall, and Zafar (2015).

[24]These estimates are based on occupation specific risk measures. Hartog (2011) notes, however, that education based risk measures may lead to lower elasticity estimates.

[25]Table 4.4.4 only shows the coefficient of the females indicator. For the full estimation results see Table A4.6 in the Appendix.

The role of differences in career motives is shown in row (5). Including how important particular aspects are for students' job choice decreases the gender gap by 1.0 pp, which corresponds to 6.7% of the raw gap. Being more extrinsically motivated is significantly related to higher expected earnings, suggesting that students whose job choice is motivated by earning a high income, having good promotion possibilities or a highly recognized job expect higher average earnings.

Table 4.4.4: Explaining the gender gap in expected average earnings

| Dep.var: Log of expected mean earnings with a Master's degree | Female coeff. (1) | Gender gap in % (2) | Adj.$R^2$ (3) | % of raw gap (4) |
|---|---|---|---|---|
| (1) Raw difference | -0.154*** (0.038) | 14.3% | 0.029 | 100% |
| (2) Baseline characteristics | -0.161*** (0.037) | 14.9% | 0.036 | -4.2% |
| (3) Performance and cognitive skills | -0.158*** (0.043) | 14.6% | 0.024 | -2.5% |
| (4) Intended college major | -0.137*** (0.044) | 12.8% | 0.058 | 10.3% |
| (5) Career motives | -0.143*** (0.040) | 13.3% | 0.061 | 6.7% |
| (6) Personality traits | -0.145*** (0.043) | 13.5% | 0.044 | 5.4% |
| (7) Preferences | -0.147*** (0.038) | 13.7% | 0.037 | 4.2% |
| (8) Confidence | -0.134*** (0.041) | 12.5% | 0.044 | 12.1% |
| (9) Expected earnings risk | -0.035 (0.027) | 3.4% | 0.581 | 75.9% |
| (10) Full set of covariates | -0.003 (0.031) | 0.3% | 0.619 | 97.9% |
| N | 428 | | | |

*Notes:* This table presents estimates based on Equation 4.4. Each row presents a separate regression including only a female indicator and the set of covariates as indicated by the row label. For complete regression results see Table A4.6 in the Appendix. The gender gap in expected earnings is reported in column (2) and shows the exact percentage difference, i.e. $100 * (e^{\hat{\beta}} - 1)$. The calculation in column (4) is based on the exact percentage changes, i.e. column (2), and shows how much of the gender gap can be explained by the particular set of covariates in each row. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Further, including personality traits leads to a reduction in the gender gap of 0.8 pp, thereby accounting for 5.4% of the gender gap in expected average earnings. Being more agreeable is negatively correlated with expected average earnings, showing that students who have a stronger tendency to act cooperatively and unselfishly expect to earn less and may thus indeed settle for lower earnings. A higher internal locus of control appears positively related to expected earnings; however, this correlation misses the ten percent threshold for statistical significance (p-value: 0.109).

In row (7) the role of differences in preferences is shown. Differences in risk aversion, patience and (present) time preference lead to a decrease of the gender gap of 0.6 pp. The coefficient on risk aversion is significantly negative, showing that more risk averse students expect lower earnings.

Row (8) underlines the importance of students' confidence in their abilities as well as themselves. Adding these covariates decreases the gender gap by 1.8 pp. Gender differences in confidence can account for 12.1% of the gender gap; these two variables are even more important than intended college major. Being very sure that one could successfully complete a college degree is significantly related to higher expected earnings. Similarly, being more self-confident also increases expected earnings (p-value: 0.120).

Finally, row (9) of Table 4.4.4 reveals that expected earnings risk plays a crucial role in explaining the gender gap in expected average earnings. Including only the female indicator and expected earnings risk, as measured by the variance of the individual expected earnings distribution, accounts for about three-quarters of the gender gap. Accounting for differences in expected earnings risk decreases the gender gap in expected earnings by almost 11 pp (from 14.3% to 3.4%). Recall that the extent of anticipated compensation for earnings risk does not vary significantly by gender (see Table 4.4.3) and, hence, the reduction in the gender gap is solely driven by differences in expected earnings risk. Further note that the explained variation of the model sharply increases with the inclusion of expected earnings risk.

By including the full set of explanatory variables 97.9% of the gender gap in expected average earnings can be explained, yielding an insignificant estimate of gender gap that is close to zero. The estimates in Table 4.4.4 show that anticipated compensation for earnings risk is the single most important variable in explaining the gender gap in expected average earnings.[26] Moreover, expected earnings risk plays a major role in explaining the overall variation in expected average earnings. Overall, Table 4.4.4 provides supportive evidence for the hypotheses that females expect to trade off higher earnings for lower earnings risk.

[26]The results are qualitatively similar when looking at expected earnings with a Bachelor's or a vocational degree, although the importance of anticipated risk compensation in explaining the gender gap in expected average earnings seems to decrease with lower levels of education (see Tables A4.2 and A4.4 in the Appendix).

### 4.4.3 The gender gap in earnings expectations: Decomposition results

In the previous section I focused on the importance of a single set of covariates, neglecting the correlation between different sets of covariates. Next, I analyze the gender gap in expected average earnings in the framework of an Oaxaca-Blinder decomposition. The results are presented in Table 4.4.5. As outlined in Section 4.3, the results of the decomposition depend on the weighting scheme; thus Table 4.4.5 shows the results using the pooled regression coefficient, the male coefficient, or the female coefficient.

Table 4.4.5: Oaxaca-Blinder decomposition

| | Using pooled coefficent | | Using male coefficent | | Using female coefficent | |
|---|---|---|---|---|---|---|
| | Contribution (1) | % of gap (2) | Contribution (3) | % of gap (4) | Contribution (5) | % of gap (6) |
| **Difference** | 0.1539 | 100 | 0.1539 | 100 | -0.1539 | 100 |
| Explained | | | | | | |
| Baseline characteristics | -0.0016 | -1.06 | -0.0013 | -0.86 | -0.0004 | 0.25 |
| Performance and cognitive skills | -0.0025 | -1.65 | -0.0143 | -9.26 | -0.0128 | 8.34 |
| Intended college major | 0.0078 | 5.07 | 0.0031 | 2.03 | -0.0143 | 9.27 |
| Career motives | 0.0087 | 5.64 | -0.0014 | -0.89 | -0.0251** | 16.28 |
| Personality traits | 0.0131 | 8.49 | 0.0156 | 10.15 | -0.0109 | 7.11 |
| Preferences | 0.0014 | 0.89 | 0.0005 | 0.35 | -0.0034 | 2.2 |
| Confidence | 0.0058 | 3.8 | 0.0121 | 7.89 | 0.0099 | -6.43 |
| Expected earnings risk | 0.1184*** | 76.94 | 0.1185*** | 76.96 | -0.1246*** | 80.93 |
| Total explained | 0.151 | 98.11 | 0.1329 | 86.36 | -0.1816 | 117.95 |

*Notes:* This table presents estimates of the Oaxaca-Blinder decomposition using different weighting schemes. The number of observations equals N=428. Standard errors in parentheses are clustered at the school level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Note, that the negative sign on the %-contribution of baseline characteristics, academic performance and cognitive skills using pooled coefficients indicates that if females had the same average characteristics as males the gender gap in expected average earnings would be slightly larger.

With respect to the role of expected earnings risk, the results of the decomposition confirm the finding of the previous section. Looking at the pooled coefficient decomposition first, column (2) shows that, even conditional on including the full set of explanatory variables, differences in expected earnings risk explain, at 77%, the vast majority of the gender gap in expected average earnings. Given that in a decomposition exercise the role of expected earnings risk is calculated based on conditional correlations, this result is striking. Accounting for differences in other covariates, particularly intended college major, career motives, and self-confidence, differences in expected earnings risk explain over three-quarters of the overall gender gap in expected earnings. The different weighting schemes do not affect the explained contribution substantially. Using female coefficients even results in a higher percentage contribution (81%) of expected earnings risk in explaining the gender gap.

Differences in personality traits are the second most important factor and account for 8.5% of the gender gap. Again, the contribution changes only slightly when using the male or female coefficients. Accounting for gender differences in career motives explains an additional 5.6% of the gender gap, while intended college major accounts for 5.1%. Taking the measures for confidence into account reduces the gender gap by 3.8% and differences in preferences only explain 0.9%. Comparing the different weighting schemes reveals that differences in intended college major, career motives, and preferences can account for a higher share of the gender gap if female coefficients are used to weight the differences, suggesting that theses variables are more important for females' expected average earnings than for males' expectations. In contrast, using female coefficients to weight differences in confidence results in an increase of the gender gap, while differences in academic performance and cognitive skills yield a reduction in the gender gap. The explained contribution of the full set of explanatory variables is highest when using female coefficients (118%); in that case the gender gap can even be reversed. The explained part is lowest when using the male coefficient (86%) and within that interval using pooled coefficients (98%).[27]

Overall, the decomposition analysis shows that – irrespective of the weighting scheme – including expected earnings risk substantially increases the explained part of the gender gap in expected average earnings. Expected earnings risk can explain three times as much of the gender gap as all other covariates combined, highlighting that expected earnings risk is unlikely to be captured by students' characteristics that may be more easily observed.

### 4.4.4 Sensitivity analysis

The key finding of the empirical analysis is that anticipated compensation for earnings risk explains a considerable part of the gender gap in expected average earnings. This section investigates how sensitive this finding is to different specifications. A

[27]Note that although some of the covariates were significantly related to expected earnings and also different by gender (see Table 4.2.2 and A4.6), as a set of variables only expected earnings risk is significant in the decomposition.

summary of the sensitivity analysis is presented in Table 4.4.6. I use the estimate of the reduction in the gender gap in expected average earnings when accounting for expected earnings risk (see Table 4.4.4, row 9) as a reference point to which alternative specifications are compared. This estimate is displayed again in the first row of Table 4.4.6. Column (1) presents the raw gender gap in expected earnings, i.e. the coefficient of the female indicator in a model regressing (log) expected average earnings on a female dummy. Column (2) shows how this coefficient changes if the measure for expected earnings risk is included. In addition, column (3) exhibits how much of the raw gender gap is explained by including expected earnings risk in percentage terms. In order to facilitate comparison across specifications, estimates in Table 4.4.6 do not include any additional covariates.

Table 4.4.6: Sensitivity analysis

| Dep.var: (log) expected mean earnings with a Master's degree | Raw gender gap (female coeff.) | Gender gap including measure for earnings risk (female coeff.) | % of raw gap | N |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| (1) Main | -0.154*** | -0.035 | 77.3 | 428 |
| | (0.038) | (0.027) | | |
| (2) Uniform distribution | -0.150*** | -0.032 | 78.7 | 428 |
| | (0.038) | (0.027) | | |
| (3) Measuring earnings risk by the range between $y_m$ and $y_M$ | -0.154*** | -0.033 | 78.6 | 428 |
| | (0.038) | (0.026) | | |
| (4) Using pooled data | -0.178*** | -0.056** | 68.5 | 1248 |
| | (0.026) | (0.022) | | |
| (5) Complete info on all covariates | -0.158*** | -0.027 | 82.9 | 366 |
| | (0.044) | (0.032) | | |
| (6) Including those w/o study intention | -0.146*** | -0.022 | 84.9 | 526 |
| | (0.038) | (0.025) | | |
| (7) Excluding upper/lower 5 percentiles | -0.118*** | -0.043** | 63.6 | 394 |
| | (0.028) | (0.021) | | |
| (8) Maximum number of observations | -0.176*** | -0.024 | 86.4 | 582 |
| | (0.041) | (0.029) | | |
| (9) Heckman correction for item-nonresponse | -0.167*** | -0.022 | 86.8 | 1033 |
| *Wald test-statistic: 0.04; p-value: 0.8470* | (0.045) | (0.028) | | |

*Notes:* This table presents estimates from regressing log expected earnings with a Master's degree on a female indicator (column 1) under different specifications as indicated by the row label. Column (2) reports the coefficient on the female indicator if the measure for earnings risk is added, i.e. the (log) variance of expected earnings (except for row 3). Column (3) indicates how much of the gender gap can be explained by including expected earnings risk. Standard errors in parenthesis are clustered at the school level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

In order to calculate moments of the individual expected earnings distribution, I follow the approach of Guiso, Jappelli, and Pistaferri (2002) and Attanasio and Kaufmann (2014). I assume that expected earnings are distributed triangularly over the two intervals from the minimum to the midpoint and from the midpoint to the maximum. To verify that the main finding is not driven by the distributional assumption, I repeat the estimation and assume a uniform distribution. The results are presented in row (2). The first column shows that based on this alternative distributional assumption the gender gap is with 0.150 log points or 13.9% marginally smaller. However, adding the measure of expected earnings risk reduces the gap by 79%, which is very close to the 77% in row (1). Hence, the finding that anticipated compensation for earnings risk can explain a very high share of the gender gap in expected average earnings does not hinge on the distributional assumption.

Next, I use the range between expected maximum and minimum earnings as an alternative measure for expected earnings risk. The estimates in row (3) show that using this alternative measure of earnings risk does not change the relationship between expected earnings risk and averages expected earnings. Expected earnings risk still accounts for 79% of the gender gap.

The analysis, as presented in the previous sections, focuses on explaining gender differences in expected earnings with a Master's degree. In contrast, in row (4), I use the pooled data, i.e. additionally include earnings expectations with a Bachelor's or a vocational degree. As shown in row (4) of Table 4.4.6, in this case, the raw gender gap in average expected earnings is higher compared to only focusing on earnings expectations with a Master's degree and equals 0.178 log points or 16.3%. Adding expected earnings risk, however, reduces the gender gap considerably from 0.178 to 0.056 log points. This reduction corresponds to almost 69% of the raw gender gap. Note that while using the pooled data, the share that can be attributed to differences in expected earnings risk is lower than in row (1), it still explains more than two-thirds of the gender gap in average expected earnings.

In the remainder of Table 4.4.6, I analyze how different sample restrictions and item non-response affect the role of expected earnings risk in explaining the gender gap in expected average earnings. In row (5) I include only students with complete information on all covariates, which results in a slightly larger gender gap (14.6%) and an increase in the share of the gender gap that is explained by differences in expected risk (83%). The results are similar when including students who have no intention to enroll in university (row 6). In the main specification, I only exclude earnings expectations that fell in the upper or lower one percentile of the cross-sectional distribution to account for outliers and simultaneously keep as many observations as possible. In row (7) I alternatively exclude the upper and lower five percentiles, while in the estimation in row (8) I use all available information. As shown, neglecting the outer tails of the cross-sectional distribution reduces the raw gender gap to 0.118 log points or 11.1%, while using the maximum number of observations increases the gap to 16.1%. In the latter case, accounting for differences in expected earnings risk yields a larger reduction in the gender gap than in the main specification or when focusing on earnings expectations between the 5th and the 95th percentile.

Finally, as outlined in Section 4.2, a large share of students did not answer the questions on their expected earnings. Table 4.2.1 suggests that this item non-response may not be at random. Thus, in row (9) I estimate a selection corrected model, as suggested by Heckman (1979). As exclusion restrictions I use the time each student spent on the questionnaire as well as a binary variable indicating whether a student is a stable panel member, which is defined as having participated in all five waves of the panel survey. These variables may contain information on how dedicated students are to the survey and, hence, how thoroughly they fill in the questionnaire.[28] Estimates of the selection corrected model are presented in row (9). The Wald-test fails to reject the hypothesis that the error terms in the estimation of expected average earnings and the selection equation are independent. Correspondingly, the key conclusion is not affected. Nevertheless, the gender gap in expected earnings increases to 0.167 log points or 15.4% in this model and expected earnings risk account for an even larger portion of this gap (87%).

Overall, the estimates in Table 4.4.6 suggest that the role of expected earnings risk in explaining gender differences in expected average earnings does not depend on the exact specification. Although the percentage contribution of expected earnings risk in explaining the gender gap in expected earnings varies, the general conclusion is unaffected: Expected earnings risk plays a crucial role in explaining the gender gap in expected earnings.

### 4.4.5 Are females just better informed?

The analysis presented so far provides evidence for the hypothesis that females deliberately trade off higher earnings for lower earnings risk. In this section I discuss an alternative hypothesis that could explain why females expect lower earnings and,

[28]Both variables predict response behavior on a statistically significant basis.

at the same time, expect lower earnings risk. I provide evidence that this explanation is unlikely to apply.

It is typically assumed that students base their earnings expectations on what they observe from currently active labor market participants. Further, several studies show that students tend to overestimate their own future earnings in relation to what current employees earn (Botelho and Pinto, 2004; Jerrim, 2015). Thus, it might be that females expect to earn less than males because they are better informed, i.e. their expected earnings are closer to what can be observed in the labor market. In this case, lower expected earnings risk may just reflect more accurate information. In order to explore this alternative hypothesis, I examine the importance of 'misinformation' considering all education scenarios. In Table 4.4.7, I show students' expected average earnings in the estimation sample (Panel A) and additionally report actual average earnings as calculated from the German Micro Census (Panel B).[29]

Difficulties in the comparison between expected earnings and actual earnings arise because the number of active labor market participants in Germany with a Bachelor's or a Master's degree is rather small for historical reasons.[30] It might be even less clear to students how the labor market rewards these relatively 'new' degrees in comparison to the previously awarded degrees. This may further contribute to the uncertainty students face when forming expectations about their future earnings. However, considering the years of education necessary to earn these different degrees, earnings with a Bachelor's degree should be closer to earnings with a vocational degree, while earnings with a Master's degree should be closest to earnings with the previous German tertiary degrees. As shown in Panel B of Table 4.4.7, where I additionally report average earnings for individuals with a tertiary university degree (i.e. pooled over all tertiary degrees), this is indeed the case.

[29]For the calculation of average earnings I only use the years 2010-2012. As we asked students about their expected earnings at the age of 35, the figures presented are based on individuals between 33 and 37 with an *Abitur*. Note that in the German Micro Census individuals only report their net income within specific bins, which might not necessarily consist only of labor market income. To account for this, I only include individuals who work full time and report that their main source of income derives from labor market earnings. Moreover, I use the midpoint of each bin to proxy actual earnings and divide monthly earnings by the hours an individual typically works during a month.

[30]The introduction of the two-tier system of Bachelor's and Master's degrees was only initiated in the academic year 2000/2001. This replaced the German degrees formerly awarded, like the *Diplom* or the *Magister*. Both degrees were awarded after completing 4-5 years of schooling; this is, roughly a Master's degree.

Table 4.4.7: Are females better informed?

| | Females | Males | Difference |
|---|---|---|---|
| **Panel A: Students' expected earnings (in EUR)** | | | |
| ...with a vocational degree | 1770 | 2098 | -328*** |
| ...with a Bachelor's degree | 2532 | 3109 | -577*** |
| ...with a Master's degree | 3865 | 4453 | -588*** |
| **Panel B: Actual average (population) earnings (in EUR)** | | | |
| ...with a vocational degree | 1760 | 2278 | -518 |
| ...with a Bachelor's degree | 2070 | 2739 | -669 |
| ...with a Master's degree | 2323 | 3092 | -769 |
| ...with a tertiary degree | 2325 | 3002 | -677 |
| **Panel C: Share of students overestimating earnings (in %)** | | | |
| ...with a vocational degree | 47.3 | 35.5 | 11.8** |
| ...with a Bachelor's degree | 67.3 | 56.8 | 10.5** |
| ...with a Master's degree | 82.0 | 76.0 | 6.1 |
| ...with a tertiary degree | 82.0 | 77.0 | 5.0 |
| **Panel D: Percentage deviations between expected and actual population earnings** | | | |
| ...with a vocational degree | 26.1 | 23.9 | 2.2 |
| ...with a Bachelor's degree | 35.8 | 30.7 | 5.2 |
| ...with a Master's degree | 72.8 | 51.9 | 20.9*** |
| ...with a tertiary degree | 72.7 | 55.0 | 17.7** |

*Notes:* This table presents differences between males and females. The difference in means is based on a two-sided t-test. The percentage deviation in Panel D is calculated as: $\Delta_i = (|y^e_{id} - \bar{y}^p_d|)/\bar{y}^p_d$, where $y^e_{id}$ represent student $i's$ expected earnings with education degree $d$ and $\bar{y}^p_d$ indicates actual average population earnings with education degree $d$. Calculations are based on data from Best Up as well as from the German Micro Census (2010-2012). * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Reverting to the question whether the observed gender differences in expected earnings could be partly due to females being 'better' informed, Table 4.4.7 reveals that this is unlikely. Panel C of Table 4.4.7 shows that the majority of students overestimate their own earnings (or are likely to overestimate their earnings).[31] Focusing on gender differences, females are even more likely to overestimate their earnings than males in all education scenarios. To account for the possibility of over- and underestimation, I additionally calculate the percentage absolute deviation between expected average earnings and actual population earnings on an individual basis (see Panel D).[32] Students' expected earnings with a vocational degree deviate on average by 26% for females and 24% for males. With respect to higher educational degrees, these deviations increase sharply. Comparing the deviations of expected

[31]The only exception is expected earnings with a vocational degree. One explanation for the smaller difference in expected earnings and actual earnings with a vocational degree relates to the sample of students under consideration. Recall that the survey oversampled students from lower educated backgrounds. Given this background it may be easier for students to observe individuals with a vocational degree than individuals holding a higher education degree. Hence, their information on population earnings with a vocational degree may be more accurate than their earnings expectations with a higher education degree.

[32]The percentage absolute deviation is calculated as: $\Delta_i = (|y^e_{id} - \bar{y}^p_d|)/\bar{y}^p_d$, where $y^e_{id}$ represent student $i's$ expected earnings with education degree $d$ and $\bar{y}^p_d$ indicates actual average population earnings with education degree $d$.

earnings with a Master's degree from population earnings with a tertiary degree shows that females' deviations on average amount to 73%, while males' deviations equal on average 55%. Thus, from Table 4.4.7 I conclude that females are certainly not better informed than males; if anything, females appear to be more poorly informed. Consequently, the gender gap in expected earnings and the role of expected earnings risk in accounting for this gap cannot be explained by females holding more realistic earnings expectations than males.

## 4.5 Conclusion

Several studies show that females start out with lower earnings expectations, even before entering the labor market, which partly translates into the actual gender wage gap. The main channels run through the effect of expected earnings on educational choice and the formation of reservation wages. This study investigates the gender gap in earnings expectations and provides evidence for a rarely tested explanation, namely anticipated compensation for earnings risk. Building on the theoretical reasoning of compensating wage differentials, this study investigates whether the gender gap in expected average earnings can be explained by differences in expected earnings risk. Based on actual labor market earnings, it is repeatedly shown that higher average earnings are positively related to higher earnings risk. This positive relationship suggests that females may deliberately trade off higher earnings for lower earnings risk.

Using a unique dataset in which we elicited information on the entire distribution of students' expected earnings, allows me to construct a measure of expected earnings risk by computing the individual-specific variance of expected earnings. The results of the empirical analysis can be summarized in three key findings. First, students anticipate compensation for earnings risk, i.e. higher expected average earnings are positively correlated with higher expected earnings risk. This finding supports the evidence provided by Schweri, Hartog, and Wolter (2011). Additionally, the extent of anticipated risk compensation does not differ by gender. Second, a considerably large share of the gender gap in expected average earnings can be explained by differences in expected earnings risk. For earnings expectations with a Master's degree this share amounts to three-quarters of the gender gap in expected average earnings. Females expect to earn less in all education scenarios and, simultaneously, expect lower earnings risk than their male counterparts. This observation cannot be explained by females being better informed about actual labor market earnings. Given the extensive set of additional covariates included in the analysis that cover alternative explanations for the gender gap, the importance of expected earnings risk in explaining the gender gap in expected average earnings is emphasized. And third, a supplementary analysis shows that gender differences in expected average earnings help to explain gender differences in college major choice. More specifically, differences in expected average earnings account for 20% of the gender differences in choosing a high paying college major.

Overall, the results of this study suggest that females expect to earn less because they are willing to trade off higher earnings for lower earnings risk. This finding provides supporting evidence for a conscious selection of females into lower paying careers in exchange for lower earnings risk. While the results of this article cannot be interpreted as causal evidence, it sheds further light on why women self-select into lower paying careers and provide a fruitful perspective for future research. A particularly interesting question is whether the self-selection process into different majors and/or occupations could be altered if earnings perspectives would be more stable. Providing causal evidence in this direction could have important policy implications.

## Appendix A: Relevance of earnings expectations

The relevance of the gender gap in expected earnings depends on whether these differences are significantly associated with other relevant choices that can partly account for the actual gender wage gap. For a sub-sample of students I can inspect whether gender differences in expected average earnings are related to gender differences in actual college major choice.[33]

Based on data from the German Micro Census (2005-2012), a representative dataset covering one percent of the German population, I calculate average hourly wages for each college major. Note that in the German Micro Census individuals only report their net income within specific bins, which must not necessarily consist of labor market income. To account for this, I only include individuals who work full time and report that their main source of income derives from labor market earnings. Moreover, I use the midpoint of each bin to proxy actual earnings and divide monthly earnings by the hours an individual typically works during a month. I further restrict the sample to individuals with *Abitur*. For illustration purposes, I group college majors into high, medium, and low paying fields according to their rank in terms of hourly wages. I then match students' chosen study programs to the classification of majors as implemented in the Micro Census and generate an indicator variable equal to one if a student chooses a high paying major and zero otherwise. The results of this estimation are presented in Table A4.1.

Table A4.1: Relevance of expected earnings

| Dep.var: Choosing a high paying college major | (1) | (2) | (3) |
|---|---|---|---|
| Female | -0.128** | -0.103** | -0.086 |
| | (0.049) | (0.048) | (0.067) |
| Average expected earnings | | 0.155** | 0.071 |
| | | (0.070) | (0.061) |
| Controls | No | No | Yes |
| Adj. $R^2$ | 0.0138 | 0.0301 | 0.0828 |
| N | 302 | 302 | 302 |

*Notes:* This table presents estimates from regressing a binary variable indicating whether a student chose a high paying college major on a female indicator (column 1) and expected average earnings (column 2). The full set of covariates includes all variables as described in Section 4.2. Standard errors in parentheses are clustered at the school level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

[33] Out of the 428 students in the analysis sample, about 72% enrolled in university within one year after completing high school (N= 308) and 71% provided information on their college major choice (N= 302).

The first column includes only the female indicator showing that females are 12.8 pp less likely to choose a high paying major. This estimate drops to 10.3 pp when additionally including students' (log) expected average earnings conditional on earning a Master's degree in the second column. Accounting for the expected average earnings decreases the gender gap in choosing a high paying college major by almost 20%, pointing toward a considerable role of earnings expectations in explaining gender differences in college major choice. Adding the full set of covariates in column (3) further reduces the gender gap to 8.6 pp. Although this difference is no longer statistically significant, the size of the gender gap is still large. The coefficient on expected earnings in column (2) is positive and statistically significant at the 5% significance level, indicating that a one percent increase in expected average earnings is associated with a 15.5 pp increase in the probability to choose a high paying major. Given the small sample size the coefficient on expected earnings loses statistical significance in column (3) when the full set of covariates is added. Nevertheless, the size of the coefficient still suggests that expected earnings are positively related to choosing a high paying major.[34]

The estimates in Table A4.1 show that higher expected average earnings are positively related to choosing a high paying major and that gender differences in expected earnings can partly explain gender differences in choosing these majors. Overall, Table A4.1 provides empirical evidence for the relevance of students' earnings expectations and their role in explaining gender differences in educational choices.

[34]Note that although higher expected earnings are significantly associated with choosing a high paying major, the overall explanatory power of the included variables is rather low as indicated by a relatively low adjusted $R^2$.

# Appendix B: Additional tables

Table A4.2: Explaining the gender gap in expected average earnings with a Bachelor's degree

| Dep.var: Log of expected mean earnings with a Bachelor's degree | Female coeff. (1) | Gender gap in % (2) | Adj.$R^2$ (3) | % of raw gap (4) |
|---|---|---|---|---|
| (1) Raw difference | -0.207*** (0.030) | 18.7% | 0.076 | 100% |
| (2) Baseline characteristics | -0.210*** (0.030) | 18.9% | 0.075 | -1.3% |
| (3) Performance and cognitive skills | -0.211*** (0.034) | 19.0% | 0.077 | -1.7% |
| (4) Intended college major | -0.202*** (0.039) | 18.3% | 0.070 | 2.2% |
| (5) Career motives | -0.198*** (0.035) | 18.0% | 0.100 | 4.0% |
| (6) Personality traits | -0.190*** (0.034) | 17.3% | 0.083 | 7.5% |
| (7) Preferences | -0.198*** (0.030) | 18.0% | 0.090 | 4.0% |
| (8) Confidence | -0.191*** (0.032) | 17.4% | 0.096 | 7.0% |
| (9) Expected earnings risk | -0.111*** (0.025) | 10.5% | 0.473 | 43.8% |
| (10) Full set of covariates | -0.077** (0.036) | 7.4% | 0.499 | 60.4% |
| N | 419 | | | |

*Notes:* This table presents estimates based on Equation 4.4. Each row presents a separate regression including only a female indicator and the set of covariates as indicated by the row label. The gender gap in expected earnings is reported in column (2) and shows the exact percentage difference, i.e. $100 * (e^{\hat{\beta}} - 1)$. The calculation in column (4) is based on the exact percentage changes, i.e. column (2), and shows how much of the gender gap can be explained by the particular set of covariates in each row. * $p$<0.1, ** $p$<0.05, *** $p$<0.01.

Table A4.3: Oaxaca-Blinder decomposition for expected earnings with a Bachelor's degree

| | Using pooled coefficent | | Using male coefficent | | Using female coefficent | |
|---|---|---|---|---|---|---|
| | Contribution (1) | % of gap (2) | Contribution (3) | % of gap (4) | Contribution (5) | % of gap (6) |
| Difference | 0.2068 | 100 | 0.2068 | 100 | -0.2068 | 100 |
| Explained | | | | | | |
| Baseline characteristics | -0.0014 | -0.69 | -0.0015 | -0.71 | -0.0028 | 1.38 |
| Performance and cognitive skills | -0.0019 | -0.94 | -0.0161 | -7.77 | -0.0145 | 7.00 |
| Intended college major | -0.0045 | -2.19 | -0.0083 | -3.99 | 0.0015 | -0.73 |
| Career motives | 0.016 | 7.75 | 0.0125 | 6.05 | -0.0246 | 11.89 |
| Personality traits | 0.0174 | 8.39 | 0.024 | 11.6 | -0.0116 | 5.63 |
| Preferences | 0.0007 | 0.34 | 0.0021 | 1.04 | -0.0007 | 0.33 |
| Confidence | 0.005 | 2.41 | 0.0052 | 2.54 | 0.0036 | -1.74 |
| Expected earnings risk | 0.0987 | 47.72 | 0.094 | 45.47 | -0.116 | 56.06 |
| Total explained | 0.1299 | 62.79 | 0.1122 | 54.22 | -0.1651 | 79.81 |

*Notes:* This table presents estimates of the Oaxaca-Blinder decomposition using different weighting schemes. The number of observations equals N=419. Standard errors in parentheses are clustered at the school level. * $p$<0.1, ** $p$<0.05, *** $p$<0.01.

Table A4.4: Explaining the gender gap in expected average earnings with a vocational degree

| Dep.var: Log of expected mean earnings with a vocational degree | Female coeff. (1) | Gender gap in % (2) | Adj.$R^2$ (3) | % of raw gap (4) |
|---|---|---|---|---|
| (1) Raw difference | -0.177*** (0.022) | 16.2% | 0.073 | 100% |
| (2) Baseline characteristics | -0.176*** (0.022) | 16.1% | 0.069 | 0.5% |
| (3) Performance and cognitive skills | -0.174*** (0.022) | 16.0% | 0.065 | 1.5% |
| (4) Career motives | -0.162*** (0.026) | 15.0% | 0.085 | 7.7% |
| (5) Personality traits | -0.152*** (0.027) | 14.1% | 0.070 | 13.1% |
| (6) Preferences | -0.175*** (0.022) | 16.1% | 0.073 | 1.0% |
| (7) Confidence | -0.166*** (0.022) | 15.3% | 0.084 | 5.7% |
| (8) Expected earnings risk | -0.110*** (0.021) | 10.4% | 0.322 | 35.8% |
| (9) Full set of covariates | -0.072*** (0.023) | 6.9% | 0.328 | 57.2% |
| N | 401 | | | |

*Notes:* This table presents estimates based on Equation 4.4. Each row presents a separate regression including only a female indicator and the set of covariates as indicated by the row label. The gender gap in expected earnings is reported in column (2) and shows the exact percentage difference, i.e. $100 * (e^{\hat{\beta}} - 1)$. The calculation in column (4) is based on the exact percentage changes, i.e. column (2), and shows how much of the gender gap can be explained by the particular set of covariates in each row. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A4.5: Oaxaca-Blinder decomposition for expected earnings with a vocational degree

| | Using pooled coefficent | | Using male coefficent | | Using female coefficent | |
|---|---|---|---|---|---|---|
| | Contribution (1) | % of gap (2) | Contribution (3) | % of gap (4) | Contribution (5) | % of gap (6) |
| **Difference** | 0.177 | 100 | 0.177 | 100 | -0.177 | -100 |
| Explained | | | | | | |
| Baseline characteristics | 0.0007 | 0.37 | 0.0013 | 0.74 | -0.001 | -0.56 |
| Performance and cognitive skills | -0.0023 | -1.33 | -0.0129 | -7.27 | -0.0109 | -6.17 |
| Career motives | 0.0142 | 8.05 | 0.0104 | 5.89 | -0.0153 | -8.63 |
| Personality traits | 0.0155 | 8.77 | 0.0261 | 14.74 | -0.0085 | -4.8 |
| Preferences | -0.0003 | -0.14 | 0 | -0.01 | 0.0005 | 0.3 |
| Confidence | 0.0062 | 3.53 | -.0017 | -0.93 | -0.0039 | -2.22 |
| Expected earnings risk | 0.0709 | 40.03 | 0.0763 | 43.12 | -0.065 | -36.72 |
| Total explained | 0.1049 | 59.28 | 0.0996 | 56.27 | -0.1041 | -58.81 |

*Notes:* This table presents estimates of the Oaxaca-Blinder decomposition using different weighting schemes. The number of observations equals N=401. Standard errors in parentheses are clustered at the school level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A4.6: Explaining the gender gap in expected average earnings: Full regression results

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| Female | -0.154*** | -0.161*** | -0.158*** | -0.137*** | -0.143*** | -0.145*** | -0.147*** | -0.134*** | -0.035 | -0.003 |
| | (0.038) | (0.037) | (0.043) | (0.044) | (0.040) | (0.043) | (0.038) | (0.041) | (0.027) | (0.031) |
| **Background characteristics** | | | | | | | | | | |
| Information intervention | | 0.004 | | | | | | | | -0.003 |
| | | (0.051) | | | | | | | | (0.027) |
| Migration background | | 0.104** | | | | | | | | 0.041 |
| | | (0.049) | | | | | | | | (0.030) |
| Non-academic fam.backgr. | | -0.043 | | | | | | | | 0.026 |
| | | (0.038) | | | | | | | | (0.028) |
| Academic high school | | 0.030 | | | | | | | | 0.003 |
| | | (0.049) | | | | | | | | (0.029) |
| Integrated high school | | 0.027 | | | | | | | | 0.001 |
| | | (0.052) | | | | | | | | (0.026) |
| **Cognitive skills** | | | | | | | | | | |
| Final high school GPA | | | -0.013 | | | | | | | 0.003 |
| | | | (0.021) | | | | | | | (0.016) |
| Verbal cognitive skills | | | -0.005 | | | | | | | -0.011 |
| | | | (0.026) | | | | | | | (0.020) |
| Figural cognitive skills | | | -0.014 | | | | | | | -0.014 |
| | | | (0.021) | | | | | | | (0.015) |
| **Intended college major** | | | | | | | | | | |
| Social Sciences | | | | -0.282* | | | | | | -0.245** |
| | | | | (0.158) | | | | | | (0.102) |
| Business and Economics | | | | 0.065 | | | | | | -0.091 |
| | | | | (0.114) | | | | | | (0.080) |
| STEM | | | | 0.072 | | | | | | -0.019 |
| | | | | (0.118) | | | | | | (0.069) |
| Teaching | | | | 0.132 | | | | | | 0.073 |
| | | | | (0.125) | | | | | | (0.064) |
| Law & Management Sciences | | | | 0.150 | | | | | | -0.008 |
| | | | | (0.128) | | | | | | (0.101) |
| Medicine | | | | 0.021 | | | | | | -0.087 |
| | | | | (0.117) | | | | | | (0.083) |
| Psychology | | | | 0.086 | | | | | | -0.037 |
| | | | | (0.155) | | | | | | (0.094) |
| Arts and Sports | | | | -0.094 | | | | | | -0.087 |
| | | | | (0.174) | | | | | | (0.110) |
| Others | | | | -0.052 | | | | | | -0.102 |
| | | | | (0.198) | | | | | | (0.115) |
| **Career motives** | | | | | | | | | | |
| Extrinsically motivated | | | | | 0.088*** | | | | | 0.029** |
| | | | | | (0.022) | | | | | (0.012) |
| Intrinsically motivated | | | | | -0.007 | | | | | -0.019 |
| | | | | | (0.024) | | | | | (0.016) |
| Socially motivated | | | | | -0.024 | | | | | -0.011 |
| | | | | | (0.026) | | | | | (0.012) |
| Work-Life-Balance motivated | | | | | -0.022 | | | | | -0.007 |
| | | | | | (0.026) | | | | | (0.015) |
| **Personality tratis** | | | | | | | | | | |
| Openness | | | | | | -0.022 | | | | -0.024 |
| | | | | | | (0.019) | | | | (0.016) |
| Extraversion | | | | | | 0.037 | | | | 0.011 |
| | | | | | | (0.022) | | | | (0.014) |
| Conscientiousness | | | | | | 0.019 | | | | 0.014 |
| | | | | | | (0.025) | | | | (0.019) |
| Neuroticism | | | | | | 0.005 | | | | -0.009 |
| | | | | | | (0.020) | | | | (0.016) |
| Agreeableness | | | | | | -0.033* | | | | -0.023** |

continued on next page

**Table A4.6 – continued from previous page**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | (0.017) | | | | (0.009) |
| External Locus of Control | | | | | | -0.019 | | | | 0.019 |
| | | | | | | (0.020) | | | | (0.014) |
| Internal Locus of Control | | | | | | 0.046 | | | | 0.035** |
| | | | | | | (0.027) | | | | (0.016) |
| **Preferences** | | | | | | | | | | |
| Riskaversion | | | | | | | -0.047** | | | -0.005 |
| | | | | | | | (0.021) | | | (0.015) |
| Patience | | | | | | | -0.017 | | | -0.011 |
| | | | | | | | (0.020) | | | (0.015) |
| Time preference for present | | | | | | | -0.024 | | | -0.013 |
| | | | | | | | (0.020) | | | (0.013) |
| **Confidence** | | | | | | | | | | |
| Confidence in own ability | | | | | | | | 0.100* | | 0.034 |
| | | | | | | | | (0.052) | | (0.033) |
| Self-confidence | | | | | | | | 0.027 | | 0.008 |
| | | | | | | | | (0.017) | | (0.010) |
| **Expected earnings risk** | | | | | | | | | | |
| (log) Variance of expected earnings | | | | | | | | | 0.171*** | 0.170*** |
| | | | | | | | | | (0.009) | (0.010) |
| Cons. | 8.312*** | 8.269*** | 8.318*** | 8.270*** | 8.299*** | 8.304*** | 8.308*** | 8.140*** | 6.240*** | 6.190*** |
| | (0.028) | (0.064) | (0.035) | (0.099) | (0.031) | (0.034) | (0.028) | (0.097) | (0.111) | (0.124) |

*Notes:* This table presents estimates based on Equation 4.4.* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Chapter 5

# Conclusion

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This dissertation investigates determinants of college enrollment and provides empirical evidence on how this choice is affected by various aspects. It acknowledges the complexity of this choice by considering the three different levels of influencing factors – the individual (Chapter 4), the environmental (Chapter 2) and the institutional (Chapter 3) levels. As with all empirical studies, the results of this dissertation must be interpreted in light of several limitations that are discussed in the following. Moreover, I highlight the policy implications of each chapter and point toward directions for future research.

***Chapter 2*** investigates how the effect of students' family background on enrollment decisions can be reduced by evaluating a randomized field experiment in which students in randomly selected high schools were given information on the benefits of as well as on different funding possibilities for college education. The results of this chapter show that the provision of information increases intended college enrollment for students from a non-academic family background, in the short and medium run. In contrast, it leads students from academic backgrounds to lower their enrollment intentions in the short run, whereas in the medium run no statistically significant effects can be detected.

While the analysis in Chapter 2 draws on a randomized field experiment, which generally ensures a high internal validity, the main caveat of this study relates to its external validity. The randomized field experiment was implemented in Berlin, a large city with around 50 higher education institutions. The proximity to, and the extensive supply of, different institutions may partly contribute to the positive effect for students from non-academic family backgrounds as it is shown that the distance to higher education institutions affects enrollment choices (e.g. Spieß and Wrohlich, 2010). It remains unclear whether the findings hold in other, less urban, settings or more generally in other parts of Germany. Moreover, selected schools were located in districts with a high share of low-educated individuals. This was necessary to increase the likelihood of sampling sufficient students originating from non-academic families. However, it implies that differences between students with different educational backgrounds are likely to be underestimated as students from academic backgrounds in this sample are likely to be more similar to their peers from non-academic backgrounds than to other students from academic families who attend a high school in a more well-off neighborhood.

In addition, although a causal effect of information provision is found, the question of which specific information triggered this result, is less clear. The information treatment consisted of a bundle of information on post-secondary education. Hence, we cannot ascertain whether information on labor market benefits or funding options persuaded students from non-academic families to pursue college education. Conducting a randomized field experiment with different treatment arms and providing students either with information on the benefits or funding options could shed more light on this question. Furthermore, the study in Chapter 2 would benefit from a larger sample size. This would not only allow to estimate more precise effects through increased statistical power, but at the same time enable us to investigate further heterogeneities within the subgroups of students from different educational backgrounds. Examining the treatment effect by academic performance or gender would, for example, be particularly interesting analyses. However, cell sizes quickly become too small to yield reliable results. Finally, a natural and interesting extension of this study would be to follow students and not only observe their actual college enrollment but also their progress in college and ultimately examine whether they obtain a college degree.

From a policy perspective, the results of Chapter 2 bear interesting implications. The fact that students from different educational backgrounds respond in opposite direction to the information treatment in the short term, suggests that the information sets of students may indeed be biased toward the educational level that prevails in their environment. Where students from non-academic family backgrounds may lack information about university education, students from academic backgrounds may have an information deficit about options other than university education. It might be that for students from academic backgrounds, raising the awareness for alternatives to university education and providing further information on vocational education may indeed induce them to choose this path.[1]

With respect to educational inequality, the findings of Chapter 2 suggest that the 'education gap' – measured by the difference in students' intended college enrollment by parental educational – can be reduced by providing students with relevant information. Thus, a tailored information workshop may indeed be an appropriate and inexpensive policy tool to narrow the gap in take up of university education. However, instead of only focusing on increasing college enrollment for students from non-academic backgrounds, it seems similarly important to inform students from academic backgrounds about the alternatives to college education. This way, students' post-secondary educational choices may more generally become less background dependent and educational matches may improve.

***Chapter 3*** examines how the length of secondary schooling affects students' college enrollment choice by using a secondary school reform as a quasi-experiment. Exploiting the variation of the reform implementation over time and across states in a difference-in-differences approach, this chapter shows that reducing the length of secondary schooling while simultaneously increasing instruction hours in the remaining years leads to a significant decrease in college enrollment rates. Moreover, students are more likely to delay their enrollment, to drop out of university, and to change their major.

The reliability of these results rests on the plausibility of the common trend assumption that is necessary to identify causal effects. A comprehensive set of additional specifications and robustness checks support the validity of this assumption. As such, the effects can be credibly interpreted as causal effects of the reform. The main caveat of this analysis is that we cannot provide concrete and robust evidence on the channels through which the reform affects enrollment choices. While Chapter 3 includes a section that aims to shed some light on the mechanisms, the results of this analysis remain suggestive. If policy makers intend to mitigate the negative effects on enrollment rates, further research is needed to provide more robust evidence on starting points for suitable interventions. Relatedly, using administrative data

[1]In this regard it should be considered that enrolling in the German vocational education system, especially in the dual system, requires more timely effort and initiative from students than enrolling in college. Hence, familiarizing students with labor market returns and funding options for different post-secondary educational options is likely to be most effective if information is provided no later than the beginning of the penultimate year of high school (*Oberstufe*).

covering the universe of students comes with the drawback of lacking further information on student characteristics. This prevents us from investigating further effect heterogeneities by, for example, performance or family background. More detailed subgroup analyses would, however, provide insights on which students have difficulties in coping with the higher workload and should correspondingly be specifically targeted.

In addition, the effects on the timing of enrollment and study progress during the first year of college education have to be interpreted in light of the reform induced reduction in general enrollment rates (conditional-on-positives interpretation). These outcomes are only defined for students who enroll in college. Thus, potentially it could be that the effect on the timing of enrollment and regular study progress are partly explained by compositional changes in the population of enrollees. This concern may be more relevant for the timing of enrollment than for regular study progress. Given that we find a negative effect on regular study progress, we would expect the compositional change to increase the share of comparatively low-performing students. However, it seems rather unlikely that the reform leads comparatively high-performing students to refrain from college enrollment; yet, if it is the lower performing students who decide not to enroll, the general student body at university would be better performing on average. Hence, compositional changes are less likely to explain the effects on regular study progress. Nevertheless, this limitation must be kept in mind when interpreting the findings. Strictly speaking, the effects on the timing of enrollment and regular study progress can only partly be causally tied to a reduction in the length of secondary schooling, even though they can be interpreted as causal effects of the reform. This concern does not apply to the effect on college enrollment as the reform did not affect high school graduation rates (see Appendix of Chapter 3).

Apart from investigating the effect of the length of secondary schooling on college enrollment, the analysis in Chapter 3 contributes to the evaluation of one of the largest German education reforms since reunification in 1990. The goal of the reform was to allow for an earlier labor market entry without affecting students' human capital.[2] From that angle, the effects on higher education choices constitute unintended consequences of the reform that need to be taken into account when evaluating the main goal of the reform. The fact that, under the new regime, students

[2]This was precisely why the number of instruction hours was kept unchanged.

are more likely to delay their enrollment and change their college major implies that the potential of a full-year reduction in age at labor market entry will not be fully realized.[3] Moreover, as students are less likely to enroll in college and more likely to drop out of it due to the reform, their formal level of human capital at labor market entry will be lower. Considering that German policy makers aim to increase the number of university graduates, the results of this chapter point toward rather negative effects of the reform.[4] Nevertheless, students under the new regime will most likely enter the labor market earlier than students under the old regime, on average. While some federal states have already decided to switch back to the old regime, others are still discussing this possibility. The results of this chapter may contribute to a more informed debate on this topic.

Finally, the results of this chapter are not only informative for the German context, but also for policy makers in other countries who are trying to increase the number of active labor market participants in order to address the challenges of an aging society. Generally, policy makers face a trade-off between an earlier labor market entry and constant levels of education. Thus, increasing education efficiency by reducing the years of schooling and simultaneously increasing weekly instruction hours sounds like a tempting policy option. However, the empirical evidence presented in Chapter 3 shows that this policy might not come without unintended consequences regarding students' higher education decisions.

***Chapter 4*** builds on the theoretical reasoning of compensating differentials and shows that a large part of the gender gap in expected earnings can be explained by differences in expected earnings risk as measured by the individual-specific expected earnings dispersion. This relationship suggests that females are willing to trade off higher earnings for lower earnings risk.

While the analysis in this Chapter is the first to examine the link between the gender gap in earnings expectations and expected earnings risk, thereby assessing whether educational choices are driven by anticipated compensation for earnings

[3]An earlier study on the effects of the reform shows that even at high school graduation, the age of students is only reduced by ten months, not the expected twelve months (Huebener and Marcus, 2017).

[4]This is not to say that the reform necessarily affects individual students negatively. It is unclear which long-term positive benefits this reform may entail for students who, for example, used the time between high school graduation and college enrollment to spend a year abroad or perform voluntary services. In addition, even students who refrain from college enrollment due to the reform, may be better off with this choice as it might increase their educational match.

risk, the study has several caveats. The relatively small number of observations makes statistical inference challenging. A larger sample size would allow for more precise estimates and an in-depth analysis of heterogeneities. Examining heterogeneities across the distribution of expected earnings would be particularly informative. For example, studying whether anticipated compensation for earnings risk is more important at the upper than at the lower end of the earnings distribution could yield additional insights as to why women might be underrepresented at the upper end of the earnings distribution. Similarly, examining whether gender differences in earnings expectations are generally larger at the upper end and smaller at the lower end of the earnings distribution could provide evidence on whether the 'glass ceiling effect' is already prevalent based on earnings expectations. Related to the small sample size, the study suffers from a relatively high item non-response rate that is not random. Although correcting for this sample selection does not change the results of this study, concerns about the selectivity of the sample remain. In addition, the selectivity prevents an analysis of the relationship between expected earnings and the decision to enroll in college and its importance in accounting for gender differences in the enrollment choice; instead the analysis looked at the broader enrollment choice by considering which major to enroll in.

The results of this chapter highlight the relevance of risk in educational choices. A natural extension of the analysis in Chapter 4 is to examine whether educational choices of females can be changed by altering their perception of earnings risk. One possibility would be to confront students with different hypothetical choice scenarios that vary with respect to average earnings and earnings risk. Observing gender differences in these choices could potentially shed more light on whether females are indeed willing to trade off higher earnings for lower earnings risk.

On a more survey methodological note, the elicitation of expected earnings risk should receive more attention in future research. As this study is among the very few to elicit not only information on average expected earnings but on the entire earnings distribution, a thorough validation of the instrument itself is needed. A starting point would be to use the two existing approaches to elicit expected earnings risk on the same individuals and document differences regarding expected earnings risk. Beyond testing different elicitation approaches, it is more generally necessary to determine to what extent the elicitation approach is affected by (over)confidence, optimism and general risk attitudes. Disentangling these aspects is important in order to enhance interpretation of this measure. The results of this study suggest, however, that expected earnings risk does not only reflect risk attitudes or confidence.

The policy implications of Chapter 4 are, in contrast to Chapter 2 and 3, less clear. While the result of this chapter show that expected earnings risk is an important factor in explaining gender differences in educational choices, this does not imply that altering expected earnings risk would change the gender specific choice pattern. However, the analysis points toward the possibility that the gender specific occupational sorting in the labor market may not only result from differences in tastes for job content or preferences for workplace flexibility, which is often emphasized, but also from preferences for low earnings risk. Given that the literature documents that females are more risk averse than males, it may be that females choose occupations that exhibit low earnings risk and, as a consequence of compensating differentials, earn less. The analysis in Chapter 4 shows that students' earnings expectations reflect this relationship and that it accounts for a considerably large share of the gender gap in expected earnings. Thus, it provides a fruitful direction for future research that is needed in order to draw more concrete policy implications on how to achieve the goal of reducing the gender wage gap.

Despite the aforementioned caveats, this dissertation contributes to the literature on higher education in several ways. First, it provides empirical evidence on the enrollment choice in Germany, which adds to the literature that largely focuses on English-speaking countries, in particular the United States. Given the vastly different institutional setting in these countries, it is unclear whether, and to what extent, the findings in the literature can be extrapolated to the German context.

Second, this dissertation also considers and combines different strands of the higher education literature, as highlighted by the diversity of aspects examined in each chapter. Chapter 2 not only adds to the literature on educational inequality at the transition to college education but also on the literature considering the role of information in educational choices more generally. Chapter 3 contributes to the literature on how the institutional structure of secondary schooling affects higher education decisions, while Chapter 4 extends our knowledge on the role of earnings expectations and combines this literature with the literature on the gender wage gap.

Third, the thesis further demonstrates the different methodological approaches that can be used to investigate specific research question. It applies experimental (Chapter 2) and quasi-experimental estimation methods (Chapter 3) to identify causal effects, as well as a decomposition analysis (Chapter 4) to shed light on the relevance of a rarely studied explanation.

Fourth, by using different data sets, this dissertation exemplifies the benefits of using various types of data: The analysis in Chapter 3 is based on administrative data covering the universe of students that, despite its apparent advantages, is not frequently used in the literature. The construction of new outcome variables therefore provides guidance for other researchers interested in using this data set to analyze college enrollment choices in Germany. In addition, the data set used in Chapter 2 and 4 was collected in the context of a larger project called *"Berliner Studienberechtigten Panel"* and provides unique insights in the enrollment choice of students. This is the first German study to examine the effect of providing students with information on the benefits and funding options for college education in the framework of a randomized field experiment. Furthermore, as a part of this project team, I could include survey questions designed to elicit students' earnings expectations and their expected earnings risk – a topic that can hardly be analyzed with other data sets.

And fifth, this dissertation provides not just empirical evidence on the effectiveness of a potential policy measure to reduce educational inequality at the transition to college education (Chapter 2), but it also contributes to the evaluation of a recent policy reform *(G8 reform)* that shortened the length of secondary schooling at academic high schools (Chapter 3). This reform is still heavily discussed in several states and the results of Chapter 3 may further inform this debate.

To conclude, there are several aspects that unfold from this dissertation for future scope of action from researchers and policy makers alike. With this work I contribute to a better understanding of the determinants of college enrollment and to the discussion on how policy interventions can shape this choice.

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