Determinants of Human Capital Formation

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Roughly five years ago, I started the doctoral program at the BGSE with a mindset towards research that can be best described as uninformed optimism.[1] In the years that followed, reality set in, projects did not always work out as expected, and I encountered occasions of pessimism and a few times even despair. Yet, these difficult moments were also the times in which I grew the most, both academically as well as personally. Moreover, the hard times have brought me the informed optimistic view on research that I have today, and for which I am grateful. Nevertheless, this all would not have been possible if it were not for the people who stood by me along the way.

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[1] Common phases of a PhD are described in Julio Peironcely's blog on NextScientist.com

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Chapter 1

Introduction

Human capital is a key source of economic growth in current developed societies. More than 70 percent of the total amount of wealth in high-income OECD countries stems from human capital (Hamilton et al., 2018). Likewise, cross-country differences in GDP growth can often be traced back to variation in cognitive skill levels (Hanushek and Woessmann, 2012). Also from an individual's perspective accumulated human capital is important for economic prosperity. Key later-life outcomes such as labor earnings and probability to be unemployed are increasingly dependent on an individual's human capital (Acemoglu and Autor, 2011). As such, differences in human capital are largely responsible for existing inequalities within society, making it essential to understand the determinants of individual human capital formation.

The foundations of economic research regarding human capital formation are made by the work of Becker (1962). Human capital can be seen as broad as an individual's knowledge, skills, ideas, and health that improves the efficiency of the human factor, and Becker was one of the first to approach this as an economic concept. He saw human capital no different than any other type of capital, in terms of the investments that can be made and the returns it generates. His original human capital investment models focus on investments in education, which is one of the main contributors to human capital. In these models individuals are assumed to invest in education until their marginal returns equal their marginal costs, and variation in investment levels mainly comes from individuals facing different financial constraints (Becker, 1994).

Since the development of the original human capital investment models there has been an enormous increase of economic studies that focus on gaining a better understanding of why individuals accumulate different levels of human capital. The prevalent explanations in the literature can roughly be divided along two dimensions. First, there may be obstacles or constraints on the investment side that hinder people to accumulate human capital. Second, variation in human capital formation may be explained by differences in returns between individuals, both in terms of actual and expected returns to obtaining human capital.

This dissertation presents three empirical papers that explore various hypotheses related to both the investment and return dimension, of why there may be differences in individual human capital formation: Do people expect returns from accumulating human capital, and if so where do they belief these returns originate from? Can temporary events of distress in childhood have longstanding negative consequences on human capital formation in the presence of standardized tests? Do worsening local economic conditions incentivize parents to invest in children's human capital? By answering these questions, this dissertation contributes to coming a step closer to identifying determinants of human capital formation.

Chapter 2 provides new insights to the long-standing debate between human capital formation versus signaling as an explanation for returns from attending higher education. According to the signaling theory obtaining an educational degree does not form human capital, it merely reveals it, as only high-ability individuals can obtain the signal (Spence, 1973). The paper is joint work with Laura Ehrmantraut and Pia Pinger, and its contribution is twofold: first, we estimate the perceived premium to obtaining higher education for university students; second, we investigate whether students ascribe the premium to acquired human capital or the signaling value of the degree. Accordingly, we conducted a survey among a large and diverse sample of German students at different stages of higher education to elicit counterfactual labor market expectations for the hypothetical scenarios of leaving university with or without a degree. These expectations are collected for the time when individuals start their first job and at age 40 and 55 to explore developments throughout the working life.

Our findings indicate substantial perceived returns to finishing higher education, not only in terms of earnings but also with respect to job satisfaction and the probability of finding a suitable job. To estimate the perceived importance of signaling in generating these returns, we employ a within-individual fixed effects model. This strategy circumvents selection bias between university-leavers and university-graduates, as it compares the leaving and graduating scenario within individuals. We document that the perceived returns from signaling are substantial, as obtaining a degree raises returns by roughly 20 percent, whereas one more semester of accumulating human capital in university does not significantly raise returns. Moreover, the importance of signaling at the start of one's career is expected to largely persist over an individual's working life. As the findings show that people expect an extensive part of the returns to come from signaling, differences in human capital formation may partially reflect that people have different inherent abilities and obtain different signals.

Chapter 3 contributes to the extensive stream of literature that explains differences in human capital investments by the existence of various obstacles, such as constraints related to income, time, attention, or institutions. In this chapter I look at how the consequences of another obstacle, namely experiencing an event of temporary family distress, may be aggravated or diminished by prevalent features of education systems. In particular, I investigate how children's educational outcomes are affected by experiencing a common form of family distress - the death of a grandparent - shortly before taking a high-stakes standardized test. I use administrative registers from the Netherlands, as the Dutch context bears the advantage of combining an objective standardized test with a subjective teacher track recommendation to determine secondary school track placement. To obtain causal estimates I exploit the quasi-random timing of grandparental death in the three-month window surrounding the track placement test, and compare children losing a grandparent before the test to children losing a grandparent after.

The results show that grandparental loss at an unfortunate time leads to reduced test performance of roughly 3 percent of a standard deviation. Moreover, the subjective teacher recommendation does not compensate for this decreased performance, as children who lost a grandparent before the test also receive a lower track advice. Accordingly, treated children have an increased likelihood of being placed in the lowest track of secondary education. The possibility to participate in a makeup test and switch tracks later-on does mitigate part of the negative effects, but are not able to fully offset the initial setback. Hence, four years after the loss of a grandparent children in the treatment group still have a higher probability to attend the lowest track. The findings underline that even a relatively mild event of family distress can have lasting negative consequences on educational attainment in a context with high-stakes standardized testing, and that it is hard to recover from the initial setback. Consequently, differences in human capital as an adult can stem from temporary disadvantages at crucial times during childhood.

Finally, chapter 4 relates to recent emerged studies that focus on what incentivize people to invest in human capital rather than looking at what may prevent them from doing so. The main underlying idea is that returns from human capital may depend on a person's circumstances and surroundings (see e.g. Doepke and Zilibotti, 2017). Chapter 4 follows this intuition and analyses how parental investments respond to economic incentives set by a family's living environment. I investigate whether the regional unemployment rate influences investments related to children's human capital. The hypothesis is that higher unemployment rates raise the importance of human capital as it becomes harder to find a job, thereby stimulating parents to invest in their children. I use data from the German Socio-Economic Panel, and employ a regional- and time-fixed effect approach to circumvent that the results are driven by unobserved invariant heterogeneities.

The outcomes show that an increase of the unemployment rate in a family's federal state, raises measures of maternal support, academic interest and homework assistance. Furthermore, the responsiveness of parenting behavior on economic incentives differs by parental and child background characteristics, such as maternal locus of control and secondary school track recommendation. The results indicate that the local economic environment can provide incentives to invest in cognitive skills, which may explain differences in human capital formation in the long-run.

Each chapter in this dissertation addresses a small part of why we may observe differences in human capital between individuals. Taken together, this dissertation emphasizes that we need a comprehensive view and consider all aspects of the accumulation process to be able to identify the determinants of human capital formation.

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Chapter 2

The (Expected) Signaling Value of Higher Education

Joint with Laura Ehrmantraut and Pia Pinger

2.1 Introduction

Higher education is a major determinant of labor earnings as university graduates earn substantially more over the life cycle than individuals with a high-school degree (Cunha et al., 2011; Piopiunik et al., 2017; OECD, 2017). The importance of education for labor market outcomes is reflected in economic theory (Becker and Chiswick, 1966; Mincer, 1958, 1974) and has been documented in a vast body of empirical literature (for reviews see, e.g. Card, 1999; Patrinos and Psacharopoulos, 2020). Moreover, many recent papers show that individuals are aware of existing returns and adopt their educational decision-making accordingly (McMahon and Wagner, 1981; Manski, 2004; Delavande and Zafar, 2019).

Nonetheless, the sources of the education premium are less well understood. After all, education may both enhance productivity as well as reflect it. On the one hand, the human capital hypothesis (Becker, 1962; Schultz, 1963; Mincer, 1974) states that education augments productivity because individuals acquire knowledge and useful skills during their studies. On the other hand, the signaling hypothesis pioneered by Spence (1973) and Stiglitz and Weiss (1990) advocates that education is merely a signal of productivity. Here, the (psychic) costs of education correlate with worker productivity such that a separating equilibrium emerges where high-productivity individuals use education as a signal to earn higher wages and firms screen workers for their education to attract high-productivity-type workers. [1]

[1] A third hypothesis states that (higher) education premia arise because university attendance is a screening or selection device that induces students to resolve uncertainty about their individual returns. According to this presumption, only those students with sufficiently large returns decide to finish a degree (Chiswick, 1973; Lange and Topel, 2006).

The empirical evidence on the relative importance of human capital versus signaling for (higher) education premia remains inconclusive (Patrinos and Psacharopoulos, 2020). While some studies report findings mostly in support of the human capital hypothesis (e.g. Layard and Psacharopoulos, 1974; Chevalier et al., 2004; Kroch and Sjoblom, 1994; Aryal et al., 2019) others report substantive evidence of signaling effects (e.g. Hungerford and Solon, 1987; Jaeger and Page, 1996; Park, 1999; Bedard, 2001; Chatterji et al., 2003; Caplan, 2018). This discrepancy arises because both theories are largely observationally equivalent: *Ex-post*, individuals with education credentials are more productive, which entails a positive relation between education on wages.[2]

In this paper, we circumvent this identification problem and provide new evidence on the perceived *ex-ante* signaling value to higher education. In particular, we ask two questions: Do students recognize considerable premia to obtaining higher education? If so, do they ascribe them to the human capital acquired or the signaling value of the degree certificate?

To answer these questions, we have collected new data on subjective pecuniary and non-pecuniary returns to finishing higher education in a large and diverse sample of students currently enrolled at a university or college of applied sciences in Germany. In particular, we elicit expected wage information among individuals who are at different stages of higher education for the hypothetical scenarios of leaving university with or without a degree certificate. Besides, the data also comprise information on expected job satisfaction, the probability of finding a suitable job, expected working hours, and a large array of background variables. All expectations were elicited for the time when individuals start working and at two later points in the life cycle (at the age of 40 and 55). The data thus allow us to circumvent selection and estimate *ex-ante within-individual* graduation premia as well as to distinguish between the *perceived* signaling and human capital values of higher education.

The analysis proceeds in three steps. First, we provide general evidence on the expected returns to continued higher education, including estimates of the perceived lifetime return on investment and the perceived internal rate of return. Second, using expected wages for counterfactual scenarios of leaving university with or without a degree, we estimate within-person fixed effects models to obtain perceived wage and non-wage (job satisfaction, probability of finding a suitable job) signaling and human capital values of education. As part of this analysis, we also unveil the perceived long-term development of the graduation premium, i.e. the expected persistence of signaling and the respective perceived speed of employer learning. Third, we investigate heterogeneities in the signaling value and the importance of returns for leaving university without a degree.

[2]For a long time, this identification problem seemed insurmountable. As an example, Lang and Kropp (1986, p. 609) write: "[M]any members of the profession maintain (at least privately) that these hypotheses cannot be tested against each other and that the debate must therefore by relegated to the realm of ideology." See also Huntington-Klein (2020).

Our estimates for master students indicate high perceived individual returns to degree completion, with an average discounted lifetime return of €334,400. Moreover, the model parameters from a within-person fixed effects analysis suggest a signaling value of roughly 20% in terms of wages, more than a standard deviation in terms of job satisfaction, and more than half of a standard deviation regarding appropriate employment. At the same time, the estimated human capital value is very small and mostly not significantly different from zero. We thus observe a considerable perceived labor market advantage of an individual who recently received a credential over someone who is just about to receive it. We also find lasting effects of the graduation signal, whereby even individuals with a high subjective on-the-job productivity do not believe that employer learning will outweigh the initial signaling value of a degree in the longer run. Finally, we find that the expected earnings premium plays a rather small role in the choice to leave university without a degree. Instead, variables that proxy for student satisfaction and psychic costs hold stronger importance. This finding is congruent with a large body of literature documenting small educational choice responses to monetary incentives (e.g. Arcidiacono, 2004; Beffy et al., 2012; Wiswall and Zafar, 2015). It is also in line with the signaling hypothesis, which implies that the decision to select out of education should be driven by the (psychic) cost of education only, and not the potential earnings gained from finishing.

Whether education premia arise due to human capital augmentation or signaling holds important implications for young people's motivation to obtain higher education, as well as their educational decision-making. If education merely increases productivity, then for individuals who want to work in a high-productivity job or position, attending higher education (or at least studying the material) is without alternative. However, if education only relates to signaling, high-productivity types will only obtain a degree if there is no other cheaper (but equally credible) way to document their future productivity. Similarly, if signaling prevails, leaving a higher educational institution just before finishing a degree is very costly in terms of later wages, while it should matter little under the human capital hypothesis.[3]

The analysis in this paper builds upon and extends prior work regarding the importance of so-called graduation premia, signaling, diploma, or sheepskin effects (see e.g. Weiss, 1995; Lange and Topel, 2006, for reviews). Part of this research relies on a matching assumption for identification, as researchers regress wages on the number of years of schooling and degree attainment and then interpret the wage differential between degree and non-degree workers conditional on years of schooling as signaling (Hungerford and Solon, 1987; Frazis, 1993; Ferrer and Riddell, 2002; Jaeger and Page, 1996; Park,

[3]The type of regime also has implications for societal investments. For example, if education augments human capital, society may subsidize it to reap positive externalities in the form of productive worker interactions, better citizenship, or knowledge spillovers. If education is simply a means to convey information, society might as well leave it to the individual to pay for it, unless it effectively reduces uncertainty about the quality of labor input to firms, which may increase total output (Wolpin, 1977).

1999). [4] Another part uses instruments or discontinuities to identify the graduation premium for individuals at the margin (see e.g. Acemoglu and Angrist, 1999; Tyler et al., 2000; Clark and Martorell, 2014; Barrera-Osorio and Bayona-Rodríguez, 2019). Similarly, some papers exploit changes in the curriculum, years, or intensity of schooling to investigate exogenous changes in the human capital accumulation process on wages (see e.g. Arteaga, 2018; Goodman, 2019). Our approach differs from this literature in two respects. First, we only look at the supply side, e.g. by estimating signaling effects among (future) labor market participants, thus abstracting from equilibrium effects. Second, we estimate the graduation premium from within-person variation, enabling us to estimate average instead of local effects.

This paper also adds to various strands of the literature on subjective expectations. In particular, it relates to work on the role of expectations of returns when making educational decisions, such as starting tertiary education (Boneva and Rauh, 2017; Attanasio and Kaufmann, 2014, 2017), major and occupation choice (Arcidiacono et al., 2017; Wiswall and Zafar, 2015) or completing tertiary education (Stinebrickner and Stinebrickner, 2014; Wiswall and Zafar, 2016; Hastings et al., 2016). While much of this work relies on data from small, selective samples, we are able to draw on a large and diverse sample, i.e. allowing us to make statements about a substantive population of students.

In addition, our findings pertain to a large body of literature on employer learning (Farber and Gibbons, 1996; Altonji and Pierret, 2001). This research investigates the extent to which statistical discrimination by employers based on degree signals fades over time, i.e. as employers learn about the true underlying productivity of new employees (Farber and Gibbons, 1996; Lange, 2007). It also shows that employer learning may differ by the type of degree or the observability of educational content (Arcidiacono et al., 2010; Bauer and Haisken-DeNew, 2001; Aryal et al., 2019). We add to this strand of research by providing insights into the extent to which individuals anticipate signaling and employer learning effects to affect their wages in the longer run.

Finally, our paper relates to research on the role of psychic costs and non-monetary outcomes for educational decision-making (Cunha et al., 2005; Heckman et al., 2006; Jacob et al., 2018; Boneva and Rauh, 2017). This literature documents that both psychic costs and non-pecuniary factors are important determinants of educational decision-making, which is in line with our findings.

The remainder of the paper is organized as follows. In section 2.2, we provide information on the data collection procedure, describe our sample and main measures. Section 2.3 provides some descriptive insights into the data. Subsequently, section 2.4 contains our empirical strategy and main results for the perceived signaling value. Section 2.5 then tests two implications of the signaling theory. Finally, section 2.6 concludes.

[4] See also Fang (2006) for a structural model of education choices to disentangle signaling and human capital effects.

2.2 Data

This section provides detailed information on our sample and questionnaire measures. We start by describing the data collection procedure, before we report on our measures related to expected labor market outcomes, future employment, university experience and various background characteristics. Finally, we present summary statistics of the main background variables.

2.2.1 Data Collection

Our sample was recruited as part of the German student study "Fachkraft 2020".[5] Students on the mailing list of a popular nationwide job board were contacted via email and asked to complete an online questionnaire with items related to future labor market expectations, current study experiences, university dropout and a broad range of background characteristics.[6] The surveys were conducted in September 2014 and March 2015 and participation in the study was incentivized using Amazon vouchers amounting to €5,000.[7]

2.2.2 Measures

Labor Market Expectations As we are interested in individuals' expected labor market outcomes for different studying scenarios, we obtain students' counterfactual labor market expectations. Specifically, we elicit job prospects for two different scenarios: (i) when students graduate from their preferred major (graduating scenario) and (ii) when they leave university without obtaining any further academic degree (leaving scenario), see appendix section 2.B for the survey items. We assume that for the leaving scenario students think about leaving university immediately, and hence the current semester is seen as the semester in which they would hypothetically leave. For students in the last semester of studying, this is straightforward. For students at the start of their studies, it is reasonable to assume that students would expect to leave university immediately due to the high opportunity costs of studying. For each scenario, the students indicate their expectations with respect to gross yearly labor earnings, weekly working hours, the probability of finding a suitable job, and job satisfaction, where the latter is measured on a scale from 1 to 10.[8] From the specified earnings and working hours, we construct expected hourly wages, and full-time wages.

Moreover, in order to gain a better understanding of the development of perceived labor market expectations over the life course, all wage expectations were elicited for

[5] See Seegers et al. (2016) for more information.

[6] The job board jobmensa.de is operated by Studitemps GmbH and is the largest platform for student jobs in Germany.

[7] 1 x €1,000, 4 x €250, 10 x €100, and 40 x €50 vouchers.

[8] In the survey students were asked for the probability of *not* finding a suitable job. However, for readability we recode this as the job-finding probability.

three different points in time: at the age when a person first starts working, at the age of 40 and 55. [9] With this information, we approximate lifetime wage trajectories by assuming a standard Mincer-type earnings function where wages ($w_{i,t}^c$) are a quadratic function of work experience:

$$w_{i,t}^c = \alpha_i^c + \beta_i^c experience_{i,t}^c + \gamma_i^c (experience_{i,t}^c)^2 \tag{2.1}$$

Experience in time t is calculated by deducting the expected age at labor market entry by the age at time t.[10] We solve equation 2.1 for each individual i and counterfactual c to obtain scenario- and individual-specific parameters β_i^c and γ_i^c.[11] We use these parameters to calculate expected wages for each year of a person's working life for both the graduating and leaving scenario.

Studies that explore patterns of actual wage trajectories find that they tend to exhibit a concave growth pattern over the working life (for a review see Polachek et al., 2008). To investigate whether expected wage trajectories behave similarly, we identify the most common patterns. Figure 2.1 shows that concave, linear and convex growth patterns are most prevalent. Less than two percent remains unclassified, which mainly originates from expected wage developments that decrease over time. In accordance with actual wage patterns, the concave pattern is most prevalent for both scenarios, with a share of 69.9 percent for graduating and 45.3 percent for leaving. This is followed by a convex pattern of earnings growth, with shares of 24.4 percent and 31.8 percent respectively. Finally, 5.5 percent of students expect a linear increase in earnings after graduating, and 21.7 percent after leaving. For the scenario of leaving university we observe more linear and convex patterns, which is mainly due to a lower earnings growth at the beginning of the work life (see appendix figure 2.A2). This observation is in line with existing literature that shows that actual wage growth is steeper for higher levels of schooling (Belzil, 2008; Dustmann and Meghir, 2005).

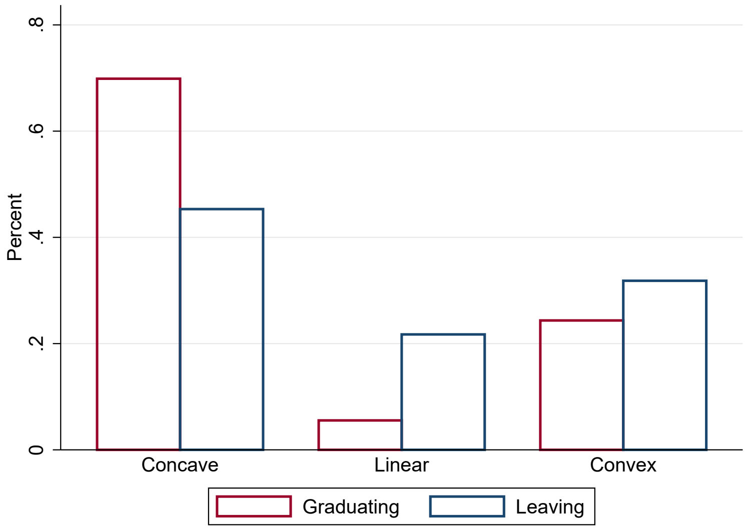
Future Employment Further, respondents were asked about the profession they plan to pursue after graduating from their current studies. They could choose out of 429 predefined occupations or make use of a free text field. This information allows us to classify whether people plan to pursue a profession that is legally regulated, meaning that individuals need to have a license in the form of a specific degree to pursue this occupation. We follow the classification of the German federal employment agency for regulated professions (Bundesagentur für Arbeit, 2020). Typical occupations for which this applies are physicians, lawyers or engineers. In addition, we inquire whether individuals would like to become a civil servant, as in Germany civil servants have a

[9]Expected job satisfaction and the probability of finding a suitable job were only elicited for labor market entry and the age of 40, not for the age of 55.

[10]Students indicated their current age and how long they still need to study until they finish their degree. With this information, we were able to calculate the expected age at labor market entry.

[11]See appendix figure 2.A1 for the distribution of parameters β and γ

Figure 2.1: Shares of expected wage trajectory patterns



Notes: Figure 2.1 shows the share of expected wage trajectory patterns over the working life by scenario. The concave, linear and convex growth patterns are classified based on the parameters computed from equation 2.1.

special status, with fixed wage regulations depending on experience and education. This information allows us to control for a licensing effect after graduation.

University Experience The survey also contains questions about various aspects of students' university experience. First, with respect to the study phase, we ask which degree respondents aim to obtain. In addition, we ask how many semesters they have studied, both with respect to their current studies as well as overall, and how many semester they still expect to need to finish their current degree[12]. Second, respondents were asked to report their study subject from a list of fifteen study directions. We group these subjects into five main categories: medicine/health, STEM, law, economics, and humanities/social sciences. Third, to know more about students' performance, we inquire about individuals' grade point average. Furthermore, we ask them to estimate their perceived relative position in the distribution of all students regarding academic ability and work-related ability on a scale from 0 to 100. Fourth, to better understand the relevance of the leaving scenario, we ask students about their perceived probability of leaving university *without any further degree*, where this probability excludes switching to an alternative university study. Finally, we elicit their overall satisfaction with their studies.

[12]In Germany, only roughly 30% of all students obtain a degree in regular study time (Destatis, 2018). Often internships, side jobs or stays abroad prolong the study time. We thus elicit both semesters studied and semesters left to study to approximate the students' current stage of studying.

Background Characteristics We also collect data on a broad range of individual characteristics, such as gender, age, migrant background and state of residence. Moreover, we inquire about respondents' high-school GPA to have information on pre-university ability. Finally, we ask individuals to state whether neither, one, or both of their parents attended university, which is a proxy for socioeconomic background.

2.2.3 Summary Statistics

After dropping observations who indicated implausible wage returns or missed essential variables of interest, we obtain a sample of 6,306 students.[13] Table 2.1 provides summary statistics of the main background variables for our sample, and compares them to the entire population of students in Germany in the 2014/2015 academic year. Overall, the table shows that we have a diverse sample of students, which closely compares to the overall population of German students in terms of age, migration background, region, degree type and high-school GPA. An exception is that females are slightly overrepresented, potentially due to higher responsiveness to surveys among females in general. In addition, there are 29.3% economics students in our sample, which is 15 percentage points higher than the population share in this subject category. This higher share of economics majors mainly comes at the cost of a lower fraction of students in humanities, social sciences and law. This unbalance might reflect that all students were approached via a job agency and having a side job could be more common for economics students. In our analysis, we take these differences into account and control for all relevant background characteristics.

In addition to the statistics presented in table 2.1, our sample is also diverse with respect to respondents' study phase. For respondents aiming to obtain a master (bachelor) degree, 31.7% (10.0%) are in semester 1-2, 37.4% (26.0%) in semester 3-4, 19.6% (27.4%) in semester 5-6 and 11.3% (36.6%) in their 7th or higher semester. This variation is essential to estimate the value of human capital accumulation.

2.3 Descriptive Evidence

In this section, we first characterize the wage and non-wage returns that students perceive from both graduating and leaving university without a degree. Subsequently, we provide descriptive evidence on where these returns originate. Although (expected) returns to higher education are well documented in the literature, we can still contribute to existing evidence due to the uniqueness of our data. First, we show returns for different scenarios *within* individuals, which means they are not biased by students selecting themselves out of university once they have started studying. Second, we look at the most relevant returns with respect to individual decision-making, namely the *perceived ex-ante* returns.

[13] See section 2.C in the appendix for more information on the data-cleaning procedure.

Table 2.1: Summary statistics

		Our sample	Student cohort 2014/15*
Age		23.5	23.4
Male (%)		47.1	52.2
Migration background (%)		16.7	16.2
Federal state(%)	Baden-Wuerttem.	11.4	13.2
	Bayern	17.0	13.6
	Berlin	7.1	6.3
	Brandenburg	2.0	1.8
	Bremen	1.7	1.3
	Hamburg	2.8	3.6
	Hessen	8.7	8.8
	Mecklenburg-Vorp.	1.5	1.4
	Niedersachsen	7.1	7.1
	Nordrhein-Westfalen	23.3	26.9
	Rheinland-Pfalz	4.8	4.5
	Saarland	0.5	1.1
	Saxony	4.5	4.2
	Saxony-Anhalt	2.5	2.0
	Schleswig-Holstein	2.8	2.1
	Thueringen	2.4	1.9
Bsc. student (%)		77.0	78.1
Subject (%)	Medicine	5.7	6.0
	STEM	37.4	39.2
	Law	1.3	4.9
	Econ.	29.3	15.5
	Human./Social	26.3	34.5
High-school GPA		2.42	2.45
Observations		6,306	2,698,910

Notes: Table 2.1 compares the summary statistics of several background characteristics between our sample and the overall German student cohort in 2014/15. The statistics for the total student cohort originate from Destatis (2020) and Govdata (2020). Regarding the high-school GPA, in Germany the best grade is 1.0 and the worst passing grade is 4.0.

Finally, we observe the perceived wage and non-wage returns for a diverse and large sample of students, which is rare in this literature.

2.3.1 Perceived Wage Returns

Our data allow us to calculate expected wage returns for a diverse sample of students, which can be done in various ways. In the most straightforward sense, we can compare the indicated perceived graduation wage to the perceived university-leaving wage at the time of labor market entry. The top panel of figure 2.2 plots the density of these two measures. In addition to substantial variation in expected starting wages between individuals, the graph clearly shows that students expect their leaving wages to be considerably lower than their graduation wages. On average, students expect €27,400 of yearly earnings when leaving university instead of €38,000 when graduating, with the averages being weighted by major and gender. The perceived graduation wage average fits well with the observed labor market entry wage for university graduates, which in 2014 amounted to €36,600 (Destatis, 2017). Furthermore, the patterns of earnings expectations between university majors and gender are plausible, with on average higher expected earnings for males and STEM majors (see figure 2.A3 in the appendix). Note that our respondents are also fairly accurate in terms of estimating earnings further in the future. The observed yearly earnings at age 60 after obtaining a university degree in 2014 is €60,700, while our sample's expected weighted average at age 60 is €69,200 (Destatis, 2017).[14]

To analyze expected returns in a more formal manner, we also use our data to estimate the lifetime earnings return, which is the discounted sum of wage income after graduating minus wages earned when leaving and potential study costs. Furthermore, we calculate the internal rate of return (IRR), namely the discount rate that would make an individual indifferent between finishing their degree and leaving at their current study phase. For this purpose, we estimate the following two equations:

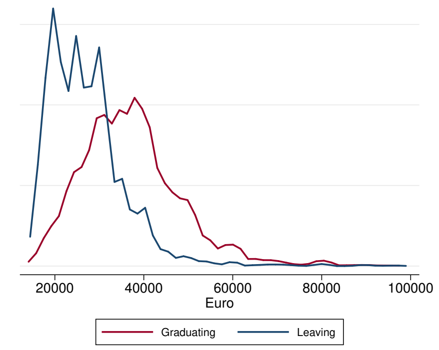
$$V_i^* = \sum_{t=t_i^f}^{65} \delta^{t-t_i^f} W_i^f(t) - \sum_{t=t_i^l}^{65} \delta^{t-t_i^l} W_i^l(t) - \sum_{t=t_i^l}^{t_i^f} \delta^{t-t_i^l} C_i \tag{2.2}$$

$$\sum_{t=t_i^l}^{65} \frac{W_i^f(t) - W_i^l(t)}{(1+\rho)^{(t-t_i^l)}} = \sum_{t=t_i^l}^{t_i^f} \frac{C_i}{(1+\rho)^{(t-t_i^l)}} \tag{2.3}$$

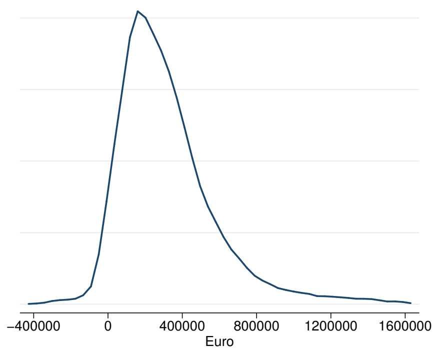
where V_i^* are the lifetime returns for individual i and $W_i^f(t)$ and $W_i^l(t)$ indicate expected wages after finishing studies (f) and leaving (l) at time t. Accordingly, t_i^f and t_i^l is the age at which an individual i is expected to start working when she respectively finishes studying or leaves university. C_i are the yearly study costs an individual incurs,

[14] We cannot easily compare the expected leaving wages to observed values, as any observed measure would be heavily influenced by selection.

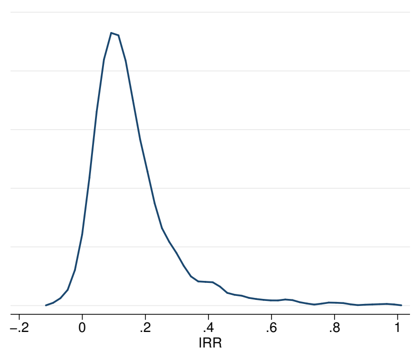
Figure 2.2: Density of starting wages and returns



(a) Expected starting wages by scenario



(b) Life time wage return



(c) Internal rate of return

Notes: Panel A of figure 2.2 shows the density of the expected wage at labor market entry for graduating and leaving university without a degree. Panel B shows the density of the lifetime wage return of graduating, which is calculated according to equation 2.2. Finally, panel C portrays the density of the internal rate of return of finishing university, as estimated in equation 2.3.

and they are assumed to stay constant over time. Study costs include only explicit costs such as tuition fees, spending for books or other materials needed, and were elicited in the survey. Furthermore, in equation 2.2 δ is the time discount rate, which in main specification is set at 0.95, although we also calculated the returns for $\delta = 1$ to estimate an upper bound for lifetime returns. In equation 2.3, ρ is the internal rate of return. An individual chooses to obtain a higher education degree if $V_i^* > 0$ or $\rho > 0$.

The density graphs of the returns can be found in panels B and C of figure 2.2. Panel B shows that almost all respondents in our sample expect positive discounted lifetime earnings returns from graduating, with the average being around €334,400 until retirement.[15] Panel C shows a similar pattern for the estimated IRR, with only 3.2 percent of all respondents expecting a negative return and an average rate of return of 17.9%. Accordingly, if students in our sample face the decision whether to complete their current degree or leave university without graduating, they on average expect to encounter a 17.9% return to *completing* their studies. This percentage is substantially higher than the IRRs generally reported in the literature for the *initial* choice of starting a university study or not, e.g. the observed initial IRR within Germany in 2014 is 7.5% OECD (2014). First, this is due to the fact that the students in our sample self-selected into university. Second, we observe the IRR for *completing* a degree, and hence students have already paid some of the direct and indirect costs of studying. Moreover, the discrepancy between initial and "course of study" IRRs points to returns mostly accruing towards the end of one's studies, while the costs are born at the beginning. Therefore, we also look at the IRR of students who only recently started studying, where for students in their first or second semester we find an IRR of 11.4%, which comes closer to the observed initial IRR.

2.3.2 Perceived Non-Wage Returns

Along with the wage returns of finishing a university degree, expected non-wage returns are also considered an important labor market outcome for students (e.g. Wiswall and Zafar 2016). Figure 2.3 shows the expected job satisfaction and the job-finding probability when finishing and leaving university. Similar to the expected wage returns of graduating, students expect large non-wage returns to a university degree. Panel A displays substantial differences in the distribution of job satisfaction between the two scenarios. While the mean expected job satisfaction is 7.2 out of 10 for graduating, it is only 4.0 for leaving university. The density of the expected job-finding probability by the age of 40 for each scenario can be seen in panel B of figure 2.3. We look at the expected job-finding rate at the age of 40 instead of at labor market entry to prevent the results being driven by the fact that many first-time employees need some time to

[15] If we calculate an upper bound for the lifetime returns, setting the discount rate $\delta = 1$, the average expected return is €792,200 instead. The substantial difference between setting different discount rates is reasonable, as with discounting the higher expected wages in the future are weighted less compared to not discounting at all.

initially find a suitable job.[16] The expected return to graduation is substantial, with a mean expected probability of finding a suitable job after graduating of 81.9% compared to 56.7% after leaving university.

2.3.3 Origins of Returns

Overall, the expected wage returns closely mirror the observed measures, which is promising for the validity of our results. To gain a first insight into the perceived origins of the returns, we show descriptive evidence on the immediate graduation premium, as well as the development of expected returns over the course of studying.

With respect to the graduation premium, we are interested in the impact of obtaining a degree certificate on students' wage expectations. For this purpose, we compare perceived starting wages after graduating to perceived starting wages when leaving university for master students who indicate being in either their last or second-last semester before finishing their studies. Restricting the descriptive comparison to a sample of students who have almost completed their degree ensures that the difference in returns over scenarios is not mainly driven by accumulating human capital during one's studies.[17] Moreover, as we compare the wage expectations within an individual across the two scenarios, this comparison is free from selection bias. Panel A of figure 2.4 shows that there is a substantial difference between the average expected leaving wage and the average expected graduation wage for students in their last semester. The expected premium to graduation is 24.5%, which corresponds to €7,400 yearly gross income (€37,600 versus €30,200). This is a sizable difference, especially considering that we are only looking at master students, i.e. those who have already completed a first university degree.

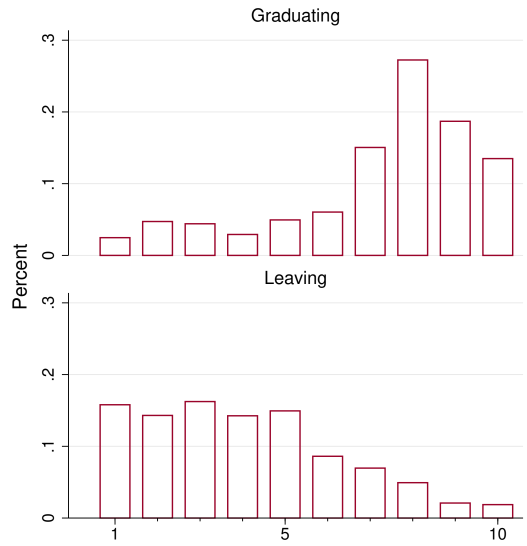
In addition, we look at how the perceived returns when leaving university without a degree evolve over the course of studying, which can be interpreted as an indication of the expected accumulation of human capital over semesters. For the following comparison (and for our estimations in section 2.4), we assume that a higher number of semesters studied is associated with a higher human capital value.[18] Panel B of figure 2.4 shows the perceived starting wage after leaving by current semester studied for master students. As we compare expected leaving wages between individuals over different semesters, we control for background characteristics such as gender, major and age. According to the human capital theory, we should see an upward trend in expected leaving wages, as

[16]The results for the job-finding probability at labor market entry are qualitatively similar, and can be found in figure 2.A4 in the appendix.

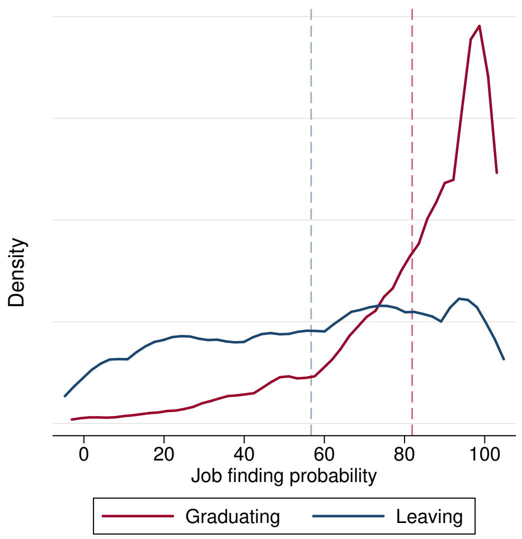
[17]Besides, we focus on master students as they obtain an additional degree, which is different from obtaining a first academic degree, as is the case for most bachelor students. See appendix 2.D for a more extensive explanation.

[18]This assumption is credible as in general every semester studied involves coursework, mandatory internships, writing a thesis or the like. However, there might be some students who obtain fewer or no credits in a given semester. One can imagine that an extension in study time often comes due to stays abroad, (voluntary) internships or side jobs, which can also be seen as enhancing human capital. Thus, one more semester studied should be associated with a higher or at least similar human capital compared to the previous semester, even if students take more time to study than the regular study time.

Figure 2.3: Density of job satisfaction and job-finding probability



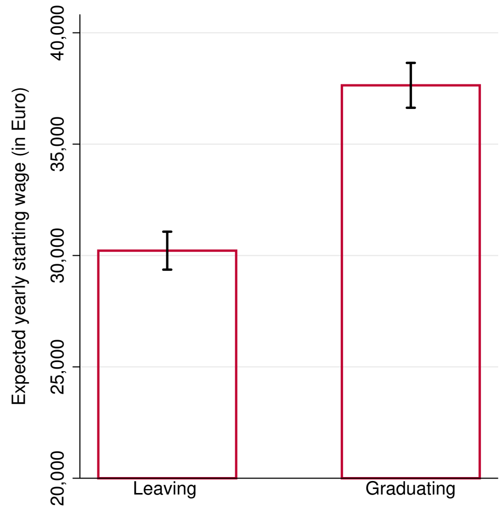
(a) Expected job satisfaction by scenario



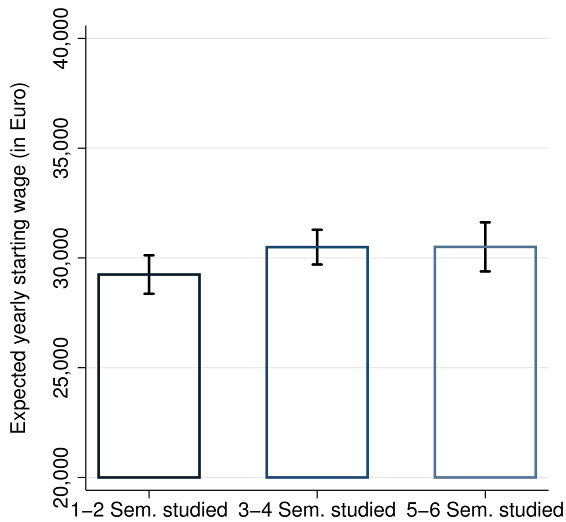
(b) Expected job-finding probability at age 40 by scenario

Notes: Panel A of figure 2.3 shows the distribution of expected job satisfaction at labor market entry for the scenarios of graduating and leaving university, measured on a scale from 1 to 10. Panel B displays the density of the expected probability of finding a suitable job at the age of 40 for both scenarios. The dashed lines indicate the average expected job-finding probability at the age of 40, which is 81.9% for graduating and 56.7% for leaving university without a degree.

Figure 2.4: Graduation premium and the development of university-leave wages



(a) Graduation premium



(b) University-leave wages by semester studied

Notes: Panel A of figure 2.4 compares the expected yearly starting wage for leaving university with graduating on a within-individual basis. It includes only master students who are in their (second to) last semester. Panel B compares the expected yearly starting wage when leaving university for master students at different stages of their studies. We control for differences in gender, age, ability, SES, major and perceived work ability.

more productive human capital is accumulated over the course of studying, giving rise to higher expected wages when leaving university. However, we do not observe a conclusive pattern. Wages slightly increase between students who are in their first year compared to students in their second year of master studies by around €1,400, but the difference is not statistically significant. We do not observe any difference in expected leaving wages between students in their second and third year. Moreover, the magnitude of the effect is much less substantial than the premium of obtaining the degree.

2.4 Perceived Signaling Value of Higher Education

The descriptive findings strongly suggest that students expect substantial labor market returns from finishing their studies, which seems to be largely driven by a graduation premium. In this section, we estimate the perceived signaling effect of a degree and proxy the value of human capital accumulation more precisely, making use of the unique individual counterfactual expectations data that we possess.

2.4.1 Immediate Wage Returns

Our strategy of eliciting counterfactuals through carefully-designed survey questions allows us to estimate the effect of obtaining a degree on a within-person basis, i.e. without having to worry about other confounding factors. Here, identification relies on two key assumptions, namely that (i) individuals respond truthfully and that (ii) they are able to form reasonable expectations about counterfactual scenarios and related probabilities. A growing body of literature relying on hypothetical scenarios, beliefs, and counterfactual labor expectations has shown that these assumptions are reasonable and in particular that stated expectations and preferences tend to be close to actual realizations and informative about actual choices (see e.g. Wiswall and Zafar, 2016; Mas and Pallais, 2017). In addition, we document a high average accuracy of wage expectations in section 2.3 by comparing expected starting wages to actual wage realizations for the general population. However, note that even if elicited labor market expectations were biased (but truthful), they are nevertheless informative about those (subjective) beliefs that enter the individual decision-making process.[19]

Using the counterfactuals, we can identify the effect of a degree by comparing the two different scenarios on a within-person basis, eliminating the individual fixed effect. Additionally, we approximate the human capital effect by comparing leaving wages between individuals who are in different semesters of their studies and assume that human capital accumulates linearly over time. [20] As the signal is most prevalent at labor market entry, we first concentrate on the immediate returns from graduating, where we

[19] Note that truthful reporting is an underlying assumption of any analysis based on survey data.

[20] We restrict the sample to students who indicate having at most eight semesters left to study, changing the sample size to 3,945 and 1,284 for bachelor and master students, respectively. See the robustness checks for the relaxation of this assumption.

will also look at the long-term development of the graduation premium in section 2.4.3. Accordingly, equation 2.4 shows our main specification for immediate returns:

$$W_i^c = \beta_0 + \beta_1 degree_i^c + \beta_2 semesters_i^c + \gamma_i \quad (2.4)$$

W_i^c represents the expected yearly starting wage of individual i in scenario c, with $c = f$ for graduating and $c = l$ for leaving. In this and the following equation in this section, all variables used are expectations about the time of labor market entry, and hence W_i^c stands for $W_i^c(start)$, with $t = start$ indicating the time at which individual i starts working. Moreover, $degree_i^c$ is a dummy variable indicating the graduation wage, which is one for the scenario of obtaining a degree and zero for leaving without a degree. $semesters_i^c$ indicates how many more semesters an individual still has to study to finish, which is zero in the scenario of graduating.[21] The individual fixed effects are captured by γ_i, which controls for an individual's scenario invariant characteristics. Hence, β_1 measures the value of the degree certificate, while β_2 captures the expected wage premium for getting one semester closer to the degree.

The interpretation of the above analysis rests on the assumption that graduating results in a positive signaling value. However, it is conceivable that leaving university without a degree yields a negative signal instead. In this case, the overall signaling value that we estimate would be unaffected, although its interpretation would change. We provide a detailed account of this possibility in appendix section 2.E. Throughout the paper, we stick to the interpretation of a positive signaling value for obtaining the degree certificate, as this is most in line with the existing literature.[22] Under this assumption, β_1 can be interpreted as the (positive) signaling effect of a degree and β_2 can be interpreted as the human capital value per semester.

We calculate equation 2.4 separately for bachelor and master students and focus on master students throughout the main analysis, as they face less ambiguity with respect to both their own ability (Stinebrickner and Stinebrickner, 2012; Arcidiacono et al., 2016) and potential labor market outcomes (see appendix 2.D for an extensive discussion).

Table 2.2 shows our main results with expected starting wages as the outcome variable. In column 1, we estimate the model for wage levels, whereas the other columns use log wages as the outcome variable. The first coefficient estimated in column 1 indicates that the effect of coming one semester closer to graduating is positive but statistically insignificant, with roughly a €210 increase in expected starting wages on average. By contrast, graduating is expected to increase returns by €7,100. Column 2 shows that

[21]To make the estimates more comprehensive, we used a negative sign on the semester variable such that a higher (less negative) semester variable means getting closer to the degree. Of course, the coefficients are unaffected by this manipulation, whereby only the sign is positive instead of negative.

[22]We believe that this is also more plausible since labor market applicants have some leeway in informing future employers about (the reasons for) leaving university without a degree. Of course, this is not always possible, as it depends among others on the time studied, although very often applicants only include accomplishments and positive signals in their application and not failures.

Table 2.2: Immediate wage returns

	(1) Starting wage levels	(2) Starting wage logs	(3) Starting wage logs
Semesters	212.3	0.00680	0.00687
	(157.3)	(0.00439)	(0.00438)
Degree	7099.7***	0.209***	0.202***
	(549.4)	(0.0151)	(0.0156)
Interaction effects:			
Licence*Degree			0.0289*
			(0.0162)
Civil servant*Degree			0.00335
			(0.0176)
Constant	30639.6***	10.29***	10.29***
	(520.8)	(0.0144)	(0.0144)
N	1381	1381	1381
adj. R^2	0.461	0.506	0.507

Notes: Column 1 in table 2.2 shows the effects of semesters studied and obtaining a degree on the level of yearly starting wages, while the dependent variable in columns 2 and 3 comprises of the log starting wage. The sample only includes master students who have maximum eight semesters left until reaching their degree. Standard errors are in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

this translates into a wage increase insignificantly different from zero for an additional semester, whereas the degree raises wage by 20.9%. The size of the expected signaling effect is notable, especially since we only consider the returns to a master's degree, i.e. such that leaving still means being able to start working with a bachelor's degree.

However, a potential problem with the estimated coefficients arises in case of licensed occupations. For certain (often high-paid) professions, the returns from graduating might be largely driven by legally-binding requirements of having obtained a certain degree. Licensing may thus capture something very distinct from future productivity. Therefore, in column 3 we include two interaction terms: first, a dummy indicating whether an individual plans to work in a legally-regulated occupation; and second, a dummy indicating whether a person plans to work as a civil servant. In Germany, many positions as a civil servant also require a completed degree and the earnings are predefined by a collective wage structure depending among others on the highest degree. The results in column 3 show that the coefficients of interests are robust to including these additional terms, where the interaction term for licensed professions is positive and marginally statistically significant. Nonetheless, the effect size is relatively small. At the same time, we do not observe an effect of planning to work as a civil servant. One explanation might be that although having a master's degree allows individuals to earn more when working in a public institution, the earnings potential is generally still lower than in the private sector.

In appendix table 2.A1, we present estimates for bachelor students. The results show a similar pattern as for the master students, with a positive but small increase of expected earnings over semesters (0.62%), and a large signaling value of graduating (32.1%). It is reasonable that the effect size of graduating is stronger for bachelor students, as graduating yields their first academic degree, possibly allowing them to enter a different segment of the labor market.

2.4.2 Immediate Non-Wage Returns

In addition, we estimate the fixed effects model for expected non-wage returns, namely job satisfaction and the probability of finding a suitable job. At present, little is known about the extent to which signaling expands to non-wage returns. There are two possible scenarios. First, if wage and non-wage returns are positively correlated, we would expect to see a positive signaling value for both the perceived job-finding probability and job satisfaction. Instead, if they are negatively correlated – for example, due to compensating wage differentials (Rosen, 1974) – we would expect to see opposite or insignificant results. For the estimation of the fixed effects model, we standardize both variables across scenarios, using the value in the leaving scenario as the baseline to adjust both leaving and graduating values:

$$S_i^c = \frac{sat_i^c - \mu_{sat}^l}{\sigma_{sat}^l} \tag{2.5}$$

with S_i^c as the standardized outcome variable (here satisfaction). sat_i^c is the expected satisfaction of individual i for scenario c and μ_{sat}^l and σ_{sat}^l are the mean and standard deviation of the perceived satisfaction when leaving university.

Table 2.3 shows the results for the expected non-wage returns, where the first two columns examine satisfaction at labor market entry and the last two relate to the job-finding probability. For both measures, we observe similar patterns compared to wage returns. There is a large perceived graduation premium, which is statistically significant across all specifications. We observe that the degree raises expected satisfaction by 1.04 of a standard deviation, and expected job-finding probability by 0.46 of a standard deviation. At the same time, the expected human capital effect is not statistically significant for both measures, although the signs of the effects are as expected and consistent with our previous findings. Moreover, licensed occupations do not significantly affect expected job satisfaction. However, for the expected suitable job-finding probability licensing or becoming a civil servant substantially increases the probability.

In appendix table 2.A2, we present the findings for the non-wage returns of bachelor students. These results are similar to our main findings, where graduation yields even stronger effects, i.e. approximately a 1.5 standard deviation increase in job satisfaction, and a 0.8 standard deviation increase in the job-finding probability.

Table 2.3: Immediate non-wage returns

	(1) Satisfaction	(2) Satisfaction	(3) Job finding probability	(4) Job finding probability
Semesters	0.0204	0.0211	0.00770	0.00864
	(0.0264)	(0.0263)	(0.0184)	(0.0184)
Degree	1.091***	1.043***	0.519***	0.465***
	(0.0932)	(0.0952)	(0.0658)	(0.0672)
Interaction effects:				
Licence*Degree		0.151		0.124*
		(0.0930)		(0.0687)
Civil servant*Degree		0.0855		0.146**
		(0.108)		(0.0736)
Constant	0.0671	0.0693	0.0253	0.0283
	(0.0882)	(0.0878)	(0.0617)	(0.0618)
N	1381	1381	1381	1381
adj. R^2	0.424	0.425	0.240	0.244

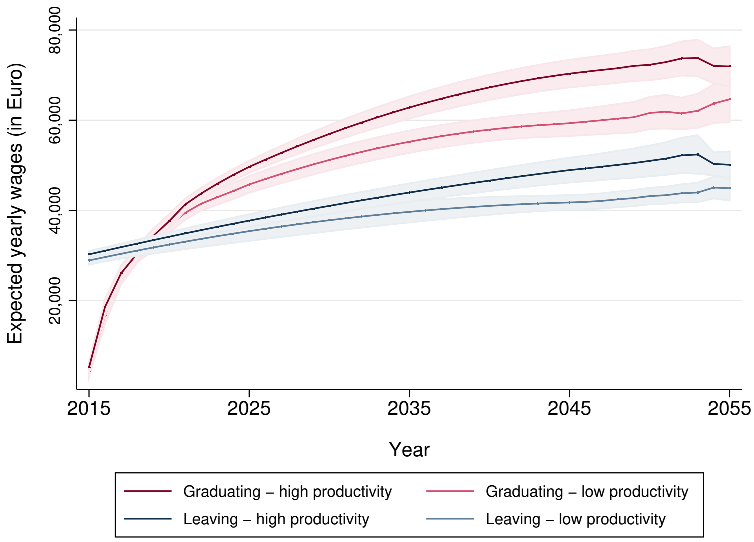
Notes: Columns 1 and 2 in table 2.3 show the effects of semesters studied and obtaining a degree on expected job satisfaction at labor market entry, while the dependent variable in columns 3 and 4 is the expected probability of finding a suitable job. Both job satisfaction and job-finding probability are expressed in standard deviations according to equation 2.5. The sample only includes master students who have maximum eight semesters left until they reach their degree. Standard errors are in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

2.4.3 Persistence of the Graduation Premium

So far, our results show that students perceive that the immediate returns from graduating mainly stems from signaling their ability to employers in the labor market rather than from accumulating human capital. However, in the longer run this might be different, as individuals can demonstrate their abilities and reveal their true productivity types to employers while working. As we collected data on the expected wage returns for three points in time and computed wage expectations over the whole life span for both scenarios accordingly, we are able to examine how the initial difference between graduates and university leavers evolves over time. In addition, we can investigate heterogeneities in perceived work ability to assess the degree of perceived employer learning (see e.g. Farber and Gibbons, 1996; Lange and Topel, 2006; Aryal et al., 2019, for a discussion and evidence regarding actual wage outcomes) and the extent to which it may outweigh the signaling effect in the long run.

Figure 2.5 displays the development of expected wages over the working life after graduating (red lines) and after leaving university without a degree (blue lines), where the darker lines of each color resemble the upper 50% of the perceived work ability distribution and the lighter lines resemble the bottom 50% of perceived work ability. We use the indicated perceived work ability of each individual as a proxy for later (perceived) productivity in the labor market. From the figure it is apparent that independent of the

Figure 2.5: Expected yearly wage over the life time by work productivity



Notes: Figure 2.5 shows the development of the expected yearly wage over the life time for master students. The red lines correspond to graduating, and the blue lines to leaving university without a degree, where the darker (top) lines of each color correspond to the upper 50% of the perceived work ability distribution and the lighter (bottom) lines correspond to the bottom 50% of the perceived work ability distribution. The colored areas around the lines indicate the 95% confidence intervals.

productivity types students expect to earn more at every point in time as graduates than as university leavers. Besides, the perceived earnings gradient is higher in the graduating scenario, meaning that there is not only an initial wage gap between the two scenarios, but also the expected earnings potential of graduating seems to be higher.

In addition, looking at the productivity types separately, one could expect that after some time in the labor market the high-productivity types can reveal their true ability and the initial setback of leaving university without a degree can be reduced or even fully offset. Nonetheless, we do not find evidence of a reduction in the initial gap between the two scenarios. At the start of the working life, there is only little difference between high- and low-productivity types in both scenarios, which is in line with the signaling theory claiming that productivity is initially unobserved by employers and that by definition the signal should have the same worth to everyone who obtains it. Over the course of time, as employers learn more about individual ability, the difference between the low- and high-ability employees within the scenarios increases, as the slope reflecting the earnings increase is stronger for the high-productivity types. Nevertheless, the graph also shows that even high-ability types in the leaving scenario on average do not expect their earnings to catch up nor come closer to the graduating scenario.

In fact, from all master students only 8.9 percent expect to be able to diminish part of the wage gap between the graduating and leaving scenario at some point in their career. Moreover, merely 4.2 percent of master students belief they can fully close the gap. For bachelor students this percentages are even lower, with 6.5 and 2.6 percent respectively. Besides, it is not apparent that the students who believe they can mitigate the wage difference expect this to happen due to employer learning, as the average wage trajectories of these students show that the narrowing of the gap happens relatively late in the career and we expect that employer learning to rather happen in the first years of employment (see appendix figure 2.A5).

We can only speculate on the reasons for the low support for diminishing the initial graduation premium. One explanation is that graduating not only leads to higher perceived lifetime returns through increased starting wages, but that it also helps job beginners to get into different kind of jobs compared to university leavers. Moreover, they may believe that initial assignment to a high-earning job allows individuals to acquire specific human capital that outweighs their latent productivity.[23] These jobs might then have stronger potential for wage increases over time. Nonetheless, we need to recall that our main results and figure 2.5 only refer to master students who already have a university degree even in the scenario of quitting their current studies. Hence, it is not quite straightforward to expect that students with only a bachelor degree will end up in substantially different jobs compared to master students. Although the mechanisms behind this result are not completely evident, we can conclude that the initial expected graduation premium caused by the signaling value is persistent and for a majority of students even increases over time.

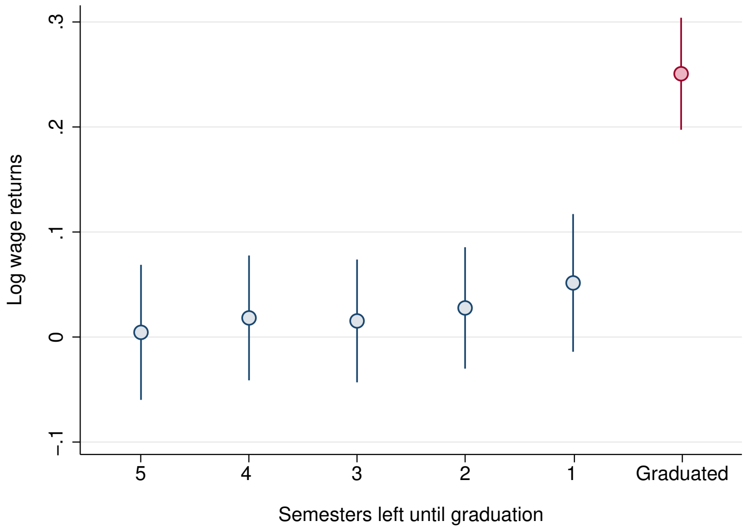
2.4.4 Robustness Checks

In this subsection, we assess the robustness of our results. For this purpose, we first relax the linearity and homogeneity assumptions that we make to estimate the human capital effect. We then study potential biases that may arise from dynamic selection related to student dropout over time. Finally, we assess the sensitivity of our results with respect to sample selection.

Linearity of Human Capital Accumulation First, we assume that the human capital effect is linear in semesters. This is a reasonable assumption as credit points at university normally build up linearly with an increasing number of semesters completed. However, from an individual perspective this does not always hold true. Besides, some courses or activities might be perceived as creating more human capital than others. Therefore, we estimate an alternative fixed effects specification easing the assumption that human capital accumulation is a linear process by looking at the effect of each

[23]The same effect could arise from productivity spillovers from high-performing co-workers or if the signal grants advantages in promotions, e.g. because early earnings are a signal for later earnings (see e.g. Waldman, 2016).

Figure 2.6: Coefficients of fixed effect model with semester dummies



Notes: Figure 2.6 displays the coefficients and 95% confidence intervals from estimating equation 2.6. The blue dots correspond to β_n and the red dot to β_1. The coefficients are set against a baseline of having six or more semesters until graduation. The regression only includes master students who have a maximum of eight semesters left to study.

semester separately. Equation 2.6 shows the respective specification:

$$W_i^c = \beta_0 + \beta_1 degree_i^c + \beta_n \mathbb{1}_{n,i}^c + \gamma_i, \tag{2.6}$$

where $\mathbb{1}_{n,i}^c$ is an indicator function representing a set of dummy variables for the number of semesters n that individual i still needs to study. The baseline is having 6, 7 or 8 semesters more to study, as we bundled the "high semester" students in one category due to the small number of observations.

Figure 2.6 visualizes the results of the fixed effects model with semester dummies and displays the estimated coefficients with 95% confidence intervals.[24] The coefficients indicate how expected starting wages after leaving change compared to the baseline of having 6 to 8 semesters left to study. It seems that the development over semesters is slightly increasing, although in line with the model estimated in section 2.4.1 none of the coefficients are significantly different from zero and we do not see any non-linearities. The graph shows that graduating with a master degree causes a considerable jump in expected wages of 25.1% compared to the baseline, which is in line with the estimated effect of a degree of 20.6% in our main model specification. As before, this is a substantially stronger effect compared to the value of an additional semester studied.

Increasing Human Capital by Semesters A second assumption that we make to approximate the human capital effect is that with fewer semesters left to study the

[24]See appendix table 2.A3 for the regression results.

human capital value should increase. However, this is not straightforward, as students who have the same number of semesters left to study are not necessarily at the exact same stage of their studies. We test this assumption by restricting the sample to students who are studying in regular study time, meaning that the sum of semesters left to study and semesters already studied cannot exceed the regular study time plus one. Fixing the sum of these two variables ensures strong comparability of semesters between students as they are all participating in a master's program that they are about to finish in regular study time. In table 2.4, columns 1 to 3, we show that the estimated effect of obtaining a degree slightly decreases but remains at a significant 18.7% wage increase (compared to 20.6%). The estimated human capital effect remains statistically insignificant. Overall, our estimation of the signaling effect is robust to this subsample analysis.

Dynamic Selection Third, so far we have abstracted from dynamic selection. Although we have students at all study stages in our sample, the students in the later semesters of their studies might be a selected sample as they have already reached a later stage of studying. At the same time, students with a higher expected graduation premium might be less likely to leave university than students with lower expected returns of graduating, in which case we might overestimate the signaling value. To test whether our results are affected by this selection, we estimate the signaling effect only for students who finished high school with an average grade in the top third of our sample. According to Isphording and Wozny (2018), a better high-school grade is highly predictive of graduating within Germany. Hence, if we restrict our analysis to the top performers in high school, this should reduce potential dynamic selection, while also improving comparability between students across the different study stages. Columns 4 to 6 of table 2.4 present the estimates for this sample. We observe a signaling effect of roughly 18%, which is close to the results in our main analysis. The human capital effect turns statistically significant and increases slightly compared to our main analysis, although with a 1.5% wage return per semester it remains considerably lower than the effect of the degree.

Sensitivity with respect to Sample Selection Finally, in our main specification we restrict the sample to students who indicate having at most eight semesters left to study in the main analysis[25]. To test the sensitivity of our findings with respect to the exact thresholds of semesters, columns 7 to 9 in table 2.4 show the results for a sample including students who have 9, 10, 11 or 12 semesters left to study (capturing more than 99% of all students). The results show that the magnitude of the graduation premium is robust to expanding the sample to these students.

Overall, we can conclude that the expected signaling effect is substantial and robust

[25] As the regular study time for master students is four semesters in Germany, we restricted the sample to double the amount of time needed for studying

Table 2.4: Robustness analyses

	Regular study time			Best third in high-school			Max 12 semesters		
	(1) Starting wage levels	(2) Starting wage logs	(3) Starting wage logs	(4) Starting wage levels	(5) Starting wage logs	(6) Starting wage logs	(7) Starting wage levels	(8) Starting wage logs	(9) Starting wage logs
Semesters	124.4	0.0118	0.0121	493.8*	0.0148**	0.0148**	181.0	0.00561	0.00556
	(298.8)	(0.00784)	(0.00784)	(253.1)	(0.00692)	(0.00696)	(133.3)	(0.00378)	(0.00379)
Degree	7229.1***	0.191***	0.187***	5924.1***	0.180***	0.188***	7191.7***	0.212***	0.205***
	(988.1)	(0.0250)	(0.0253)	(828.5)	(0.0235)	(0.0236)	(493.6)	(0.0137)	(0.0144)
Interaction effects:									
Licence*Degree			0.0108			0.00112			0.0324**
			(0.0215)			(0.0258)			(0.0162)
Civil servant*Degree			0.00683			-0.0545*			0.00709
			(0.0236)			(0.0296)			(0.0175)
Constant	30675.5***	10.31***	10.31***	32090.4***	10.32***	10.32***	30530.6***	10.28***	10.28***
	(947.1)	(0.0244)	(0.0244)	(810.2)	(0.0226)	(0.0227)	(463.4)	(0.0130)	(0.0130)
N	688	688	688	523	523	523	1411	1411	1411
adj. R^2	0.455	0.522	0.522	0.436	0.499	0.502	0.459	0.503	0.504

Notes: Table 2.4 shows robustness analyses of the effects of semesters studied and obtaining a degree on yearly starting wages. Columns 1 to 3 comprise students who are expected to finish within regular study time, i.e. four semesters in total. Columns 4 to 6 include every student who had a high-school GPA in the highest 33% of the sample. Column 7 to 9 includes all students who are in the 12th semester or less. The sample only includes master students. Standard errors are in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

across all specifications. Throughout, the human capital value remains positive but small and its relative importance to the signaling effect continues to be minor. For bachelor students, we repeat all robustness checks and find that the signaling value also remains robust across specifications (see table 2.A4 in the appendix).

2.5 Implications of the Signaling Theory

The previous sections have shown that students predominantly believe that the signaling value is responsible for the largest part of the returns to graduating. A natural next step is to check whether further implications of the signaling theory also hold in our sample. Regarding our analysis, there are two testable implications of Spence's signaling theory. First, as the degree is assumed to be the only way to signal productivity in the labor market, the short-term returns should be the same for everyone who obtained the signal, independent of any unobservable skills or background characteristics. Second, as the immediate returns from graduating should not differ between individuals, the decision to leave university should be mostly driven by the (psychic) cost of education, rather than the potential earnings after finishing.

2.5.1 Heterogeneities in Signaling

A key assumption of the signaling model is that an individual's productivity type is not directly observable and that employers therefore use the signal to infer an individual's productivity. If a degree is no more than a way of signaling (future) productivity, then the expected returns should ideally apply to everybody who obtains that signal, and the signaling value should not vary between individuals with the same observable (but different unobservable) characteristics.

However, it is important to realize that Spence's signaling theory did not include any kind of labor market discrimination. Some background characteristics are usually observable in the application process and discrimination with respect to wage or other labor market outcomes is a widely-documented phenomenon in Western labor markets. Hence, one could expect to observe heterogeneities in the signaling value concerning characteristics that are discriminated against, such as gender (see Belman and Heywood, 1991, for earlier evidence on heterogeneities in signaling values for women and minorities). Moreover, the model of Spence abstracts from the fact that various educational degrees exist, e.g. graduating from different fields or majors. These degrees can be interpreted as distinct signals, which are valued differently in the labor market. Hereby, each type of degree may signal different underlying unobservable characteristics, such as stamina, on-the-job productivity, or creativity.

To test whether there are heterogeneous signaling values, we include interaction terms between the degree dummy and various background variables. We estimate the following equation:

$$W_i^c = \beta_0 + \beta_1 degree_i^c + \beta_2 semesters_i^c + \beta_3(degree_i^c * X_i) + \gamma_i \tag{2.7}$$

where X_i is a set of background characteristics comprising gender, socioeconomic background, study characteristics and perceived relative job ability, to test whether these characteristics matter for the value of the degree signal in the labor market.

Table 2.5 displays the regression results. Overall, it seems that the expected returns from the degree do not strongly depend on individual skills or background characteristics, with two main exceptions: gender and major. The interaction term with the gender dummy shows a statistically significant positive effect for males, where the expected signaling value is roughly three percentage points higher for males than for females. The existence of gender discrimination in the labor market is an intuitive explanation for this finding. In addition, the interaction terms with the major categories (humanities/social sciences, medicine, STEM, law and economics/business) are statistically significant. With the humanities/social sciences major as a baseline, we observe a higher signaling value for medicine and STEM majors. As explained before, this result is reasonable as graduating in a different major can be interpreted as acquiring a different signal.

On the other hand, we do not see any significant heterogeneities based on socioeconomic background, perceived job ability or GPA. Regarding GPA, it is surprising that grades do not seem to play a role for the valuation of the signal, as a high GPA could function as an additional signal in the labor market. However, grades may often be specific to the university, the study program or the federal state in which the degree was obtained and hence might be difficult for employers to evaluate. Furthermore, grades reflect *academic* ability, which is assumed to be correlated to job ability but is not necessarily similar to worker productivity, which could explain why GPA does not function as an additional signal.

In addition, the two characteristics associated with socioeconomic status – i.e. the indicators for migration background and having at least one parent with an academic degree – do not appear to affect the value of the signal. As especially parents' educational background is unobserved by potential employers, the lack of a significant interaction term is suggestive evidence of the signaling theory, which states that the signal should be independent of unobservable characteristics. The same holds true for perceived work ability, as table 2.5 presents evidence that the perceived work ability of students has no effect on the value of the expected signal. The signaling interpretation of this finding is further supported by the outcomes in section 2.4.3, which shows that students do expect their work ability to yield wage returns in the long run.

In appendix table 2.A5, we show the same results for bachelor students. The findings with respect to gender and majors are similar to those for master students. Nonetheless, for bachelor students we observe heterogeneities based on migration background and

Table 2.5: Heterogeneities in immediate wage returns

	Starting wage (logs)				
	(1)	(2)	(3)	(4)	(5)
Semesters	0.00624	0.00646	0.00690	0.00562	0.00493
	(0.00434)	(0.00437)	(0.00439)	(0.00435)	(0.00432)
Degree	0.187***	0.203***	0.201***	0.217***	0.202***
	(0.0160)	(0.0173)	(0.0171)	(0.0445)	(0.0449)
Interaction effects:					
Sex*Degree	0.0447***				0.0302**
	(0.0126)				(0.0130)
Academic*Degree		-0.00634			-0.00568
		(0.0122)			(0.0120)
Migrat*Degree		0.0278			0.0262
		(0.0182)			(0.0179)
Perc. job ability*Degree			0.00140		-0.000707
			(0.0124)		(0.0124)
Gpa*Degree				-0.00799	-0.00636
				(0.00600)	(0.00609)
Majors:					
Medicine*Degree				0.0720**	0.0737**
				(0.0308)	(0.0311)
STEM*Degree				0.0844***	0.0765***
				(0.0171)	(0.0176)
Law*Degree				0.0302	0.0287
				(0.0693)	(0.0675)
Economics*Degree				0.0201	0.0152
				(0.0163)	(0.0164)
Constant	10.29***	10.29***	10.29***	10.28***	10.28***
	(0.0143)	(0.0144)	(0.0144)	(0.0143)	(0.0143)
N	1381	1381	1381	1381	1381
adj. R^2	0.511	0.508	0.507	0.520	0.522

Notes: Table 2.5 shows the interaction between the degree premium and background characteristics on log yearly starting wages. All regressions are controlled for licensing effects. The baseline major is humanities and social sciences. The sample only includes master students who have a maximum of eight semesters left until they reach their degree. Standard errors are in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

parental SES.[26] We discuss this finding in detail in appendix section 2.D.

2.5.2 Determinants of Leaving

The second implication from the signaling theory relates to students' decision whether or not to complete tertiary education. As the returns from graduating should not substantially differ between individuals sending the same signal, the decision to select out of education should be driven by the (psychic) cost of education only, and not the potential earnings gain from finishing. Besides testing this implication of Spence's theory in our data, the following analysis is also informative about the determinants of student dropout. This is a relevant issue as our previous analysis has shown that the largest part of the return to studying is associated with graduating, and hence leaving university earlier is very costly. Nonetheless, 11% of all master students in Germany leave university without a degree (Heublein and Schmelzer, 2014).[27]

To test the second hypothesis, we regress the perceived probability of leaving university without a degree on the immediate wage and non-wage returns to graduating, study performance and satisfaction, and background characteristics. For the wage returns, we compute the absolute difference of expected entry wages between the graduation and leaving scenario. For the non-wage returns, we use standardized differences of expected immediate returns between scenarios. The results are presented in table 2.6. In columns 1 and 2, we include both wage and non-wage returns and test whether the returns from graduating predict expected leaving probabilities. As we know that the signaling value depends on the chosen major, we additionally control for majors in column 2 to test whether the probability to leave is affected by major-specific wage returns. The table shows that expected wage returns do not seem to affect students' leaving probability. This finding is in line with the hypothesis that wage returns should not matter for deciding whether to obtain the signal, as the returns are the same for everybody who acquires the signal. For non-wage returns, it is less clear what to expect, as they might not be perfectly correlated with wage returns and – unlike wage returns – they may differ between individuals with the same type of degree. We indeed see that increased job satisfaction and job-finding probability returns reduce the probability of leaving.

Concentrating on the cost-related variables included in column 3, we find additional support for the second hypothesis. Study satisfaction – which is an indicator of the current consumption utility of studying and a proxy of psychic costs – is strongly associated with the probability of leaving university. Being satisfied instead of dissatisfied with one's studies reduces the leaving probability by over five percentage points. Further, we include ability measures that can be thought of as being related to effort costs,

[26] Higher perceived returns among younger students from high SES backgrounds have also been documented in e.g. Boneva and Rauh (2017).

[27] For bachelor students, the observed dropout rate is even 28%. These data were collected in Germany and refer to the student cohort graduating in 2012.

Table 2.6: Determinants of probability to leave university

	Leaving probability		
	(1)	(2)	(3)
Wage returns (in 1,000 Euro)	-0.0255	-0.0220	-0.0514
	(0.0546)	(0.0565)	(0.0550)
Job satisfaction return	-1.333***	-1.431***	-1.425***
	(0.497)	(0.506)	(0.496)
Job finding prob. return	-1.708***	-1.679***	-1.494***
	(0.576)	(0.571)	(0.569)
Satisfied with studies			-5.345***
			(1.347)
Male			1.017
			(0.897)
Academic parent(s)			0.110
			(0.897)
Migration background			2.254
			(1.488)
Study GPA			-1.366***
			(0.408)
High-school GPA			0.215
			(0.284)
Perceived academic ability			-0.0271
			(0.0268)
Constant	7.452***	9.150***	22.89***
	(0.677)	(2.437)	(4.059)
N	1381	1381	1381
adj. R^2	0.012	0.012	0.041
Controlled for major	No	Yes	Yes
Mean leaving probability	7.75	7.75	7.75

Notes: Table 2.6 shows the effects of the expected returns from graduating and several background characteristics on the probability to leave university without a further degree. The wage returns are computed by taking the absolute difference of expected labor market entry wages between the graduation and leaving scenario. The returns of both job satisfaction and job-finding probability are expressed in standard deviations according to equation 2.5. The sample only includes master students who have a maximum of eight semesters left until they reach their degree. Standard errors are in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

as a lower academic ability may make studying more difficult. Accordingly, we find that having a higher study GPA reduces the leaving probability.

Taken together, we find support for the second testable implication of the signaling theory. Students seem to mainly base their decision whether or not to leave university at an early stage on cost-related factors, while wage returns are not predictive for leaving.

2.6 Conclusion

While substantial returns to university education have been documented in a large body of empirical literature, the extent to which these returns reflect the signaling versus productivity-enhancing human capital effect of education remains open to debate. Based on innovative data with measures of counterfactual labor market outcomes for graduating and leaving university without a degree, this paper documents large perceived returns to degree completion. In particular, estimates from within-person fixed effects models display substantial signaling effects of around 20% in terms of starting wages for a master degree, exceeding the human capital effect of education by 3-5 times over the course of studies. Besides, the perceived signaling value is persistent or even increasing over time, such that employer learning seems to be relatively unimportant for expected life-cycle wage developments.

Although in terms of methodology our approach differs from the existing literature, our findings are complementary. First, we provide novel evidence that among current students *perceived* signaling tends be important and highly persistent in terms of lifetime wages. Second, our findings are in line with two predictions from the signaling theory: (i) heterogeneities in perceived signaling – albeit for different fields of study – are relatively unimportant when compared to the overall effect of obtaining a degree, and (ii) when compared to the psychic cost of studying, the graduation premium matters little for the perceived probability of leaving university without a (further) degree. Third, using within-individual variation and information on students' grades we can largely dismiss an alternative (selection) hypothesis that dates back to Chiswick (1973) (see also Lange and Topel (2006)), stating that the graduation premium arises because graduates are disproportionately comprised of individuals whose returns to education are particularly large. If this hypothesis held true, it would be unlikely to observe homogeneously high within-individual returns to degree completion.

Our results hold implications for understanding students' motivations to study and for economic policy. First, given their expectation of substantive signaling effects, students' main motivation to attend higher educational institutions should be to obtain credentials rather than to learn new skills, concepts, and material. Thus, in light of our findings, common complaints among professors regarding their students' limited willingness to study material beyond what is on the exam seem warranted. Moreover, our findings provide a rationale for the sustained demand for enrollment in selective educational institutions, even though many studies find no benefits in terms of learning achievements or actual wages (see e.g. Dale and Krueger, 2002). In terms of policy, the fact that most of the perceived returns to education are private implies that tuition fees should have little effect on student enrollment. Thus, our findings may explain why a temporary introduction of tuition fees in Germany – although contested politically – had only small effects on study take-up (Hübner, 2012). Finally, the finding that perceived returns are unable to predict perceived university-leaving probabilities suggests that policies to fight student dropout should focus on measures that target the psychic costs of studying rather than e.g. the perception of future returns.

The paper also opens up several avenues for future research. First, our results only hold for individuals who are currently enrolled at a university or college of applied sciences. In this sense, it would be valuable to extend the analysis to high-school students, e.g. to study the effect of the perceived graduation premium for the extensive margin of student enrollment. Second, it would be informative to investigate whether the labor demand side (e.g. human resource managers) holds similar perceptions regarding the relative importance of signaling and human capital values and how perceptions on either side translate into equilibrium wage outcomes.

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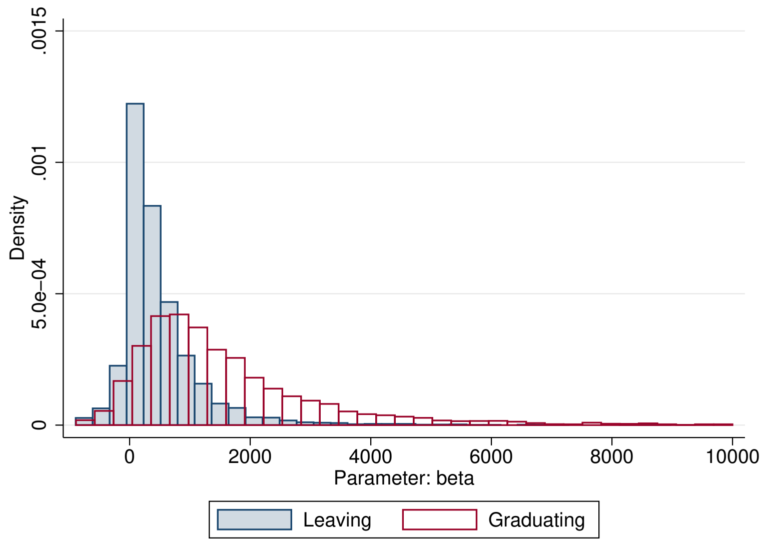
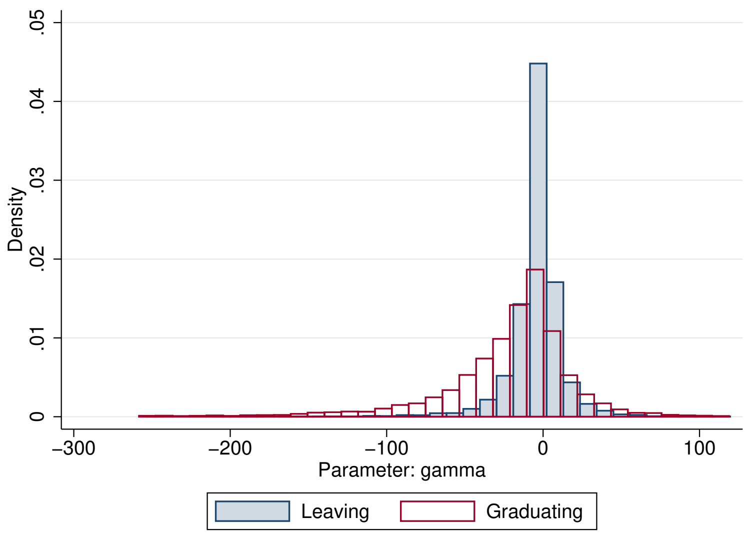
Appendix

2.A Additional Figures and Tables

Figure 2.A1: Computed parameters of the mincer wage equation



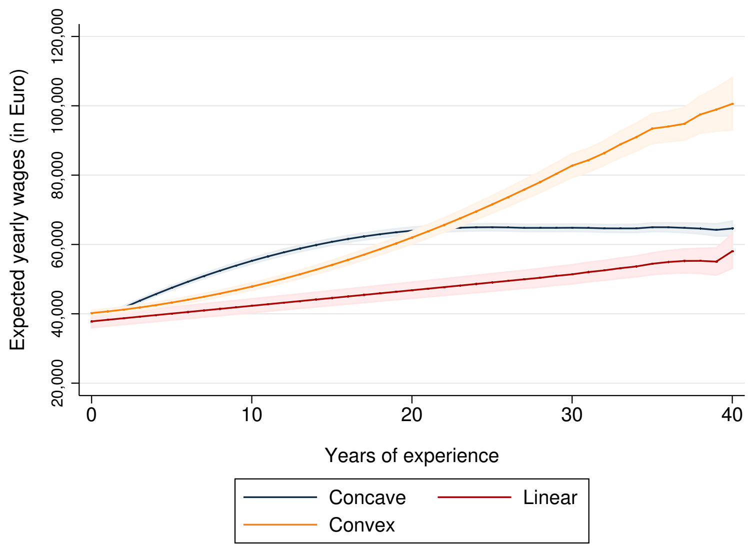
(a) Density of slope parameter



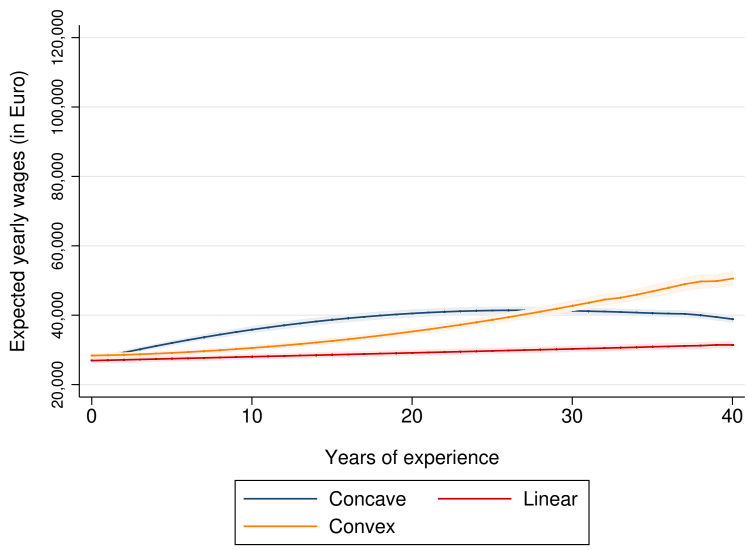
(b) Density of curvature parameter

Notes: Panel A of figure 2.A1 shows the distribution of the computed slope parameter β from equation 2.1 by the scenarios of graduating and leaving. Panel B shows for both scenarios the respective curvature parameter γ of equation 2.1. The underlying sample of both figures is winsorized at a one percent level.

Figure 2.A2: Expected wage trajectory patterns



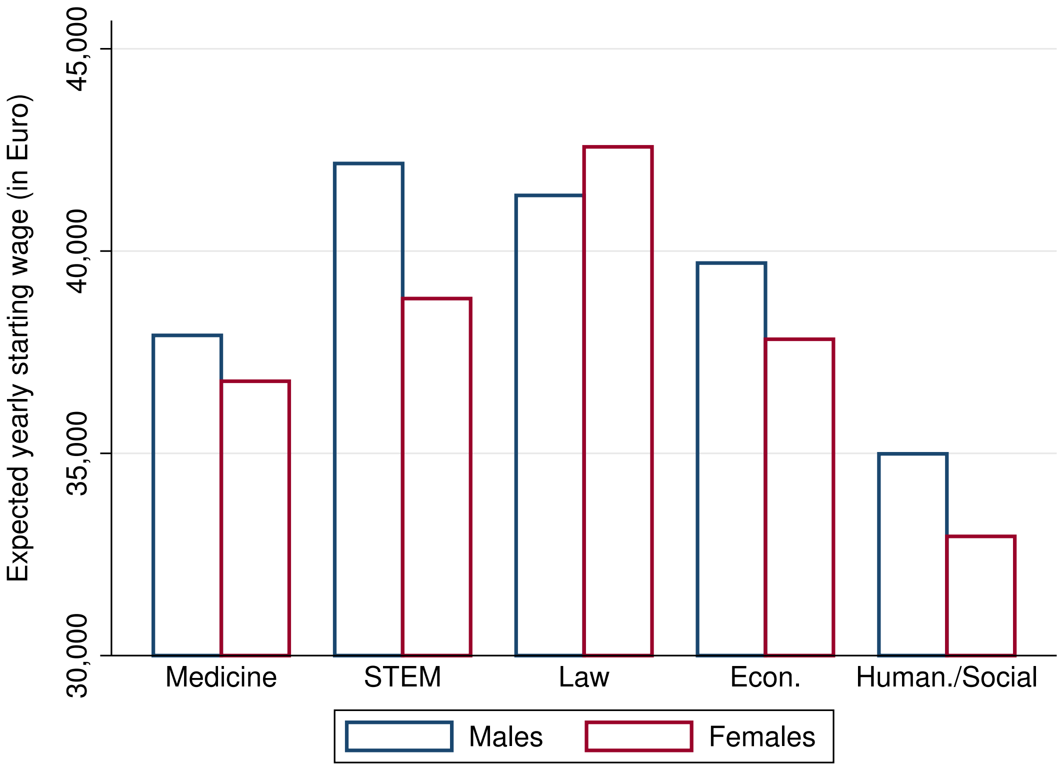
(a) Average wage after graduation



(b) Average wage after leaving

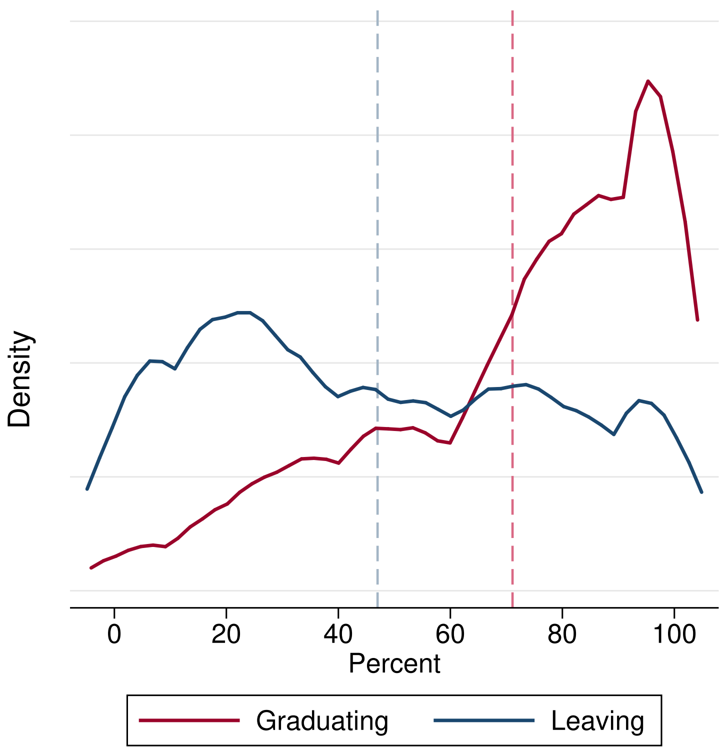
Notes: Figure 2.A2 shows the expected wage trajectories patterns over the working life. The concave, linear and convex growth patterns are classified based on the parameters computed from equation 2.1. Panel A shows the wage trajectories for the scenario of graduating and panel B for the scenario of leaving university.

Figure 2.A3: Expected starting wage after graduating by gender and major



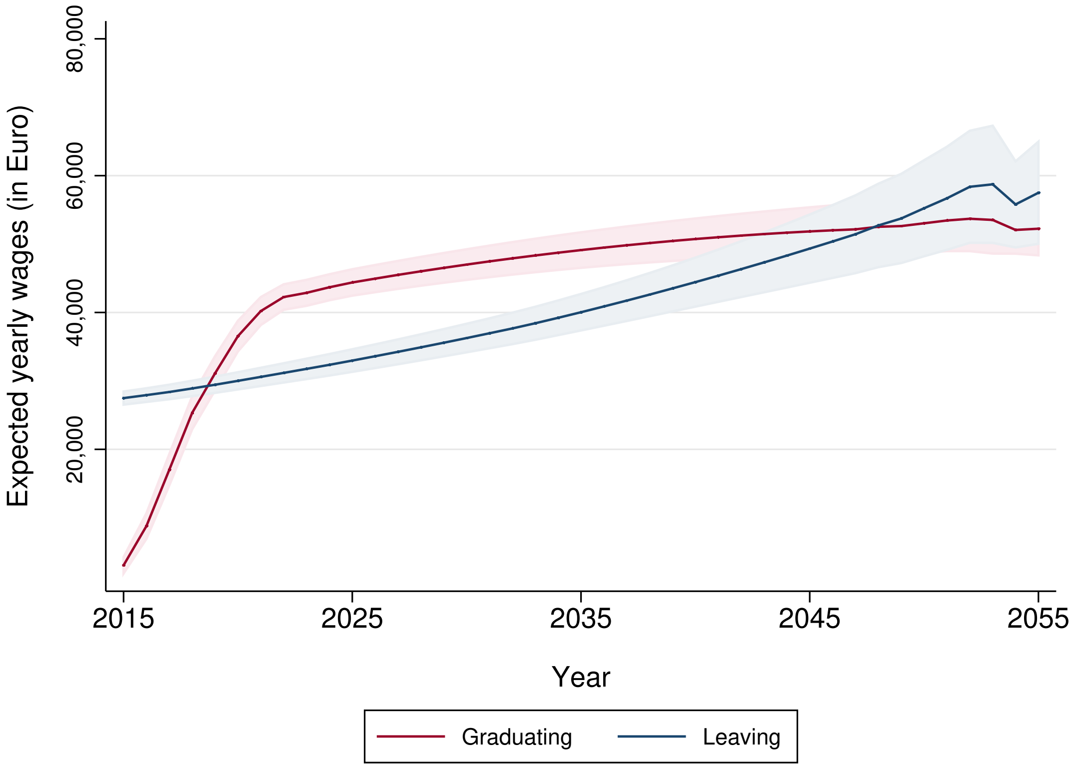
Notes: Figure 2.A3 displays the averages of the expected yearly wage at labor market entry after graduating university by gender and major.

Figure 2.A4: Density of job finding probability at labor market entry



Notes: Figure 2.A4 displays the density of the expected probability of finding a suitable job at labor market entry for both scenarios. The dashed lines indicate the average expected job-finding probability at labor market entry, which is 71.1% for graduating and 47.0% for leaving university without a degree.

Figure 2.A5: Expected yearly wage over the life time, conditional on diminishing wage differences



Notes: Figure 2.A5 shows the development of the expected yearly wage over the life time for students who expect to diminish the wage difference between the graduating and leaving scenario. The colored areas around the lines indicate the 95% confidence intervals.

Table 2.A1: Immediate wage returns (bachelor students)

	(1) Starting wage levels	(2) Starting wage logs	(3) Starting wage logs
Semesters	253.1***	0.00592***	0.00616***
	(92.24)	(0.00220)	(0.00219)
Degree	10491.2***	0.326***	0.321***
	(405.8)	(0.0102)	(0.0105)
Interaction effects:			
Licence*Degree			0.0362***
			(0.0101)
Civil servant*Degree			-0.0261**
			(0.0124)
Constant	27991.6***	10.17***	10.17***
	(396.7)	(0.00973)	(0.00972)
N	4384	4384	4384
adj. R^2	0.486	0.598	0.600

Notes: Column 1 in table 2.A1 shows the effects of semesters studied and obtaining a degree on the level of yearly starting wages, while the dependent variable in columns 2 and 3 comprises of the log starting wages. The sample only includes bachelor students who have maximum eight semesters left until they reach their degree. Standard errors are in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 2.A2: Immediate non-wage returns (bachelor students)

	(1) Satisfaction	(2) Satisfaction	(3) Job finding probability	(4) Job finding probability
Semesters	0.00679	0.00805	0.00855	0.00995
	(0.0126)	(0.0126)	(0.00877)	(0.00874)
Degree	1.484***	1.463***	0.815***	0.774***
	(0.0603)	(0.0618)	(0.0423)	(0.0433)
Interaction effects:				
Licence*Degree		0.177***		0.238***
		(0.0609)		(0.0419)
Civil servant*Degree		-0.161**		-0.128**
		(0.0683)		(0.0500)
Constant	0.0300	0.0356	0.0378	0.0440
	(0.0567)	(0.0566)	(0.0396)	(0.0395)
N	4384	4384	4384	4384
adj. R^2	0.461	0.463	0.347	0.352

Notes: Columns 1 and 2 in table 2.A2 show the effects of semesters studied and obtaining a degree on expected job satisfaction at labor market entry, while the dependent variable in columns 3 and 4 is the expected probability to find a suitable job. Both job satisfaction and job-finding probability are expressed in standard deviations according to equation 2.5. The sample only includes bachelor students who have maximum eight semesters left until they reach their degree. Standard errors are in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 2.A3: Immediate wage returns with semester dummies

	Immediate returns
5 Semes. until degree	0.00445
	(0.0328)
4 Semes. until next degree	0.0182
	(0.0303)
3 Semes. until next degree	0.0153
	(0.0298)
2 Semes. until next degree	0.0277
	(0.0295)
1 Semes. until degree	0.0515
	(0.0334)
Degree	0.251***
	(0.0272)
N	1381
adj. R^2	0.506

Notes: Table 2.A3 shows the effects of semesters studied and obtaining a degree on the log starting wage, according to equation 2.6. The sample only includes master students who have maximum eight semesters left until they reach their degree. The baseline is to have six or more semesters until graduation. Standard errors are in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 2.A4: Robustness analyses (bachelor students)

	Regular study time			Best third in high-school			Max 12 semesters		
	(1) Starting wage levels	(2) Starting wage logs	(3) Starting wage logs	(4) Starting wage levels	(5) Starting wage logs	(6) Starting wage logs	(7) Starting wage levels	(8) Starting wage logs	(9) Starting wage logs
Semesters	90.06	0.00814	0.00863	62.17	-0.000812	-0.000421	70.80	0.00200	0.00216
	(244.6)	(0.00577)	(0.00573)	(163.9)	(0.00386)	(0.00386)	(68.49)	(0.00170)	(0.00170)
Degree	10884.9***	0.312***	0.303***	11374.9***	0.362***	0.352***	11166.6***	0.341***	0.336***
	(932.6)	(0.0230)	(0.0231)	(749.5)	(0.0187)	(0.0195)	(348.6)	(0.00898)	(0.00934)
Interaction effects:									
Licence*Degree			0.0615***			0.0425**			0.0356***
			(0.0201)			(0.0183)			(0.00979)
Civil servant*Degree			-0.0323			-0.00976			-0.0236**
			(0.0238)			(0.0229)			(0.0118)
Constant	27121.9***	10.17***	10.18***	26977.5***	10.13***	10.14***	27149.0***	10.15***	10.16***
	(921.2)	(0.0222)	(0.0221)	(726.2)	(0.0176)	(0.0176)	(330.1)	(0.00838)	(0.00838)
N	1311	1311	1311	1421	1421	1421	4794	4794	4794
adj. R^2	0.452	0.566	0.569	0.488	0.600	0.602	0.483	0.596	0.597

Notes: Table 2.A4shows robustness analyses of the effects of semesters studied and obtaining a degree on yearly starting wages. Columns 1 to 3 comprise students who are expected to finish within regular study time, i.e. four semesters in total. Columns 4 to 6 include every student who had a high-school GPA in the highest 33% of the sample. Column 7 to 9 includes all students who are in the 12th semester or less. The sample only includes bachelor students. Standard errors are in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 2.A5: Heterogeneities in immediate wage returns (bachelor students)

	Starting wage (logs)				
	(1)	(2)	(3)	(4)	(5)
Semesters	0.00601***	0.00606***	0.00640***	0.00534**	0.00545**
	(0.00218)	(0.00219)	(0.00219)	(0.00215)	(0.00214)
Degree	0.288***	0.306***	0.309***	0.248***	0.207***
	(0.0109)	(0.0111)	(0.0115)	(0.0214)	(0.0221)
Interaction effects:					
Sex*Degree	0.0722***				0.0402***
	(0.00871)				(0.00920)
Academic*Signal		0.0158*			0.0198**
		(0.00874)			(0.00851)
Migrat*Degree		0.0510***			0.0411***
		(0.0119)			(0.0117)
Perc. job ability*Degree			0.0212**		0.0157*
			(0.00878)		(0.00880)
Gpa*Degree				-0.00377	-0.00289
				(0.00315)	(0.00323)
Majors:					
Medicine*Degree				0.0801***	0.0822***
				(0.0202)	(0.0200)
STEM*Degree				0.160***	0.146***
				(0.0116)	(0.0121)
Law*Degree				0.168***	0.164***
				(0.0452)	(0.0454)
Economics*Degree				0.103***	0.0956***
				(0.0114)	(0.0115)
Constant	10.17***	10.17***	10.17***	10.17***	10.17***
	(0.00963)	(0.00970)	(0.00970)	(0.00954)	(0.00948)
N	4384	4384	4384	4384	4384
adj. R^2	0.606	0.602	0.600	0.619	0.623

Notes: Table 2.A5 shows the interaction between the degree premium and background characteristics on log yearly starting wages. All regressions are controlled for licensing effects. The baseline major is humanities and social sciences. The sample only includes bachelor students who have a maximum of eight semesters left until they reach their degree. Standard errors are in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

2.B Counterfactual Labor Market Questions

How do you expect your future workday when you finish your first choice [...]? Estimate the following variables for the different stages of life.

	Working hours/Week	Salary/Year (gross in €)
at career start	[ ]	[ ]
at age 40	[ ]	[ ]
at age 55	[ ]	[ ]

(Original: *Wie erwarten Sie Ihren zukünftigen Arbeitsalltag, wenn Sie ihre erste Wahl [...] zu Ende studieren? Schätzen Sie die folgenden Variablen jeweils für die verschiedenen Lebensabschnitte.*)

How do you expect your future workday when you cannot complete a degree and start working without a degree? Estimate the following variables for the different stages of life.

	Working hours/Week	Salary/Year (gross in €)
at career start	[ ]	[ ]
at age 40	[ ]	[ ]
at age 55	[ ]	[ ]

(Original: *Wie erwarten Sie Ihren zukünftigen Arbeitsalltag, wenn Sie kein Studium abschließen können und ohne Studienabschluss beginnen zu arbeiten? Schätzen Sie die folgenden Variablen jeweils für die verschiedenen Lebensabschnitte.*)

How do you rate the likelihood of not finding a suitable job for the various scenarios at the time of starting your career?

Completion first choice [...]	[ ]
Dropout - no degree	[ ]

(Original: *Wie schätzen Sie die Wahrscheinlichkeit zum Zeitpunkt des Berufseinstiegs keinen passenden Job zu finden für die verschiedenen Szenarien ein?*)

How do you rate the likelihood of not finding a suitable job for the various scenarios at age 40?

Completion first choice [...]	[ ]
Dropout - no degree	[ ]

(Original: *Wie schätzen Sie die Wahrscheinlichkeit mit 40 Jahren keinen passenden Job zu finden für die verschiedenen Szenarien ein?*)

How do you rate your professional satisfaction at the time you started your career for the various scenarios?
$1 \rightarrow$ very dissatisfied, $10 \rightarrow$ very satisfied

	1	2	3	4	5	6	7	8	9	10
Completion first choice [...]	○	○	○	○	○	○	○	○	○	○
Dropout - no degree	○	○	○	○	○	○	○	○	○	○

(Original: *Wie schätzen Sie Ihre berufliche Zufriedenheit zum Zeitpunkt des Berufseinstiegs für die verschiedenen Szenarien ein?*)

How do you rate your professional satisfaction at age 40 for the various scenarios?
$1 \rightarrow$ very dissatisfied, $10 \rightarrow$ very satisfied

	1	2	3	4	5	6	7	8	9	10
Completion first choice [...]	○	○	○	○	○	○	○	○	○	○
Dropout - no degree	○	○	○	○	○	○	○	○	○	○

(Original: *Wie schätzen Sie Ihre berufliche Zufriedenheit mit 40 Jahren für die verschiedenen Szenarien ein?*)

2.C Data-Cleaning Rules

For our analysis, it was important that all included individuals filled in the following variables: expected labor market outcomes for the leaving and finishing scenarios at all points in time, probability of leaving university, gender, age, degree enrolled in, semesters done, semesters left until next degree, perceived academic ability, perceived job ability, GPA, high-school GPA, study costs, study satisfaction, university major, academic parents, and migration background. If one of these were missing, we excluded the individual from our sample.

As individuals could fill in any expected wage and working hours, we clean them to remove implausible values. With respect to working hours, this means that we exclude values above 168 hours, as this is the maximum amount of hours within a week (amounts to less than 0.05% of our sample). For wages, we first calculated the wage per hour by dividing the yearly wage by 52 and the indicated working hours per week. We then exclude everybody who has a hourly wage of below €7.50, which is even lower than the minimum wage of €8.50 that was introduced in Germany at the beginning of 2015. In addition, we exclude people who have an hourly wage above €80 at labor market entry or above €240 at age 40 and 55. For the remaining sample, we multiply the hourly wage by 2080 to obtain yearly full-time wage expectations.

2.D Bachelor vs. Master Students

In our analysis of the signaling effect, we focus on master students, given that they face less ambiguity about both their own abilities and the possible pathways in the counterfactual labor market scenarios. While a master's degree is an additional university degree on top of an existing bachelor degree, bachelor students only achieve their first academic degree when graduating. Therefore, leaving bachelor studies is likely to be associated with higher uncertainty compared with leaving master studies.

First, the potential pathways in the labor market after leaving are more straightforward for master students. Bachelor students who do not obtain a degree will enter the labor market without any academic degree, while leaving master studies always comes with the outside option of "falling back" on one's first academic degree. As most job opportunities for master students are also open for bachelor graduates, job prospects for leaving are much closer to the graduating plans that master students would pursue. For bachelor students, there exists not only uncertainty with respect to the wage when leaving, but also with respect to the type of job they can do. Non-degree leavers might need to apply to different kind of jobs – potentially even in a different sector – compared to graduates. We mitigate this effect by controlling for licensing, although compared to master students the uncertainty bachelor students face remains higher.

Second, students might face some ambiguity with respect to their own study and work ability. When survey respondents estimate future wages for the two labor market scenarios, they might condition their beliefs on their own abilities, which are ex-ante still unknown to themselves. For the leaving scenario, they might be expecting to find themselves in a bad state, in which their ability turned out to be worse than for the graduating scenario. In the master studies, prior study experience should help to resolve the uncertainty about own study ability and the productivity-enhancing effect of obtaining a degree. However, for bachelor students, graduating informs them about their abilities and part of the premium that we observe for bachelor students might stem from individuals conditioning the counterfactual expectations on the signal about their productivity (Stinebrickner and Stinebrickner, 2012; Arcidiacono et al., 2016). This would lead to an overestimation of the signaling effect. For master students, the premium to finishing the degree is less affected by ambiguity about own ability, as students have already spent several years at university. They thus dispose of information on their skills from their bachelor studies.

When we look at our results, the higher uncertainty for bachelor students makes it unsurprising that we indeed find the magnitude of the estimated signal to be higher for bachelor students (32.8%) than for master students (20.6%). Nonetheless, the patterns for bachelor and master students are still closely comparable for our results in section 2.4. However, the differences between bachelor and master students become more prominent when we examine heterogeneities in section 2.5.1. For master students, the signal in general does not depend on background characteristics, which is in line with the signaling theory. For bachelor students, a migration background, having academic parents and having a higher perceived job ability positively influence the importance of the signal, although the magnitude of the effects remains moderate compared to the effect size of the signal itself. These heterogeneous effects are likely to be driven by the larger ambiguity that bachelor students face about the two scenarios. For instance, if there is high uncertainty about the segment of the labor market in which a person can work after leaving, and having academic parents is only beneficial if the student enters an academic job, a discrepancy based on parental background may arise.

2.E Negative Signaling

In this paper, we assume that obtaining a degree from university yields a positive signaling value in the labor market. Alternatively, it is conceivable that leaving university without a degree sends a negative signal to the labor market. Similar to a positive signal when graduating, leaving university might inform potential employers about unobservable abilities, such as a lack of perseverance or motivation. In the following, we explain why we think the assumption of a positive signaling value is reasonable. We also show that even without this assumption, the absolute size of our estimated signaling value remains valid.

Assuming that education sends a positive signal in the labor market is in line with most of the literature. The latter assumption is also reasonable as individuals usually have the freedom not to inform employers about an unfinished degree. As leaving university without graduating is not a (negative) signal that has to be necessarily send in the labor market, individuals very often would not mention it in their application. When applying to a job, students who left before graduating would most of the time only include their highest education level obtained and if possible would not make dropout salient. Thus, education can be used as a positive signal in the labor market, although it is unlikely to be used as a negative signal.

Nevertheless, even if a (partly) negative signal exists, the overall value of the signal stays the same. The main difference between graduating yielding a positive signal and graduating meaning to avoid sending a negative signal lies in the relative importance of the human capital effect. The equations described below show the implications of this assumption.

In our data, we observe the university-leaving wage $W_i^l(T)$ and the graduation wage $W_i^f(T)$ both in expectation. Obtaining a positive signal when graduating implies that the university-leaving wage shortly before the degree (at time T) resembles the human capital effect $HC_i^+(T)$, where the "+" indicates that we assume a positive signal: $signal_i^+$ (likewise a "$-$" indicates the supposition of a negative signal: $signal_i^-$). The following equations show how the positive signal is calculated:

$$\begin{aligned} HC_i^+(T) &= W_i^l(T) \\ HC_i^+(T) + signal_i^+ &= W_i^f(T) \\ \Rightarrow \boldsymbol{signal_i^+} &= \boldsymbol{W_i^f(T) - W_i^l(T)} \end{aligned}$$

Now we can calculate the signal under the assumption that graduating means avoiding to send a negative signal in the labor market. Hence, the expected graduation wage corresponds to the full human capital value $HC_i^-(T)$, whereas the university-leaving wage resembles the human capital value minus the absolute value of the negative signal: $|signal_i^-|$.

$$
\begin{aligned}
HC_i^-(T) &= W_i^f(T) \\
HC_i^-(T) - |signal_i^-| &= W_i^l(T) \\
\Rightarrow \boldsymbol{|signal_i^-|} &= \boldsymbol{W_i^f(T) - W_i^l(T)}
\end{aligned}
$$

We can see that the absolute value of the signaling value is unaffected by the assumption regarding the sign of the signal as $|signal_i^-| = signal_i^+$. However, as we assume that $W_i^f(T) < W_i^l(T)$, the human capital value differs between the two suppositions, with a smaller human capital value under the assumption of a positive signaling value: $HC_i^+(T) < HC_i^-(T)$.
Note that both outcomes also hold true if we assume that graduating leads to both a positive signal due to the degree and the avoidance of a negative signal that would be associated with leaving university:

$$
\begin{aligned}
HC_i^{both}(T) &= W_i^l(T) + |signal_i^-| \\
HC_i^{both}(T) + signal_i^+ &= W_i^f(T) \\
\Rightarrow W_i^f(T) &= W_i^l(T) + |signal_i^-| + signal_i^+ \\
\Rightarrow \boldsymbol{|signal_i^-| + signal_i^+} &= \boldsymbol{W_i^f(T) - W_i^l(T)}
\end{aligned}
$$

In this case, measuring the human capital value is not possible without making further assumptions on the size of the two signals, as there exists no state of the world in which no signal is send. Nevertheless, one could calculate a lower and upper bound as the magnitude of the human capital value must lie between the other two scenarios $HC_i^+(T) < HC_i^{both}(T) < HC_i^-(T)$.
Altogether, the assumptions regarding the sign of the signaling value has an impact on how to interpret the human capital vale. However, our estimate of the signaling value is valid under all possible assumptions.

Chapter 3

A Setback Set Right? Unfortunate Timing of Family Distress and Educational Outcomes

3.1 Introduction

Many children experience some type of family distress during school age. Examples are divorce, parental unemployment, and illness or death of a family member. From the literature we know that these events can have a negative effect on children's educational accomplishments (e.g. Oreopoulos et al., 2008; Francesconi et al., 2010; Amato and Anthony, 2014). However, it is less apparent whether the lower school performance arises purely from the family setback altering childhood conditions, or if the institutional setup might prevent these children to reach their full potential? In particular, it is unclear how the consequences of family distress are mediated by one of the most important determinants of equality of opportunity: the educational environment.

To enhance educational equality one of the key instruments many countries employ is standardized testing to assess children's qualifications. The idea behind standardized testing is that it provides an objective measure of ability, which is free of biases regarding children's background characteristics. Consequently, standardized tests are often used to inform important educational decisions such as secondary school track placement or admission to higher education. Yet, in the light of family distress, standardized tests may introduce new biases and inequalities if they are taken under unequal test conditions. Even a relatively minor family setback could possibly have long-term negative consequences if its timing with respect to a critical standardized test is unfortunate. At the same time, the educational environment might comprise of features that better adopt to temporary distress situations such as teacher evaluations, repetitive testing, or re-assessments of educational decisions.

In this paper I am concerned with educational equality of opportunity after children experience adverse life-events during childhood. First, I explore if the consequences of family distress are aggravated in settings that employ high-stakes standardized testing. Second, I analyze whether there are educational practices that can mitigate the potential negative effects of family distress.

To answer these questions, I investigate how children's educational outcomes are affected by experiencing a common form of childhood distress - grandparental death - shortly before taking a high-stakes standardized test. I focus on grandparental death as it is a relatively common event in children's life that can cause immediate distress. In many families, grandparents are key figures in a child's life, with more than 40 percent of grandparents in Europe frequently caring for their grandchildren (Glaser et al., 2013). Distress after grandparental death materializes through two main potential pathways: via the child itself or via the child's parents. When children experience emotional distress this can lead to poorer educational performance, for instance by lowering the ability to concentrate. In addition, when parents are grieving this may reduce the mental and time resources available to children, which could in turn adversely affect a child's educational outcomes. As emotional distress after the death of a grandparent is likely to be of a temporary nature, at first sight it appears that this should not have lasting consequences for educational outcomes per se. However, the literature shows that even short-lived disadvantages before birth can have persisting effects in adulthood (for a review see Almond et al., 2018). Especially due to the dynamic nature of human capital development, temporary setbacks at crucial moments in a child's life may have lasting consequences (Cunha and Heckman, 2007; Cunha et al., 2010).

Regarding the high-stakes standardized test, I use a feature of the Dutch education system where children in the final grade of primary school participate in a standardized test that co-determines their secondary school track placement. This is a suitable setting to investigate the research questions, as first it allows me to estimate the immediate effects of experiencing the death of a grandparent shortly before the test on test scores itself, as well as the short- and long-term effects on track placement, track attendance and graduation performance. Second, the Dutch context bears the advantage that I can analyze several educational practices that may mitigate the consequences of the initial shock. In particular, I investigate the role of the option to take a makeup test, the existence of teacher recommendations, as well as the possibility to switch tracks after the initial placement.

I identify potential effects by taking advantage of the quasi-random timing of grandparental death in relation to the track placement test, in a similar vein to Persson and Rossin-Slater (2018). Using rich administrative data from the Netherlands, I construct the treatment and control group based on whether children experience grandparent bereavement respectively shortly before or after the test. By comparing the educational outcomes of both groups I identify the causal effect of grandparental death if conditional on the occurrence of a grandparent dying, the exact timing with respect to the test is random. As both the treatment and control group experience grandparental death in the three months surrounding the test, this strategy solves two main threats for causal identification. First, the strategy addresses potential selection bias that arises as grandparental death might be correlated with parental age, life-style and socioeconomic background. Second, the approach subtracts other effects the death of a grandparent may have on child education outcomes, for example due to a change in financial resources or childcare support. As a result, the estimated effect solely reflects how the standardized test influences the consequences grandparental death has on children's educational outcomes.

The findings show that experiencing grandparent bereavement shortly before a high-stakes standardized test can have longstanding negative effects on educational outcomes. Losing a grandparent during the three months prior to the test, lowers children's test score by roughly 3 percent of a standard deviation. I observe this decrease in test performance despite that the participation rate of the makeup test doubles for the treatment group, most likely because the fraction of children who takes the makeup test remains minor. As the teacher recommendation co-determines track placement, it could offset the impact of the poorer test outcomes. However, I find that children in the treatment group receive lower recommendations, thereby aggravating the impact grandparental death has on track placement, especially for those children that also perform worse on the test. As a consequence, treated children have a 0.87 percentage point higher chance to be placed in the vocational track at the start of secondary school, compared to starting in the general, academic or combined track. To mitigate the poorer initial track placement children may change to higher tracks later-on. I indeed find that treated children have a 0.22 percentage point higher chance to switch to a track upward during the first few years of secondary school. Nonetheless, for most children the negative consequences persist until the end of secondary school. Four years after initial track placement, treated children have a 1.08 percentage point higher chance to attend the vocational track and a 0.92 percentage point lower chance to attend the academic track. These effects do not differ depending on child-grandparent characteristics such as gender, geographical distance, or maternal vs. paternal family side.

This paper adds to a large stream of literature that analyses how permanent child characteristics interact with educational policies. It is well-documented that there are achievement gaps in standardized test scores between genders, socioeconomic status and migration background (Guiso et al., 2008; Fryer and Levitt, 2010; Schütz et al., 2008; Schnepf, 2007; Schneeweis, 2011; Ammermueller, 2007). Likewise, studies that explore the impartiality of teachers' evaluations, find that subjective assessments of ability tend to be influenced by children's gender and family background (Ready and Wright, 2011; Burgess and Greaves, 2013; Carlana, 2019; Lüdemann and Schwerdt, 2013; Lavy and

Sand, 2018; Timmermans et al., 2015). Educational tracking is another policy whose impact on equality of opportunity is extensively investigated. Most papers observe that early tracking reinforces the influence of parental background (for a review of the literature see: Betts, 2011). Track switching later-on can to a limited extend help children to overcome initial disadvantages in the tracking process, although this option is more often employed by children from more favorable socioeconomic backgrounds (Dustmann et al., 2017; Mühlenweg and Puhani, 2010; Dutch Inspectorate of Education, 2017). In contrast to this extensive literature on the interaction between permanent characteristics and educational practices, how a temporary family setback interacts with the educational environment received little attention at present.

This paper also relates to the literature that focuses on the general educational consequences of more severe events of family distress, for example divorce, parental unemployment, illness or death. Most papers find that these setbacks have negative impacts on children's educational and labor market outcomes such as school grades (e.g. Rege et al., 2011; Amato and Anthony, 2014), educational attainment (e.g. Francesconi et al., 2010; Coelli, 2011; Johnson and Reynolds, 2013) and adult labor earnings (e.g. Fronstin et al., 2001; Oreopoulos et al., 2008; Gruber, 2004; Adda et al., 2011). However, the existing literature leaves it unclear whether (part of) the observed negative effects on educational outcomes after family distress are influenced by features of the education system. An exception is the paper by Steele et al. (2009) who provide suggestive evidence that educational transitions are particularly important instances where children face negative consequences after experiencing family distress.

The rest of the paper is structured as follows. In section 3.2 I provide background information on the Dutch education system, and describe the data in more detail. Section 3.3 sets out the empirical strategy and the underlying identifying assumption. The results are described and discussed in section 3.4. Section 3.5 presents the robustness analysis. Finally, section 3.6 concludes.

3.2 Background and Data

In this section I first describe the institutional features of the Dutch education system that make the Netherlands a well-suited setting to study the effects of unfortunate timing of distress. In particular the presence of a high-stakes standardized test that, together with a teacher recommendation, informs children's secondary school track placement. Second, I describe the Dutch administrative records on which I base the empirical analysis. The high-quality administrative data make it possible to link family members to each other, and merge a wide range of background variables at an individual level.

3.2.1 The Dutch Education System

In the Netherlands, children enter primary school at the age of four.[1] The first two years consist of kindergarten, after which six years of general primary education follows. Consequently, most children are twelve years old when they transition to secondary school. Within secondary education children sort into tracks based on ability. There are three main tracks: preparatory vocational secondary education (vocational), senior general secondary education (general) and university preparatory education (academic). The tracks differ in course content, duration and entry-qualifications they provide for post-secondary education.[2] In addition, the vocational track consists of several sub-tracks that vary the weight they place on theoretical versus practical content.

Parents and children are free to choose the secondary school they apply to. However, the decision whether a child is admitted to a school, as well as which track a child will attend, lies with the secondary school. Secondary schools base their decision on the educational report primary schools prepare for each pupil at the end of the sixth grade. This educational report consists of two key components: outcomes of standardized track placement tests, and a teacher recommendation. Often secondary schools set fixed requirements concerning a minimum test score or track level recommendation to be admitted to a specific track.[3] Some secondary schools offer the possibility to start in so-called "bridge classes", which combine two tracks together, and postpone the final track decision for one or two years. Moreover, under certain circumstances it is possible to switch to a different track during the first three years of secondary school. Changing tracks is often bound to strict conditions based on particularly good or poor performance of a child. In recent years roughly 50 percent of all children attended the vocational track, 24 percent the general track and 20 percent the academic track (Dutch Inspectorate of Education, 2018).[4]

The setting of the high-stakes standardized test makes it a particularly appropriate context to investigate the consequences of the unfortunate occurrence of grandparent death. For one, although legally it is not mandatory to conduct a specific track placement test, almost all primary schools do so.[5] The most commonly employed standardized placement test is designed by the Cito organization, with a participation rate of roughly 85 percent of all primary schools. A second advantage is that the answer sheets are

[1]Education is mandatory from the age of 5 to 16, which makes the first year optional, although it is common practice to attend the first year.

[2]See figure 3.A1 in the appendix for an overview of the complete education system, including post-secondary education.

[3]As secondary schools are held accountable for how many of their pupils pass the centralized exams at the end of secondary school, they have an incentive to place children in a track that aligns with a child's abilities.

[4]The other 6 percent of pupils followed practical or special needs education.

[5]In 2015 new regulations have been implemented surrounding the transition to secondary school, therefore the analysis focuses on the years prior to 2015. Among others it became mandatory for all schools to conduct a track-placement test, for teachers to give their recommendation before the test is conducted, and prohibits secondary schools to inquire about a child's test score.

mechanically graded by the Cito organization, and therefore not compromised by teachers' beliefs. Cito's placement test consists of questions on three parts: Dutch language (100 items), mathematics (60 items), and study skills (40 items). The number of correct answers are converted into scores that range from 501 to 550 points, with an average score of roughly 535. Cito aims to keep an equal level of difficulty throughout the years, and if necessary they calibrate scores to facilitate comparison. A last advantage of this setting is that the test is administered during three pre-determined days in February in the whole of the Netherlands. When children are sick or otherwise absent during these days, it is possible to take part in a makeup test, which is conducted a few weeks later. Both parents and the school receive the test outcomes which include the final score, as well as a recommendation which secondary school track, or combination of tracks, fits best according to the test score. The primary school teacher often uses this information to form a definitive track recommendation, which is generally given after the test results are known. Besides the track placement outcome, teachers also consider beliefs on ability, soft skills, motivation and home environment to a greater or lesser extent in determining their track recommendation (Timmermans et al., 2016).

3.2.2 Data

This paper uses administrative records provided by Statistics Netherlands.[6] The records include data on the universe of children who participated in the track placement test between 2006 and 2014. For each child the records contain the number of correct answers for the different parts of the test, the final score, and whether they took the regular or makeup test.[7] In addition, for part of the pupil population the records include a tentative teacher track recommendation which is filled in at the time of the test. Children obtain the definitive teacher recommendation after the test results are known, however this information is unfortunately not available. I exclude children who do not participate in secondary education in the year after the test (1.7 percent), that could not be linked to their parents (0.9 percent), or who had any missing background information (5.8 percent). The baseline sample consists of 1,101,571 children.

To identify the occurrence of grandparent death I link each child to their grandparents, and combine this with information from the death registers which contain the exact date and cause of death of all Dutch inhabitants. From the baseline sample roughly half of all children lost at least one grandparent until the end of primary school, and 5.9 percent lost a grandparent in the final grade. The causes of death are categorized according to the International Classification of Diseases (ICD-10) codes of the World Health Organization. The two most common causes of death of grandparents are cancer (33.8 percent) and heart diseases (28.7 percent). As the impact of a grandparent dying

[6]Under certain conditions, these microdata are accessible for statistical and scientific research. For further information see microdata@cbs.nl.

[7]If children take part in the test more than once, I only keep the most recent score. Children can make the test more than once in case they have to repeat the final grade of primary school.

Table 3.1: Percentage of children per track by grade

	Vocational track	General track	Academic track	Bridge class
Grade 7	24.5	3.2	13.3	59.0
Grade 8	30.0	15.2	21.7	33.1
Grade 9	44.5	25.3	26.1	4.1
Grade 10	45.5	29.7	24.8	0

Notes: Table 3.1 shows the percentage of children in each track for the first four years of secondary school. The numbers are based on a sample of 34,022 children who lost a grandparent three months before and after the standardized test.

may depend on the foreseeable nature of the loss, I distinguish between expected and unexpected deaths. In line with existing studies I classify unexpected causes of death as heart attacks, cardiac arrests, congestive heart failures, strokes, traffic and other accidents, violence and sudden deaths from unknown causes (Andersen and Nielsen, 2010).[8] From all grandparents who passed away in the final grade of primary school roughly 14 percent died from unexpected causes.

The secondary education registers comprise of children's post-transition school outcomes. For each year of secondary school I observe the track a child attends, including whether a child is in a bridge class which combines multiple tracks. Unfortunately I can not observe which exact tracks are combined for children attending bridge classes. The widespread use of bridge classes complicates classifying children as attending one specific track. Instead, I construct two indicators that capture whether a child is placed directly in respectively the vocational or academic track, instead of the other tracks or a bridge class.[9] From the tenth grade upwards bridge classes are no longer made use of, and I observe for all children which of the three main tracks they attend. In addition, for the cohorts who took the placement test between 2006 and 2011, I have data on which track children graduated from and their centralized exam scores of Dutch and English at the end of secondary school. Table 3.1 displays the relative size of the vocational, general, academic and bridge track for grades 7 to 10.

Finally, the administrative records provide information on a wide range of background characteristics. This consists among others of basic child demographics such as age, gender, migrant status, number of siblings and birth order.[10] From parents I observe their age, receipt of unemployment-, social- or disability benefits, and whether they have siblings.[11] Regarding household characteristics, the registers contain data on whether it is a single-parent household, yearly disposable income, and geographical location of the household. All variables are measured on the first of January the year before the child

[8] The corresponding ICD-10 codes are: I22, I23, I46, I50, I60-69, R95-97, V00-99, W00-99, X00-59 and X86-90.

[9] I do not construct an indicator variable of the general track as it is unclear what it means to start directly in this track compared to a bridge class.

[10] Siblings are defined as children with the same mother.

[11] Unfortunately, the educational registers are incomplete for older generations.

takes the track placement test, to prevent grandparental death affecting any background characteristics, such as household income and composition.

3.3 Empirical Strategy

This section sets out the strategy to answer whether the consequences of a grandparent's death are aggravated or softened by high-stakes standardized tests and other common educational practices. The first part describes the estimation approach, while the second part discusses the underlying identifying assumption.

3.3.1 Estimation Approach

There are two main threats to address when causally estimating the impact of unfortunate timing of grandparental death. First, families experiencing grandparental death when children are in primary school may be different from households that experience this later in life. If unobserved family characteristics are correlated with the occurrence of grandparental death during school age, a selection bias arises. The presence of selection is probable, as mortality coincides with among others families' socioeconomic background (Glied et al., 2012). Second, a problem arises when grandparental death impacts track choice not only via its interaction with the standardized test, but also via different pathways. For example, a family may receive a positive income shock after the death of a grandparent due to the inheritance of money, or parents might have more free time as they don't have to provide informal care to their elderly anymore. In this case, it becomes difficult to separate the different effects from each other.

To solve both concerns this paper exploits the random timing of a grandparent's death during the months before and after the standardized high-stakes test, a strategy similar to the one employed by Persson and Rossin-Slater (2018). In particular, I compare children experiencing the death of a grandparent in the three months prior to the track placement test, with children experiencing the same setback during the three months after the placement test.[12] As respectively both the treatment and control group experience grandparental death in the final grade of primary school, this comparison is less susceptible to selection bias. Moreover, it allows me to disentangle the intermediating effect of the standardized test from any other effects of losing a grandparent, as only the treatment group experiences grandparental death before the test is conducted.

[12] I do not perform a full regression discontinuity (RD) analysis, as treatment is not an uniform concept in this setting. In a regular RD design all observations below the cutoff receives the *same* treatment. However, in this setting the treatment a child receives depends on the date a grandparent dies and hence differs between children in the treatment group. For example, it is unclear whether it is worse to lose a grandparent a week before the test when the grieving processes just started, or three months before the test when it might distort test preparations. The aspect all children in the treatment group have in common, is that they all lose a grandparent before the test, which is what I exploit with my empirical strategy. In addition, to capture any time patterns in the robustness analysis I implement month dummies.

Accordingly, I estimate the following regression model:

$$Y_i = \beta_0 + \beta_1 DeathBefore_i + \gamma' X_i + \epsilon_i \tag{3.1}$$

where Y_i is the set of educational outcomes of child i. $DeathBefore_i$ is the treatment dummy, which is one when children experience the death of a grandparent during the three months before the track placement test, and zero if children experience it during the three months after. Consequently, β_1 is the coefficient of interest and captures the effect of experiencing grandparental death before - instead of after - the high-stakes test.[13] X_i contains a set of background characteristics, including: gender, age, migration background, mother's age, number of siblings and disposable household income. All regressions are clustered by mother's ID.

3.3.2 Identifying Assumption

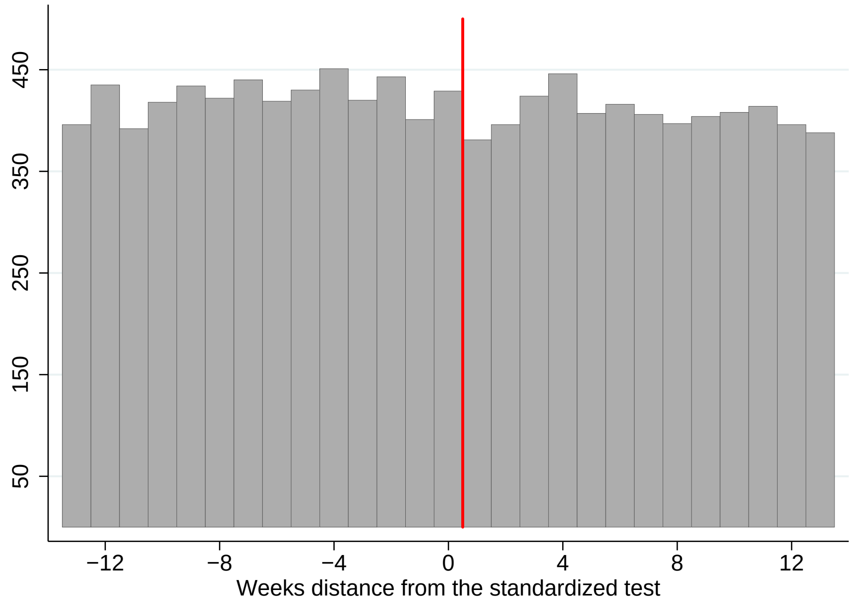
The identifying assumption that needs to hold for the above strategy to estimate causal effects is that conditional on the death of a grandparent, the exact timing of death within the six-month period surrounding the standardized test is random. This assumption holds two testable implications. First of all, it should not be possible to control or manipulate the timing of grandparental death. It is reasonable to assume that this holds, as death is an event over which people have little to no control. Figure 3.1 underlines this, as it shows the weekly frequency of grandparental bereavement in the three month-period before and after the standardized test. There is no significant change visible in the prevalence of grandparental death around the test week. Moreover, the graph shows no signs of seasonal patterns, which is advantageous as seasonal patterns can cause selection problems in case they correlate with background characteristics.

The second implication from the identifying assumption is that it should not be possible to influence the timing of the standardized test. As the test dates are fixed by the Cito organization, parents cannot change the date their child takes the test. However, as the test is not mandatory, it is possible for children to not take the test at all. Therefore, I analyze whether children who lose a grandparent prior to the test more often select out of taking the test. I merge the baseline sample with all registered sixth graders, to include those children that do not participate in the placement test.[14] With this extended sample I check whether the probability of being in the baseline sample - put differently, to take the test - is correlated to a child's treatment status. The results are presented in table 3.A1 in the appendix and show that losing a grandparent before the test date, does not predict participating in the track placement test.

[13]The estimated coefficient could represent a lower-bound effect, due to children in the control group being somewhat affected at the time of the test. For instance, when grandparents are sick before they pass away this could have already caused distress to children. In the robustness analysis I test this possibility.

[14]Due to data limitations this is only possible from the school year 2008/2009 onward.

Figure 3.1: Weekly frequency of grandparental death



Notes: Figure 3.1 shows the frequency of grandparental death by week. Week "zero" refers to the week the track placement test is conducted.

Table 3.2 presents descriptive statistics for the baseline population, as well as the treatment and control group. The descriptives show that the empirical strategy is successful in constructing a treatment and control group that have similar pre-treatment observables. The only exception is parental age, with children in the treatment group having statistically significant older parents. However, as the age difference comes down to a bit over one month, the effect size is negligible. The balance regressions confirm the similarity between treatment and control, with the only statistically significant coefficient being mother's age (see appendix table 3.A2). Nonetheless, it is worthy to note that the children in the treatment and control group come from more advantaged households than the average child in the population. On average, the children experiencing the death of a grandparent in sixth grade have older parents, less often have a migration background, have fewer siblings, are less likely to grow up in a single-parent household, and have parents who are less frequently on benefits and who earn a higher income.

3.4 Results

This section starts with analyzing the effects of experiencing pre-test grandparental death on educational outcomes in the immediate-, short- and long-term. This is followed by examining heterogeneous responses based on family background characteristics.[15]

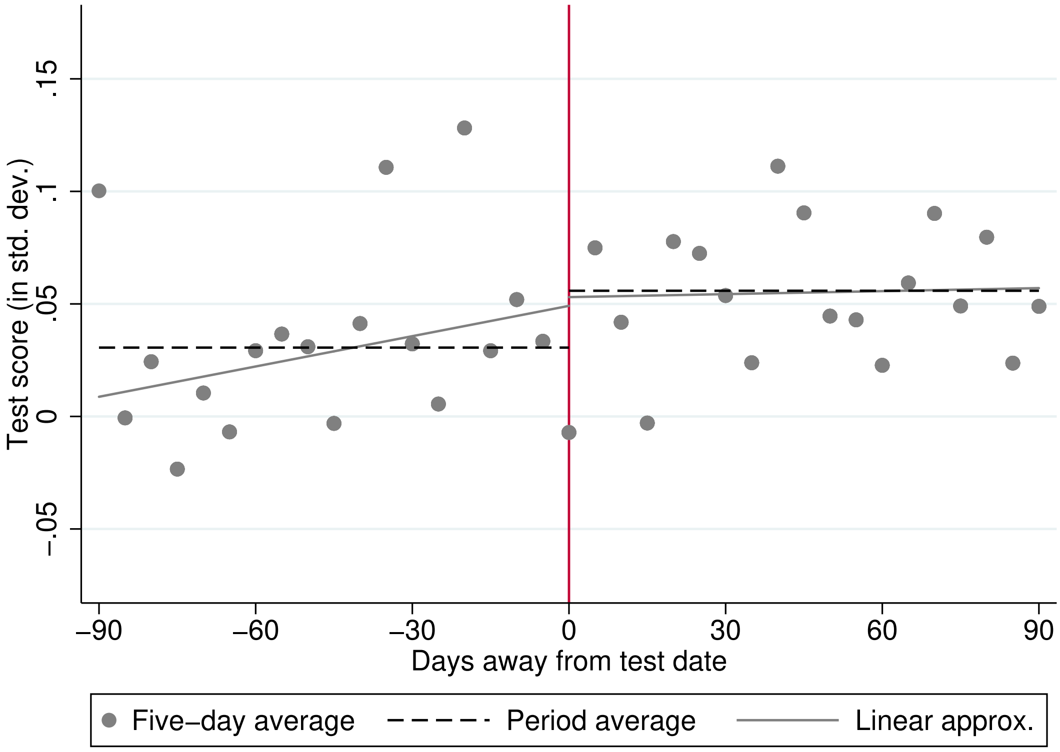
[15]The results are based on calculations by the author using non-public microdata from Statistics Netherlands.

Table 3.2: Descriptive statistics

		Children who lost a grandparent		
	(1) All children	(2) Treatment	(3) Control	(4) Difference
Child characteristics				
Age	11.46	11.46	11.45	-0.01
Boy (%)	49.74	49.65	49.69	0.04
Migrant background (%)	19.87	9.87	9.56	-0.31
Oldest child (%)	47.03	53.05	52.43	-0.62
Household characteristics				
No. of children	2.56	2.53	2.53	-0.00
Single-parent (%)	13.04	11.22	11.34	0.12
Disposable yearly income (€)	44,747	46,380	46,250	-130
Parental characteristics				
Mother's age	42.13	43.35	43.25	-0.10**
Mother has siblings (%)	82.94	90.00	90.26	0.26
Unemployment benefits - mother (%)	1.28	1.25	1.30	0.05
Social assistence - mother (%)	4.76	2.43	2.27	-0.17
Disability insurance - mother (%)	2.92	2.55	2.62	0.07
Father's age	44.83	45.93	45.82	-0.11**
Father has siblings (%)	80.19	88.93	89.42	0.49
Unemployment benefits - father (%)	1.46	1.23	1.35	0.12
Social assistence - father (%)	2.14	0.96	0.96	-0.00
Disability insurance - father (%)	2.45	2.08	1.93	-0.15
Grandparental characteristics				
Death of grandfather (%)		58.21	58.78	0.57
Death on mother's side (%)		45.96	46.51	0.55
Unexpected cause of death (%)		14.07	13.99	-0.08
N	1,101,571	17,214	16,808	34,022

Notes: Table 3.2 shows the descriptive statistics of the main background characteristics. Column 1 includes the baseline sample, which consists of the population of children who made the track placement test between 2006 and 2014. Column 2 and 3 include those children from the baseline sample who respectively lost a grandparent in the three months before and after the track placement test is conducted. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Figure 3.2: Track placement test score by time of grandparental death



Notes: Figure 3.2 shows the track placement test score in standard deviation at the end of sixth grade by time of grandparental death. The red vertical line indicates the time of the track placement test. The solid gray line shows the periods' time trends, while the dotted black line presents the periods' averages.

3.4.1 Immediate Effects

The direct impact of a grandparent dying shortly before the standardized placement test is on test performance itself. The potential negative effects on test performance can be mitigated by participating in the makeup test, as it is conducted a few weeks later than the regular test.

Standardized Placement Test Figure 3.2 displays the raw test score averages by time of grandparental death, and gives a first indication that children who lose a grandparent before the test indeed perform worse than children who lose a grandparent afterwards. Moreover, the solid lines suggest that the effect of grandparental death is stronger if it happens two to three months, instead of two to three weeks, before the test date.

Table 3.3 presents the corresponding regression results of the effects of experiencing grandparental death on test performance in sixth grade.[16] Column 1 shows that losing

[16]This is including the children who take part in the makeup test.

Table 3.3: Effect of grandparental death on track placement test outcomes

	(1) Total score	(2) Language score	(3) Math score	(4) Study-skills score
Grandparental death	-0.0293***	-0.0340***	-0.0207**	-0.0193*
	(0.0101)	(0.0100)	(0.0102)	(0.0101)
N	34,022	34,022	34,022	34,022
Controls	Yes	Yes	Yes	Yes

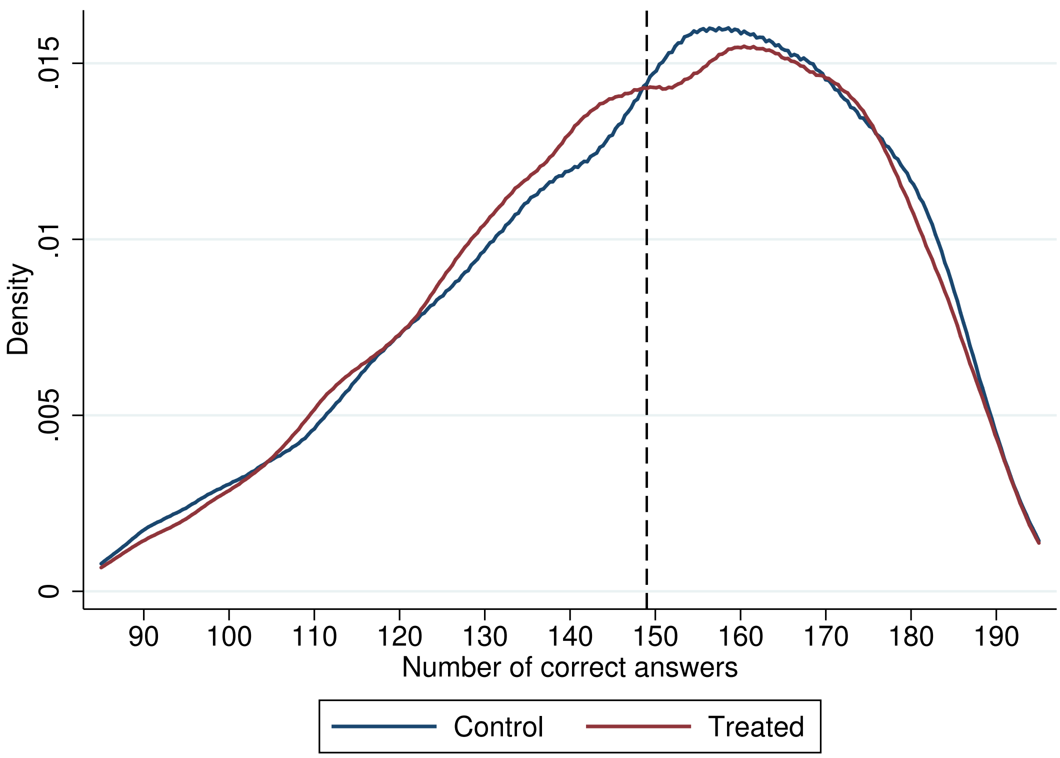
Notes: Table 3.3 presents the effect of pre-test grandparental death on the track placement test. The estimated coefficients are expressed in standard deviations. Standard errors are clustered at mother ID level in parentheses. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income. Significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

a grandparent in the three months leading up to the test reduces the total test score by 0.0293 of a standard deviation. Columns 2 to 4 display that the negative treatment effect holds for all three parts of the test separately, ranging from -0.0340 of a standard deviation for language to -0.0193 of a standard deviation for study skills. The reduction in test scores of roughly 3 percent of a standard deviation after experiencing grandparental death, is slightly smaller than the impact of other types of disturbances that may influence test outcomes. For instance, a drop in the Air Quality Index by one standard deviation has been associated with a decrease of exam performance of 3.9 percent of a standard deviation (Ebenstein et al., 2016). Whereas a one standard deviation increase of temperature has been found to induce a decline in test performance of 5.5 percent of a standard deviation (Park, 2020).

In addition, figure 3.3 compares the distribution of the number of correct answers between the control and treatment group. The figure shows that the treatment group comparatively has fewer children scoring slightly above the mean, and more children scoring just below it. At the tails, however, I do not observe any significant differences. This implies that the bereavement effect seems to mainly materialize around the mean, while particularly low- or high-performing pupils are less affected.

Makeup Test Table 3.4 shows what happens with the take up rate of the makeup test after experiencing the death of a grandparent. Column 1 demonstrates that children who lose a grandparent before the test are 0.37 percentage points more likely to take part in the makeup test. Since of the overall population of Dutch pupils only 1.1 percent of all children take part in the makeup test, this is a substantial increase. As it is so rare to take the makeup test, I look closer at whether the exact timing of grandparent bereavement matters for who makes use of this possibility. Column 2 shows the effect by month of death, and the results indicate that the effect is solely driven by children who lose a grandparent during the month directly before the track placement test takes place.

Figure 3.3: Density of number of correct answers by treatment status



Notes: Figure 3.3 shows the density of the number of correct answers on the track placement test at the end of sixth grade by treatment status. The dotted black line presents the sample's average.

Table 3.4: Effect of grandparental death on makeup test participation

	(1) Makeup test	(2) Makeup test
Grandparental death	0.0037***	
	(0.0012)	
Grandparental death: 0-1 months		0.0099**
		(0.0020)
Grandparental death: 1-2 months		0.0006
		(0.0016)
Grandparental death: 2-3 months		0.0001
		(0.0016)
N	34,022	34,022
Controls	Yes	Yes

Notes: Table 3.4 presents the effect of pre-test grandparental death on makeup test participation. The estimated coefficients are expressed as average marginal effects. Standard errors are clustered at mother ID level in parentheses. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

For this group of children I observe an increased probability of taking the makeup test by almost 1 percentage point, which is equal to a 91 percent increase compared to the control group's average. This time pattern of makeup test participation partially explains the weaker negative effect on test performance the month before the test portrayed in figure 3.2. However, it cannot fully explain the weaker effect as still only 2.1 percent of all treated children take the makeup test. Another explanation could be worsened school behavior during the crucial months leading up to the test, where the closer a grandparent's death occurs to the test date, the less a child misses out on and is affected.

3.4.2 Short-Term Effects

In the short run the lower standardized test scores may have consequences for children's initial track placement at the beginning of secondary school. Besides the standardized test, the teacher's recommendation determines a child's track placement. Hence, primary school teachers are in theory able to compensate for the negative effects of poorer test outcomes through their track recommendation.

Teacher Recommendation Table 3.5 regresses pre-test grandparental death on a tentative teacher recommendation which is available for a subsample of children. This tentative advice is filled in before the test outcomes are known, and can differ from the definitive recommendation children receive.[17] The results show that experiencing

[17]The definitive teacher track recommendation is unfortunately not available in the administrative data.

Table 3.5: Effect of grandparental death on teacher advice

	(1) Advice: Vocational	(2) Advice: Academic
Grandparental death	0.0159***	-0.0077*
	(0.0060)	(0.0045)
N	24,381	24,381
Controls	Yes	Yes

Notes: Table 3.5 presents the effect of pre-test grandparental death on the teacher track recommendation. The estimated coefficients are expressed as average marginal effects. Standard errors are clustered at mother ID level in parentheses. As the information on the teacher's track recommendation is not available for all children, this is a subsample of the 34,022 children who lost a grandparent during the three months before and after the track placement test. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income. Significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

grandparental bereavement increases the chance of receiving a vocational track recommendation by 1.59 percentage points, while it decreases the likelihood of an academic track recommendation by 0.77 percentage points. Hence, instead of compensating the lower test performance, teachers seem to recommend lower tracks for children in treatment group.

To test whether the children receiving a lower advice are also the ones performing worse on the test I look at the disparity between test outcomes and tentative teacher advice. If those children that perform poorly on the test are not the same children that receive a lower recommendation, I would expect to observe more often a misalignment between the test score and teacher advice. However, I do not find any difference in the frequency of disparities between the treatment and control group (see table 3.A3 in the appendix). This suggests that teachers award lower recommendations to children who afterwards also perform worse on the test.

One explanation could be that due to losing a grandparent children display different classroom behavior. This may not only negatively influence their test score, but also their track recommendation when teachers mis-attribute the poorer classroom performance to lower child abilities instead experiencing distress. It is unlikely that any potential mis-attribution is caused by teachers being unaware of the child losing a grandparent. In an own-conducted survey among a representative sample of 1012 Dutch parents with children aged between 6 and 24 years old, I asked whether parents informed the school of their child after the loss of a grandparent. As 87.3 percent of parents answered affirmatively, it is unlikely that teachers are not informed when a grandparent dies.

Initial Track Placement Table 3.6 shows the consequences of the reduced test performance and teacher recommendation on initial track placement in seventh grade. The

Table 3.6: Effect of grandparental death on initial track placement

	(1) Grade 7: Vocational	(2) Grade 7: Academic
Grandparental death	0.0087* (0.0046)	-0.0052 (0.0036)
N	34,022	34,022
Controls	Yes	Yes

Notes: Table 3.6 presents the effect of pre-test grandparental death on track placement in grade 7. The estimated coefficients are expressed as average marginal effects. Standard errors are clustered at mother ID level in parentheses. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income. Significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

estimates in column 1 show that experiencing grandparental bereavement shortly before the test leads to a 0.87 percentage point higher probability of being directly placed in the vocational track, compared to starting in the other tracks or a bridge class. Simultaneously, column 2 displays a decrease of 0.52 percentage points in the likelihood to start in the academic track, although it is not statistically significant. Even though the large share of children attending a bridge class in seventh grade partly blurs the picture, the findings are indicative that losing a grandparent at the end of primary school has negative effects at the beginning of secondary school.

As I do not observe the definitive teacher recommendation, I investigate whether the estimated increase in likelihood of going to the vocational track can solely be explained by the drop in test scores. When I regress test outcomes on vocational track placement for the entire population I find that a one standard deviation increase in test scores leads to a 21 percentage points lower probability to be directly placed in the vocational track (see table 3.A4 in the appendix). Assuming that the effect is constant across the distribution, a 0.0293 standard deviation decrease in test scores corresponds to an increase of 0.62 percentage point of attending the vocational track. As I find an increase of 0.87 percentage point, this suggests that indeed the definitive teacher recommendation is lower for children in the treatment group, making it more likely that they attend the vocational track.

3.4.3 Long-Term Effects

In the long run, I investigate the effects on children's tenth grade track attendance and graduation performance. After initial track placement, children may under certain conditions change tracks during the first years of secondary school. Therefore, track switching is a way through which potential lasting negative consequences of pre-test grandparental death can be overcome.

Table 3.7: Effect of grandparental death on switching tracks

	Main tracks only		Main and sub-tracks	
	(1) Switch up	(2) Switch down	(3) Switch up	(4) Switch down
Grandparental death	0.0022*	0.0014	0.0047**	0.0025
	(0.0012)	(0.0021)	(0.0020)	(0.0025)
N	34,022	34,022	34,022	34,022
Controls	Yes	Yes	Yes	Yes

Notes: Table 3.7 presents the effect of pre-test grandparental death on switching tracks during the first four years of secondary school. The estimated coefficients are expressed as average marginal effects. Standard errors are clustered at mother ID level in parentheses. Columns 1 and 2 look at switches solely between the three main tracks, while columns 3 and 4 also include switches between sub-tracks within the vocational track. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income. Significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Track Switching Table 3.7 shows the effect of pre-test grandparental death on the probability to switch tracks during the first three years of secondary school. As treated children have an increased likelihood to initially be placed in a lower track, intuitively later on they may more often change to a track upward and less often to a downward track. Columns 1 and 2 show that children in the treatment group have a 0.22 percentage point larger probability to switch to a higher track than children in the control group, while there is no statistically significant difference in switching to a lower track. The effect size of 0.22 percentage points is not negligible since the population average of children changing to a higher track is only 3.9 percent. In addition, columns 3 and 4 allow for switches between sub-tracks, making the positive effect on upward track mobility even stronger with an increase of 0.47 percentage points.[18] Hence, some children in the treatment group seem to be able to counter the initial disadvantage by switching to a higher track at a later point in time.

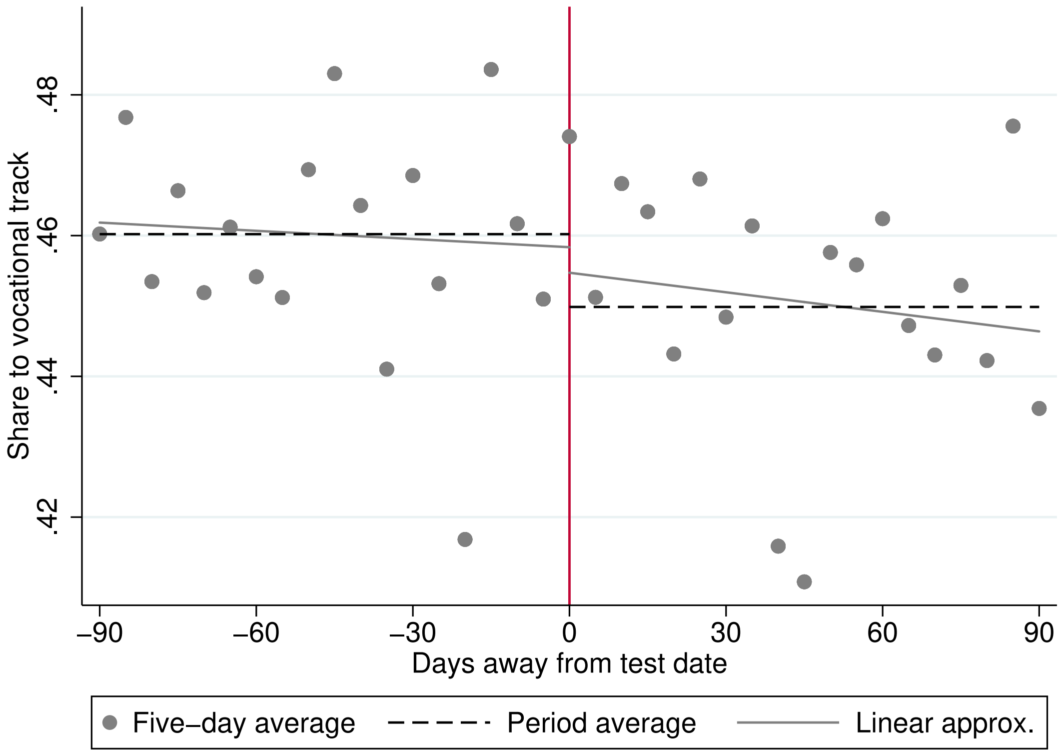
Track Attendance Figure 3.4 displays the raw shares of children attending the vocational track in tenth grade by time of grandparental death. The figure shows that children experiencing grandparental loss at an unfortunate time at the end of primary school still have a higher likelihood to attend the vocational track in tenth grade. The increase in upward track mobility seems to be insufficient to undo the negative effects on initial track placement.

Table 3.8 shows the regression outcomes of the impact of pre-test grandparental death on tenth grade track attendance. The track division in tenth grade has the advantage that it is not blurred by the existence of bridge classes anymore, this makes it possible to look at all three tracks separately.[19] Column 1 shows that children who ex-

[18]Only the vocational track contains multiple sub-tracks.

[19]I focus on tenth grade instead of any higher grades, as grade 10 is the final grade of the vocational

Figure 3.4: Probability of attending the vocational track by time of grandparental death



Notes: Figure 3.4 shows the likelihood of attending the vocational track in tenth grade by time of grandparental death. The red vertical line indicates the time of the track placement test. The solid gray line shows the periods' time trends, while the dotted black line presents the periods' averages.

Table 3.8: Effect of grandparental death on track attendance

	(1) Grade 10: Vocational	(2) Grade 10: General	(3) Grade 10: Academic
Grandparental death	0.0108**	-0.0007	-0.0092**
	(0.0052)	(0.0050)	(0.0045)
N	34,022	34,022	34,022
Controls	Yes	Yes	Yes

Notes: Table 3.8 presents the effect of pre-test grandparental death on track attendance in grade 10. The estimated coefficients are expressed as average marginal effects. Standard errors are clustered at mother ID level in parentheses. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

perience grandparental death before the standardized test have a 1.08 percentage points higher probability to attend the vocational track in grade 10, compared to the other two tracks. Simultaneously, treated children have a 0.92 percentage points lower probability to attend the academic track. As the effect sizes for the vocational and academic track are roughly the same, I do not see an effect on the middle track.[20] These findings show that despite that on average treated children more often switch to a higher track, there are still children who experience the negative consequences of the unfortunate timing of grandparental death four years after it happened.[21]

By extension, the tenth-grade findings translate into an increased likelihood to graduate from the vocational track for the early cohorts (see appendix table 3.A5). When affected children on the margin now graduate from a lower track, they might perform better within their track as there is positive selection. Therefore, I investigate what happens to the outcomes of the centralized exams in Dutch and English at the end of secondary school. However, I do not observe an improvement in children's exam performance (see appendix table 3.A5).

3.4.4 Heterogeneities

Not all children may respond the same to the death of a grandparent. In a first step I analyze whether the treatment effect on test performance differs by background characteristics or the intensity of family distress experienced by either children or parents. The regression estimates are displayed in appendix table 3.A6. An important determinant

track.

[20]Children who otherwise would attend the academic track move down one track, and a roughly equal amount of children move away from the general track to the vocational track, leaving the overall number of students in the general track the same.

[21]Intuitively, based on the track switching results one would expect the treatment effect size to decrease between grade 7 and 10. However, due to the existence of bridge classes this is not straightforward, see appendix section 3.B for a detailed explanation.

of the level of distress is the bond children and parents have with the (grand)parent. In this respect, I consider whether a bereaved grandparent lived in the same municipality or is from the mother's side of the family, as daughters are often closer to their parents than sons (e.g. Bianchi, 2006).[22] However, these factors do not seem to influence the effect of grandparental death on test scores. A potential explanation for the lack of differences by proxies of distress, is that they may coincide with experiencing a heavier care burden towards the end of a grandparent's life. As this care burden is lifted after the grandparent dies, this may weaken or cancel out the negative consequences due to emotional distress after the death of a family member (Siflinger, 2017). In addition, the degree of distress could depend on the practical hassle that often follows after a death such as organizing the funeral or dividing the inheritance. These practical concerns are generally smaller in case there are more family members around to help. Therefore, I include interaction terms with indicators of whether there is a surviving partner or siblings of the parent present. Although the estimated coefficients hint indeed to weaker effects when there are more relatives around, they are not statistically significant. Furthermore, I interact the treatment dummy with background characteristics related to gender and socioeconomic status, as they may also influence how a child responds to the death of a grandparent. Again I do not find heterogeneous responses based on the child's gender, the grandparent's gender, having the same gender, single parenthood, low household income, or having a migration background.

Moreover, there may be heterogeneous treatment effects regarding the take up of mitigation possibilities. For example, in general we observe that children from advantaged families are more likely to switch tracks during secondary school than children from disadvantaged families. Therefore, as a second step I investigate whether a child's socioeconomic background influences if a child makes use of the makeup test, teacher recommendation or track switching after the loss of a grandparent. The results can be found in table 3.A7 in the appendix. The point estimates suggest that after a grandparent dies children from more disadvantaged backgrounds - in terms of migration status, household income and single-parenthood - more frequently take the makeup test, while they are more often advised the vocational track and switch less often to higher tracks in secondary school. However, unfortunately the results are too noisy to make conclusive statements.

3.5 Robustness

There are several concerns with the empirical strategy that could influence the interpretation of the findings. First, the treatment effect may be underestimated if the control group experiences some degree of family distress at the time of the test, for instance when

[22] I assume both grandparents live together, and therefore only consider the grandfather's place of residence.

grandparents are already sick in the months prior to their death. As a robustness check I construct a control group which consists only of children who lost a grandparent from an unexpected cause of death. I assume that in the case of an unexpected loss children do not experience distress prior to the death of a grandparent. The first three columns of table 3.9 show significantly larger effect sizes on the test score and tenth grade track attendance after losing a grandparent unexpectedly. Test performance reduces by 0.0938 of a standard deviation, and the probability to attend the vocational (academic) track increases (decreases) by roughly 3 percentage points. An explanation for the stronger effects could be that now the control group no longer experiences distress at the time of the test, although alternatively the level of distress could be higher when a death is unexpected. Therefore, I conduct a second robustness check where the control group consists of children who lost a grandparent exactly one year after the treatment group. Since the control group now experiences the death of a grandparent a full year later, I assume that these children are not affected at the time of the test. The results are presented in columns 4 to 6 of table 3.9 and show actually smaller coefficients, making it unlikely that the effects of the main specification are greatly underestimated. However, the decrease of the effect sizes might partly be caused by the control group becoming slightly less advantaged than the treatment group.

A second concern is any unobserved selection bias that I fail to control for. For instance, children who lost a grandparent mere days before the test date but still participated, might be academically stronger, thereby potentially causing a selection problem. A similar reasoning holds true for children who lost a grandparent only days after the test took place. Table 3.10 presents results where I drop all children who lost a grandparent during the week before or after the test from the analysis. Columns 1 to 3 show that excluding these children does not significantly alter the point estimates. In addition, as I am unable to control for parental education I might be unaware of important unobserved heterogeneity related to socioeconomic status. Therefore, in columns 4 to 6 of table 3.10 I include additional controls for parental unemployment and social security usage. The findings are robust to including these indicators related to children's socioeconomic background. As a last check that accidental unobserved differences between the treatment and control group are not causing the results, I conduct a placebo test. I compare children who lose a grandparent four to six months after the test, to those losing a grandparent seven to nine months after the test. If my findings are solely caused by the difference in the timing of grandparental death, and not by random unobservable differences, I should not find an effect for this placebo test. The results are shown in columns 7 to 9, and indeed do not display any significant effects, which underlines the validity of the identification strategy.

A final concern relates to whether the time of grandparental death matters. In columns 1 to 3 in table 3.11 I extend the included time span from three to six months. The estimates show that doubling the time span reduces the magnitude of the negative

Table 3.9: Robustness analysis: treated control group

	(1) Total score	(2) Grade 10: Voc.	(3) Grade 10: Aca.	(4) Total score	(5) Grade 10: Voc.	(6) Grade 10: Aca.
Grandparental death	-0.0938***	0.0288**	-0.0331***	-0.0195**	0.0065	-0.0063
	(0.0269)	(0.0137)	(0.0122)	(0.0098)	(0.0050)	(0.0044)
N	4,773	4,773	4,773	35,150	35,150	35,150
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Specification	Unexpected	Unexpected	Unexpected	One year lag	One year lag	One year lag

Notes: Table 3.9 presents several robustness checks with respect to the effect of pre-test grandparental death on the track placement test and track attendance in grade 10. The estimated coefficients are expressed in standard deviation in columns 1 and 4, and as average marginal effects in columns 2,3,5 and 6. Standard errors are clustered at mother ID level in parentheses. Columns 1 to 3 only include children who lost a grandparent due to an unexpected cause of death. In columns 4 to 6 the control group changed to having lost a grandparent one year after the test is taken. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income. Significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.10: Robustness analysis: selection

	(1) Total score	(2) Grade 10: Voc.	(3) Grade 10: Aca.	(4) Total score	(5) Grade 10: Voc.	(6) Grade 10: Aca.	(7) Total score	(8) Grade 10: Voc.	(9) Grade 10: Aca.
Grandparental death	-0.0289***	0.0124**	-0.0085*	-0.0293***	0.0104**	-0.0096**	0.0095	-0.0012	0.0018
	(0.0105)	(0.0054)	(0.0047)	(0.0102)	(0.0052)	(0.0046)	(0.0104)	(0.0053)	(0.0046)
N	31,390	31,390	31,390	33,081	33,081	33,081	32,367	32,367	32,367
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Specification	Donut	Donut	Donut	Add. cov.	Add. cov.	Add. cov.	Placebo	Placebo	Placebo

Notes: Table 3.10 presents several robustness checks with respect to the effect of pre-test grandparental death on the track placement test and track attendance in grade 10. The estimated coefficients are expressed in standard deviation in columns 1, 4 and 7, and as average marginal effects in columns 2,3,5,6,8 and 9. Standard errors are clustered at mother ID level in parentheses. Columns 1 to 3 exclude children who lost a grandparent one week before or after the track placement test. In columns 4 to 6 I added controls for unemployment assistance, disability assistance and social security assistance. Columns 7 to 9 present the results of a placebo test where I compare children who lose a grandparent four to six months after the test, to those losing a grandparent seven to nine months after the test. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.11: Robustness analysis: time patterns

	(1) Total score	(2) Grade 10: Voc.	(3) Grade 10: Aca.	(4) Total score	(5) Grade 10: Voc.	(6) Grade 10: Aca.
Grandparental death	-0.0153**	0.0038	-0.0044			
	(0.0073)	(0.0037)	(0.0033)			
Grandparental death: 0-1 months				-0.0158	0.0051	-0.0086
				(0.0139)	(0.0071)	(0.0062)
Grandparental death: 1-2 months				-0.0196	0.0125*	-0.0059
				(0.0140)	(0.0072)	(0.0063)
Grandparental death: 2-3 months				-0.0559***	0.0154**	-0.0136**
				(0.0148)	(0.0075)	(0.0065)
N	64,840	64,840	64,840	34,022	34,022	34,022
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Specification	6 months	6 months	6 months	Dummies	Dummies	Dummies

Notes: Table 3.11 presents several robustness checks with respect to the effect of pre-test grandparental death on the track placement test and track attendance in grade 10. The estimated coefficients are expressed in standard deviation in columns 1 and 4, and as average marginal effects in columns 2,3,5 and 6. Standard errors are clustered at mother ID level in parentheses. Columns 1 to 3 include children who lost a grandparent six months before or after the track placement test. In columns 4 to 6 I included month dummies. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

effect on test score and grade 10 track placement by roughly a half. The findings indicate that the effects fade out over time, which is intuitive as grandparental death is a relatively mild event. In addition, I analyze whether there are interesting time patterns visible within the three-month period. Hence, in columns 4 to 6 I include month dummies. Although all months show negative point estimates, the coefficient corresponding to a death two to three months prior to the test is largest and statistically significant. The children who lose a grandparent two to three months before the test face on average a reduction in test scores of 0.0559 of a standard deviation. This pattern is in line with figure 3.2, and can be partially explained by the time trends with respect to makeup test participation. In addition, worsened school behavior during the crucial months leading up to the test could play a role. Whereas, if a child loses a grandparent merely days before the test, all school work preparing for the test has already been done, potentially diminishing the negative consequences of grandparental death.

3.6 Conclusion

This paper shows that in a setting with high-stakes standardized testing, even mild events of family distress such as losing a grandparent, can have long-term repercussions on educational outcomes, thereby hampering equality of opportunity. I find that children who experience the death of a grandparental shortly before the standardized test obtain roughly 3 percent of a standard deviation lower test scores than children who lose a grandparent shortly after the test. The poorer test performance occurs despite the higher likelihood for treated children to take advantage of the makeup test, most likely because the overall take up rate remains minor. The subjective teacher's track recommendation fails to compensate children's poorer test performance, and even aggravates the negative impact as these children also receive lower track recommendations. Due to the poorer test scores and track recommendation, children in the treatment group have an increased chance to be placed in the vocational track at the start of secondary school compared to the general, academic or combined track. The possibility to change tracks during the first years of secondary school seems to allow some children to overcome the initial negative consequences of grandparental loss, as treated children are more likely to switch a track upward than their control-group counterparts. Nonetheless, it cannot prevent that there are children who four years after losing a grandparent still experience the negative consequences, as in tenth grade treated children have roughly a one percentage point higher chance to attend the vocational track instead of the general or academic track.

Although the effect sizes I observe are relatively small, their consequences can be large: in 2012 the difference in the yearly average personal income between children who stay on the vocational track versus the general or academic track amounts to €19,500 (Statistics Netherlands, 2014). Hence, there may be severe negative consequence for adult labor market outcomes when a child graduates from a lower track due to losing a grandparent shortly before the standardized test at the end of primary school. Further research is necessary to explore what causes one child to perform poorly after a grandparent dies, while another child's performance stays unaffected.

The results highlight an important drawback of employing high-stakes standardized tests: the weight that these tests put on performance at one moment at time, allowing even mild setbacks to have a lasting negative impact. This finding implies that in the case of high-stakes standardized testing, temporary shocks may create an uneven playing field between children who take the test. In this sense, the findings of this paper relate to a wider literature on the long-term consequences of idiosyncratic disturbances during high stakes tests. Examples are the worsening of air quality (Ebenstein et al., 2016), temperature (Park, 2020), or time of the day (Sievertsen et al., 2016), which are found to negatively affect high-stakes exam results and by extension educational attainment and earnings. Hence, in the face of idiosyncratic events, standardized tests may provide a disproportional noisy measure of true ability, which can lead to inefficient and unequal educational decisions.

Finally, the results of this paper imply that the consequences of family setbacks are influenced by the prevailing educational policies. Therefore, when evaluating the fairness of educational practices, we should not only consider potential interactions with permanent background characteristics, but also take into account how they respond to temporary events of family distress. For one, the findings underline that not only the objective standardized test is influenced by a short-lived setback, but also the subjective teacher recommendation has problems separating children's inherent capabilities from the temporary consequences of the death of a grandparent. Moreover, early setbacks are not easily overcome, it seems difficult for children to redeem themselves, even with several educational policies in place that potentially can counter negative effects. As providing children the opportunity to switch tracks later-on proves to be partially effective, policies that allow for reevaluating children's capabilities might be more promising in setting initial setbacks right.

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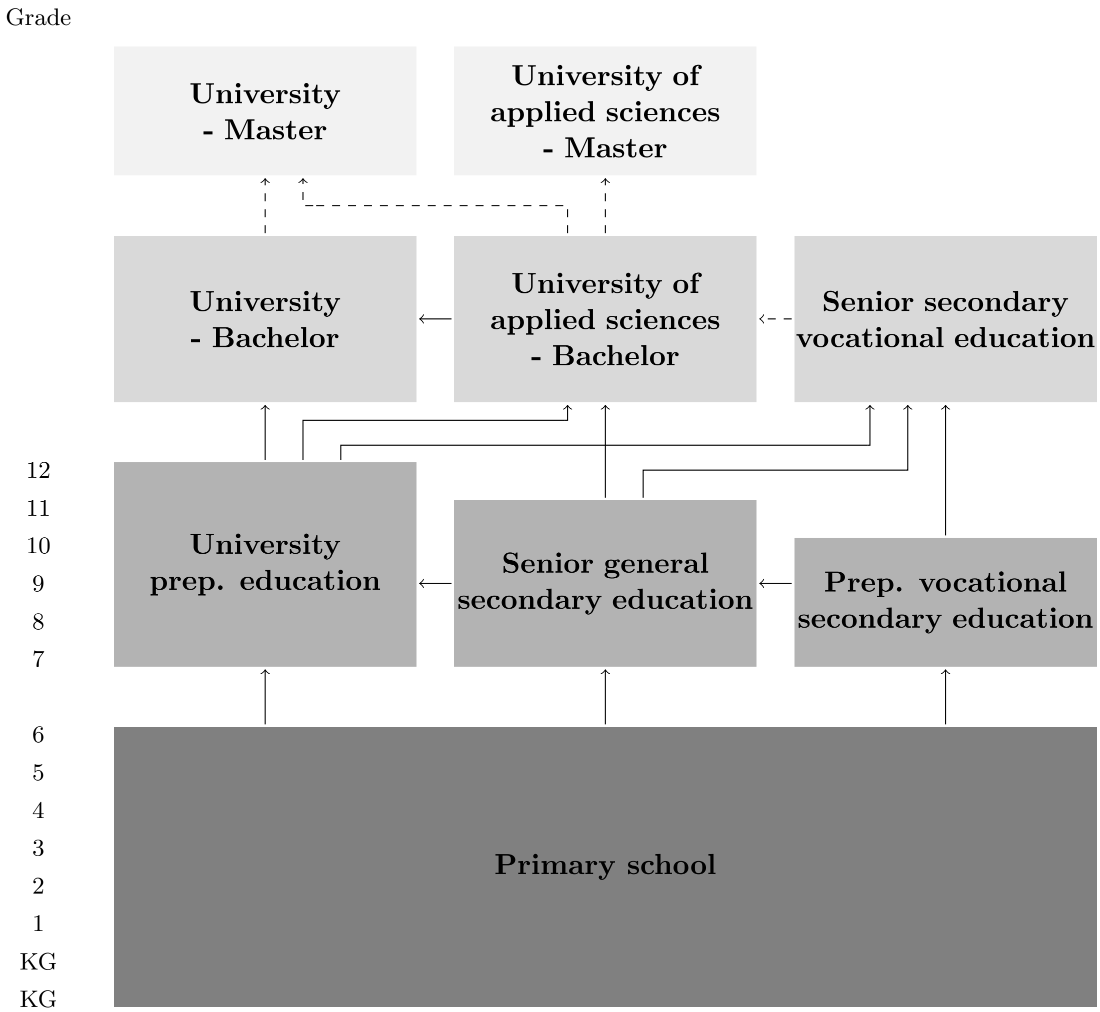
Appendix

3.A Additional Figures and Tables

Figure 3.A1: The Dutch education system



Notes: Figure 3.A1 presents the education system in the Netherlands. The solid lines indicate that finishing a certain degree gives automatic permission to start the next degree. The dotted lines indicate transitions where additional conditions need to be fulfilled.

Table 3.A1: Sample selection

	(1) Test participation	(2) Test participation
Grandparental death	-0.0055	-0.0055
	(0.0050)	(0.0038)
N	33,770	26,099
Controls	Yes	Yes
Specification	Unconditional	Conditional

Notes: Table 3.A1 presents the effect of pre-test grandparental death on participation in the track placement test, i.e. being present in the baseline sample. The estimated coefficients are expressed as average marginal effects. Standard errors are clustered at mother ID level in parentheses. Column 1 includes all children who were registered in 6th grade between 2008 and 2014 and lost a grandparent in the three months before or after the track placement test. Column 2 presents the effects for children where the majority of pupils in their school take the track placement test. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income, siblings of mother and father. Significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.A2: Balance tests

	(1) Grandparental death	(2) Grandparental death
Age	0.0043	0.0039
	(0.0051)	(0.0052)
Boy	-0.0006	-0.0009
	(0.0054)	(0.0055)
Migrant background	0.0067	0.0048
	(0.0098)	(0.0102)
Oldest child	0.0015	0.0002
	(0.0061)	(0.0062)
Mother's age	0.0015**	0.0014
	(0.0007)	(0.0009)
Grandfather died	-0.0052	-0.0036
	(0.0055)	(0.0056)
Grandparent from mother	-0.0073	-0.0056
	(0.0056)	(0.0057)
No. of children	-0.0001	-0.0004
	(0.0027)	(0.0027)
Single-parent	-0.0076	-0.0084
	(0.0100)	(0.0110)
Percentile disposable income	-0.0001	-0.0000
	(0.0001)	(0.0001)
Mother has siblings	-0.0037	-0.0041
	(0.0095)	(0.0097)
Father has siblings	-0.0129	-0.0180*
	(0.0091)	(0.0095)
Unemployment benefits - mother		-0.0148
		(0.0251)
Social assistence - mother		0.0191
		(0.0221)
Disability insurance - mother		-0.0052
		(0.0176)
Father's age		0.0003
		(0.0008)
Unemployment benefits - father		-0.0304
		(0.0247)
Social assistence - father		-0.0193
		(0.0311)
Disability insurance - father		0.0172
		(0.0200)
N	34,022	33,081

Notes: Table 3.A2 presents the correlations between background characteristics and pre-test grandparental death. The estimated coefficients are expressed as average marginal effects. Standard errors are clustered at mother ID level in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.A3: Discrepancy teacher advice and standardized test performance by treatment status

	(1) Treatment	(2) Control	(3) Difference
Teacher advice $\neq$ test outcome (%)	29.12	28.81	-0.31
N	12,332	12,049	24,381

Notes: Table 3.A3 presents the average share of children receiving a teacher recommendation that is not aligned with the track placement test outcome by treatment status. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.A4: Effect of standardized test on initial track placement

	(1) Grade 7: Vocational	(2) Grade 7: Academic
Test score	-0.2067***	0.2298***
	(0.0003)	(0.0004)
N	1,101,571	1,101,571
Controls	Yes	Yes

Notes: Table 3.A4 presents the correlation of the track placement test score on track placement in the vocational track in grade 7. The estimated coefficients are expressed as average marginal effects. Standard errors are clustered at mother ID level in parentheses. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.A5: Effect of grandparental death on graduation outcomes

	(1) Degree: Vocational	(2) Degree: General	(3) Degree: Academic	(4) Exam: Dutch	(5) Exam: English
Grandparental death	0.0135*	-0.0034	-0.0095	-0.0153	-0.0108
	(0.0077)	(0.0072)	(0.0066)	(0.0158)	(0.0150)
N	15,303	15,303	15,303	15,303	15,303
Controls	Yes	Yes	Yes	Yes	Yes

Notes: Table 3.A5 presents the effect of pre-test grandparental death on track graduation and centralized exam scores. The estimated coefficients are expressed as average marginal effects in columns 1 to 3, and in standard deviations in columns 4 and 5. Standard errors are clustered at mother ID level in parentheses. Children who participated in the standardized test after 2011 are excluded as they did not graduate yet. The effects are larger than I observe in table 3.8, because of the selection of cohorts. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income, as well as for track in columns 4 and 5. Significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.A6: Heterogeneous effects of grandparental death on track placement test outcomes

	(1) Total score	(2) Total score	(3) Total score	(4) Total score	(5) Total score	(6) Total score	(7) Total score	(8) Total score	(9) Total score
Gp. death	-0.0324**	-0.0273**	-0.0381*	-0.0810*	-0.0337**	-0.0206	-0.0351***	-0.0323***	-0.0297**
	(0.0130)	(0.0138)	(0.0206)	(0.0420)	(0.0142)	(0.0157)	(0.0107)	(0.0105)	(0.0106)
Gp. death*Same municipality	0.0059								
	(0.0205)								
Gp. death*Mother's side		-0.0047							
		(0.0202)							
Gp. death*Surviving partner			0.0114						
			(0.0237)						
Gp. death*Aunts/uncles				0.0552					
				(0.0432)					
Gp. death*Boy					0.0088				
					(0.0201)				
Gp. death*Grandfather						-0.0147			
						(0.0205)			
Gp. death*Single parent							0.0517		
							(0.0325)		
Gp. death*Low income								0.0354	
								(0.0358)	
Gp. death*Migrant									0.0044
									(0.0355)
N	34,022	34,022	34,022	34,022	34,022	34,022	34,022	34,022	34,022
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 3.A6 presents heterogeneous effects of pre-test grandparental death on the track placement test. The estimated coefficients are expressed in standard deviations. Standard errors are clustered at mother ID level in parentheses. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income. Significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3.A7: Heterogeneous effects of grandparental death on makeup test participation, teacher recommendation and track switching

	(1) Makeup test	(2) Makeup test	(3) Makeup test	(4) Advice: Voc.	(5) Advice: Voc.	(6) Advice: Voc.	(7) Switch up	(8) Switch up	(9) Switch up
Gp. death	0.2646**	0.2866***	0.2748***	0.0685**	0.0691**	0.0638**	0.1486**	0.1601**	0.1579**
	(0.1048)	(0.1043)	(0.1028)	(0.0296)	(0.0295)	(0.0294)	(0.0631)	(0.0626)	(0.0629)
Gp. death*Single parent	0.2902			0.0427			-0.0919		
	(0.3145)			(0.0864)			(0.1885)		
Gp. death*Low income		0.1068			0.0306			-0.2357	
		(0.3237)			(0.0896)			(0.2012)	
Gp. death*Migrant			0.2722			0.0942			-0.1890
			(0.3653)			(0.0905)			(0.1941)
N	34,022	34,022	34,022	24,381	24,381	24,381	34,022	34,022	34,022
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 3.A7 presents heterogeneous effects of pre-test grandparental death on makeup test participation, teacher recommendation and track switching. The estimated coefficients are expressed as logit coefficients. Standard errors are clustered at mother ID level in parentheses. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

Table 3.A8: Effect of grandparental death on track attendance excl. bridge classes

	(1) Grade 7: Voc.	(2) Grade 7: Aca	(3) Grade 8: Voc.	(4) Grade 8: Aca.	(5) Grade 9: Voc.	(6) Grade 9: Aca.	(7) Grade 10: Voc.	(8) Grade 10: Aca.
Grandparental death	0.0203***	-0.0158**	0.0188**	-0.0142*	0.0176**	-0.0158**	0.0177**	-0.0152**
	(0.0078)	(0.0074)	(0.0078)	(0.0074)	(0.0078)	(0.0074)	(0.0078)	(0.0072)
N	13,640	13,640	13,640	13,640	13,640	13,640	13,640	13,640
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 3.A8 presents the effect of pre-test grandparental death on track attendance in grades 7 to 10. The estimated coefficients are expressed as average marginal effects. Standard errors are clustered at mother ID level in parentheses. The sample excludes children who were placed in a bridge class in grade 7. The set of controls include: children's age, gender, migrant background, birth order, number of siblings, mother's age, single-parent household, percentile disposable income. Significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

3.B Bridge Class Ambiguity

Some of the results presented in this paper may at first glance seem counterintuitive. On the one hand, I observe an increased treatment effect between seventh grade vocational track placement and tenth grade vocational track attendance. While on the other hand, I show that treated children more often switch to a higher track during secondary school, which would result in a reduction of the treatment effect. These contradicting findings can be explained by the presence of bridge classes in seventh grade. To illustrate how the presence of bridge classes may impact the development of the treatment effect across grades, I construct a hypothetical division of track attendance shares in grade 7 and 10 for the control and treatment group in table 3.B1.

Table 3.B1: Hypothetical percentages of children per track in grade 7 and 10

		Voc.	Voc./Gen.	Gen.	Gen./Aca.	Aca.
Control	Grade 7	35	20	10	20	15
	Grade 10	45	0	30	0	25
Treatment	Grade 7	36	21	10	19	14
	Grade 10	46.5	0	30	0	23.5

In this hypothetical scenario I make two assumptions. First, in seventh grade treated children are more often placed in a bridge class that combines the vocational and general track, instead of the general with the academic track, than the control group. Second, half of the children in a bridge class end up in the lower track of the two, and half in the higher track. Accordingly, the treatment effect in grade 7 is a 1 percentage point increase (decrease) in the likelihood to be placed in the vocational (academic) track. While in grade 10 the treatment effect is respectively a 1.5 percentage points increase (decrease). The track indicators in seventh grade do not take into account that potentially more children are in lower bridge classes, while this effect is captured in grade 10. Therefore, even in the light of increased upward track switching, the seventh grade coefficients can be smaller than the ones in tenth grade.

Unfortunately I cannot check this hypothesis as the data contains no information on the type of bridge class a child attends. However, as a sanity check I look at the development of the treatment effect between grade 7 and 10 excluding children who started seventh grade in a bridge class. In table 3.A8 I indeed observe a decreasing trend of the treatment effect from grade 7 to grade 10, which is in line with treated children switching more often to a track upwards.

Chapter 4

Parental Investments and Environmental Incentives

4.1 Introduction

Parents spend a lot of time, money and effort to raise successful children. There exists extensive evidence that parental inputs are indeed important for children's human capital development and later-life outcomes (see e.g. Todd and Wolpin, 2007; Cunha and Heckman, 2008; Del Boca et al., 2014; Fiorini and Keane, 2014). Yet, the intensity with which parents invest in their children varies substantially between families and over time (Kalil, 2015).

The first intergenerational human capital investment models explain differences in parental investments by variation in background characteristics such as the financial constraints parents are bound to (Becker and Tomes, 1979, 1986). Accordingly, the economics literature devotes a lot of attention to investigate the role of limitations within the family environment in generating variation in parenting behavior. For instance, there is ample evidence that disadvantaged family backgrounds – related to parental education, household income, or parental unemployment status – are associated with less favorable parenting practices (see e.g. Kalil, 2015; Heckman and Mosso, 2014; Lareau, 2011; Cunha et al., 2006; Cobb-Clark et al., 2018).

More recently, studies emerged that explore whether variation in parental investments can be explained by parents facing different external incentives to invest. A paper by Doepke and Zilibotti (2017) formalizes the idea that parenting styles may react to environmental conditions. The authors present a model similar to the original human capital investment models, however they allow the returns to investments to depend on a family's socioeconomic environment. The broad intuition being that different socioeconomic circumstances require different types of cognitive and non-cognitive skills to do well in life. The authors also provide suggestive empirical evidence of important heterogeneities at the country-level, and find significant correlations between parenting styles and measures of returns to education, inequality and redistribution policies that support their theoretical model. A paper by Dohmen et al. (2019), confirms that parenting styles adapt to the external environment, as the authors observe a decrease in the use of permissive parenting if the expected returns to education are higher.

This paper contributes to this emerging stream of literature and investigates what happens to parents' investment choices when they face changing economic conditions due to higher unemployment rates. The expectation is that in environments with high unemployment rates, finding employment is hard, and an individual's human capital becomes relatively more important for being successful. This reasoning is supported by evidence of the Great Recession where the least educated people were most severely hit by the economic downturn (OECD, 2016). Moreover, it has been widely documented by the literature that incentives stemming from increased unemployment rates can motivate individuals to invest more in their own human capital (see e.g. Rice, 1999; Clark, 2011; Barr and Turner, 2015; Sievertsen, 2016). Therefore, I expect that declining economic circumstances can also foster incentives for parental investments, as they raise the significance of human capital for ensuring favorable later-life outcomes.

To investigate the relationship between the living environment and parenting behavior, I make use of survey data from the German Socio-Economic Panel. The data combine measures of parental investments with detailed information on child, parent and household characteristics. I link the survey data to regional unemployment data, which serves as a proxy for the broader economic environment parents encounter. The main challenge in estimating the causal effect of the unemployment rate on parental investments, is endogeneity bias that arises if e.g. parents with different investment capabilities sort into particular types of economic environments. Therefore, I estimate a state- and year-fixed effect model to control for regional- and time-invariant heterogeneity. In addition, I control for a broad array of background characteristics that may vary over time such as parent's unemployment status, to ensure that the effects cannot be explained by changes in families' personal circumstances.

The findings provide evidence that parents respond to environmental incentives, despite the relative crudeness of the economic environment proxy. I find that an increase in the regional unemployment rate significantly increases supportive parenting practices, raises the chance that parents are interested in their child's academic performance, and increases the chance of offering homework help.

In addition, the results show that parental and child background characteristics can influence how responsive parents are to incentives from the external environment. First, the estimates point towards stronger responses for parents with lower locus of control levels, which is intuitive since a lower locus of control implies that parents attach a larger value to the role of the environment in determining life outcomes. Second, I find stronger responses when children received a lower secondary school track recommendation. This is in line with lower educated individuals being hit harder by the negative consequences of recessions, causing these children to be at a higher risk to be affected by worsened economic situations. Third, families in the lowest quartile of the income distribution show larger increases in parental investments measures. It is likely that parents from disadvantaged backgrounds are more attentive to worsened economic conditions, or see them as more relevant, and therefore respond stronger. Finally, I observe a weaker response to environmental incentives when parents are less educated. Although it can not be said with certainty, for parents without a secondary school qualification, the additional stress and worry for their own employment that comes with a worsened economic situation may outweigh the higher awareness and result in lower investments (see e.g. Kalil, 2013).

These findings can have implications for studies that investigate the importance of perceived returns for parental investments. It has been shown that beliefs about returns to investments are highly predictive for actual investments and that there are substantial heterogeneities in beliefs between parents from different socioeconomic backgrounds (see e.g. Cunha et al., 2013; Boneva and Rauh, 2018). The results presented by this paper are suggestive that the regional economic environment may influence parents' return expectations and that the perception of the environment may differ by background characteristics of parents and children.

This paper also relates to the empirical literature that investigates how parental behavior is influenced by a household's local neighborhood environment. The evidence here is mixed: some studies show that, in richer neighborhoods, parents become more involved in school and read more often to their child; other studies find a decrease of supportive parenting when families are randomly relocated to better neighborhoods (Leventhal and Brooks-Gunn, 2000, 2001; Kohen et al., 2008; Schonberg and Shaw, 2007; Patacchini and Zenou, 2011). However, on the neighborhood level it is difficult to distinguish between what is driven by a family's personal circumstances or sorting and what is caused by the external environment.

The remainder of the paper is structured as follows. In the next section I describe the survey and measures in more detail. Section 4.3 explains the empirical strategy and the underlying identifying assumptions. Section 4.4 presents the results, including robustness tests. Finally, section 4.5 provides concluding remarks.

4.2 Data

The analysis draws primarily on data from the German Socio-Economic Panel (SOEP), a representative household survey that is conducted annually since 1984 (DIW, 2019). The survey follows roughly 11,000 households over time, consisting of more than 30,000 individuals (Wagner et al., 2007). The themes inquired by SOEP cover a wide range of individual characteristics such as education level, unemployment status, migration background and personality traits. Also with respect to household characteristics the

SOEP gathers detailed information, for example on the number of children within a household and the disposable income. In addition, the SOEP administers since the beginning of the century a specific youth questionnaire to children in the year they turn 17, which I use to obtain knowledge on parental investments.

The youth survey contains several questions where children are asked about parental behavior. I classify behavior as parental investment when 1) it requires parents to spend time, money or attention, and 2) it fosters children's development. Several survey items fulfill these requirements. First, maternal (paternal) support measures the degree to which the mother (father) expresses a supportive and predominantly authoritative parenting style. Both support variables are standardized and constructed by factor analysis.[1] Moreover, the youth survey inquires about whether or not parents display interest for children's academic performance, if they provide actual support with homework and whether they have hired a tutor to help their children with school work. Lastly, the questionnaire asks about ways parents have contact with the child's school. From this an ordinal measure is constructed counting the number of ways parents contact school.[2]

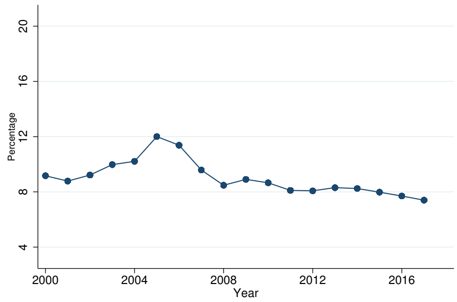
The economic environment is measured by the local prevalent unemployment rate. As this is a relatively crude measure for economic incentives, it should rather be seen as a proxy for the broader economic environment that parents face in their region. I make use of the unemployment data stemming from the German Federal Institute for Research on Building, Urban Affairs and Spatial Development. The unemployment rate reflects the percentage of individuals aged 15 to 65 that are unemployed according to the German Unemployment Agency, and is measured in the main specifications at the federal state level (BBSR, 2017a). As the SOEP surveys are predominately conducted within the first four months of the year, the unemployment rate is measured in the year before the survey took place. Figure 4.1 presents the yearly development of the unemployment rate for all 16 German federal states over the sample period, which shows quite some variation both across states as well as over time.

For the empirical analysis to be relevant I only include children who follow education at the time of the survey, and who live in the same household as their mother. Moreover, I exclude children for whom not all family and parental background variables are available. The final sample consists of 5009 children, surveyed in the years 2001 to 2018.

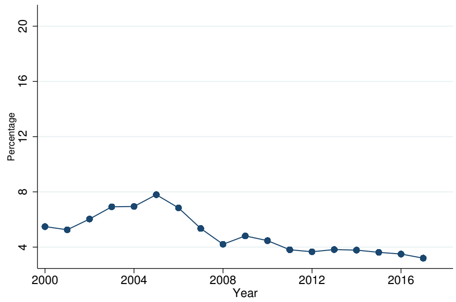
[1] The included items overlap with the items of the parenting style and dimension questionnaire (PSDQ) that define an authoritative parenting style (Robinson et al., 2001).

[2] For the exact items of the maternal and paternal support, as well of school contact, please see table 4.A1 in the appendix.

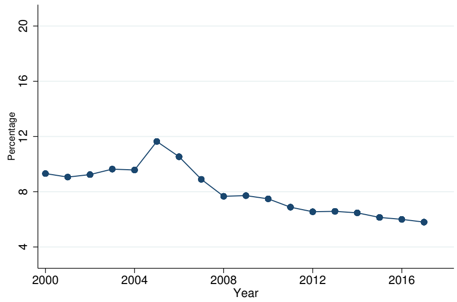
Figure 4.1: Yearly unemployment rate by federal state



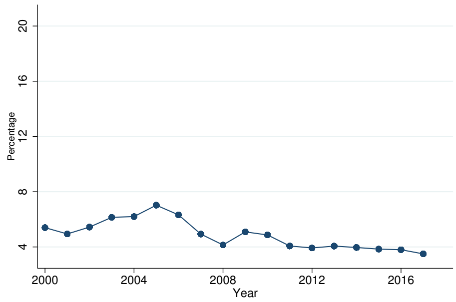
(a) North Rhine-Westphalia



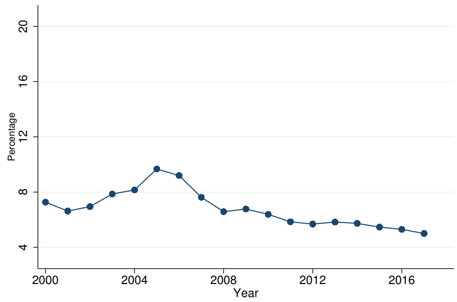
(b) Bavaria



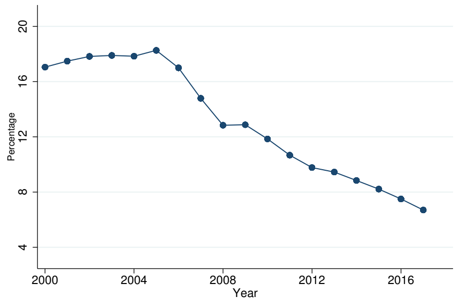
(c) Lower Saxony



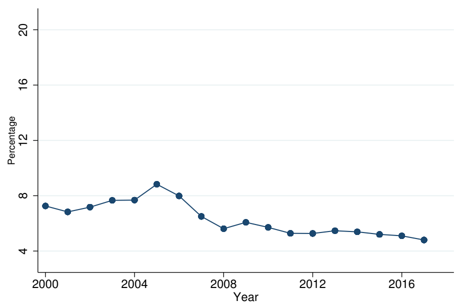
(d) Baden-Wuerttemburg



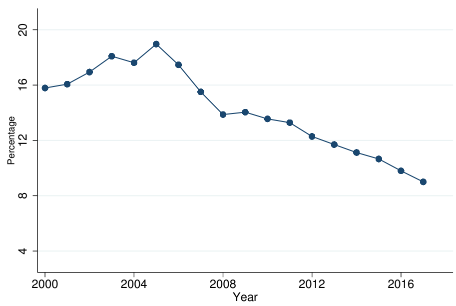
(e) Hessen



(f) Saxony



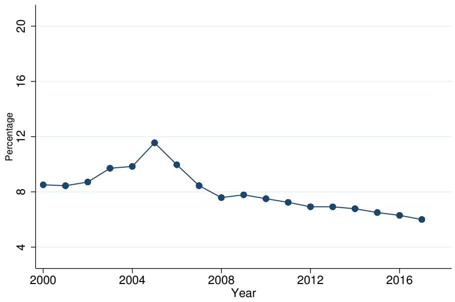
(g) Rhineland-Palatinate



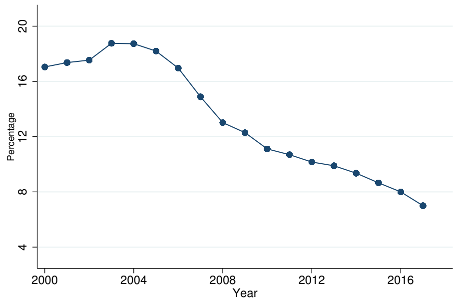
(h) Berlin

Notes: Figure 4.1 shows the development of the unemployment rate for each German state from 2000 to 2017 (BBSR, 2017a).

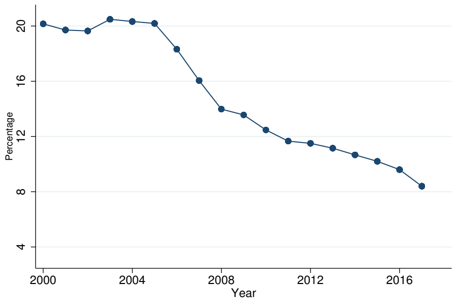
Figure 4.1 (continued): Yearly unemployment rate by federal state



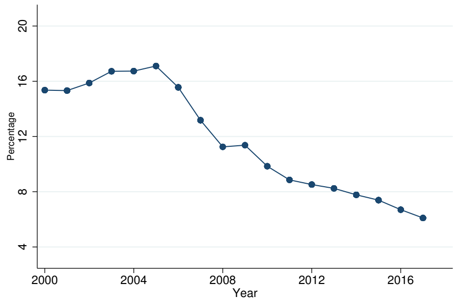
(i) Schleswig-Holstein



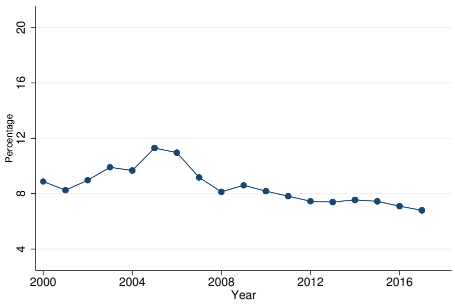
(j) Brandenburg



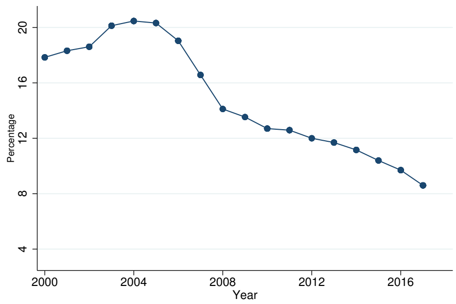
(k) Saxony-Anhalt



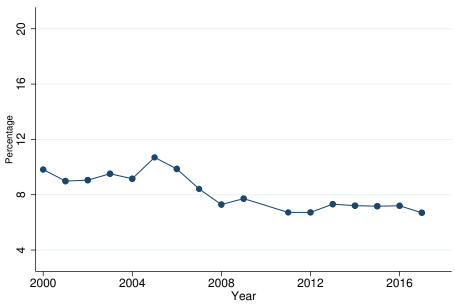
(l) Thuringia



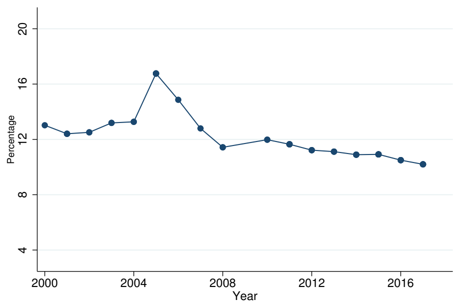
(m) Hamburg



(n) Mecklenburg-Vorpommern



(o) Saarland



(p) Bremen

Notes: Figure 4.1 shows the development of the unemployment rate for each German state from 2000 to 2017 (BBSR, 2017a).

4.3 Empirical Strategy

As the prime goal of this paper is to analyze the impact of the economic environment on parents' investment choices, I estimate the following equation:

$$y_{i,r,t} = \beta_0 + \beta_1 U_{r,t-1} + \delta X_{i,t} + \rho_t + \omega_r + \epsilon_{i,r,t} \tag{4.1}$$

where $y_{i,r,t}$ is a vector of the parental investment measures of child i, who lives in state r, in year t. $U_{r,t-1}$ denotes the lagged unemployment rate at the federal state level. The terms ρ_t and ω_r are sets of state and year dummies that respectively capture regional and time fixed effects. Depending on whether the parental investment variable is continuous or binary the equation is estimated by means of ordinary least squares (OLS) or logit regression. Moreover, as the main variation of the unemployment rate emerges across states, the error term $\epsilon_{i,t}$ is clustered at the state level.

$X_{i,t}$ is a set of control variables. Following the extensive literature on individual determinants of parenting behavior, I include controls for parental education, household income, number of children and a measure of maternal locus of control.[3] In addition, I control for parental unemployment and single-parenthood, of which there is evidence that their likelihood of occurring is affected by the state of the economy (see e.g. Amato and Beattie, 2011). Arguably, these indicators could be seen as a mechanism of how regional economic circumstances impact parenting practices. However, as the aim of this paper is to detect the effect of economic incentives, rather than indirect effects through changed personal circumstances, I include both measures as controls. Finally, I control for a child's gender, migration background, and mother's age.

The main challenge in estimating the causal effect of environmental incentives on parental investments is endogeneity, as there are several potential variables that could simultaneously influence a family's economic environment as well as their parenting behavior. For instance, in Germany states are to a certain extent free to design their own education system. Differences in the educational set-up might not only influence parenting practices, but could simultaneously impact a state's unemployment rate. A similar reasoning applies to other institutional or cultural differences between states. The state fixed effects ensure that these invariant state characteristics are controlled for. Moreover, the year fixed effects take care of spurious correlations originating from broader time trends of parenting behavior and the economy. Assuming that the state and year fixed effects capture all unobserved heterogeneity that is correlated with both parental investments and the economic environment, the β_1-coefficient estimates the causal effect of changes in the unemployment rate within states, over time, on parental investments.

Furthermore, this paper aims to analyze whether the response of parental investments

[3] I base individual parent controls only on maternal characteristics, as for all children the mother was surveyed, while this holds not for fathers.

to environmental incentives depend on certain parental background characteristics. To capture potential heterogeneous responses, I estimate a second equation where I interact the independent variable of interest with specific background variables:

$$y_{i,r,t} = \beta_0 + \beta_1 U_{r,t-1} + \beta_2 U_{r,t} * q_{i,t} + \delta X_{i,t} + \rho_t + \omega_r + \epsilon_{i,r,t} \tag{4.2}$$

here $q_{i,t}$ is a subset of the control variables in $X_{i,t}$, for which heterogeneous effects may be expected. In particular, $q_{i,t}$ includes a measure for maternal locus of control, a child's secondary school track recommendation, household's disposable income and maternal education level. All these measures may influence the perceived risk by parents that their child is affected by an unfavorable economic environment.

4.4 Results

4.4.1 Main Results

Table 4.1 shows the impact of the state unemployment rate on the different parenting investment measures. Column 1 presents a positive statistically significant effect of the unemployment rate, with maternal support as the dependent variable. A one percentage point increase in regional unemployment increases maternal support such as talking about a child's worries, by 0.018 standard deviations. I also find a positive effect for paternal support in column 2, although this is not statistically significant. In addition, column 3 shows a positive effect for parental interest in a child's academic performance. The estimated marginal effect implies that a one percent point increase in the unemployment rate increases the probability of parents being interested in their child's academic performance by 1.1 percent. The increased academic interest seems to translate in also providing active assistance with children's homework, as is portrayed in column 4. It shows that parents who are faced with a one percentage point increase in the state unemployment rate are 1.5 percent more likely to help their child with homework. For the investment measures in the last two columns, that is hiring a tutor and contact intensity with a child's school, I observe no significant effects of the regional unemployment rate, with the standard errors ruling out any effect larger than 0.5 percentage point.[4]

The results indicate that when the unemployment rate in a household's surroundings go up, parents react by increasing certain investments into their children, which is in accordance with the reasoning laid out in the introduction. The magnitude of the effect of the unemployment rate on the different investment measures is meaningful, considering that it is merely a rough proxy for people's broader economic living surroundings and their personal situation did not change. Especially in times of a recession when unemployment rates can go up several percentage points per year, its effect on parental

[4]For ease of interpretation column 6 is estimated by means of OLS regression, however estimating an ordered logit model instead lead to qualitative similar results.

Table 4.1: The effect of regional unemployment on parental investments

	(1) Maternal support	(2) Paternal support	(3) Academic interest	(4) Homework help	(5) Paid tutor	(6) Contact with school
Unem. rate	0.018**	0.012	0.011**	0.015***	-0.004	-0.005
	(0.008)	(0.015)	(0.005)	(0.005)	(0.005)	(0.006)
N	5009	5009	5009	5009	5009	5009
State & Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 4.1 shows the effect of the state unemployment rate on measures of parental investment. Maternal and paternal support are standardized as a Z-score. Academic interest, homework help and paid tutor are binary variables. Contact with school is measured on a five-point scale. The set of controls include: children's gender, secondary school track, household income vigintile, number of children within the household, single-parenthood, parents' unemployment status, parents' education, immigrant background, mother's age and locus of control. Columns 1, 2 and 6 are estimated by means of OLS, while columns 3 and 4 are estimated by logit regressions. The logit coefficients are displayed as average marginal effects. Standard errors are clustered at the federal state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

investment measures can be substantial.[5]

The absence of an effect for hiring a tutor and school contact intensity, could have several reasons. Potentially, these type of investments require more or different resources from parents than the other investment measures, and are therefore less susceptible or more difficult to change. For example, it can be quite expensive to hire a private tutor. Another explanation can be that parents perceive these types of investments as less relevant for children's human capital development.

4.4.2 Heterogeneous Responses

In addition to the general effect of the economic environment, this paper analyses whether parents with certain background characteristics are more incentivized by the environment than others. As stated in section 4.3, I look at maternal locus of control, a child's secondary school track recommendation, household's disposable income and maternal education level, since these measures may influence parents' perceived risk for their child to be harmed by increasing unemployment rates.

First, maternal locus of control reflects the importance mothers attach to the environment for determining life outcomes. Hence, mothers with a low locus of control, measured as being in the lowest quartile of the distribution, are expected to have a stronger response to worsening economic conditions as they are more likely to belief that these conditions negatively impact their children. The results for the continuous maternal support investment variable are presented in table 4.2, while the outcomes for the binary variables of academic interest and homework support are presented by marginal plots in figures 4.2 and 4.3.[6] Column 1 in table 4.2 shows that, in line with

[5] See table 4.A2 in the appendix for an overview of the effect sizes of the background variables

[6] For the results of the other three investment measures see table 4.A3 and figure 4.A1 in the appendix.

Table 4.2: Heterogeneous effects of regional unemployment on maternal support

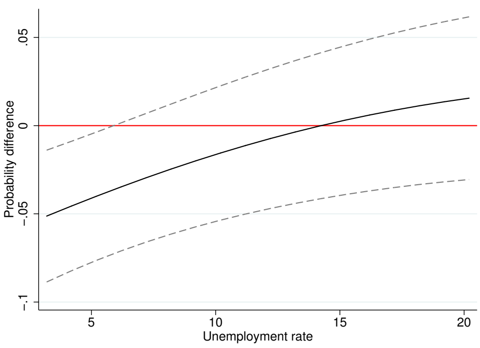
	Maternal support			
	(1)	(2)	(3)	(4)
Unem. rate	0.019**	0.024**	0.021**	0.020**
	(0.007)	(0.010)	(0.007)	(0.007)
Unem.*Low loc	0.009**			
	(0.003)			
Unem.*Vocational track		-0.004		
		(0.009)		
Unem.*Low educated			-0.032*	
			(0.017)	
Unem.*Low income				0.008
				(0.014)
N	5009	5009	5009	5009
State & Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes

Notes: Table 4.2 shows the interaction effects of the state unemployment rate and several background characteristics on the maternal support measure. Maternal support is standardized as a Z-score. The set of controls include: children's gender, secondary school track, household income vigintile, number of children within the household, single-parenthood, parents' unemployment status, parents' education, immigrant background, mother's age and locus of control. In addition, I include indicators of low maternal locus of control, vocational track recommendation, low parental education, and low household income. The regressions are estimated by means of OLS. Standard errors are clustered at the federal state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

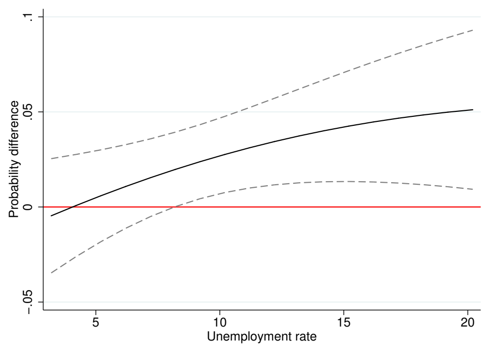
expectations, mothers with a low locus of control provide more support when the unemployment rate rises than mothers with a higher locus of control. Nonetheless, panel A of figures 4.2 and 4.3 show no significant difference in the marginal effect between low and high locus of control parents on the probability of academic interest and homework assistance. This could be because although parents with a lower locus of control think the environment matters more, at the same time we know that these parents underestimate the impact their investments have on their child's development (Cunha et al., 2013; Lekfuangfu et al., 2018).

Second, as the least educated individuals are hurt the most by high unemployment rates, parents with children in lower secondary school tracks might be more concerned. Therefore, I look at whether the effect on parental investments differs by the track recommendation children received at the age of ten. I compare children who received a vocational track recommendation, to those who received an academic track recommendation, where I expect to see stronger parental responses for children who were advised

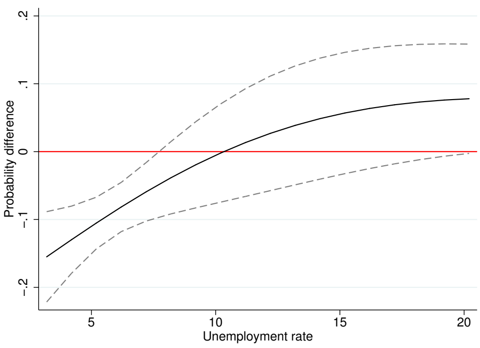
Figure 4.2: Heterogeneous effects of regional unemployment on parental academic interest



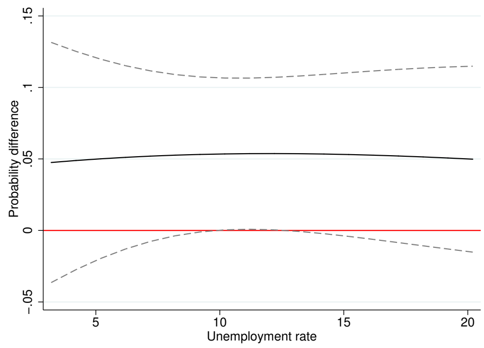
(a) By locus of control



(b) By vocational track recommendation



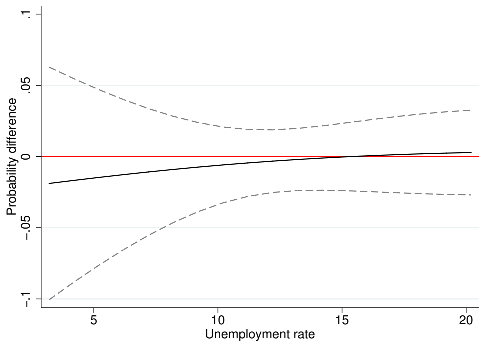
(c) By parental education



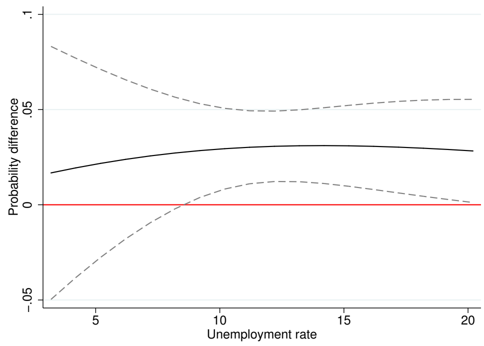
(d) By parental income

Notes: Figure 4.2 shows the difference in the marginal effect of a one percentage point increase in state unemployment on the probability of parents showing academic interest by certain background characteristics. Panel A presents the difference between parents with a locus of control measure in the lowest quartile versus parents with a measure belonging to one of the three highest quartiles. Panel B shows the difference between children who received a vocational track recommendation and children receiving an academic track recommendation. Panel C displays the difference based on whether parents have less than a secondary school qualification. Finally, panel D presents the difference between households with a disposable income in the lowest quartile versus households with an income belonging to one of the three highest quartiles. The dotted gray lines represent the 90 percent confidence intervals.

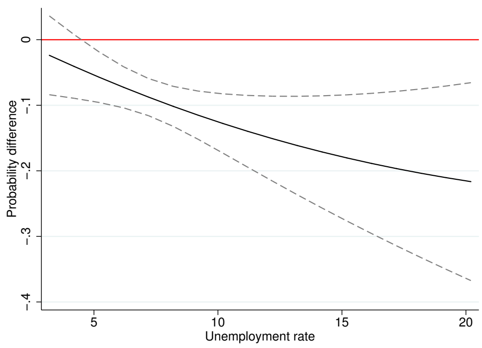
Figure 4.3: Heterogeneous effects of regional unemployment on parental homework help



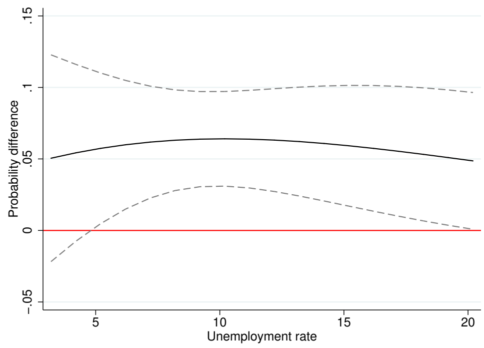
(a) By locus of control



(b) By vocational track recommendation



(c) By parental education



(d) By parental income

Notes: Figure 4.3 shows the difference in the marginal effect of a one percentage point increase in state unemployment on the probability of parents helping with homework by certain background characteristics. Panel A presents the difference between parents with a locus of control measure in the lowest quartile versus parents with a measure belonging to one of the three highest quartiles. Panel B shows the difference between children who received a vocational track recommendation and children receiving an academic track recommendation. Panel C displays the difference based on whether parents have less than a secondary school qualification. Finally, panel D presents the difference between households with a disposable income in the lowest quartile versus households with an income belonging to one of the three highest quartiles. The dotted gray lines represent the 90 percent confidence intervals.

a vocational track.[7] Column 2 of table 4.2 displays no difference in the provision of maternal support for children with a vocational track recommendation. However, panel B of figures 4.2 and 4.3 indeed portrays marginally significant positive effects on the indicators of academic interest and homework assistance.[8]

Third, parents' own educational background might play a role in how parents perceive the economic environment. On the one hand, lower educated parents, i.e. without a secondary school qualification, may be more attentive to worsening economic circumstances, and therefore be more incentivized by them. On the other hand, low-educated parents themselves are at higher risk to be hurt by rising unemployment rates. This might cause them to experience higher stress levels, leading to decreasing attention for parental investments (Kalil, 2013).[9] In accordance with the latter reasoning, column 3 of table 4.2, as well as panel C of figure 4.3, show a negative interaction effect for maternal support and homework help, respectively. Whereas panel C of figure 4.2 displays no significant difference in the probability to show academic interest.[10]

Finally, a similar trade-off could hold for household income; parents with lower income could be more attentive towards the unemployment rate, while at the same time they might be more pressured by it. As can be observed from panel D of figures 4.2 and 4.3, I find positive interaction effects between being in the lowest quartile of the income distribution and indicators of academic interest and homework help. These outcomes suggests that it is rather educational background than income, that matters when it comes to experiencing stress due to higher unemployment rates. Instead, lower income families respond to increased unemployment rates by raising parental investments.

4.4.3 Additional Results

To test the sensitivity of the main results with respect to the included sample and the economic indicator, I perform several robustness analyses. First, until now I assume that the set of controls capture all heterogeneity that is left after including the fixed effects and is correlated with both the unemployment rate and the error term. However, selection bias could still arise when families move to, or away from, a certain region, explicitly taking into consideration the changing environmental context, generating endogenous contextual conditions. Most likely this would lead to an underestimation of the results, as intuitively parents who care the most about the economic environment move to more advantageous surroundings. To explore this possibility, I restrict the sample to only those

[7] Depending on the state the percentage of children going to vocational tracks ranges from 55 to 68 percent (Statista, 2019).

[8] Although there is no general effect of the unemployment rate on the probability of hiring a tutor and intensity of school contact, column 6 of table 4.A3 and panel B of figure 4.A1 in the appendix do present a marginally significant positive interaction effect for children with a vocational track recommendation.

[9] Note that as I control for parental unemployment status effects cannot be caused by parents becoming unemployed themselves.

[10] In addition, column 7 of table 4.A3 in the appendix presents a negative interaction effect for intensity of contact with school.

families that did not move to another municipality in the last three years.[11] Table 4.3, columns 2, 5 and 8, show similar, or even slightly lower, point estimates and significance levels of the effect of the unemployment rate on maternal support, academic interest and homework assistance for this restricted sample.[12] Hence, there is no reason to belief that the baseline estimates are underestimated due to parents moving to more prosperous regions.

Second, I test whether the federal state is the relevant regional level to consider. It could be that incentives stemming from a more local economic environment have a higher relevance for parents. Therefore, the coefficients in columns 3, 6 and 9 of table 4.3 are estimated with the unemployment rate measured at the level of the regional economic center (RoR), and include RoR fixed effects. In total there are 96 regional economic centers in Germany, which are constructed based on local labor markets and commuting areas (BBSR, 2017b). For all three parental investment variables employing the RoR unemployment rate reduces its impact, both in terms of effect size and significance level. This suggests that regarding investment choices, parents are more incentivized by the state level environment than by the local labor market environment. A potential explanation could be that when it comes to a child's future, parents rather consider the state environment to be relevant than the local labor market. Alternatively, this finding could be related to the way how information is distributed, as (economic) news is often reported at the federal state level.

Third, the unemployment rate is merely one potential proxy for the environmental incentives parents face. An alternative indicator that is frequently mentioned in this literature is the prevalent level of income inequality (see e.g. Doepke and Zilibotti, 2017). The underlying intuition is the same; in more unequal surroundings the relative income loss of not succeeding is larger, and hence there lies more weight on human capital to succeed. I therefore investigate how parental investments respond to changes in inequality, where income inequality is defined by the 90/10 ratio, which is a common indicator for inequality (OECD, 2019).[13] Table 4.A5 in the appendix shows that the inequality rate does not significantly influences any of the parental investment measures.

The insignificant results for the inequality rate raise the question when parents actually react to incentives from the economic environment? The most straightforward answer is whenever the economic environment alters beliefs of parents about the future chances of their children. To test whether this holds for the environmental proxies, table 4.4 regresses an indicator of being worried about the economic development on both

[11]As the analysis includes state fixed effects, it would be sufficient to restrict the sample to those households that did not move outside their state. However, as this information is not available, I take the more conservative approach and restrict the sample to those who did not move to a different municipality.

[12]The results for the other parental investment measures can be found in table 4.A4 in the appendix, but also seem unaffected.

[13]The 90/10 ratio demonstrates the relationship between the income of the 90th percentile compared to the income of the 10th percentile. I construct this ratio based on personal income information of the entire (weighted) SOEP sample, and to ease interpretation standardize it.

Table 4.3: Robustness of the effect of regional unemployment on parental investments

	Maternal support			Academic interest			Homework help		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Unem. rate	0.018**	0.016*	0.015	0.011**	0.010**	0.007	0.015***	0.013**	0.011**
	(0.008)	(0.009)	(0.010)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
N	5009	4648	5009	5009	4648	5009	5009	4648	5009
State & Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Specification	Baseline	Not moved	RoR	Baseline	Not moved	RoR	Baseline	Not moved	RoR

Notes: Table 4.3 shows the robustness of the state unemployment rate on parental investments. Maternal support is standardized as a Z-score. Academic interest and homework help are binary variables. Columns 1, 4 and 7 present the baseline specification. Columns 2, 5 and 8 restrict the sample to families who did not move municipalities during the last three years. Columns 3, 6 and 9 measures the unemployment rate at the regional economic center level, and accordingly includes regional economic center fixed effects. The set of controls include: children's gender, secondary school track, household income vigintile, number of children within the household, single-parenthood, parents' unemployment status, parents' education, immigrant background, mother's age and locus of control. Columns 1 to 3 are estimated by means of OLS, while columns 4 to 9 are estimated by logit regressions. The logit coefficients are displayed as average marginal effects. Standard errors are clustered at the federal state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.4: The effect of regional unemployment and inequality on parental worries.

	Maternal worry: eco. dev.		Paternal worry: eco. dev.	
	(1)	(2)	(3)	(4)
Unem. rate	0.010***		0.023***	
	(0.003)		(0.005)	
Ineq. ratio		0.009		-0.005
		(0.010)		(0.013)
N	5009	5009	4168	4168
State & Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes

Notes: Table 4.4 shows the effect of the state unemployment rate and inequality ratio on maternal and paternal worries about economic development. Both maternal and paternal worries are binary variables. Columns 3 and 4 have less observations as less fathers filled in the survey. The set of controls include: children's gender, secondary school track, household income vigintile, number of children within the household, single-parenthood, parents' unemployment status, parents' education, immigrant background, mother's age and locus of control. The regressions are estimated by means of logit regression. All coefficients are displayed as average marginal effects. Standard errors are clustered at the federal state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the unemployment and inequality rate. Columns 1 and 3 show that both mothers and fathers are more likely to be concerned when the unemployment rate goes up. A one percentage point increase in the state unemployment rate, increases the probability that mothers and fathers are worried about the economic development by respectively 1.0 and 2.3 percent. By contrast, columns 2 and 4 make clear that changes in the regional inequality ratio do not affect parents' economic concerns. The outcomes of table 4.4 provide suggestive evidence that the unemployment rate indeed generates changes of parental beliefs about the economic chances of their children.

4.5 Conclusion

It is well-established that parental investments are important for children's development and later success in life. Nonetheless, the intensity of these investments varies greatly between parents. The current literature predominantly analyses the role of parental and family background characteristics to explain differences in parenting behavior. Instead, this paper investigates the role of the external living environment to explain parental investment choices. I employ German survey data, in a regional- and time-fixed effect setting, to estimate the causal impact of variation of the regional unemployment rate on multiple investment measures.

The results show that the economic environment indeed matters for the investments choices parents make. I observe that a rise of the state unemployment rate causes an increase in measures of maternal support, academic interest and homework help. The positive effects of the unemployment rate are in line with the hypothesis laid out in the introduction, which states that worsening economic conditions can incentivize parental investments by raising the importance of human capital accumulation for becoming successful. Moreover, the findings fit well with recent theoretical and empirical papers claiming that the prevailing economic surroundings incentivize parental behavior that relates to children's human capital development (Doepke and Zilibotti, 2017; Dohmen et al., 2019). In addition, the observed heterogeneous effects provide suggestive evidence that especially parents with lower locus of control, income and having a child at a lower educational track are incentivized by the external environment. By contrast, parents who themselves have no educational qualification seem to lower investments, potentially due to increased stress.

The outcomes of this paper provide three main insights. First, the findings help explain observed differences in parenting behavior between families facing different economic circumstances. Accordingly, papers that model parental investment decisions should take the economic environment of families into account, as parents actively respond to environmental incentives. Observed differences in parental investment levels between families might therefore be valid given differences in prevailing living surroundings. Second, the heterogeneous effects based on families' background characteristics show that parents do not all respond similar to incentives set by the environment. Hence, the effect of the external environment should not be looked at in isolation, but rather in combination with the family environment. In particular it is worrisome that on average parents increase investments when the unemployment rate rises, while lower-educated parents instead diminish investments. Previous research shows that during recessions especially the human capital development of disadvantaged children is harmed, for example due to the consequences of parental unemployment and income instability within a family (see Kalil, 2013, for a review of the literature). This paper shows that – even in the absence of changes in personal circumstances – inequality between children from different socioeconomic backgrounds may increase during economic downturns due to different parental investment responses. Third, the analysis demonstrates broader insights in how childhood experiences can be formed by the state of the economy. Several studies find that experiencing recessions as a child influences economic behavior later in life (Malmendier and Nagel, 2011; Malmendier et al., 2011). The results in this paper could indicate that children who grow up in an economically deprived surrounding might also perceive higher amounts of pressure from their parents to perform well.

The paper opens up several avenues for future research. The results show that parents react to changes in the regional unemployment rate, although not to changes of the inequality ratio. These different effects raise the question what determines parents' perception of the economic environment, and which economic factors could play a role? This question is also interesting with respect to the heterogeneous responses based on parental and child background characteristics, as it indicates that economic factors not always uniformly translate into increased concerns about children's chances in life.

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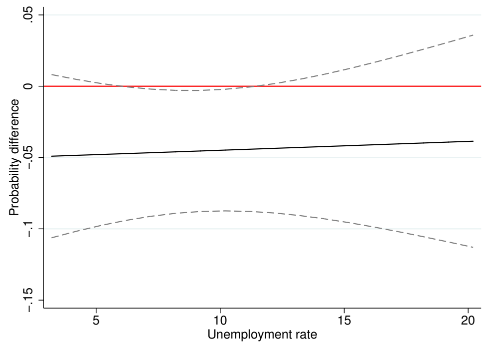
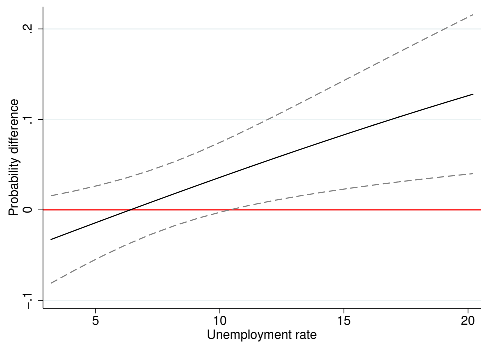
Appendix

4.A Additional Figures and Tables

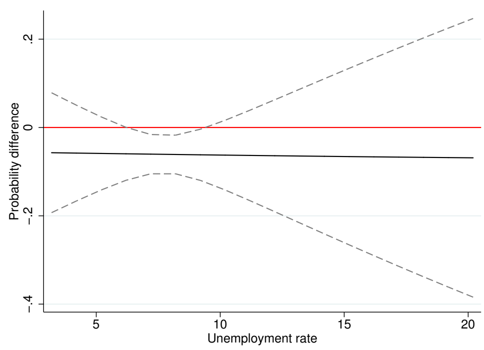
Figure 4.A1: Heterogeneous effects of regional unemployment on hiring a tutor



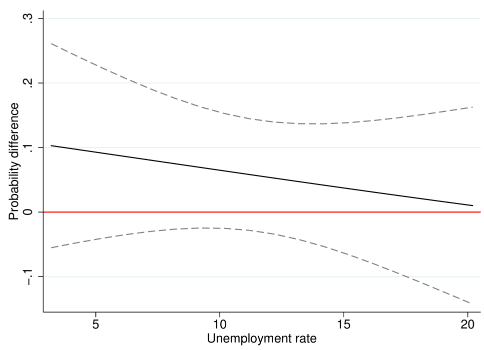
(a) By low locus of control



(b) By vocational track recommendation



(c) By low parental education



(d) By low parental income

Notes: Figure 4.A1 shows the difference in the marginal effect of a one percentage point increase in state unemployment on the probability of parents hiring a tutor by certain background characteristics. Panel A presents the difference between parents with a locus of control measure in the lowest quartile versus parents with a measure belonging to one of the three highest quartiles. Panel B shows the difference between children who received a vocational track recommendation and children receiving an academic track recommendation. Panel C displays the difference based on whether parents have less than a secondary school qualification. Finally, panel D presents the difference between households with a disposable income in the lowest quartile versus households with an income belonging to one of the three highest quartiles. The dotted gray lines represent the 90 percent confidence intervals.

Table 4.A1: Survey items of parental investment variables

Variable	Item description	Min	Max
Maternal/paternal support	Mother/father talks about things you do	1	5
	Mother/father talks about things that worry you	1	5
	Mother/father asks you prior to making decisions	1	5
	Mother/father expresses opinion on something you do	1	5
	Mother/father able to solve problems with you	1	5
	Mother/father has impression of trusting you	1	5
	Mother/father asks your opinion on family matters	1	5
	Mother/father gives reason for making decision	1	5
	Mother/father shows that she loves you	1	5
School contact	Parents take part in parents evening	0	1
	Parents consult teachers	0	1
	Parents are engaged as parent representatives	0	1
	Parents are involved as parents representative	0	1

Notes: Table 4.A1 presents details on the survey items that are employed for the parental investment measures. The items of maternal and paternal support are used in confirmatory factor analysis to create the final investment measures. For school contact I count the number of activities undertaken by parents.

Table 4.A2: The effect of regional unemployment and background characteristics on parental investments

	(1) Maternal support	(2) Paternal support	(3) Academic interest	(4) Homework help	(5) Paid tutor	(6) Contact with school
Unem. rate	0.018**	0.012	0.011**	0.015***	-0.004	-0.005
	(0.008)	(0.015)	(0.005)	(0.005)	(0.005)	(0.006)
Boy	-0.111***	0.062**	0.050***	-0.025***	0.002	0.094***
	(0.021)	(0.027)	(0.010)	(0.009)	(0.017)	(0.024)
Income vig.	-0.001	0.002	0.002**	0.002	0.005**	0.006**
	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)	(0.003)
No. of children	-0.053***	-0.021	-0.023***	-0.015**	-0.030***	-0.023
	(0.009)	(0.015)	(0.007)	(0.007)	(0.006)	(0.018)
Unemployed	-0.030	-0.096**	-0.003	-0.006	-0.070***	-0.017
	(0.028)	(0.035)	(0.019)	(0.015)	(0.020)	(0.033)
Middle voc. edu.	-0.081	0.011	0.059***	0.079***	0.062**	0.224**
	(0.077)	(0.079)	(0.018)	(0.024)	(0.027)	(0.100)
Higher voc. edu.	-0.008	0.129*	0.073***	0.087**	0.071**	0.266**
	(0.079)	(0.070)	(0.021)	(0.034)	(0.028)	(0.110)
Higher edu.	0.051	0.179**	0.054**	0.124***	0.059	0.384***
	(0.072)	(0.070)	(0.021)	(0.021)	(0.038)	(0.091)
Native parent(s)	-0.062	-0.187***	0.032	0.178***	0.017	0.304***
	(0.042)	(0.053)	(0.030)	(0.027)	(0.020)	(0.085)
Mother's age	-0.001	0.011**	-0.002**	-0.000	-0.000	0.005*
	(0.005)	(0.004)	(0.001)	(0.002)	(0.002)	(0.003)
Locus of control	0.063***	0.076***	0.019***	0.009*	0.017**	0.012
	(0.009)	(0.013)	(0.005)	(0.005)	(0.008)	(0.016)
Highest track	0.179***	0.174***	-0.089***	-0.079***	-0.044**	-0.047
	(0.032)	(0.031)	(0.009)	(0.016)	(0.020)	(0.038)
Single parent	-0.035	-0.921***	-0.082***	-0.047***	0.036	0.039
	(0.040)	(0.054)	(0.018)	(0.015)	(0.027)	(0.036)
N	5009	5009	5009	5009	5009	5009
State & Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 4.A2 shows the effect of the state unemployment rate and all control variables on measures of parental investment. Maternal and paternal support are standardized as a Z-score. Academic interest, homework help and paid tutor are binary variables. Contact with school is measured on a five-point scale. Columns 1, 2 and 6 are estimated by means of OLS, while columns 3 and 4 are estimated by logit regressions. The logit coefficients are displayed as average marginal effects. Standard errors are clustered at the federal state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.A3: Heterogeneous effects of regional unemployment on paternal support and school contact

	Paternal support				Contact school			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unem. rate	0.013	0.021	0.014	0.016	-0.002	-0.009	-0.003	-0.001
	(0.015)	(0.016)	(0.015)	(0.015)	(0.007)	(0.007)	(0.007)	(0.007)
Unem.*Low loc	0.005				-0.002			
	(0.006)				(0.006)			
Unem.*Vocational track		-0.010*				0.012**		
		(0.005)				(0.005)		
Unem.*Low educated			-0.046				-0.070***	
			(0.033)				(0.016)	
Unem.*Low income				-0.009				-0.013
				(0.016)				(0.013)
N	5009	5009	5009	5009	5009	5009	5009	5009
State & Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 4.A3 shows the interaction effects of the state unemployment rate and several background characteristics on the paternal support and school contact measure. Paternal support is standardized as a Z-score. Contact with school is measured on a five-point scale. The set of controls include: children's gender, secondary school track, household income vigintile, number of children within the household, single-parenthood, parents' unemployment status, parents' education, immigrant background, mother's age and locus of control. In addition, I included indicators of low maternal locus of control, vocational track recommendation, low parental education, and low household income. The regressions are estimated by means of OLS. Standard errors are clustered at the federal state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.A4: Robustness of the effect of regional unemployment on parental investments

	Paternal support			Paid tutor			Contact school		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Unem. rate	0.012	0.013	0.012	-0.004	-0.005	-0.003	-0.005	-0.009	-0.011
	(0.015)	(0.016)	(0.015)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.009)
N	5009	4648	5009	5009	4648	5009	5009	4648	5009
State & Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Specification	Baseline	Not moved	RoR	Baseline	Not moved	RoR	Baseline	Not moved	RoR

Notes: Table 4.A4 shows the robustness of the state unemployment rate on parental investments. Paternal support is standardized as a Z-score. Paid tutor is a binary variable. Contact with school is measured on a five-point scale. Columns 1, 4 and 7 present the baseline specification. Columns 2, 5 and 8 restrict the sample to families who did not move municipalities during the last three years. Columns 3, 6 and 9 measures the unemployment rate at the regional economic center level, and accordingly includes regional economic center fixed effects. The set of controls include: children's gender, secondary school track, household income vigintile, number of children within the household, single-parenthood, parents' unemployment status, parents' education, immigrant background, mother's age and locus of control. Columns 1 to 3 and 7 to 9 are estimated by means of OLS, while columns 4 to 6 are estimated by logit regressions. The logit coefficients are displayed as average marginal effects. Standard errors are clustered at the federal state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.A5: The effect of regional inequality on parental investments

	(1) Maternal support	(2) Paternal support	(3) Academic interest	(4) Homework help	(5) Paid tutor	(6) Contact with school
Ineq. ratio	0.007	-0.016	-0.007	-0.002	0.012	0.034
	(0.024)	(0.037)	(0.018)	(0.011)	(0.010)	(0.037)
N	5009	5009	5009	5009	5009	5009
State & Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 4.A5 shows the effect of the state inequality ratio on measures of parental investment. Maternal and paternal support are standardized as a Z-score. Academic interest, homework help and paid tutor are binary variables. Contact with school is measured on a five-point scale. The set of controls include: children's gender, secondary school track, household income vigintile, number of children within the household, single-parenthood, parents' unemployment status, parents' education, immigrant background, mother's age and locus of control. Columns 1, 2 and 6 are estimated by means of OLS, while columns 3 and 4 are estimated by logit regressions. The logit coefficients are displayed as average marginal effects. Standard errors are clustered at the federal state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.