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Preface

Natalie Obergruber prepared this study while she was working at the ifo Center for the Economics of Education. The study was completed in March 2018 and accepted as doctoral thesis by the Department of Economics at the University of Munich. It consists of four distinct empirical analyses – two on the determinants of education and two on the determinants of occupational choice. For both choices, individual and institutional determinants are investigated. The econometric analyses are based on panel data from the National Educational Panel Study (NEPS), historical census data of the Statistical Office of the German Empire, historical maps and studies from Sering 1897, Verein für Socialpolitik 1883, Grossherzogliches Ministerium des Inneren 1883, Miaskowski 1882-1884, Fick 1895, and Krafft 1930 which are combined with modern data from the German Statistical Office and the BBSR. This study analyses which individual and institutional factors (causally) influence individuals in their educational career and in their choice for an occupation. Chapter 2 explores consequences of parental separation for cognitive skill development of children. In the year before parental separation occurs children are negatively affected in their cognitive skill development. Chapter 3 investigates the consequences of an institutional reform in the German school system which awards high track school drop outs with lower track school degrees if they accomplished 9th grade. After the reform students are less likely to switch between schools and tracks and surprisingly are more likely to successfully finish the high-track school and enter university. Chapter 4 predicts the choice of math-intensive occupations by school grades. School grades are affected by students' ability and tastes and may furthermore contain pure signals of achievement (based on rank in class). We find that the strong association between grades and math intensity of occupations is completely explained by individuals' differences in tastes. Chapter 5 shows that occupational choice is influenced by the distribution of land, a store of wealth in an agricultural society. We find that areas with more equally distributed land started to industrialize earlier particularly in innovative manufacturing.

Keywords: difference-in-differences, economic inequality, industrialization, panel data, NEPS

JEL-Codes: I21, I24, I28, J12, J13, J24, N13, N33, O33, O47

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1 Introduction

Since World War II, average education levels have increased hand in hand with returns to education see e.g., Autor (2014); Jones and Romer (2010); Autor, Katz, and Krueger (1998). The growth in demand for skilled workers therefore necessarily exceeded the growth in supply. Tinbergen (1974, 1975) was the first to link the excess demand for skilled workers to recent technological change. The process of technological change offers many opportunities for individuals but also poses a challenge to policymakers who should ensure that people are well-prepared to take-up these opportunities.

The main characteristic of the ongoing technological change are improvements in productivity and decline in real prices of information and communication technologies (Acemoglu and Autor, 2011). New technologies substitute labor-intensive, well-defined, and codifiable tasks - routine tasks - by capital which are mainly performed by medium skilled workers. Complementary, non-routine tasks are largely performed by high skilled workers. However, manual tasks are partly non-routine as well when they ask for situational adaptability, visual and language recognition, and face-to-face interactions. These tasks are for example concentrated in the service sector and can be performed by low skilled workers (Acemoglu and Autor, 2011). The 'routinization' hypothesis predicts the substitution of labor in routine tasks by technology and therefore an erosion of medium skilled occupations in favor of high and low skilled occupations. This is in line with employment data (Acemoglu and Autor, 2011). Over the past two decades employment shares in high skill/high wage and low skill/low wage occupations increased while medium skill/medium wage occupations lost on average 8 percent of their employment share in the US and the EU until 2010.[1] As real wage levels in high skilled occupations are far higher than in low skilled occupations, the observed job polarization increases inequality in real wages (Acemoglu and Autor, 2011). Although the exact consequences of income inequality are still unclear, there is some evidence that it harms the institutional quality of democracies (Kotschy and Sunde, 2017; Jung and Sunde, 2014).

The described consequences of technological change challenge policymakers in several perspectives: how can the increased demand for high skilled workers be supplied? How should the education system particularly in the area which trains people for medium skilled occupations deal with the worse future employment perspectives in these occupations? How can real wage

[1] Job polarization and increasing real wage inequality are not US specific but hold for most industrialized countries (Goos and Manning, 2009; Atkinson, 2008; Katz and Autor, 1999).

inequality be ameliorated? Tackling these issues will particularly ask for reforms and interventions in the education system. The aims would include increases in average skill levels, more flexibility in the vocational education system, and incentives to chose innovative occupations in sustainable branches. Tailoring such educational policies require a solid understanding of individual and institutional determinants of education and occupational choice.

This thesis provides insights into these determinants and consists of two parts: The first investigates determinants of education outcomes and the second determinants of occupational choice - both once from an individual and once from an institutional perspective. The first part deals on the individual level with students who are disadvantaged due to changes in their family background. On the institutional level, the first part investigates the consequences of a reform which reduced the risk of dropping out without any school degree. The second part of this thesis deals with the influence of individual characteristics on the decision for specializing in a (medium skilled) occupation. Then, it investigates how an institution which influences wealth distribution historically changed occupational choice and in the long-run improved adaption of people to technological change. This chapter proceeds as follows: Section 1.1 discusses the literature on determinants of education with a focus on family background and institutions. Section 1.2 gives an overview of the literature on determinants of occupational choice. Section 1.3 introduces the chapters of this thesis in more detail and section 1.4 discusses the conclusions and recommendations which can be drawn from this thesis.

1.1 Determinants of education

In line with a typical economics production function Hanushek (1970, 1979) conceptualized the process of education in the *education production function*. The idea is that different inputs determine the level of education an individual achieves - measured in cognitive skills. Recent formalizations distinguish four separate determinants (Hanushek and Woessmann, 2011):

$$Y_i = f(F_i, R_i, I_i, A_i). \tag{1.1}$$

The outcome of the educational production process Y_i of individual i is a function of individual characteristics and family background (F_i), school resources (R_i), institutional features of the school system (I_i), and individual ability A_i. The literature investigates the influence of the four defined determinants separately. Individuals' ability A_i and family background F_i are often taken as given and rigorously controlled for. However, early-childhood interventions, mentoring pro-grams, or financial support of disadvantaged families are policies to reduce negative influences of family background e.g., Cunha, Heckman, Lochner, and Masterov (2006); Kosse, Deckers, Schildberg-Hörisch, and Falk (2016). The majority of the literature deals with the influence of school resources R_i and institutional features I_i because these determinants are particularly relevant for policy recommendations. From a policy perspective changes in school resources, often interpreted as spending per student, are more easily introduced compared to changes in institutional features of the school system. Concretely, more spending per student can lead to more or better teachers per student and to more or better equipment like books, computers, school excursions, etc. While many studies show that quantitative increases in spending like decreases in class size hardly increase educational outcomes of children (Hanushek and Woessmann, 2011), Jackson, Johnson, and Persico (2016) show long-run positive effects of increased spending. Overall, the effects might depend on how the increased spending is used. If spending increases the quality of school inputs e.g. the quality of teachers, there is strong evidence that this will have strong and long-run positive effects on student outcomes (Chetty, Friedman, and Rockoff, 2014a, 2014b). Institutions are so far mainly investigated in an international perspective as school systems mainly vary on a national level. Internationally comparable tests like PISA (Programme for International Student Assessment) and TIMSS (Trends in International Mathematics and Science Study) offer the opportunity to compare students from different school systems within the same testing and survey framework. Woessmann (2016) provides and overview of these analyses. Conclusions from the findings, however, rely on the assumption that the introduction of the same institutional feature has the same effect in each country regardless of other national institutional features and cultural factors - the issues all cross-country studies face (Hanushek and Woessmann, 2011). The remaining section discusses family background and institutional features as determinants of education in detail.

1.1.1 Family background

Family background explains about 50 percent of the variation in years of schooling (Bjoerklund and Salvanes, 2011). In estimating education production functions like equation 1.1 proxy variables for family background are highly statistically and economically significant (Hanushek and Woessmann, 2011). A number of different family characteristics approximate family background: number of books at home, parents' job classification, parents' working status, parents' education level, household income, and the relationship status of the parents see e.g., Kalil, Mogstad, Rege, and Votruba (2016); Hanushek and Woessmann (2011); Schuetz, Ursprung, and Woessmann (2008); Woessmann (2008). As family background is also related to other inputs - particularly school resources - it is a source of potential bias in identifying any effect on education production. The relation comes from parents selecting their children into specific schools or schools selecting children by their family background. At least controlling for family back-ground is therefore crucial to any study estimating an education production function.

However, considering equal opportunities of students, the influence of family background on education production is also a research field on its own. The aim is to ameliorate negative family influences. As a first step in that direction it is necessary to determine how much of the observed influence is driven by nurture i.e. arises by social contact as this is the potential reach of interventions. Bjoerklund, Lindahl, and Plug (2006) show for adopted children in Sweden that the effect of biological parents and adoptive parents on the transmission of education and income has almost the same magnitude and adds up to the parents' effect on transmission for children born and raised by their biological parents. These results suggest that about 50 percent of the family background effect can be targeted by interventions. Experimental early-childhood interventions show very positive effects on school careers of children from disadvantaged backgrounds see for an overview Cunha et al. (2006). The effects are even positive and significant for the very long-run as Heckman, Moon, Pinto, Savelyev, and Yavitz (2010) show for 40-year old participants of the Perry Preschool experiment. Chapter 2 adds to the literature which aims to understand the mechanism how family background influences education production by investigating the timing, causality, and potential mechanisms of the effect of parental separation on cognitive skill development of children.

1.1.2 Institutions

Institutions which summarize all structural characteristics of the school system provide incentives for education and shape individual behavior (Hanushek and Woessmann, 2011). The challenge for identification of institutional effects is that institutions mainly vary on the national level. International achievement tests like PISA and TIMSS provide the opportunity to evaluate in repeated cross-sections the influence of specific institutional features on educational outcomes of children. These international comparisons allow to identify long-term general equilibrium effects instead of short-term effects of recent reforms. The disadvantage of international data are that they are repeated cross-sections. It is not possible to follow students over their school career and into the labor market. Therefore, identifying the incentives provided by a school system for educational choices is harder. For such analyses panel data which follows students over time would allow more detailed analyses. Such data are for administrative reasons rather available on the national level. Population data from Scandinavian countries for example follow cohorts throughout their school and labor market career. In these settings causal short-run effects of reforms on treated cohorts can be identified. Good examples for such identification strategies are (Meghir and Palme, 2005; Pekkarinen, Uusitalo, and Kerr, 2009) who study the introduction of tracking reforms in Norway and Finland and exploit the different timing of the reforms in different parts of the country. They find that equality of opportunities is increased if tracking is reduced. In chapter 3 we apply a similar approach exploiting a reform in Germany as natural experiment which was introduced in the 11 West-German states at different points in time from 1965 on with retrospective spell data on school and labor market careers.

1.2 Determinants of occupational choice

Theoretically, occupational choice is seen as the last step in a sequence of binary decisions on education — first general education and later occupation-specific or specialized education (Altonji, Arcidiacono, and Maurel, 2016; Altonji, Blom, and Meghir, 2012). These decisions are taken under uncertainty about one's own ability and about the returns to ability in the different occupations. Individuals choose the occupation which maximizes their utility based on believed pecuniary and non-pecuniary returns on believed ability. The recent empirical literature on college major choice tries to identify the influence of belief updating and expected future income on college major choice using survey experiments see e.g. Wiswall and Zafar (2015); Stinebrickner and Stinebrickner (2014). One main finding is that residual factors which are non-pecuniary play a larger role for college major choice than pecuniary factors. There is also a literature documenting correlations of occupations of children and parents (Hellerstein and Morrill, 2011; Constant and Zimmermann, 2004) and clustering of specific non-cognitive skills within occupations (John and Thompsen, 2014).[2] In chapter 4 we investigate in a rich panel data set how ability and ability signals influence occupational choice of teenagers in Germany in comparison to parental occupations, and direct measures for individual tastes.

The effects of institutions on occupational choice are difficult to study as international comparable data is hardly available for the transition of teenagers and young adults into the labor market. In the literature on college major choice (Kirkeboen, Leuven, and Mogstad, 2016) show that there are significant income jumps at the cut-off of acceptance for college majors. This suggests that institutions which restrict who may study which subject probably have a large impact on occupational choice. Similar institutional interventions into the choice of field of study in Germany and Austria are for example the limitations to the study field of medicine. In light of the ongoing technological change institutions might become even more important for low and medium skilled people. Hampf and Woessmann (2017) provide evidence that institutions with a very early specialization into one occupation turn out to be more rigid in the long-run as it is harder for people to switch occupations. Chapter 5 adds to this literature investigating the effects of institutions on occupational choice during a past era of technological change. An institution which distributed land — the key store of wealth in an agricultural society — more evenly during industrialization encouraged people to seek employment in innovative occupations.

[2] In the view of (Altonji et al., 2016) non-cognitive skills are part of *ability*.

1.3 Outline of the thesis

This thesis is a collection of four self-contained empirical studies in the field of economics of education and its overlap with labor economics. Chapters 2 and 3 investigate the influence of family background on cognitive skill development and of institutions on school careers. Chapter 4 determines the important factors for occupational choice of German teenagers today. Chapter 5 shows how equal distribution of wealth via occupational choice may influence long-run development positively.

Chapter 2 explores consequences of parental separation for cognitive skill development of children. In general, growing up with a single parent is related to lower educational achievement. Identification of a causal effect is, however, challenging as growing up with a single parent is correlated with other disadvantageous aspects of family background and separation is an endogenous decision of parents. We use panel data from the National Educational Panel Study (NEPS) on children in 5th grade. These data provide annual reports on living with a single parent at home and bi-annual data on cognitive skills. While using a a value-added setting conditions out fixed characteristics of family background, the issue of the endogenous separation decision is not solved. The panel structure of the data allows to investigate the timing of the onset of a negative effect of separation on cognitive skills. Particularly, in the year before parental separation occurs children are negatively affected in their cognitive skill development. This suggests that parental conflicts at home or the threat of separation is more disadvantageous than the event itself. After separation cognitive skill development is not significantly negatively affected anymore. In terms of mechanisms the degree parents help with homework is the only family input with a similar pattern in the timing as the cognitive skill development.

Chapter 3 investigates the consequences of an institutional reform in the German school system which awards high track school drop outs with lower track school degrees if they accomplished 9th grade. Drop-outs who lack a school degree face difficulties in the labor market as a school degree is an important signal for any future employer. The reform under study focuses on high-track school drop-outs who fail to successfully complete 13 years of schooling. In general, in most data sets it is not possible to track students who did not finish the high track school in Germany in a representative sample of adults and explore their past school careers. We exploit detailed retrospective spell data on school and labor market careers from the adult cohort of the NEPS from 2010. As the reform was introduced at different points in time between 1965 and 1982 in the 11 West-German states, we apply a difference-in-differences approach for identification. Our results show that after the reform students are less likely to switch between schools and tracks. Surprisingly, we find that the reform led to an increase in the number of high-track students who successfully finish the high-track school and enter university. The reform reduced the perceived risk of trying longer in the high-track school and thus led to an increase in the high-track completion rate.

Chapter 4 predicts the choice of math-intensive occupations by school grades. School grades are an interesting determinant of occupational choice to investigate because grades are affected by students' ability and tastes and may furthermore contain pure signals of achievement (based on rank in class). So far, the literature on occupational choice focuses on college students choosing their college major which is for academics the first moment when education becomes specialized. The results of the existing literature show that non-pecuniary aspects of occupations have a large influence on occupational choice. These aspects are mostly residuals and interpreted as individual tastes. We use NEPS data, which follows 9th-grade students in Germany into the labor market. These are students who generally will not go into higher education and who are much more affected by the recent technological change. For them occupational choice is a crucial moment for future employment and life-time earnings. The rich data allow us to link students' math and reading achievement, their teacher-assigned school grades, and tastes for specific activities to their choice of apprenticeship. To measure the math intensity of occupations, we use the adult cohort of NEPS to compute average math skills of workers in each occupation and the Qualification and Career Survey (QaC) to compute the math skill requirements per occupation. We find that better math grades are robustly associated with the math intensity of chosen occupations. This relationship is, however, not explained by better math ability or by parents' occupation. Furthermore, we exploit the fact that teachers tend to grade on a curve and instrument a student's school grades by her classmates' average achievement. This allows us to estimate the causal effect of a pure ability signal on occupational choice, for which we find no evidence. Using extensive information on individuals' interests in various domains, we find that the strong association between grades and math intensity of occupations is completely explained by individuals' differences in tastes. This is in line with existing studies, which find that non-pecuniary residuals are often the main driver of occupational choice and are interpreted as differences in tastes (without observing them directly).

Chapter 5 shows that occupational choice is influenced by the distribution of land, a store of wealth in an agricultural society. The old and remaining question in economics on the consequences of inequality is hard to answer as suitable data and natural experiments in inequality are scarce. In this chapter we exploit spatial variation in an exogenous institution - agricultural inheritance rules. Within Germany, inheritance rules for land differed sharply across locations with some areas featuring rules prescribing an equal division of land among children while others featured rules prescribing land to be indivisible. Leveraging a geographic regression discontinuity design at the boundary between equal and unequal division areas, we document that equal division areas started to industrialize earlier particularly in innovative industries which show a higher patenting activity. Other potential drivers of economic growth are smooth at the boundary and using various data sets we can show that the head start in economic development is only visible from the onset of industrialization on. Today, historical equal division areas still draw on the advantage and show substantially higher employment in the service sector and the share of university educated people is higher compared to unequal division areas.

The next section discusses the results of the four chapters briefly and draws conclusions for policy interventions tackling the challenges of technological change.

1.4 What do we learn?

The current technological change characterized by increasing speed and lowering prices of standardized computations puts education policy in front of new challenges. This thesis provides a starting point for such policies as it provides insights into the individual and institutional determinants of education and occupational choice.

The findings of chapter 2 suggest that parental separation is a crisis to cognitive skill development of children which has the largest impact in the year before separation occurs. After separation cognitive skill development is hardly affected but level differences might persist. Interventions might prevent that children are missing too much of the material covered in school and put on a lower schooling trajectory from the crisis on. Such individual support potentially increases average skill levels of children. Simultaneously to such supportive individual interventions the institutional framework influences school careers. Risk-reducing, low-cost, institutional reforms as analyzed in chapter 3 can incentivize to stick to the investment into higher education. If students are encouraged to invest into university education average skill levels in the cohort are increased as well.

The overview on determinants of occupational choice of German teenagers in chapter 4 suggests that a major driver of occupational choice is individual taste. Tastes, however, do not seem to be pre-determined. There is scope for school interventions to encourage students individually to take-up innovative or sustainable occupations. Last but not least, chapter 5 suggests that institutions play a role for occupational choice particularly in times of technological change. One role of institutions is the distribution of prerequisites which incentivize individuals to take-up occupations in new, rising branches.

Overall, there are many opportunities for education policy to increase average skill levels, and to make individuals more flexible in their occupational choice. Introducing reforms and interventions in these directions might improve the ongoing transition process from an industrialized economy to a digitized one.

2 Separating Parents and the Timing of Consequences for Children's Cognitive Skill Development

2.1 Introduction

Family background explains about half of the variation in education (Bjoerklund and Salvanes, 2011). Growing up with separated parents is one aspect of family background which is studied by a large interdisciplinary literature since the 1960s.[1] Children growing up with separated parents obtain less education, participate more in deviant behavior like smoking, and have lower health outcomes compared to children growing up with two parents at home (Tartari, 2015; Francesconi, Jenkins, and Siedler, 2010a, 2010b; Wuertz Rasmussen, 2009; Ermisch, Francesconi, and Pevalin, 2004; Ermisch and Francesconi, 2001). In adulthood children who grew up with separated parents have lower employment rates, lower earnings, and their relationships are more often unstable (Gruber, 2004; Corak, 2001). While these correlations are strong, it is unclear if these differences arise due to separation or independently of parental relationship status. Studies trying to identify a causal effect of parental separation, so far, suggest very small or even zero effects (Bjoerklund, Ginther, and Sundstroem, 2007; Sanz-de Galdeano and Vuri, 2007; Bjoerklund and Sundstroem, 2006; Gruber, 2004; Corak, 2001).

This chapter sheds light on the timing and mechanism how disadvantages in educational achievements of children who experience a separation of their parents emerge. In rich panel data from the German National Educational Panel Study (NEPS)[2] 10-year-old children from Germany are tracked from 2010 to 2014. The data provide cognitive skill measures in form of standardized reading test scores in 2010 (first year of observation) and 2012 and reports on family type, and household inputs into education production on an annual basis. The effect of separation is estimated in a value-added setting which controls for test scores of the previous testing wave linearly. This value-added approach eliminates all unobserved factors which influence test scores in 2010 and 2012 simultaneously. The estimated pattern suggests that separation affects cognitive skill development of children negatively and strongest in the year before separation occurs. After parents separated a slight catching-up of skill development occurs. This pattern of cognitive skill development over time is different from patterns of development for household income and proxy variables for other family inputs: the number of books at home and household size. However, we deliver evidence that parental time investments

1 See for example Gruber (2004) and Haveman and Wolfe (1995) for an overview of the economics and sociology literature on consequences of growing up with separated parents.

2 For more information about the study see Blossfeld, Roßbach, and von Maurice (2011).

play a role measured in the degree of parental help with homework. As cognitive skill development, the degree of parental help with homework is already reduced before a separation occurs.

Most of the studies which find negative influences of separated parents on children estimate correlations with cross-sectional data. Studies using longitudinal data applying difference-in-differences approaches or family-fixed effects do not find effects of parental separation on educational outcomes of children.Sanz-de Galdeano and Vuri (2007) apply a difference-in-differences approach and compare in two periods the educational achievement of UK school children who live with two parents with children who live with a single parent. Bjoerklund and Sundstroem (2006) apply family-fixed effects and treat one sibling who leaves home before parents separate as a counter-factual for the other sibling who is still at home. Both approaches fail to identify a causal effect of separation. However, if children are affected by separation before it occurs neither the pre-separation outcome measure nor the sibling leaving home is unaffected by future separation see for discussion of pre-separation effects Corak (2001); Piketty (2003).[3] In longitudinal settings, lead and lag effects of separation might introduce misspecification. This chapter sheds light on the issue of lead and lag effects of parental separation on educational outcomes of the child and gives first insights from when on and for how long children are affected in cognitive skill development if their parents separate.

In Germany about 17 percent of children under the age of 18 live in single-parent households (Bundesministerium für Familie, Senioren, Frauen und Jugend, 2014). Although Germany has a well established welfare state that supports low-income and single-parent households educational achievement depends strongly on family background. The report on PISA 2012 (OECD, 2013) ranked Germany above the OECD average for the influence of family background on educational achievement. Woessmann (2015) compares the disadvantage in PISA test scores of single-parent children internationally and finds that also in Germany a disadvantage exists although it is smaller than in other OECD countries. Earlier studies on Germany find that children living with a single parent are more likely to drop out of school (Bohrhardt, 2000), less likely to attend the highest secondary school track (Mahler and Winkelmann, 2004), and less likely to obtain university-entrance qualification (Francesconi et al., 2010a). Policy measures which support children of separated parents might potentially reduce inequality in the German school system.

To tailor policy interventions specifically to the needs of children who experience a separation of their parents it is necessary to understand the mechanism how a separation affects cognitive

[3] There is also a literature in health economics documenting that divorces or deaths of spouses affect adults before the event occurs if they are pre-announced (Laporte and Windmeijer, 2005; Siflinger, 2016).

skill development. Theoretically, the literature on intergenerational transmission of education[4] suggests that household resources, parental education, preferences, and skills are input factors into the educational achievement of children. A separation affects many of these family inputs. A policy intervention should focus on the input which is most influential or most affected by a separation. In line with the literature on the importance of parental time investments for educational achievement (Avvisati, Gurgand, and Maurin, 2014; Bergman, 2014; Banerji, Berry, and Shotland, 2013; Kraft and Dougherty, 2013), this chapter suggests that the timing of reduction in the degree of parental help with homework coincides most with the timing of the reduction in cognitive skill development.[5]

The rest of the chapter proceeds as follows: section 2 presents the data. Section 3 discusses the empirical strategy to identify a lead and a recent separation effect and heterogeneous separation effects. Section 4 presents the results on the lead effect of separation and section 5 presents results on heterogeneity. Section 6 shows robustness checks. Section 7 concludes.

2.2 Data

The NEPS starting cohort 3 is a representative panel on German children which starts in 2010 when children are in 5th grade in regular schools.[6]. Our sample consists of 3287 children from this cohort. The data feature repeated subject specific tests in math, language competencies, and science administered in the class environment. These tests allow to track cognitive skill development of children over time. Additionally, the data include detailed information on family background and school environment reported repeatedly from parents, teachers, and principals.

2.2.1 NEPS students within the German school system

In 2010, the first observation period, most children in the sample are in the first year of secondary school in a new school. In the German school system children are usually tracked at age 10 after four years of comprehensive primary school.[7] There are 6 school types distinguished in NEPS. These 6 school types more or less represent the three tracks in the German school system from 5th grade on: lower, middle, and high track. While lower track provides grades 5

4 see e.g.Bjoerklund and Salvanes (2011) or Black and Devereux (2011)

5 Although a reduction in parental help with homework is in general — estimated for children affected and unaffected of a separation of their parents — not a significant driver of cognitive skill development.

6 The sampling procedure has two steps. First, it randomly selects the schools and second, within each of these schools it samples two classes in 5th grade. If the school has only one class in 5th grade, then one class is sampled

7 Exceptions are Berlin and Brandenburg where children are tracked after 6 years of primary school.

to 9[8], middle track provides grades 5 to 10. Both school tracks primarily prepare for vocational training and are less academically demanding. The high track provides grades 5 to 13[9] and awards students who complete the track with a university entrance qualification. The used NEPS data tracks students from 5th grade to 9th grade in all school tracks. During that period no compulsory switching of schools occurs.[10]

2.2.2 Data structure

One challenge of the analysis is that the time-distance between the reports of information varies by variable. For example, while children are asked annually with whom they live in a household, they are tested every second year. The time structure of the used variables is therefore a crucial characteristic of the data. Figure 2.1 gives an overview of the tests and reports on separation. In general, child questionnaires and parent interviews are conducted in 2010, 2011, 2012, 2013, and 2014 (fall)/2015 (spring). Separation can occur either in S_0, S_1, S_2, or S_3. The **T** in 2010, 2012, and 2014 indicates when children are tested in reading and math.[11] NEPS-trained interviewers conduct the written tests within the classrooms of the children.[12] In the analysis we focus on two time periods for testing: 2010 and 2012.[13] For consistency we use the observation of 2010 and 2012 for all outcome variables even if they would be available on a more regular basis. To capture the timing of the separation effect, however, we exploit that we know if a separation occurred on an annual basis.

2.2.3 Definition of separation

The treatment variable indicates a separation i.e. when children who lived with two parents start to live with a single parent. This indicator uses the information from parents and children on the annually reported family type.[14] Family type is a dummy which equals 1 if the child lives

8 There is an optional 10th grade available.

9 If there are 12 or 13 grades necessary to obtain university entrance qualification depends on the state.

10 Children can voluntarily switch schools. In that case NEPS individually tracks these students and interviews and tests them at home. However, for those children test score data in 2012 and 2015 is not available.

11 Although children are tested in reading and math in 2014, in the used data publication only reading test scores of 2014 are available.

12 In 2010, besides reading and math, cognitive basic skills and reading speed is tested. In 2011 scientific literacy, listening comprehension of vocabulary, and ICT literacy is tested. In 2012, besides reading and math, orthography, and Russian or Turkish for children with Russian or Turkish as mother tongue is tested. In 2014/15 tests are performed on: cognitive basic skills, reading speed, listening comprehension, scientific literacy, Russian and Turkish, ICT literacy, and orthography.

13 In 2014 a quarter of our sample does not participate in the testing and first analysis suggest that participants are positively selected.

14 For 2012 there is only information from parents available.

with a single parent and 0 if she lives with two parents.[15] Table A-1 in the appendix lists the questions which are used to categorize a family type. If information from parents and children is available, the information of parents is favored.[16] A child lives with a single parent in year t if:

1. the parent does not report to live with a partner in year t, or
2. the information if the parent is living with a partner is missing but the parent reports that there is only one person older than 15 living in the household, or
3. the parent reports in year $t+1$ that there was no partner present in year t, or
4. the child reports to live only with one biological parent and neither with the other one nor with any other partner of the biological parent.

If the information of 1) is available, the algorithm for defining if the child lives with a single parent stops there. If the information is missing the algorithm proceeds. If no information for any of the four conditions is available, family type is set to missing for year t. Missing values in t are filled if the report on family type is available for $t-1$ and $t+1$ and the family type in $t-1$ equals the one in $t+1$. If the child reports living with two parents in $t-1$ and as living with a single parent after $t+1$, it is assumed that parents separated between t and $t+1$.[17] In the final sample 169 children experience a separation between 2010 and 2014.

2.2.4 Summary statistics

Table 2.1 shows summary statistics for our sample of 3287 children. About 5 percent of children in our sample experience a separation of their parents between 2010 and 2014. For our main analysis we distinguish four groups among those children: those who experience a separation until 2011, until 2012, until 2013, and until 2014 (always compared to the last observation one year earlier). About 8 percent of our sample live with a single parent in 2010. These children are in the control group as they live in a stable family environment from 2010 to 2014.

[15] Two parents include two biological parents or a patchwork family. There are 19 children who live in a foster family which are dropped due to the low number.

[16] For 46.84 percent of children reports of parents and children are available. In 91.91 percent the information on family type is the same. This is mainly driven by the children living with two parents. When children report to live with a single parent, 81.71 percent of their parents also report to be a single parent. If the parents report to be a single parent only 15.95 percent of their children also report to live with a single parent. Regulations in which the child lives for some days with one parent and for some days with the other might drive these findings.

[17] Separation is always assumed to have happened in the year before the first report of living with a single parent.

Cognitive skills are tested in reading and math in 2010 and 2012. They are standardized to mean 0 and standard deviation 1 in 2010 and in 2012 standardized to the 2010 mean and 2010 standard deviation. The positive mean for reading and math in 2012 reflect that students improve in their cognitive skills over time. Figure 2.2 shows for our control group and the four separation cohorts means and differences in reading test scores for 2010 (left graph) and for 2012 (right graph). In 2010 test scores for the five groups do not differ significantly (on the 5 percent level), however, the mean is clearly more negative for children who will experience a separation until 2011 and until 2012. For children who will experience a separation until 2013 or until 2014 the mean is close to zero which is the mean of the control group. In 2012, test scores do not differ significantly (on the 5 percent level) either but again it is visible that the mean is lower for children who have experienced a separation within the past two years and those who will experience a separation in the next year (2013).

The *degree help with homework* is a five-scale variable in which children report in 2010 and 2012 how much their parents help them with their homework. It is one of the few available panel variables (so far) in the NEPS data which captures parental time involvement in their children's educational achievements. The value of 5 means that parents help their children very often with their homework, the value of 1 means that they do not help them at all. The mean reduces slightly from 2010 to 2012 which might capture the fact that children become more independent in studying between age 10 and age 14.

The individual characteristics and the number of books at home are our control variables. We use an indicator for being female and having migration background (the child or at least one parent is born abroad). The log household income per household member is calculated from two variables – one summarizes reports from parents on the household income in 2010 (either in form of an open question or of categories), the other one states the number of household members. As the household income contains many missing values we impute household income with variables on the number of books at home, the school track the child attends, migration background, the family type, highest parental education, home possessions, and household size. An indicator captures for 35 percent of children in our sample that we imputed their household income. There are 6 categories for the number of books at home which we include as dummies into our analysis. Additionally, we control for year of birth with a majority of 94.28 percent born in 1999 and 2000.

2.3 Empirical strategy

To estimate when a separation affects cognitive skill development most, we apply a value-added approach with the following equation:

$$T_{i,2012} = \beta_0 + \sum_{k=2011}^{2014} \beta_{1,k} separation_{i,k} + \beta_2 T_{i,2010} + X_{i,2010}\beta_3 + \epsilon_{i,2012}. \tag{2.1}$$

The outcome variable $T_{i,2012}$ is the level of cognitive skills of child i in 2012 measured in test scores. The coefficients of interest are $\beta_{1,k}$ which captures the coefficients of skill development for the cohort of children who experience a separation between 2010 and 2011, between 2011 and 2012, between 2012 and 2013, and between 2013 and 2014, separately. The value-added approach is visible through the variable $T_{i,2010}$ on the right hand side. $T_{i,2010}$ controls for the test score level of child i in 2010. The matrix X_i includes a dummy for being female, having migration background, dummies for the year of birth, dummies for the number of books at home, and the imputed income of households (including an indicator for household income being imputed). These variables are measured in 2010 to exclude that they are already influenced by separation. $\epsilon_{i,2012}$ is the error term. Standard errors are clustered on school level.

The value-added approach is similar to a first-difference approach with the feature that the coefficient of the past value is not restricted to being 1.[18] $T_{i,2010}$ captures all observed and unobserved influences into cognitive skills which influence the test scores in 2010 as well as in 2012. Therefore, all unobserved factors which do not change in their influence on test scores in 2010 and 2012 do not bias our results. Sources of potential bias might only come from unobserved changes in education production inputs. Changes in family inputs – like a drop of income, household size, etc. – are assumed to be driven by separation only. If we controlled for levels in these variables in 2012 or for changes, we would introduce a bad control problem. Section 2.5 shows that a separation shocks these inputs suggesting that they should not be included as control variables.

Unobserved inputs which might change from 2010 to 2012 are school inputs. This is problematic if children who experience a separation are not randomly distributed to schools. In the robustness checks, we estimate equation 2.1 with school-fixed effects. In that setting we compare only children within the same school who would be prone to the same changes of school inputs between 2010 and 2012.

[18] Estimated coefficients of $T_{i,2010}$ range from about 0.5 to 0.7 and are highly statistically significant. The assumption that the coefficient is not 1 is therefore plausible.

Classical analysis of separation effects using longitudinal data assumes that separation affects children from the moment it happens on. However, separation is not exogenous but pre-announced by parental conflicts. Separation might be anticipated by children (Piketty, 2003; Corak, 2001) and then it is unclear for any longitudinal study which pre-separation measure of the outcome variable is unaffected by future separation.[19] Identifying causal effects of separation in a longitudinal setting is not possible if it is unclear when the treatment starts. This might explain why difference-in-difference approaches so far failed to identify an effect of separation. We do not claim that our data contain an unaffected measure of cognitive skills. However, distinguishing four different pints in time when separation happens with respect to the moment when cognitive skills are measured, delivers at least descriptive insights into the evolution of a separation effect. For interpretation of the evolution of the separation effect, we assume that children who experience a separation in 2011, 2012, 2013, or 2014 do not differ significantly in unobserved factors. In the robustness checks we provide some descriptive evidence that this is the case.

2.4 Separation and cognitive skills

In this section we present our main findings on the effect of parental separation on cognitive skill levels and cognitive skill development of children. We document in our data that children who live with a single parent achieve lower test scores than children who live with two parents. These are the gaps we want to explain by investigating what happens to cognitive skill development in the years before and after separation.

2.4.1 Level differences in cognitive skills

The first step of our analysis is to show that level differences in cognitive skills exist between children who live with a single parent and children who live with two parents exist. Table 2.2 shows that children who live with a single parent in 2010 achieve significantly lower reading test scores in 2010 (column 1) and 2012 (column 2) compared to children who live with two parents. This is robust to including control variables (column 3).[20] However, applying a first-difference approach as in column 4 or a value-added approach as in column 5 reveals that children who live with a single parent do not differ significantly in their cognitive skill *development*. Their gains in reading skills are comparable to children who live with two parents. This suggests that

19 Laporte and Windmeijer (2005) and Siflinger (2016) show that for adults lead and lag effects of divorce and death of a spouse should be taken into account.

20 These gaps vanish if school-track or school-fixed effects are included. For the German system which tracks after 4th grade there might be issues of selecting children with separated parents to low or middle track schools.

at some point in the unobserved past the gaps in cognitive skills emerged for children who live with a single parent in 2010 and although they improve their skills they are unable to close the gap over time.

2.4.2 Differences in cognitive skill development

As we observe large level differences between children who live with a single parent and children who live with two parents, a question naturally arising is, if the gap emerges in the moment of separation. For children who experience a separation of their parents between 2010 and 2014 we can investigate this question. Table 2.3 uses reading test scores of 2012 as outcome variable. It shows that a separation which lies one or two years in the past does not significantly reduce test score gains of these children (column 1). However, the small sample size of children who experience a separation inhibits that an effect size of 10 percent of a standard deviation can be identified.[21] Taking future separations into account (column 2) increases the magnitude of the coefficient and it becomes significant at the 10 percent level. In column 3 we distinguish all four separation cohorts estimating an effect separately for children who experience a separation in 2011, 2012, 2013, and 2014. The results show that the negative effect of a separation between 2010 and 2014 on reading test scores in 2012 is mainly driven by the cohort experiencing a separation in 2013 – during the next year. With a p-value of 0.157 this effect is slightly above conventional significance levels.

To exclude that these results are driven by changes in school inputs, we include school-fixed effects in table A-2. These patterns that future separations influence test scores more than past separations are robust. The pattern is also not specific to reading test scores. Table A-3 shows that future separations affect cognitive skill development in math also more than past separations. However, it seems that the pattern is driven by separations in 2014, two years ahead (column 3).

To explore the influence of past and future separations on cognitive skill development further, we apply an event study design following the four separation cohorts from 2010 to 2014. Figure 2.3 plots the demeaned reading test scores of children in the four separation cohorts against the year-distance to separation of their parents. The graph suggests that in the year before separation, test scores are most affected. After separation test scores improve again. We now use the additional reading test scores of 2014 and apply individual-fixed effects in a panel setting. Table A-4 column 1-3 compares the reading test score of children four and two years

[21] Power calculations show that with our sample size of 169 separations the effect size would have to be -0.196 to be significant at a 10 percent level and a power of 0.8. To find a significant effect with a magnitude of -0.1 we would need at least 500 observed separations.

before and 2 years after their parents separate with the test score they achieve in the year of separation. Column 4-6 compares reading test scores of children three years before and one or three years after their parents separate with the test score they achieve one year before separation. While columns 1-3 suggest that test scores do not significantly differ over time compared to the test scores in the year of separation, column 4 and 5 suggest that test score gains reach a clear minimum one year before parents get separated. The event study design, therefore, confirms the findings of table 2.3.

2.4.3 Grades

The test scores provided in the NEPS data are good measures for cognitive skills which make children comparable over all schools in Germany and over time. However, these measures are not reported to children, teachers, or parents. The influence of these test scores on school careers is therefore unclear. Another measure for educational achievement are grades which are awarded to the children from their teachers. Beside cognitive skills in the specific subject grades capture for example participation in class and doing one's homework. If grades are negatively affected by parental separation children could be discouraged to study, they might have to repeat a grade, or might struggle finding an apprenticeship position later on.

In table A-5 we use grades in German and math in 2012 as outcome variables. For grades the patterns are different compared to test scores. Overall, grades and particularly grades in German (column 1-3) are negatively affected by separations. However, past separations of 2011 and 2012 have a stronger negative influence on the grades of 2012 than future separations. One drawback of grades as measure for educational achievement is that they are not a longitudinal measure for increasing skills. The range is fixed and thus they do not necessarily rise over time. However, as we do find a negative effect even on grades, the results suggest that separation — maybe with delay — decreases school grades of children significantly as well.

Overall, our results on the relationship between separation and cognitive skills suggest that one to two years before parents get separated children experience a lower cognitive skill development than children who live in stable families in the next years. However, cognitive skill development catches-up after parents separated. Therefore, the years before separation are potentially the years when the educational achievement gaps between children who live with a single parent and children who live with two parents emerge. In the next section we explore if specific patterns in parental inputs match the pattern of cognitive skill development when children experience that their parents get separated.

2.5 Mechanism: Family inputs shocked by separation

Policy interventions would need information on the mechanism of the reduction of cognitive skill development. If cognitive skill development is reduced because a specific family input is reduced like parental time or household income, supportive interventions should differ respectively. In the following we explore household income, parental help with homework as a proxy for parental time, and two proxy variables for family inputs in general: the number of books at home and household size as factors which might influence cognitive skill development in case of separation. The number of books at home could capture the loss of possessions in the household or the loss of contact to the higher educated parent. Household size captures in general the reduced number of adults at home and might subsume reduced adult time and household income. As some of these variables capture similar aspects of family inputs they should be included separately into a regression to avoid bad control issues.

In a first step, table A-9 in the appendix checks which family input factors have the largest impact on cognitive skill development as policy interventions might want to tackle the most effective family input. The outcome variable in table A-9 is reading test scores of 2012 and all three specifications control for reading test scores of 2010. The family input measures stem from 2012. Column 1 includes household income and the degree of parental help with homework. While the degree of help has an almost zero effect on cognitive skill development, the effect of household income is positive and highly statistically significant. Column 2 shows that similarly, the number of books at home has a positive and highly statistically significant effect on cognitive skill development while household size is only significant in the squared term – suggesting that very large households have a negative effect on cognitive skill development. Including all family inputs into the estimation equation simultaneously confirms these patterns: household income and the number of books at home seem to be most relevant family input factors.

In a second step, we check which family input is most affected by a separation. Household income might influence cognitive skill development most, however, it might be hardly affected by a separation because of social security systems. In table 2.4 and A-6 we use the four different family inputs measured in 2012 as outcome variable controlling for the value of the family inputs in 2010. Table 2.4 shows in columns 1-3 that household income is only reduced from the moment on when parents separate. In column 3 only children who experienced a parental separation one year ago have a significantly lower household income. Although household income is an important input for cognitive skill development in general, the evolution of household income when a separation occurs is different from the evolution pattern of cognitive skill development. Therefore, household income might not be the crucial family input which could ameliorate the negative influence of a separation on cognitive skill development. The pattern for the degree of help with homework is different. Column 4 of table 2.4 suggests that past separation have hardly an effect on the degree of help with homework. However, taking future separations into account in column 5 suggests that the degree is significantly reduced by a separation. Column 3 shows that this pattern is driven by children who will experience a separation of their parents during the following year. This pattern is very similar to the pattern of cognitive skill development and might be a potential channel. Parents shortly before they separate are absorbed with their conflicts and do not find time to support their children in school anymore. Afternoon care for children in school which provides help with homework might be a reasonable policy intervention for children who experience a high degree of parental conflicts at home.

The number of books at home and household size are proxy variables for observed and unobserved family inputs. Table A-6 columns 1-3 show that the number of books at home is also particularly affected if separation occurs one year ahead. As the most plausible explanation why separation should influence the number of books at home is that one parent moves out and takes some books with her, it is unclear why the effect is so pronounced one year before separation occurs. One concern would be that this captures peculiarities of the separation cohort S_2 which experiences a separation in 2013. The robustness checks will deal with that issue. Columns 4 to 6 show that household size is significantly reduced by about one person if separation occurred in the past. However, the pre-separation effect on household size is close to zero. Future research should explore more direct measures of family inputs and specifically more measures on parental time investments if such measures are available as panel variable.

Overall, mainly the degree of parental help with homework coincides in the timing with the decrease in cognitive skill development. Although the results of table A-9 suggest that parental help with homework does not influence cognitive skill development significantly, it might ameliorate negative effects of separation for the treatment group.[22]

2.6 Robustness checks

The robustness checks address some issues on identification of a separation effect and on the interpretation of our results.

[22] An interaction of a decrease in the degree of help with homework and separation is particularly negative for cognitive skill development. If the help with homework does not change or increases, the separation effect becomes almost zero. Results are available upon request.

2.6.1 Reversed causality and omitted variables

One issue for identifying a separation effect is, if parents select into separating because of the educational achievement of their children. If this is the case causation is reversed. To address reversed causality we, first, use pre-observational measures for educational achievement: the probability of attending a high track school in 2010 and the probability of having repeated a grade in school before 2010 and check if these measures are correlated with separations between 2010 and 2014. Additionally, we check if birth weight a long-run measure for well-being of the child is significantly different for children of parents who separate between 2010 and 2014. Second, we predict a future separation with past test scores and past behavioral measures. Behavior in school might be an omitted variable driving separation and test scores.

Table 2.5 shows that the coefficients of experiencing a separation of one's parents between 2010 and 2014 are very small (2 percent and 1 percent of a standard deviation) and insignificant. This holds for the probability of attending a high track school (columns 1 and 2) and for the probability of having repeated a grade (columns 3 and 4). These findings contradict the findings of Piketty (2003) and suggest that there are no large long-run disadvantages in educational achievement for children who will experience a separation in the future (at least for 10- to 14-year olds). The earliest measure for child-well being which is studied extensively in the literature is birth weight. Birth weight affects future outcomes (Behrman and Rosenzweig, 2004) and in particular cognitive development of children (Figlio, Guryan, Karbownik, and Roth, 2014). In table 2.5, columns 5 and 6 show that children who will experience a separation 10 years in the future do not have a significantly lower weight at birth.

In table A-10 we predict future separations with test scores and measures of behavior. Columns 1 and 2 show that past test scores from 2010 do not predict future separations.[23] Similarly for measures of behavior in columns 3 and 4 the coefficients are close to zero and insignificant. Problematic behavior is measured by the strength-and-difficulties questionnaire (SDQ) in 2012.

Overall, these results deliver evidence that parents do not select into separation - neither by observed factors like test scores or behavior, nor by unobserved factors which would influence child development negatively early on.

[23] The outcome here is a dummy equal to 1 if the child experiences a separation between 2010 and 2014. Therefore, the large and significant correlation between test scores in 2010 and a separation in 2011 shown in table A-8 does not show up here.

2.6.2 Heterogeneity of separation cohorts

For interpretation of the timing of the separation effect it is crucial that the four different separation cohorts do not differ significantly. The aim is to interpret the cohort which experienced a separation in 2013 as representative for any other cohort which experiences a separation in the following year. For that we can only deliver descriptive evidence. First, table A-8 shows that also in reading test scores in 2010 (column 1) the *level* of those children who will experience a separation in 2011 – which is one year ahead of the testing – is lowest. As the panel starts in 2010 there are no pre-2010 test scores available to apply a value-added approach. Column 2, additionally shows that the results do not hold for math test scores in 2010. Second, table A-7 shows means and standard deviations of our control variables for the four separation cohorts. Column 6 shows if the the means of the specific cohort are significantly different from the mean of the cohort which experiences a separation in 2013. The first two cohorts which experience a separation in 2011 and 2012 are not significantly different except for the 2012-cohort in household income and the 2011-cohort in the share of imputed household income. The 2014-cohort differs significantly in primary school math grades, share of children with migration background, and the share of children with imputed household income. We conclude that the children who experience a separation of their parents in 2013 are overall comparable to the other separation cohorts - except for the cohort which experiences a separation in 2014.

2.6.3 External validity

One issue in the NEPS data is that sample selection or non-random panel attrition and item-non response lead to results which are not externally valid. To address this issue, first, we check in German population data on family structure and school track attendance how representative the NEPS data are in that dimensions. Second, we investigate panel attrition and item-non response patterns within the NEPS data.

Representativeness of NEPS To check whether NEPS is representative in the family structure children live in, we compare the 10-year olds in NEPS 2010 to the 10-year olds in the German Microcensus data from 2011. In the Microcensus 18 percent of 10-year-old children live with a single parent at home. In the NEPS sample only 10 percent of the 10-year olds live with a single parent. This difference suggests that single-parent families are underrepresented in the NEPS data.

More than the level difference, a positive selection on separations between age 10 and 14 matters for external validity of our findings. However, there is no representative data set available for changes in family types for 10-year olds. In the census 20 percent of the 14-year olds live with a single parent which suggests an increase by 2 percentage points compared to the 10-year olds.

However, here cohort effects cannot be disentangled from age effects. Additionally, the group of 14-year olds who live with a single parent consists of children who lived with a single parent since they are 10 and of children who experienced a separation in the past four years reduced by the group of children whose parents moved in with a new partner. The rate of new separations between age 10 and 14 might therefore be higher in reality. In the NEPS sample 5 percent of children are affected by a separation. Administrative data from 2014 on divorces shows that 9.25 percent of all children aged 0-18 experienced a divorce of their parents within the past five years. If divorces are evenly distributed over the five years about 1.85 percent experience a divorce each year. Again, the 5 percent new separations within four years in NEPS seem low compared to population data. However, it is possible that separations and divorces are not equally distributed over the age of children. It is possible that the share of children experiencing a parental divorce is lower for children between age 10 and 14 than between age 0 and 10 or age 14 and 18.

Another issue might be that NEPS selected a sample of schools which are particularly good (or bad) in dealing with children who experience a separation. Specifically, if the share of high track students is higher in the NEPS data than in the German population it is possible that the estimated effect is not representative. The allocation of 10-year-old children to school tracks gives insights into the selectivity of the NEPS sample with respect to educational achievement. In the Microcensus 40 percent of all 10-year olds attend high track schools. In the NEPS sample used for the analysis 55 percent attend the high track school. However, in the whole sample 43 percent of children attend a high track school. The over representation of high track school students in the analysis comes from non-random panel attrition and item-non response. It is unclear, if a positively selected sample concerning school tracks provides an upper or lower benchmark of the separation effect. Children who are attending a high track school might either compensate a separation of their parents better or they might be more affected as they have a higher level of parental inputs to lose. If the positive selection is particularly prevalent for the control group of children in stable families, the estimated effect is overestimated.[24]

Panel attrition and item-non response The sample used for the analysis is a sub-sample of the total sample due to panel attrition and item-non response. Panel attrition happens because of withdrawal of participation consent while item-non response occurs either through absenteeism at the test and survey day or refusal to answer a specific question. A withdrawal of participation consent can occur on the individual or on the school level. On the individual level until 2012, 47 and until 2014, 312 children have been lost to the sample. On the school level until 2014, 15 schools have withdrawn their consent reducing the sample by about 300

[24] However, we are not very much concerned about overestimation as the magnitudes of our main results are very similar when we include school-fixed effects in table A-2.

children.[25] Temporary missing values are more frequent. 38.1 percent of students miss at least one of the five survey and test days. The majority of them, 18.6 percent miss only one survey and test day. About 4 percent of students participate in only one survey and test. In 2012, 784 students missed the survey and test and in 2014, 1292 missed it. On top of these types of missing observations comes item-non response. From those who participate in the survey in 2010, 15 do not take the reading test. In 2012, this number increases to 648 students and in 2014, to 907 students (about 20 percent). For 528 children who participated in the reading tests 2010 and 2012 there is no or only one measure of family-type information. The final sample for analysis consists of 3287 children who participated in the reading tests of 2010 and 2012, who have at least two measures for their family type between 2010 and 2014, and for whom information on control variables is available.

Panel attrition and item-non response might bias the results if they occur non-randomly. If children who experience a separation are more likely to drop out of the sample e.g. because they switch schools, are more often absent from school, or are less motivated to fill out questions about their family life, the treatment group is selected. If the most affected students drop out of the sample, the separation effect is underestimated. This might be the case in the NEPS setting. Table 2.6 analyses the correlation between separation between 2010 and 2014 and non-participation in the NEPS and between reading test scores in 2010 and missing information. Column 1 shows that separation between 2010 and 2014 and non-participation in NEPS are hardly correlated. Children whose test scores in 2012 are missing perform worse than their non-missing peers but this is not a specific problem for children who experience a separation as the interaction terms show (columns 2 and 3). However, column 4 shows that children whose family type is missing perform worse in 2010 than children whose family type is available. If there are children who experience a separation but do not report it they are probably the lower performing children. This holds as well for missing parent interview data which is the first source to extract information on the family type. Overall, this suggests that some low performing children drop out of the sample and it is possible that these children might have also experienced a separation. The estimated effects in section 2.4 might therefore be underestimated. If the dropped out children are most likely living in two parent families the control group might be positively selected and the effects could be overestimated.

2.7 Conclusion

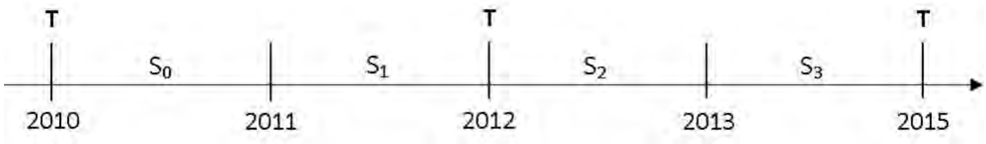
Many studies document large level differences in educational outcomes between children who grow up with a single parent and children who grow up with two parents. However, so

[25] From those, 8 schools withdraw their consent between 2010 and 2011 and additional 7 schools between 2011 and 2012.

far, the literature could not identify when and how these differences emerge. With rich panel data from the NEPS on 10-year-old children this chapter investigates when a separation starts to affect cognitive skill development of children. The panel data tracks children from 5th grade in 2010 until 9th grade in 2014. The data provide cognitive skill measures in form of standardized reading test scores in 2010 and 2012 and annual reports on family type, and household inputs into education production. Applying a value-added approach, we find that the effect of separation is negative and strongest in the year before separation occurs. After parents separated a slight catching-up of skill development occurs. These findings shed light on the issue of lead and lag effects of parental separation on child outcomes and give first insights from when on and for how long children are affected if their parents separate. With information on household income, the degree parents help with homework, the number of books at home, and household size, we further analyze which family inputs are affected by parental separation. The pattern of cognitive skill development over time coincides most with the pattern in the development of degree of parental help with homework when a separation occurs. Policy measures which support children of separated parents might take these findings into account to reduce inequality in the German school system.

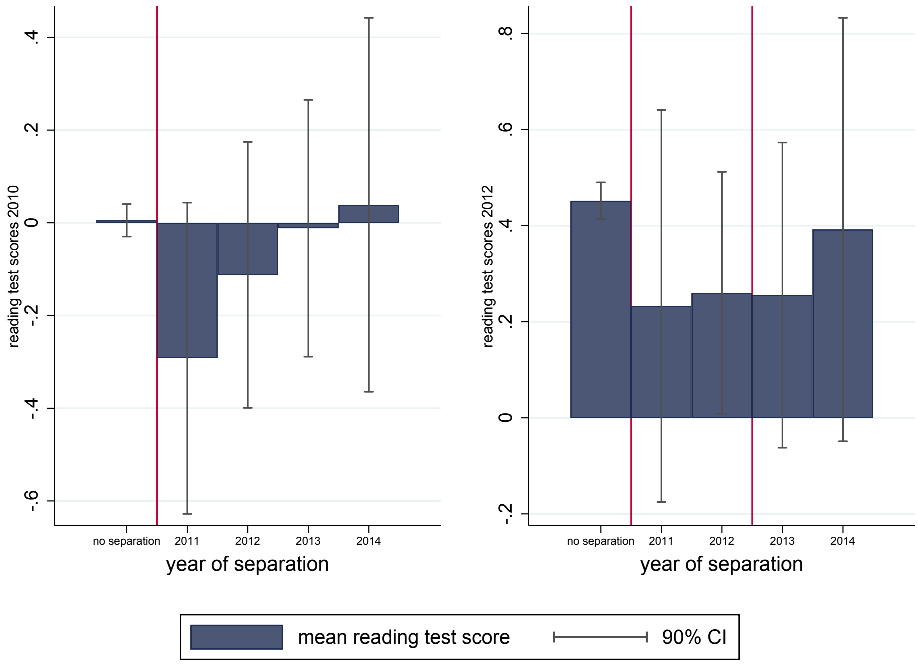
2.8 Figures and tables

Figure 2.1 : Structure of NEPS panel data on 10-year olds



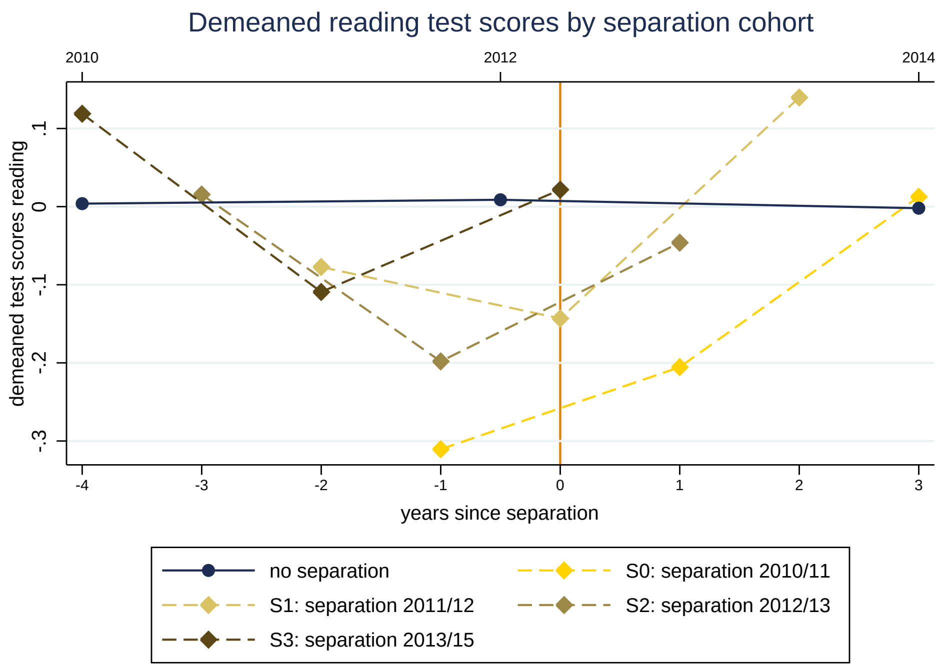
Notes: Figure 2.1 shows the structure of NEPS panel data of starting cohort 3 for the main variables: reading and math test scores and separation. Children are tested in fall 2010, 2012 and 2014/2015 (spring) in reading and math. Data on family structure of the children is available for 2010, 2011, 2012, 2013, 2014. Separation which is reported in 2011 happened between 2010 and 2011. The same holds for the other years. Three periods during which separation happens can be identified: S_0=2010/2011, S_1=2011/2012, S_2=2012/2013, and S_3=2014 (spring).

Figure 2.2 : Mean reading test scores 2010 and 2012 by year of separation and for control group



Notes: Figure 2.2 shows separately for 2010 (left) and 2012 (right) the differences in mean reading test scores for the control group of children living in stable families from 2010 to 2014 and for the four separation cohorts S_0, S_1, S_2, and S_3. The grey bars indicate the 95-percent confidence interval.

Figure 2.3 : Demeaned reading test scores by time distance to separation and separation cohort



Notes: Figure 2.3 shows the evolution of reading test scores (demeaned) for children who experience a separation of their parents in S_0=2011 (yellow), S_1=2012 (beige), S_2=2013 (brown), and S_3=2014 (olive). The dark blue line marks the benchmark of demeaned test scores by year (second x-axis) of children who do not experience a separation.

Table 2.1 : Summary statistics

	Mean	SD	Min	Max
Separation between 2010 and 2014	0.05	0.22	0	1
Living with a single parent 2010	0.08	0.27	0	1
Cognitive skills				
Reading				
2012	0.44	1.09	-3.1	4.5
2010	0.00	1.00	-3.1	3.1
Math				
2012	0.57	1.09	-4.5	4
2010	-0.00	1.00	-3.4	3.4
Degree help with homework				
2012	3.42	0.99	1	5
2010	3.64	1.01	1	5
Individual characteristics				
Female	0.49	0.50	0	1
Migration background	0.31	0.46	0	1
Log household income per member (imputed)	6.67	0.44	3.1	10
Income imputation indicator	0.35	0.48	0	1
Number of books at home				
Up to 10 books	0.03	0.17	0	1
11-25 books	0.07	0.26	0	1
26-100 books	0.24	0.43	0	1
101-200 books	0.20	0.40	0	1
201-500 books	0.27	0.44	0	1
More than 500 books	0.18	0.39	0	1
Observations	3287			

Notes: The table shows summary statistics for outcome and control variables. *Data source:* NEPS SC3 6.0.1.

Table 2.2 : Reading test score deficit of children living with a single parent

	Level differences			Differences in change	
	(1) 2010	(2) 2012	(3) 2010	(4) First difference	(5) Value-added
single parent	-0.315***	-0.277***	-0.145*	0.037	-0.074
	(0.082)	(0.095)	(0.076)	(0.076)	(0.073)
Controls	No	No	Yes	No	No
Observations	2961	2961	2961	2961	2961
R^2	0.005	0.003	0.149	0.000	0.346
Mean outcome	0.217	0.765	0.217	0.548	0.765
SD outcome	1.209	1.330	1.209	1.158	1.330

Standard errors in parentheses
$^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Notes: The table shows in column 1-3 level differences in reading test scores 2010 and 2012 for children who live with a single parent in 2010 and children who live with two parents in 2010. Column 4 estimates for the same groups of children a first difference approach with reading test scores 2012-reading test scores 2010 as outcome variable. Column 5 estimates a value-added approach with reading test scores of 2012 as outcome variable controlling on the right-hand side for reading test scores in 2010. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC3 6.0.1.

Table 2.3 : Effect of separation on reading test score development

	Reading test scores 2012		
	(1) past	(2) past and future	(3) exact year
separation btw 2010 and 2012	-0.103		
	(0.082)		
separation btw 2010 and 2014		-0.114*	
		(0.067)	
separation 2011			-0.060
			(0.136)
separation 2012			-0.132
			(0.102)
separation 2013			-0.163
			(0.113)
separation 2014			-0.060
			(0.157)
Reading test score 2010	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	3287	3287	3287
R^2	0.385	0.386	0.386
Mean outcome	0.443	0.443	0.443
SD outcome	1.090	1.090	1.090

Notes: The table estimates the effect of separation on cognitive skill development in a value-added approach with reading test scores of 2012 as outcome variable controlling for reading test scores of 2010. The first column estimates the effect if a separation happened between 2010 and 2012, the second column estimates the effect for any separation between 2010 and 2014. The third column distinguishes the four separation cohorts: S_0, S_1, S_2, and S_3. All specifications include individual and family controls from 2010. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC3 6.0.1.

Table 2.4 : Mechanism: Family inputs of money and help with homework

	Household income 2012			Help homework 2012		
	(1) past	(2) past and future	(3) exact year	(4) past	(5) past and future	(6) exact year
separation btw 2010 and 2012	-0.043			-0.075		
	(0.083)			(0.113)		
separation btw 2010 and 2014		-0.011			-0.176**	
		(0.054)			(0.086)	
separation 2011			-0.114*			-0.150
			(0.066)			(0.191)
separation 2012			0.008			-0.041
			(0.131)			(0.150)
separation 2013			0.047			-0.280**
			(0.059)			(0.136)
separation 2014			0.027			-0.276
			(0.056)			(0.179)
Outcome 2010	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1787	1787	1787	3004	3004	3004
R^2	0.565	0.565	0.566	0.094	0.096	0.096
Mean outcome	6.796	6.796	6.796	3.427	3.427	3.427
SD outcome	0.466	0.466	0.466	0.993	0.993	0.993

Notes: The table estimates the effect of separation on household income per household member (in logs) and the degree parents help their children with homework in a value-added approach. Measures of the outcome variable from 2010 are included on the right-hand side. In column 1-3 household income in 2012, in column 4-6 the degree of help with homework in 2012 is the outcome variable. The first/forth column estimates the effect of a separation which happened between 2010 and 2012, the second/fifth column estimates the effect for any separation between 2010 and 2014. The third/sixth column distinguishes the four separation cohorts: S_0, S_1, S_2, and S_3. All specifications include individual and family controls from 2010. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC3 6.0.1.

Table 2.5 : Pre-separation effects - attending highest secondary school track, having repeated a grade, birthweight

	in high track		repeated grade		birthweight	
	(1)	(2)	(3)	(4)	(5)	(6)
	ols	with controls	ols	with controls	ols	with controls
separation	0.002	-0.006	-0.001	-0.003	-19.804	-39.068
	(0.039)	(0.033)	(0.018)	(0.014)	(50.583)	(49.165)
Controls	No	Yes	No	Yes	No	Yes
Observations	3406	3406	3237	3237	2513	2513
R^2	0.000	0.174	0.000	0.308	0.000	0.024
Mean outcome	0.553	0.553	0.056	0.056	3392.203	3392.203
SD outcome	0.497	0.497	0.230	0.230	613.378	613.378

Notes: Table 2.5 shows results for regressing the probability of attending the high track school in 2010 (column 1-2) and the probability of having repeated a school grade at least once (column 3-4) on experiencing a separation between 2010 and 2014. In column 5-6 a retrospective measure of birthweight is used as outcome variable. All coefficients are estimated with OLS. Column 2, 4, and 6 add individual and household characteristics as control variables. Standard errors in parentheses clustered on school level; * = p<0.1, **=p<0.05, ***=p<0.01.

Table 2.6 : Missing patterns and school switches by family type

	non-participation	reading test scores 2010			
	(1) predict missing	(2) test scores	(3) test scores	(4) family type	(5) parent interview
separation	0.025	-0.113	-0.091		-0.009
	(0.023)	(0.101)	(0.083)		(0.077)
x missing test scores		0.155	0.202		
		(0.173)	(0.160)		
x missing parent interview					-0.251
					(0.181)
missing test scores		-0.346***	-0.082		
		(0.075)	(0.072)		
parent interview					-0.077*
					(0.043)
missing family type				-0.286***	
				(0.043)	
Controls	Yes	No	No	No	No
School track FE	No	No	Yes	Yes	Yes
Observations	4340	4171	4171	5193	4171
R^2	0.021	0.013	0.310	0.342	0.310
Mean outcome	0.218	0.126	0.126	-0.020	0.126
SD outcome	0.328	1.238	1.238	1.266	1.238

Notes: Table 2.6 explores missing patterns of the NEPS data. Column 1 shows results of regressing the probability of becoming a non-participating child on a dummy indicating that the child experiences a separation. Columns 2-5 use reading test scores 2010 as outcome variable to check if children who drop out of the sample later on perform significantly different from children who do not drop out. All coefficients are estimated applying OLS. Standard errors in parentheses clustered on school level; * = p<0.1, **=p<0.05, ***=p<0.01.

2.9 Appendix A

Table A-1 : Survey questions used to construct separation dummy

Parents	
Q1:	**Are you currently ...**
A	Married and live with your spouse,
B	Married and live apart from your spouse,
C	Divorced,
D	Widowed,
E	Single,
F	Or do you live in a registered civil partnership?
Q2:	**Do you currently live with a long-term partner?**
	Yes/No
Q3:	**Is this the same partner as in our last interview?**
	Yes/No/No partner present in the last wave
Children	
Q4:	**Who normally lives with you in your household?**
A	Biological mother, adoptive mother, foster mother
B	Stepmother or father's girlfriend
C	Biological father, adoptive father, foster father
D	Stepfather or mother's boyfriend
E	Siblings and/or step siblings
F	Grandmother and/or grandfather
G	Other people

Notes: Q1, Q2, Q3 are administered to the interviewed parent of the specific child in the yearly conducted telephone interview. Q4 is administered to the children in school by a paper-and-pencil questionnaire. Q4 is asked in 2010, 2011, and 2013. If Q1-Q4 are available for a child, the information from the parents is used. The family type is 'living with a single parent' if Q1 is not A or F and Q2 is "No". Q3 is used if information from last wave from the parents is missing. If all information from parents (Q1-Q3) is missing, the information from the children (Q4) is used. The family type is set to 'living with a single parent' if either Q4 is A but not C or D or Q4 is C but not A or B.

Table A-2 : Effect of separation on reading test score development - school fixed effects

	Reading test scores 2012		
	(1) past	(2) past and future	(3) exact year
separation btw 2010 and 2012	-0.113		
	(0.081)		
separation btw 2010 and 2014		-0.104	
		(0.063)	
separation 2011			-0.105
			(0.142)
separation 2012			-0.121
			(0.100)
separation 2013			-0.120
			(0.109)
separation 2014			-0.047
			(0.149)
School FE	Yes	Yes	Yes
Reading test score 2010	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	3287	3287	3287
R^2	0.465	0.465	0.465
Mean outcome	0.443	0.443	0.443
SD outcome	1.090	1.090	1.090

Notes: The table estimates the effect of separation on cognitive skill development in a value-added approach with reading test scores of 2012 as outcome variable controlling for reading test scores of 2010. The difference to table 2.3 is that it includes school-fixed effects in all specifications. The first column estimates the effect if a separation happened between 2010 and 2012, the second column estimates the effect for any separation between 2010 and 2014. The third column distinguishes the four separation cohorts: S_0, S_1, S_2, and S_3. All specifications include individual and family controls from 2010. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC3 6.0.1.

Table A-3 : Effect of separation on math test score development

	Math test scores 2012		
	(1) past	(2) past and future	(3) exact year
separation btw 2010 and 2012	-0.091		
	(0.072)		
separation btw 2010 and 2014		-0.124**	
		(0.056)	
separation 2011			-0.109
			(0.138)
separation 2012			-0.087
			(0.080)
separation 2013			-0.065
			(0.142)
separation 2014			-0.295***
			(0.095)
Math test score 2010	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	3283	3283	3283
R^2	0.541	0.542	0.542
Mean outcome	0.571	0.571	0.571
SD outcome	1.088	1.088	1.088

Standard errors in parentheses
$^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Notes: The table estimates the effect of separation on cognitive skill development in a value-added approach with math test scores of 2012 as outcome variable controlling for math test scores of 2010. The first column estimates the effect if a separation happened between 2010 and 2012, the second column estimates the effect for any separation between 2010 and 2014. The third column distinguishes the four separation cohorts: S_0, S_1, S_2, and S_3. All specifications include individual and family controls from 2010. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC3 6.0.1.

Table A-4 : Separation effect in event study design

	compared to year of separation			compared to one year before separation		
	(1)	(2)	(3)	(4)	(5)	(6)
	reading test scores			reading test scores		
separation in 4 years	-0.028					
	(0.236)					
separation in 2 years		0.058				
		(0.121)				
separation 2 years ago			0.199			
			(0.159)			
separation in 3 years				0.216		
				(0.144)		
separation 1 year ago					0.231**	
					(0.113)	
separation 3 years ago						0.227
						(0.194)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6181	9585	5885	7002	9558	6181
R^2	0.520	0.311	0.214	0.179	0.314	0.522
Mean outcome	0.648	0.674	1.004	0.429	0.674	0.644
SD outcome	1.326	1.330	1.281	1.316	1.330	1.327

Standard errors in parentheses
$^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Notes: The table shows results for comparing pre- and post-separation reading test score measures of 2010, 2012, and 2014 with the respective test score performance in the year of separation or in the year one year before separation occurs. In each column only two performances are compared. In column 1 performance 4 years before separation, in column 2 performance 2 years before separation, and in column 3 performance 2 years after separation is compared to the performance in the year of separation. In column 4 performance 3 years before separation, in column 5 performance 1 year after separation, and in column 6 performance 3 years after separation is compared to the performance one year before separation. The effects are estimated in a panel setting with individual-fixed effects. The sample varies as the children who experience a separation in another period are excluded from the sample. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC3 6.0.1.

Table A-5 : Effect of separation on development of grades

	German grades			Math grades		
	(1) past	(2) past and future	(3) exact year	(4) past	(5) past and future	(6) exact year
separation btw 2010 and 2012	-0.145**			-0.117		
	(0.064)			(0.081)		
separation btw 2010 and 2014		-0.097**			-0.095	
		(0.048)			(0.070)	
separation 2011			-0.171			-0.041
			(0.110)			(0.152)
separation 2012			-0.131			-0.164*
			(0.085)			(0.087)
separation 2013			0.009			-0.002
			(0.100)			(0.151)
separation 2014			-0.126			-0.170
			(0.156)			(0.157)
Grades 2010	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3154	3154	3154	3161	3161	3161
R^2	0.295	0.295	0.295	0.220	0.220	0.221
Mean outcome	3.384	3.384	3.384	3.350	3.350	3.350
SD outcome	0.758	0.758	0.758	0.870	0.870	0.870

Notes: The table estimates the effect of separation on German and math grades 2012 in a value-added approach controlling for grades of 2010. Column 1-3 estimate effects for German grades. Column 4-6 estimate effects for math grades. The first/fourth column estimates the effect if a separation happened between 2010 and 2012, the second/fifth column estimates the effect for any separation between 2010 and 2014. The third/sixth column distinguishes the four separation cohorts: S_0, S_1, S_2, and S_3. All specifications include individual and family controls from 2010. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC3 6.0.1.

Table A-6 : Mechanism: Family inputs approx. by the number of books at home and household size

	Books at home 2012			Household size 2012		
	(1) past	(2) past and future	(3) exact year	(4) past	(5) past and future	(6) exact year
separation btw 2010 and 2012	-0.063			-0.992***		
	(0.155)			(0.094)		
separation btw 2010 and 2014		-0.142			-0.605***	
		(0.104)			(0.081)	
separation 2011			-0.094			-0.911***
			(0.205)			(0.157)
separation 2012			-0.056			-1.031***
			(0.205)			(0.116)
separation 2013			-0.447***			-0.065
			(0.170)			(0.136)
separation 2014			0.110			0.103
			(0.189)			(0.069)
Outcome 2010	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3013	3013	3013	2368	2368	2368
R^2	0.373	0.373	0.374	0.824	0.812	0.824
Mean outcome	4.233	4.233	4.233	4.127	4.127	4.127
SD outcome	1.436	1.436	1.436	1.068	1.068	1.068

Notes: The table estimates the effect of separation on the number of books at home in 2012 and the household size in a value-added approach. Measures of the outcome variable from 2010 are included on the right-hand side. In column 1-3 the number of books at home in 2012, in column 4-6 the household size in 2012 is the outcome variable. The first/forth column estimates the effect of a separation which happened between 2010 and 2012, the second/fifth column estimates the effect for any separation between 2010 and 2014. The third/sixth column distinguishes the four separation cohorts: S_0, S_1, S_2, and S_3. All specifications include individual and family controls from 2010. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01.
Data source: NEPS SC3 6.0.1.

Table A-7 : Descriptive differences by separation cohort

Separation Cohort	Mean	SD	Min	Max	Different from 2013
Grade math primary school					
2011	3.88	0.79	1.5	5	
2012	3.69	0.83	1	5	
2013	3.80	0.81	1	5	-
2014	3.41	1.15	1	5	*
Grade German primary school					
2011	3.78	0.61	2.5	5	
2012	3.65	0.69	2	5	
2013	3.69	0.64	2	5	-
2014	3.56	0.94	1	5	
Female					
2011	0.38	0.49	0	1	
2012	0.36	0.48	0	1	
2013	0.41	0.50	0	1	-
2014	0.50	0.51	0	1	
Migration background					
2011	0.28	0.46	0	1	
2012	0.32	0.47	0	1	
2013	0.39	0.49	0	1	-
2014	0.19	0.40	0	1	*
Log household income per member (imputed)					
2011	6.69	0.51	6.1	7.9	
2012	6.77	0.47	5.8	8.3	*
2013	6.61	0.46	5	7.2	-
2014	6.59	0.49	5.7	7.5	
Income imputation indicator					
2011	0.19	0.40	0	1	*
2012	0.34	0.48	0	1	
2013	0.39	0.49	0	1	-
2014	0.19	0.40	0	1	*
Observations					
2011	32				
2012	56				
2013	49				
2014	32				

Notes: The table shows summary statistics for the four different separation cohorts who experience a separation of their parents in S_0=2011, S_1=2012, S_2=2013, and S_3=2014. The sixth column indicates if the mean is statistically significantly different from the cohort S_2. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC3 6.0.1.

Table A-8 : Pre-separation effects on test scores

	Test scores 2010	
	(1)	(2)
	reading	math
separation 2011	-0.369***	-0.044
	(0.140)	(0.167)
separation 2012	-0.146	-0.123
	(0.134)	(0.133)
separation 2013	0.039	-0.027
	(0.134)	(0.116)
separation 2014	0.036	-0.096
	(0.167)	(0.137)
Controls	Yes	Yes
Observations	3287	3283
R^2	0.157	0.187
Mean outcome	0.000	-0.000
SD outcome	1.000	1.000

Notes: The table shows descriptive results for level differences in reading (column 1) and math (column 2) test scores of 2010 for the four different separation cohorts. Both specifications include individual and family controls. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC3 6.0.1.

Table A-9 : Cognitive skill development and parental inputs

	Reading test scores 2012		
	(1) direct measures	(2) indirect measures	(3) horse race
help homework	-0.010		-0.015
	(0.019)		(0.019)
household income	0.199***		0.132***
	(0.042)		(0.044)
no. books at home		0.118***	0.104***
		(0.014)	(0.016)
household size linear		0.085	0.093
		(0.054)	(0.059)
household size squared		-0.013***	-0.012**
		(0.005)	(0.005)
Reading test score 2010	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	1917	2289	1883
R^2	0.362	0.377	0.376
Mean outcome	0.562	0.572	0.573
SD outcome	1.060	1.063	1.055

Notes: The table regresses reading test scores of 2012 on different family inputs measured in 2012: the degree of parental help with homework, household income per household member (in logs), the number of books at home, and the household size (linear and squared term). Column 1 includes only the degree of help with homework and household income - which are direct measures of family inputs in education production. Column 2 includes the number of books at home and household size as proxy variables for unobserved factors in family inputs. Column 3 performs a horse race among all four family input factors. All specifications include reading test scores of 2010 as control variable (value-added approach) and individual and family controls of 2010. Standard errors in parentheses clustered on school level; * = p<0.1, **=p<0.05, ***=p<0.01.

Table A-10 : Predicting separations

	future separation			
	(1) ols	(2) with controls	(3) ols	(4) with controls
reading test scores 2010	-0.005	-0.002		
	(0.005)	(0.005)		
math test scores 2010	0.002	-0.003		
	(0.005)	(0.006)		
medium low SDQ problematic behavior score			-0.006	-0.005
			(0.009)	(0.010)
medium SDQ problematic behavior score			0.007	0.001
			(0.010)	(0.010)
medium-high SDQ problematic behavior score			0.005	-0.001
			(0.014)	(0.016)
high SDQ problematic behavior score			0.022	0.015
			(0.028)	(0.029)
Controls	No	Yes	No	Yes
School FE	No	Yes	No	Yes
Observations	3284	3284	3374	3374
R^2	0.000	0.069	0.001	0.062
Mean outcome	0.051	0.051	0.042	0.042
SD outcome	0.221	0.221	0.200	0.200

Notes: Table A-10 shows the results from regressing the probability of experiencing a separation in the future on test scores in 2010 (column 1 and 2) and on 4 categories of the strengths-and-difficulties questionnaire (SDQ) score for problematic behavior (column 3 and 4). 1 is the second lowest score for problematic behavior, 4 is the highest score. The lower the score the less problematic is the behavior. Column 1 and 3 show a linear probability estimation, column 2 and 4 add controls. Standard errors in parentheses clustered on school level; * = p<0.1, **=p<0.05, ***=p<0.01.

3 Does Awarding a Low-Track School Degree Improve the Fate of High-Track Drop-Outs? Evidence from a German School Reform*†

3.1 Introduction

An accomplished school degree determines an individual's labor market career at various stages. On the one hand, it influences the transition into the labor market. In Germany, for example, it is an institutionalized prerequisite for vocational training. On the other hand, a school degree is a signal for any future employer about cognitive and soft skills. Numerous economic studies find negative effects of school dropout on individual schooling careers and employment prospects (Rumberger and Lamb, 2003; DesJardins, Ahlburg, and McCall, 2006; Oreopoulos, 2007; Brunello and De Paola, 2013; Cahuc, Carcillo, and Minea, 2017). In the European Union (EU) in 2012, 13 percent of all 18- to 24-years-olds – about 5.5 million young people – had dropped-out of upper secondary education before obtaining a degree (EU, 2013). Many countries are eager to establish education policies that reduce the number of dropouts, e.g. the Europe 2020 strategy aims at "reducing the share of early leavers of education and training to less than 10%" (Commission, 2014). Notably, school dropouts are not only a phenomenon among low-track school students. Also in high-track schools, there is a relatively stable dropout rate of up to 10 percent of students, since 1950.

In this chapter, we analyze a reform of the German school system, a system with tracking and grade repetition. The school reform under study awarded high-track dropouts, failing to successfully complete 13 years of schooling, with a low-track school degree if they accomplished at least the 9th grade. The high track in Germany is the most time-consuming secondary school track awarding students a degree after accomplishing the 13th grade. In contrast, in the low tracks (*Hauptschule*, basic school, and *Realschule*, middle school) students obtain a low-track school degree after having accomplished 9th resp. 10th grade. Before the reform, students who dropped out from the high track were left without any degree and had to qualify for the labor market otherwise, e.g. by switching to a low-track school and repeating a grade there.

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After the reform, the students received a basic school degree if they passed the 9th grade of the high track, and received a middle school degree if they passed the 10th grade of the high track.

Theoretically, such a reform could lead to various outcomes at least via the four mechanisms discussed in the following. The first set of expectations contains the group of students who actually drop out of the high track. In a mechanical way, the number of students who leave the school system without any school degree should decrease. This was the main policy goal behind implementing the reform. If high-track dropouts switched schools to obtain any other low-track school degree, the reform should result in reduced downgrading. In case of sheepskin effects holding a degree, given the same time spent in school, should improve the labor market prospects of the students in terms of employment and wage see Layard and Psacharopoulos (1974); Hungerford and Solon (1987); Jaeger and Page (1996). The second and third expectations concern incentives for students at risk of dropping out. On the one hand the reform incentivizes students to leave the high-track school after having completed the 9th resp. 10th grade to enter the labor market. This would result in a lower number of students with a high-track degree, a higher number of students with a low-track degree, and a younger labor market entry age. On the other hand, the reform reduced the risk of staying in the high track. Parents who might have favored downgrading their low-performing child from the high track to the low track before the reform, might now be more likely to keep their child in the high track. Given that the child is already awarded a low-track degree after successful completion of the 9th grade, the reform reduced the risk of staying in the high track on the relevance of risk aversion for schooling decisions, see Lavecchia, Liu, and Oreopoulos (2016); Wölfel and Heineck (2012). Staying in the high-track school could then exert a positive effect on the student's motivation to try hard to successfully finish the high track. Such behavioral response to the reform would lead to an increased completion rate of the high track, and eventually to a higher university enrollment rate. Fourth, once the reform is implemented and known by students and their parents, there could arise selection into the high track. The possibility of receiving a (low track) school degree in the high track, even in case of failure after the 9th grade, reduced the risk of high-track enrollment. Therefore, parents who might have sent their low-performing child to a low-track school before the reform might now take these new opportunities into account when deciding on their child's secondary school. This would lead to an increase in the high-track school enrollment rate.

Our study aims at estimating the causal effects of the German dropout reform and by that at detecting which of the above presented mechanisms is the most prevalent one. For identification we exploit the different timing of the reform implementation across German states. In 1965, after alarming numbers on high-track school dropouts were released (Peisert and Dahrendorf, 1965; Stamm, 2010) the *Kultusministerkonferenz* (the Standing Conference of the Ministers of

Education and Cultural Affairs of the Länder) announced the reform on rewarding dropouts from high-track schools with low-track school degrees in order to reduce the number of students without any degree and to facilitate their labor market entry. Although announced at the federal level, the German states reacted at different points in time (Helbig and Nikolai, 2015). The different timing of the reform between 1965 and 1982 in 11 West German states[1] enables us to apply a difference-in-differences (DID) framework for identification of the causal reform effect. In this approach, we compare high-track school students in the respective reform state before and after the reform and compare the changes in student outcomes to the respective changes in high-track school student outcomes in states that did not reform at the same point in time. The inclusion of state fixed effects allows furthermore to abstain from any state specific features that could affect student outcomes and the inclusion of time fixed effects controls for potential nation-wide shocks. Importantly, in the baseline specification we restrict the sample to students who were already enrolled in high-track schools at the time of the reform in order to rule out reform-induced selection into high-track schools.

For estimation, we use two unique data sources. Our first data source consists of rich and detailed survey data of the National Educational Panel Study (NEPS) adult cohort from 2010 for more detail on the data, see Blossfeld et al. (2011). The NEPS data have two specific features enabling us to tackle our research question in a methodologically clear-cut manner: first, the data include information on the respective nature of dropping-out: we can differentiate whether a students left a school for good, whether the student switched to another school resp. to another school track, and at which point in time the student effectively ended her schooling career.[2] Second, the NEPS data reports the individuals' state of schooling in addition to the state of current residence. This is very important for an analysis of state reforms in a context where migration across states during an individual's lifespan cannot be ruled out. Our second data source is the German Microcensus: administrative data covering 1 percent of the population. We combine information on the jobs held at age 20 and 35 provided by the NEPS data with income information on the occupation in the Microcensus. Additionally, we use the Microcensus for common trend assumption considerations.

Our results show compliance with the reform in the sense that students are less likely to switch between schools and tracks. This reform effect is thus in line with the first theoretical mechanism. The results, however, do not support the second theoretical mechanism. We do not find that more students leave school with a low-track school degree to enter the labor market

[1] We abstain from including the East German states in our analysis as their education system was exposed to substantial changes after reunification.

[2] This is in contrast to other studies that only rely on school data reporting how many students left a school without degree but being ignorant of the subsequent educational path of the reported dropout. Usually, dropout is not well defined as students are not tracked over time.

earlier. Instead, we find that students, rather surprisingly, stay longer in the school system and finish school with a higher school degree due to the reform. This supports the third theoretical mechanism. The reform seems to reduce the perceived risk of trying longer in the high-track and thus leads to an increase in the high-track completion rate and in the university enrollment rates. Yet, we do not find significant reform effects on individual labor market income at age 20 and age 35 which implies that sheepskin effects cannot be shown. Regarding the fourth theoretical mechanism we find that selection into high-track schools is indeed affected by the reform. Students who enroll in a high-track school after the reform, and are thus potentially (adversely) selected, are less likely to complete the high track, and more likely to downgrade to a low-track school. As regards heterogeneity of the reform effects we find that students with a low socio-economic background benefit more from the reform than students with a high socio-economic background.

This study mainly relates to two strands of literature in the economics of education. First, we relate to studies on dropouts and their school respectively labor market careers. Oreopoulos (2007) focuses on dropouts from the US and argues that compulsory schooling laws prohibiting drop out add to students' health, life satisfaction, and wealth. Yet, opposed to our approach, the authors only focus on time spent in school and not on the role of degrees. DesJardins et al. (2006) focus on interrupted enrollment on graduation from college. They find that students after having dropped-out once are more likely to experience subsequent enrollment disruptions. Such disruptions are found to be detrimental to the student's probability of graduating. So far, there are only few dropout studies that focus on the particular group of dropouts from high-track schools. One study taking the German context into consideration is the descriptive study of Peisert and Dahrendorf (1965) who focus on Baden-Württemberg – a South Western German state – and conducted interviews with dropouts from the academic track.[3] Second, we relate our study to the emerging literature on behavioral aspects of the economics of education. One closely related study is Heckman, Humphries, LaFontaine, and Rodriguez (2012) who look at the GED Testing Program in the US. This program allows dropouts to attain a degree by doing the GED which is a high school diploma equivalent. While surely meant well by the politicians the authors find that this program created incentives to drop out from high school. These findings show the importance of considering the riskiness of decisions in the educational system to understand student behavior. In the same vein, some further recent studies show that the role of the default in the education system plays an important role for educational careers and outcomes for an overview, see Lavecchia et al. (2016). The German dropout reform under

[3] The authors find that there are a number of reasons for dropouts to leave the school, mainly problems in school subjects and personal reasons. The reasons for dropout are not easily distinguishable and merge into each other.

study can be interpreted as a low-cost intervention that changed the behavior of students in a beneficial way and thus adds to this strand of literature.

Our contribution to the literature is threefold. First, we provide causal evidence on the effects of a dropout reform making use of time variation of reform implementation across German states. This is in contrast to the existing literature which is mostly relying on descriptive evidence only. Second, we can reconstruct the whole school and labor market careers of individuals surveyed in our data and link these developments to reforms on the state level while the existing literature often fails to identify the subsequent periods of individual careers after experiencing a dropout. Third, we show that student behavior can be changed by a low-cost intervention in a rather surprisingly beneficial way.

This study proceeds as follows. In Section 3.2 we provide information on the institutional background of the German school system and in particular, on the reform under study. Section 3.3 presents the main data sets and provides descriptive statistics. Section 3.4 explains the empirical framework. Section 3.5 presents the results and section 3.6 provides the heterogeneity analysis. Section 3.7 discusses a series of robustness checks. Finally, section 3.8 concludes.

3.2 Institutional background

Germany has a specific institutional setup of its education system. This setup allows to study the dropout reform effects on high-track school students' behavior in a very detailed way, including school switches, track down- and upgrading as well as grade repetition. In the following we give a brief overview of the German school system for the birth cohorts in West Germany under study and describe the reform and its timing in detail.

The German school system Germany's school system is decentralized and is at the responsibility of each of the 16 states. Although there are some differences across states, the general structure is quite uniform and illustrated in Figure 3.1. The school system tracks children at age 10 after 4 years of primary school into mainly three secondary school tracks.[4] The different tracks are characterized by the final degree and the academic requirement. In the basic school (Hauptschule) a student has to pass 5 grades until compulsory schooling ends after accomplishing 9th grade. The basic school education is completed with a basic school degree which traditionally enables students to go into vocational training (Helbig and Nikolai, 2015). In the middle school (Realschule) a student has to pass 6 grades and finishes with a middle

[4] In two states, Berlin and Brandenburg, tracking takes place at age 12 after 6 years of primary school.

school degree. The middle school degree opens-up more vocational training opportunities. In the high track (Gymnasium) the student has to pass 9 grades and finishes with an academic school degree (Abitur) which is the university entrance qualification.[5] Tracking is done by ability. Teachers in primary school recommend the highest school track which they think is suitable for the child. In some states this recommendation is limiting the school track choice to the top. Parents finally choose the secondary school track from the (limited) set of available school tracks. Upgrading from lower tracks into higher ones is in general possible if the attended lower track is completed successfully. Students may also downgrade to a lower track at any time. After secondary school, individuals may either go into vocational training, which comprises classes at a vocational school as well as practical training from an employer, or enter tertiary education.[6] For the evaluation of the reform it is important to note that high ability children are tracked into the high track. Nonetheless, it is possible to dropout before obtaining a school degree. The high track takes the longest time until a school degree is accomplished and is the academically most demanding.

The reform under study In the 1950s and 60s there was no legal regulation of the school degree high-track students would receive when dropping out from the high track.[7] Figure 3.2 shows that close to 10 percent of all students who attend the 8th grade in high-track schools drop out in all birth cohorts from 1950 on and in Baden-Württemberg it was even observed that in the 1950s and 1960s, more than 20 percent of students dropped out from the high track without a degree (Peisert and Dahrendorf, 1965). The first regulations concerned students who just did not pass the high-track school degree in 13th grade. For dropouts from lower grades, however, only from 1965 on necessary regulations were started to be discussed. In 1965 the *Kultusministerkonferenz* (the Standing Conference of the Ministers of Education and Cultural Affairs of the Länder) announced a reform making school-leaving diplomas from the high track comparable to school degrees from lower tracks.[8] The aim was to make dropouts available for vocational training. Although the reform was announced at the federal level in

[5] Helbig and Nikolai (2015) provide insights in various dimensions into the school system of Germany including at which stage which degree can be obtained.

[6] See Biewen and Tapalaga (2017) and Buchholz and Schier (2015) for a good overview and description of school careers in the German school system with the NEPS adult cohort data.

[7] There is some evidence for Baden-Wüerttemberg that the custom was to count certificates for having passed 10th grade as middle school degree at least for some occupations for example in the public sector. Similar customs are not known for other states.

[8] The original name of the reform was 'Gleichwertigkeit von Abgangszeugnissen der Realschulen und Gymnasien mit Abschlusszeugnissen der Hauptschulen' (Equality of school leaving diploma of middle and high-track schools with basic school degrees). This means that also students in the middle track schools were treated by the reform. Additionally, changes in the school careers of high and middle track students might have influenced via spillover effects students in the low-track schools. In future research we plan to investigate reform effects for these groups separately in more detail. First results from the overall reform effect suggest that these groups reacted differently than the high-track students.

1965, the German states reacted to this announcement at different points in time, see table 3.1 (Helbig and Nikolai, 2015). While the first state to reform was Baden-Württemberg, Bavaria reformed only in 1982 and the states of East Germany reformed after re-unification. Thuringia reacted latest, only after a high-track school dropout perpetrated a shooting rampage in the city of Erfurt in 2002. Following this tragedy, the media discussed the missing perspective of high-track school dropouts and politicians reacted. In some states (e.g. Berlin) the reform granted low-track school degrees also retroactively.

The reform aimed at supporting students, who although probably of high-ability, struggled to enter the labor market without a school degree. Generally, students had to obtain a low-track school degree via some other path first. In our analysis we will focus on the reforms in West Germany between 1965 and 1982. We exclude the former East German states as the school system was not completely comparable. Students from Bremen where the reform took place in 1999 are only observed in the untreated state.

As we want to evaluate if the reform improved the situation for dropouts, we would like to know what students' outside options were when dropping out of school without a degree. As dropouts are hardly tracked over time, there is little knowledge about the pre-reform outside options. There is some narrative evidence from Peisert and Dahrendorf (1965) studying the state of Baden-Württemberg. Peisert and Dahrendorf (1965) interview students from high-track schools and track dropouts for some years after they left the high track. They find that the majority of dropouts are still in education in the years after the dropout to either obtain a middle or even a high-track school degree. The second biggest group is in public service or trade occupations which rewarded a certificate of 10th grade as middle school degree. Peisert and Dahrendorf (1965) describe the following reasons for dropout before obtaining the high-track school degree in the early 1960s: personal reasons, problems in school subjects, family reasons, the school, teachers, the peer group in school, and commuting to school. Personal reasons include that students do not want to go to school anymore or that they have an occupation in mind for which an academic degree is not necessary. Taking all topics together problems in school subjects would be the most stated reason for dropout.

3.3 Data

We combine three unique data sources to estimate the reform effects. First, we make use of rich and detailed survey data of the adult cohort of the National Educational Panel Study (NEPS) from 2010.[9] We extract attendance of the high track without accomplishing a school degree

[9] see Blossfeld et al. (2011) for more detail.

and our education- and labor-market-related outcomes from spell data on individual schooling and occupational careers. Second, we use data from various waves of the German Microcensus - a representative 1 percent sample of the German population - to test the common trend assumption and to use information on income that we merge with the NEPS data. Third, we use the years reported in table 3.1 as starting point of the treatment. We created a treatment indicator from the information provided by Helbig and Nikolai (2015). The indicator states if children who dropped out of the high track received a low-track school degree or not.[10]

NEPS The NEPS data provide detailed data on individual schooling and occupational careers of a representative sample of adults living in Germany. The data have two specific features enabling us to tackle our research question in a methodologically clear-cut manner: first, the data include information on the respective nature of dropping-out: we can differentiate whether a student left a school for good, whether the student switched to another school resp. to another school track, and at which point in time the student effectively ended her schooling career.[11] Second, the NEPS data report the individuals' location of schooling on federal state level. This is crucial for an analysis of past state reforms in a context where migration across states during an individual's lifespan cannot be ruled out.

From the NEPS adult cohort we use a subsample of 3878 adults who attended secondary school in a West German state (including Berlin) and are born between 1944 and 1983. NEPS asked in 2010 retrospectively all participants about their school careers documenting school types and locations of schools. We use these spell data to construct our main outcome variables which should capture important characteristics of school careers. Table 3.2 shows descriptive statistics for these outcome variables. The first variable *years of schooling* states the number of years a person reports to be in educational spells. The variable *longer in education than standard* is an indicator which is equal to 1 if a person stayed longer in the education system than it would have been expected given her final degree. It is the difference between *years of schooling* and calculated standard years of schooling from the last CASMIN.[12] The variable *no degree in last school episode* is an indicator which is equal to 1 if a person leaves the school system at a maximum of age 19 without reporting having obtained a degree in the last episode. If a person reports to having obtained a basic school degree in the last school episode, the indicator *basic school degree in last school episode* is equal to 1. *Downgrading* is an indicator which is equal to 1 if a person switches from the high track to a lower ranked school, i.e. basic

[10] The indicator does not distinguish between basic and middle school degrees as the type of degree varies heavily by state and the complexity was too high to document this detail (Helbig and Nikolai, 2015).

[11] This is in contrast to other studies that rely on school data reporting how many students left a school without degree but being ignorant of the subsequent educational path of the reported dropout.

[12] The CASMIN is short for 'Comparative Analysis of Social Mobility in Industrial Nations', and exists as classification of education since the 1980s.

or middle school.[13] Similarly, *upgrading* indicates if a person switches from a lower ranked school type to the high track. The *number of school episodes* counts how many schools a person attends and *age at leaving school system* contains the age at which a person switches from school episodes to vocational training (including university education), employment, military, or other types of spells.

The variables on school degrees are indicators as well, capturing reported school degrees at two different points in an individual's life. The first point is at the end of the last school episode between 11 and 19. The second point is in 2010 as highest accomplished school degree. The indicators differ as people can accomplish school degrees later on in life.

The occupational outcomes capture if an individual holds a vocational degree in 2010 and job quality in terms of average income in these jobs at age 20 and 35. The vocational degree indicator is constructed from the CASMIN. The average income in the job held at age 20 or 35 is merged information from the Microcensus and is described below.

From the background questionnaire we construct the control variables: sex, birth cohort, migration background, highest parental education level, and the number of siblings. Table 3.2 shows the summary statistics for the control variables. We construct 3-year bins as birth cohort dummies - each including 3 successive birth cohort years.[14]

Microcensus While the NEPS data offer many advantages with regard to our research question like detailed school spells and the location of schools, the dataset unfortunately lacks precise information on the income of the individuals. Our second data source, the German Microcensus, helps to overcome this problem. The German Microcensus contains cross-sectional, administrative data covering 1 percent of the German population in each wave since 1970. The data include income, occupations, and schooling (current school type for people in education and final degree for people out of education). First, we use these data to construct the retrospective, average income in the jobs individuals in our NEPS sample hold at age 20 and 35. For the age group 20 to 25 and 30 to 35 we construct in state-year-occupation-gender-age cells the average income. Income is always net income in about 7 bins. An occupation cell is a

13 We cannot distinguish between these school types in the spell data.

14 We do so because the number of observations per year of birth is small. Including dummies for each year of birth does not change the results.

3-digit KldB classification.[15] Second, we use the 1991, 1996, 2001, and 2011 waves to construct the share of people with a specific school degree by age at the reform year and use it for our analysis of the common trend assumption.

3.4 Identification strategy

For identifying the causal effect of the dropout reform we exploit the different timing of the reform between 1965 and 1999 in the 11 former West-German states and apply a DID approach. Our DID approach relies on the comparison of students in the highest track pre- and post-reform within and between states. For the baseline specification, we restrict the sample to include only students who had a high-track episode in their school career. One issue of evaluating reforms in general is selection into treatment. As soon as people know that the high track awards additional lower-track school degrees, selection of people into the high track might change. To avoid selection effects, our treatment is not only the interaction of age and attending at least the high track once in life. We define our treatment group as high-track students who are 11-19 years old in the reform year. With this restriction we put 93 students in the control group who chose to attend the high track after the reform at an older age than 11. Figure 3.3 shows for the 11 West German states our treatment and control groups. Black represents the cohorts at age 11 which are treated. White represents the pre-reform control group. Grey is the treatment group we exclude due to possible selection effects. In our main specification we estimate:

$$Y_{i,s,t} = \beta_0 + \beta_1 post\,reform_s + X_i\beta_5 + \mu_s + \mu_t + \epsilon_{i,s,t}. \tag{3.1}$$

$Y_{i,s,t}$ are the various outcome measures for school- and occupational-career characteristics of student i in state s of birth cohort t. The coefficient of interest is β_1, showing the effect of the reform for students who attend the academic track in the reform year. The matrix X_i contains all control variables. State- and birth-cohort-fixed effects are captured by μ_s and μ_t respectively. In our main specification standard errors are clustered on state level as the treatment varies on state level.[16]

The coefficient β_1 represents the causal effect of the reform if the assumptions of a DID framework hold. In our setting, first, the common trend assumption has to hold which implies that – in absence of the treatment – treatment and control group would lie on the same longer-run

[15] KldB is a German classification of occupations and stands for 'Klassifikation der Berufe'. It was introduced by the Federal Agency for Labor and the German Statistical Office. The most recent classification of 2010 is highly compatible with the international classification of ISCO.

[16] As we only have 11 German states the number of clusters is small. We apply wild-cluster-bootstrap methods to account for this.

trend with respect to the outcome variables. Second, the treatment effect we pick up with β_1 should not represent any other development than the reform.

The NEPS data do not allow a thorough evaluation of pre-reform trends as the number of observations per birth cohort and state are small and decrease with age of the survey participants. Shares of people with a specific school degree, therefore, fluctuate a lot by birth cohort and state. To obtain some evidence whether the states were at different trends in the years before the reform was introduced, we pool the German Microcensus of 1991, 1996, 2001, and 2011. As the first reform was introduced in 1965 we would like to observe birth cohorts who have completed their education already in 1965. As the first Microcensus wave available is the Census of 1970 for most states it is not possible to observe the direct schooling decisions of the pre-reform cohorts. We decided on the above years as in that time education or work induced migration will potentially play a minor role. There might be even more people returning to their place of birth at older ages. In general, in the Microcensus data we do not know in which states individuals were born or where they attended schools. Note that migration might bias any result also our presented evidence for the common trend assumption which should thus be interpreted with some caution.[17] For each birth cohort we calculate the share of people with a basic, middle, or high-track school degree per state. In figure B-1, B-2, and B-3 we plot the share of people with a specific degree against the age of the birth cohort when the reform was introduced. We assume that the cohort which was 20 in the year of the reform is the last resp. youngest cohort which was not affected by the reform. Younger people could have benefited from the reform if they still attended the academic track in the reform year.[18] The second assumption for this analysis of the common trend is, that the majority of people did not move to another state between school and the Microcensus survey.[19] This might in particular be an issue for the city-states and could explain why the state of Bremen (orange line) seems to be a special case in terms of degrees.[20] Figure B-1 shows that there is a trend of decreasing basic school degrees from the cohort which is 70 in the reform year to the cohort which is 20. Simultaneously, figure B-3 shows that high-track degrees become more common. The results for middle school degrees vary more by state and between the cohorts but overall there is a positive trend the younger the cohort. Particularly for the cohorts just out of the education system in the reform years, trends look similar, although levels vary (between 50 percent to 70 percent for low-track school degrees and 10 percent to 20 percent for high-track school degrees).

17 Therefore, we refrain from presenting a DID estimation with time varying treatment effects for the Microcensus data.

18 A student attending the high track is in most cases still in school at age 19.

19 As indicated beforehand in the Microcensus we only observe people at their current state of residence.

20 Bremen, as a city state might attract people with an academic school degree for university education. Additionally, it might attract people with university degree from the surrounding areas as many companies are located in cities.

To provide further evidence for a pre-reform common trend, we use official statistics from 1962 to plot figure B-4. It shows the share of children in 7th grade who attend the high track from 1950 to 1962 by state. Again the trends seem similar although the levels vary.[21] So far, the graphical evidence is a first step to mitigate concerns that the estimated effects could be due to diverging developments of specific states.

To avoid that we pick up other developments within states with the same timing as our reform, we investigated if other education reforms affecting secondary schooling were introduced simultaneously. So far, we do not find evidence for that but we are still exploring in this direction. From an empirical point we can rule out that we are capturing state-time trends and that specific states are driving our results. These robustness checks are presented in section 3.7.

3.5 Main Results

In this section we describe and discuss the results estimating equation 3.1. First, we consider direct reform effects on school careers of affected students. Second, we estimate effects on the school degrees they obtain. Third, we estimate if there are any longer run effects on occupational outcomes. The reported effects in this section are average treatment effects for the group of high-track students. Last but not least we estimate the effect of the reform on selection into the highest track.

3.5.1 School careers and degrees

Regarding school career outcomes we expect to find that the reform made high-track students switch less between school tracks because after the reform they can receive the lower track school degrees as well in the high-track school. This should reduce down- and upgrading. As regards the time spent in school, the number of school episodes, and the age when leaving the education system, the reform effect depends on the relative role of the following two incentives. 1) The incentive to leave school and enter the labor market before obtaining a high-track school degree. 2) The incentive to stay in the high-track school and try to successfully finish 13 years of schooling with a lower risk level.
Table 3.3 shows that overall, the reform changes school careers of students in the highest track as expected in a way that they become more stable and linear. Students spend almost one year more in school (column 1) which is about 70 percent of a standard deviation such that they are about half a year older when leaving the school system (column 3). It is not likely

21 The drawback here is that the time-distance to the reform introduction varies by state. As the time series is not long enough we cannot provide a similar plot as for the Microcensus data.

that this is due to students repeating classes more often after the reform as excess time in the highest track does not significantly increase (column 2). Taking these findings together indicates that students drop out later or finish school more often due to the reform. This is in line with a stronger incentive effect for trying to finish high-track school at a lower risk. Additionally, school careers stabilize as students reduce their school episodes (column 4) by half an episode which is 60 percent of a standard deviation. This means that they switch less often between different schools. This is also true for downgrading which is switching to a lower-track school (column 5). Downgrading is reduced by almost 90 percent of a standard deviation. Upgrading is slightly reduced as well but not significantly. As students downgrade less often they mechanically cannot upgrade to the highest track as much as before. These results are robust to wild-cluster bootstrapping of the standard errors as it is recommended by Cameron and Miller (2011). The third row shows the p-value for testing if the reform coefficient is zero obtained from wild-cluster bootstrapping the standard errors. The significance levels hardly change applying wild-cluster bootstrapping methods.
The treatment group contains students aged 11 to 19. Results might be driven by a specific age group of students as it is possible, for example, that the incentive to leave school and enter the labor market earlier is larger for the older students than for the younger students. If this is the case, we would expect that the effects we find are driven by the younger students and might go into another direction for the older ones. Table B-1 in the appendix shows that this is not the case. The effects always go into the same direction for all three age groups: age 11-14, age 15-17, and age 18 and older.[22] The significance levels are also almost the same for the different age groups. This suggests that the reform hardly incentivized students to enter the labor market immediately.

One aim of the reform was to provide students who otherwise would have held no school degree with a school degree. Therefore, we expect that the share of people without any school degree decreased with the reform while the share of people with school degrees should increase (particularly for low or middle track school degrees). Table 3.4 shows the estimated reform effects on the likelihood of holding various school degrees. Column 1 to 4 use information from the retrospective school spells on the school degree obtained in the last reported school episode. Column 5 to 8 use information on the highest school degree obtained in 2010. Column 1 shows that the likelihood to end the last reported school episode without any school degree is slightly, yet not significantly, reduced. This suggests that even before the reform high-track dropouts hardly completed their school career without any degree. It is more likely and complies with the anecdotal evidence that high-track dropouts obtained a lower track school degree at a lower track school and finished their school career with such a degree. In line with that story we find that students are less likely after the reform to finish their school

22 The exception is upgrading which is of course very unlikely for the older students as they would have to downgrade first and upgrade afterwards.

career with a basic school degree (column 2) and a middle school degree (column 3). They are, however, more likely to finish their school career with a high-track school degree (column 4). The same pattern holds for the highest accomplished school degree in 2010, see column 4 to 8. People who attended the highest track in the reform year hold significantly less middle school degrees but the share of people with a high-track degree increased by 60 percent of a standard deviation compared to people attending the high track before the reform. Again, the significance levels hold when applying wild-cluster bootstrapping methods (third row). In sum, table 3.4 delivers evidence that people are incentivized to stay at the high track and try to obtain a high-track school degree with the reduced risk of ending-up without any school degree.

The reform thus seems to motivate students to stay in the high track even if they are at risk of not achieving its completion. The consequence of staying in the high track is surprisingly beneficial for the students. As the estimates show, many of them actually pass the high-track school degree and thus qualify for university and different career paths. Therefore, in the next section we analyze if we can find longer-run effects of the reform on occupational outcomes.

3.5.2 Occupational outcomes

More years of schooling and higher degrees should positively affect students' labor market careers. Table 3.5 shows the reform effect on occupational outcomes. As students are pushed away from lower school degrees they are also less likely to hold a vocational school degree (column 1) but more likely to obtain a university degree (column 2). This push into higher education, however, is so far not visible in the quality of jobs held at age 20 and 35 ranked by income. Yet, at age 20 (column 3) the sample is reduced by about 500 people and the coefficient of the reform is negative. This could be due to individuals now being in university education at age 20 and therefore not reporting an occupation at that age. Future research will explore in more detail if the reform changed labor market careers and will investigate a larger variety of moments when jobs are held.

3.5.3 Selection into the academic track

As the reform reduced the risk of dropping-out of the high track without any school degree, we expect that (lower ability) students should select into the high track. Lavecchia et al. (2016) and Wölfel and Heineck (2012) show that risk aversion plays an important role in schooling decisions. Relaxing our sample restriction from above to students who attended the high track in the reform year, we apply two approaches to investigate the degree of selection into the high track. First, we analyze if the share of students attending the high track at different points of their school career increased with the reform. Second, we analyze if there are additional effects on the outcomes of table 3.3 and 3.4 for birth cohorts who could have selected into the high track.

For our first approach, table 3.6 shows that after the reform a higher share of students attended the high track – independent of measuring attending the high track as a dummy for having at least one high-track school episode, as attending the high track as first secondary school, or as attending the high track in 8th grade. However, the positive effect is only significant for the likelihood of having any high-track school episode.[23] The effect size is 5 percentage points which is about a tenth of a standard deviation.

For our second approach, we investigate whether the reform effect changes as soon as parents and children can take the new opportunities into account when deciding on their child's secondary school. In table B-2 and B-3 in the appendix we enlarge our sample to all available birth cohorts and include an extra dummy for the interaction of being in the high track after the reform but younger than 10 in the year of the reform. The coefficient of *post reform* shows the overall reform effect, the coefficient *post reform selected* captures an additional reform effect for cohorts which could have selected into the high track after the reform. The selection effects are only significant for the number of school episodes, downgrading, and upgrading. In these cases the selection effect has the opposite sign of the overall reform effect. The same pattern holds for degrees as can bee seen in table B-3. The additional selection effect is significant for holding no degree or a high-track degree at the end of one's school career. In these cases the selection effect also goes into the opposite direction. These findings are in line with negative selection into the high track. Students who are probably less suited for the high track select into it and when they struggle they switch schools more again.

3.6 Heterogeneity by parents' school degree

The aim of the reform was to provide dropouts from the high track with better labor market opportunities. Estimating average treatment effects, therefore, might attenuate the effect the reforms have on the target population which is the group of students which is potentially affected by a dropout. Ex-post it is hard to define the group at risk because not all members of that group actually drop out. Indicators like grades or repeating a grade are not documented in our data. In the following we investigate to which extent students with parents without a high-track degree profited from the reform differently than students with parents with a high-track degree. Dropping-out of the high track might be particularly an issue for children who do not have strong parental support to pursue a high-track school career. Therefore, the group of students with parents without a high-track degree might belong to the target population of the reform. The group of students with parents who hold a high-track degree is, however, small and includes only the most privileged students.

[23] If we apply robust standard errors instead of clustered ones the effects are significant for all three measures. In future research we will investigate into this more.

Comparing the results in table 3.7 panel A with those in panel B shows that all coefficients go into the same direction for both groups. Also the magnitude is mostly comparable except for excess time in education (column 2) which is negative and insignificant for students of parents with a high-track school degree. Overall, effects are larger and therefore more significant for students with parents who do not hold a high-track degree.

In order to detect differences in final school degrees table 3.8 panel A has to be compared to panel B. Again the effects go into the same direction for both groups. Yet, the other estimates – particularly the estimated reform effect on the accomplishment of a high-track school degree (column 4 and column 8) is twice as large for students with parents without a high-track school degree. These results suggest that the reform was especially beneficial for students with parents who do not hold a high-track school degree.

3.7 Robustness checks

In this section we test the reform effect for the whole population and address some identification issues of our DID approach. First, we do not restrict the sample to high-track school students and treat everybody who is 19 or younger in the year of the reform as being treated. Second, we include linear state-time trends, third, we exclude one state at a time.

3.7.1 Effect on the whole population

In the sections above we identify effects on a very small subgroup of the whole population of students within a state, namely students in the high track. There are two arguments in favor of estimating an overall effect. First, the reform also targeted students in middle-track schools who accomplished the 9th grade and awarded them with a basic school degree. Second, there might be spillover effects on students from other school tracks. Resource constraints could prevent high-track schools from getting over-enrolled while middle-track schools will try to attract students from the low-track schools to fill their seats. On the local labor markets it is possible that high track school dropouts with a school degree crowd-out students from the middle track schools. Table 3.9 shows that for the whole population the share of people without any school degree decreased significantly with the reform, all other effects, however, are close to zero and insignificant. This suggests that effects might go into different directions for different student subgroups. In future research we will explore reform effects on students in other school tracks in more detail.

3.7.2 Linear state trends

In DID approaches it is possible that the reform effect picks-up state-specific time trends and, in this case, cannot be interpreted as a causal reform effect. To ensure that state-specific time trends are not driving our results we include linear state trends into equation 3.1. The results are shown in table 3.10. It shows that our main findings for the increased stability of school careers and a push into more high-track degrees are robust to the inclusion of linear state trends.[24]

3.7.3 Leave-one out

In Germany education policy in general differs by state. It is possible that our reform effects are driven by specific states where the setting of the education system is peculiar. To make sure that no single state is driving our results we estimate all regressions in a restricted sample leaving one state out at a time. We do not report all results but only those for the largest states which contribute most observations to our analysis: Northrhine-Westphalia and Bavaria. Additionally we show results for leaving out Berlin, which was in part East Germany at the time of the reform. Table 3.11 panel A shows the results for years of schooling and the number of school episodes. Panel B shows the results for downgrading and the holding of a high-track degree. The magnitudes of the coefficients hardly change and the effects stay significant. This holds as well for leaving out any other state (not shown here).

3.8 Conclusion

In the context of the German school system, we investigate the effects of a school reform that rewarded high-track school dropouts with a low-track school degree if they at least accomplished the 9th grade. For identification we exploit that the reform was introduced at different points in time in the 11 West German states from 1965 to 1999. Detailed retrospective spell data on school and labor market careers from the NEPS allow us to identify exactly who attended the high track regardless of completing a degree or not and where – in terms of federal state – she attended it. Both are features many other data sets lack of. Our results show compliance with the reform in the sense that students are less likely to switch between schools and tracks.

Surprisingly, however, we find that the reform led to an increase in the number of high-track students who successfully finish high-school and enter university. This is in line with theoretical considerations on risk perception that changes students' motivation: The reform seems to

24 We can also include non-linear state-decade-fixed effects with the first decade of people born before 1950, the second people born between 1950 and 1959, the third people born between 1960 and 1969, the forth people born between 1970 and 1979, and the fifth people born after 1979. These decade dummies are interacted with the state dummies. Our results do not change if we include the state-decade dummies.

have reduced the perceived risk of trying longer in the high-track school and thus led to an increase in the high-track completion rate. The reform can therefore be interpreted as a low-cost intervention that changed the behavior of students in a beneficial way.

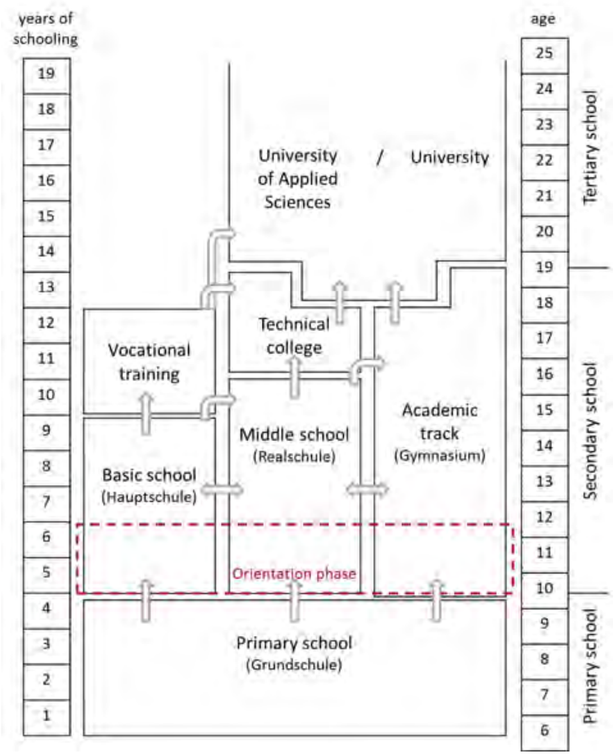
Interestingly, there is heterogeneity in the effects. Students from lower socio-economic status benefit particularly from the reform. As selection into treatment is one big issue of reform evaluations our main results stem from a reduced sample of students who were already enrolled in a high-track school in the reform year. Investigating selection effects into the high track in later cohorts suggests that selection played a role as we detect some negative selection of low-performing students into the high track.

In future research, we aim to estimate the total effect of the reform, including not only selection effects but also spillover effects in terms of enrollment patterns and completion rates in low-track schools.

One might argue that international policy conclusions that could be taken away from this reform evaluation are limited since we study a very specific context. The dropout reform took place in a system with tracking, grade repetition and strong implications of a degree for the labor market entrance. Thus, this reform might just not be applicable to another country's school system. Yet, our results show one important insight which is of importance to education policies in general: Along the school career, milestones which are awarded can motivate students to actually try longer and harder to successfully finish the next milestone. A prominent example of the introduction of such a milestone is the European Bologna reform that introduced the bachelor degree as a milestone in university graduation. The results of our evaluation serve as evidence that such milestones indeed change students' behavior and improve their outcomes.

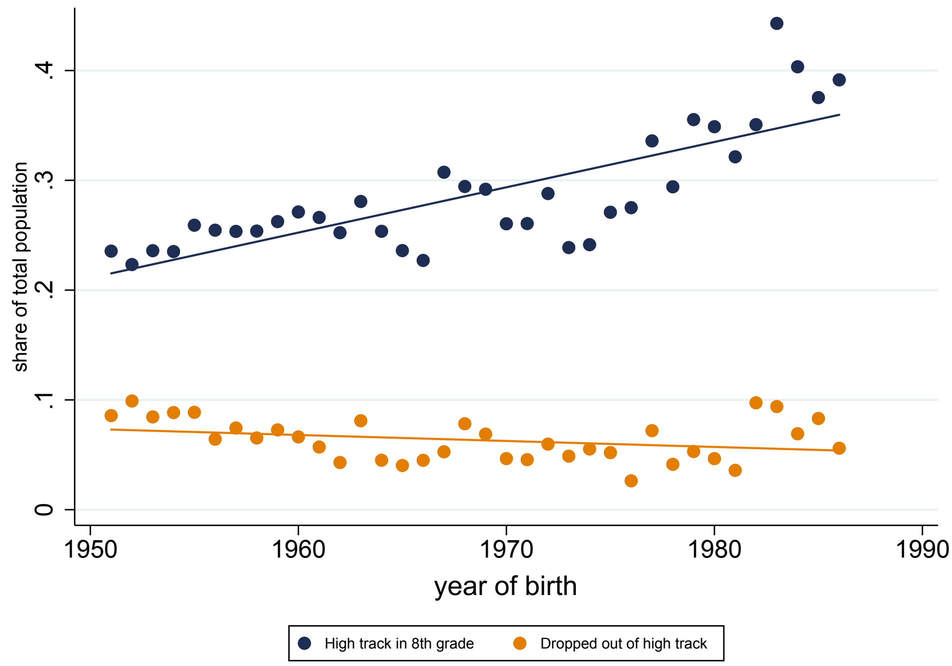
Figures and tables

Figure 3.1 : The Structure of the German School System



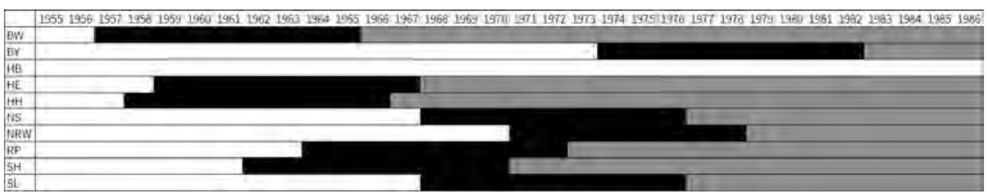
Notes: The figure depicts the German school system from primary school through university education. The left axis shows the year of schooling for each stage. The right axis the age of students in that stage.

Figure 3.2 : Dropouts from the high track in Germany



Notes: The blue dots show for birth cohorts from 1950 to 1986 the share of people who attended the high track in Germany at age 13/14 (8th grade). The orange dots shows for the same birth cohorts the share of people who dropped out from the high track among the people who attended the high track in 8th grade. The respective lines are the linear fit of the raw data points. Data source: NEPS SC6:7.0.0. Own calculations.

Figure 3.3 : Restricted sample: control and treatment group sample



Notes: The figure shows the restricted sample we use for our main analysis. The cohorts which turn 11 in the specific year are pre-reform cohorts if the cell is white and belong treatment group not prone to selection if the cell is black. Grey cells are the birth cohorts which belong to the treatment group prone to selection and are excluded from our analysis. Source: (Helbig and Nikolai, 2015).

Table 3.1 : Timing of the dropout reforms

state	reform
Baden Wuerttemberg	1965
Hamburg	1966
Hessen	1967
Schleswig Holstein	1970
Rheinland Pfalz	1972
Niedersachsen	1976
Saarland	1976
Berlin	1977
Nordrhein Westfalen	1978
Bayern	1982
Brandenburg	1991
Mecklenburg Vorpommern	1994
Bremen	1996
Sachsen	1999
Sachsen Anhalt	2002
Thueringen	2003

Notes: The table shows the timing of the dropout reforms in the German federal states. Source: (Helbig and Nikolai, 2015).

Table 3.2 : Summary statistics

	Mean	SD	Min	Max
School career				
Years of schooling	12.30	1.09	8	13
Excess time in education	0.31	0.46	0	1
Age at leaving school system	18.55	0.90	14	19
School episodes	2.56	0.85	1	8
Downgrading from high track	0.13	0.33	0	1
Upgrading into high track	0.12	0.32	0	1
School degree				
No degree in last school year	0.05	0.21	0	1
Basic school degree in last school year	0.02	0.14	0	1
Middle school degree in last school year	0.19	0.39	0	1
High track school degree in last school year	0.69	0.46	0	1
No degree in 2010	0.00	0.03	0	1
Basic school degree in 2010	0.01	0.11	0	1
Middle school degree in 2010	0.15	0.36	0	1
High track school degree in 2010	0.83	0.37	0	1
Occupational Outcomes				
University degree	0.54	0.50	0	1
Holding a vocational degree	0.40	0.49	0	1
Average income in job held at age 20	721.49	325.49	0	2,100
Average income in job held at age 35	2,180.49	1,482.49	0	7,750
Personal characteristics				
Female	0.49	0.50	0	1
Year of birth	1958.22	6.23	1944	1982
Migration background	0.08	0.26	0	1
Highest parental education				
No degree	0.00	0.05	0	1
Basic school degree	0.42	0.49	0	1
Middle school degree	0.20	0.40	0	1
High track school degree	0.11	0.32	0	1
University degree	0.27	0.44	0	1
Number of siblings				
No siblings	0.16	0.37	0	1
One sibling	0.38	0.49	0	1
Two or more siblings	0.46	0.50	0	1
Observations	1462			

Notes: The table shows summary statistics for the outcome and control variables constructed from the NEPS data.

Table 3.3 : Reform effect on school career

	(1) Years of schooling	(2) Excess time in education	(3) Age at leaving school system	(4) School episodes	(5) Downgrading	(6) Upgrading
post reform	0.725***	0.055	0.605***	-0.546***	-0.292***	-0.032
	(0.121)	(0.050)	(0.076)	(0.111)	(0.039)	(0.035)
p-value wild-cluster bootstrap	[0.000]	[0.476]	[0.000]	[0.004]	[0.004]	[0.48]
state FE	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes
observations	1462	1461	1460	1462	1460	1460
R^2	0.105	0.065	0.087	0.094	0.098	0.122
mean outcome	12.296	0.313	18.547	2.563	0.128	0.120
SD outcome	1.088	0.464	0.895	0.851	0.334	0.325

Notes: The table shows the results of estimating equation 3.1 for years of schooling, excess time in school, age at leaving the school system, the number of school episodes, downgrading, and upgrading described in detail in section 3.3. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. Standard errors are clustered on state level. The third row reports p-values of wild-cluster bootstrapping the standard errors. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 3.4 : Reform effect on school degrees

	degree in last school episode				final school degree 2010			
	(1) none	(2) basic	(3) middle	(4) high track	(5) none	(6) basic	(7) middle	(8) high track
post reform	-0.026	-0.037**	-0.256***	0.359***	0.002	-0.005	-0.235***	0.238***
	(0.026)	(0.016)	(0.040)	(0.051)	(0.001)	(0.011)	(0.030)	(0.031)
p-value	[0.456]	[0.184]	[0.004]	[0.000]	[0.372]	[0.688]	[0.004]	[0.000]
wild-cluster bootstrap								
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
observations	1460	1460	1460	1460	1462	1462	1462	1462
R^2	0.057	0.025	0.079	0.094	0.012	0.013	0.081	0.079
mean outcome	0.047	0.019	0.188	0.693	0.001	0.013	0.155	0.832
SD outcome	0.212	0.137	0.391	0.461	0.026	0.113	0.362	0.374

Notes: The table shows the results of estimating equation 3.1 for degrees obtained in the last school year (no degree, basic, middle, and high track school degree) and degrees held in 2010 (no degree, basic, middle, and high track school degree) described in detail in section 3.3. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. Standard errors are clustered on state level. The third row reports p-values of wild-cluster bootstrapping the standard errors. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Asterisks show the significance level:* = * = p<0.1, **=p<0.05, ***=p<0.01.

Table 3.5 : Reform effect on occupational outcomes

	(1) Holding vocational degree	(2) university degree	(3) Income in job at age 20	(4) Income in job at age 35
post reform	-0.140***	0.137***	-9.376	90.995
	(0.037)	(0.038)	(17.631)	(72.018)
state FE	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes
observations	1462	1462	938	1292
R^2	0.091	0.072	0.198	0.479
mean outcome	0.399	0.540	721.490	2180.486
SD outcome	0.490	0.499	325.492	1482.488

Standard errors in parentheses
$^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Notes: The table shows the results of estimating equation 3.1 for the likelihood of holding a vocational, or a university degree in 2010 and the quality of the job in terms of income held at age 20 and 35. The variables are described in detail in section 3.3. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 3.6 : Reform effect: selection into high track

	(1) Any high track episode	(2) First secondary school: high track	(3) Grade 8: high track
post reform	0.050*	0.034	0.025
	(0.027)	(0.025)	(0.022)
state FE	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes
observations	8621	8233	8621
R^2	0.180	0.175	0.177
mean outcome	0.361	0.362	0.432
SD outcome	0.480	0.481	0.495

Standard errors in parentheses
$^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Notes: For the overall population we estimate if the share of students in the high track increased after the reform. We distinguish between having any high track episode (column 1), reporting the high track as first attended secondary school (column 2), and attending the high track in 8th grade. *Post reform* captures if students are 19 or younger at the year of the reform. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 3.7 : Heterogeneity of reform effect on school careers by parental school degree

	(1) Years of schooling	(2) Excess time in education	(3) Age at leaving school system	(4) School episodes	(5) Downgrading	(6) Upgrading
Panel A. Subsample: parents with high track school degree						
post reform	0.278	-0.090	0.258*	-0.387**	-0.156**	-0.033
	(0.191)	(0.063)	(0.118)	(0.172)	(0.063)	(0.040)
observations	557	556	557	557	557	557
R^2	0.108	0.108	0.091	0.090	0.085	0.109
mean outcome	12.454	0.300	18.662	2.542	0.110	0.065
SD outcome	0.954	0.459	0.740	0.882	0.313	0.246
Panel B. Subsample: parents without high track school degree						
post reform	0.962***	0.126**	0.800***	-0.641***	-0.359***	-0.035
	(0.148)	(0.047)	(0.112)	(0.112)	(0.061)	(0.046)
observations	905	905	903	905	903	903
R^2	0.119	0.062	0.098	0.116	0.142	0.134
mean outcome	12.199	0.320	18.476	2.576	0.140	0.154
SD outcome	1.153	0.467	0.973	0.832	0.347	0.361
state FE	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the results of our subgroup analysis restricting the sample to people with parents who hold an high track school degree. We estimate equation 3.1 for this subgroup with years of schooling, excess time in school, age at leaving the school system, the number of school episodes, downgrading, and upgrading as outcome variables described in detail in section 3.3. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 3.8 : Heterogeneity of reform effect on school degrees by parental school degree

	degree in last school year				final school degree 2010			
	(1) none	(2) basic	(3) middle	(4) high track	(5) none	(6) basic	(7) middle	(8) high track
Panel A. Subsample: parents with high track school degree								
post reform	0.022	-0.038	-0.152***	0.211**	0.005	-0.002	-0.139***	0.136**
	(0.017)	(0.032)	(0.033)	(0.074)	(0.003)	(0.014)	(0.037)	(0.048)
observations	557	557	557	557	557	557	557	557
R^2	0.105	0.061	0.081	0.108	0.029	0.033	0.096	0.087
mean outcome	0.047	0.020	0.142	0.738	0.002	0.011	0.113	0.874
SD outcome	0.211	0.139	0.349	0.440	0.042	0.103	0.317	0.332
Panel B. Subsample: parents without high track school degree								
post reform	-0.053	-0.031***	-0.304***	0.429***	0.000	-0.007	-0.282***	0.289***
	(0.036)	(0.009)	(0.055)	(0.050)	(.)	(0.012)	(0.033)	(0.031)
observations	903	903	903	903	905	905	905	905
R^2	0.061	0.021	0.088	0.105	.	0.025	0.084	0.088
mean outcome	0.048	0.019	0.217	0.666	0.000	0.014	0.180	0.806
SD outcome	0.213	0.136	0.412	0.472	0.000	0.119	0.384	0.396
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the results of our subgroup analysis restricting the sample to people with parents who hold an high track school degree. We estimate equation 3.1 for this subgroup with degree obtained in the last school year (no degree, basic, middle, and high track school degree) and degrees held in 2010 (no degree, basic, middle, and high track school degree) described in detail in section 3.3. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 3.9 : Robustness check: effect on whole population

	school career			final school degree 2010			
	(1) Years of schooling	(2) School episodes	(3) Downgrading	(4) none	(5) basic	(6) middle	(7) high track
post reform	-0.087	-0.041	-0.010	-0.003**	-0.028	0.047	-0.016
	(0.136)	(0.066)	(0.009)	(0.001)	(0.032)	(0.034)	(0.031)
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
observations	3878	3878	3850	3878	3878	3878	3878
R^2	0.160	0.036	0.022	0.022	0.095	0.069	0.153
mean outcome	11.027	2.527	0.059	0.004	0.192	0.351	0.452
SD outcome	1.578	0.794	0.236	0.064	0.394	0.477	0.498

Standard errors in parentheses
$^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Notes: The table shows the reform effect on school careers and school degrees for the whole population (not only high track attendees). The coefficient of *post reform* is the treatment effect for students being younger than 19 in the year of the reform. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 3.10 : Robustness check: including linear state trends

	(1) Years of schooling	(2) No. of school episodes	(3) Downgrading	(4) no degree	(5) basic school degree	(6) middle school degree	(7) academic school degree
post reform	0.823***	-0.548***	-0.339***	0.001	-0.005	-0.276***	0.280***
	(0.072)	(0.130)	(0.028)	(0.001)	(0.012)	(0.019)	(0.024)
linear state trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
observations	1462	1462	1460	1462	1462	1462	1462
R^2	0.138	0.100	0.113	0.016	0.017	0.103	0.102
mean outcome	12.296	2.563	0.128	0.001	0.013	0.155	0.832
SD outcome	1.088	0.851	0.334	0.026	0.113	0.362	0.374

Notes: The table shows robustness checks including in our estimation equation 3.1 linear time-state trends. The outcome variables are years of schooling, school episodes, downgrading, holding no degree, a basic, middle, or high track school degree in 2010. The variables are described in detail in section 3.3. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

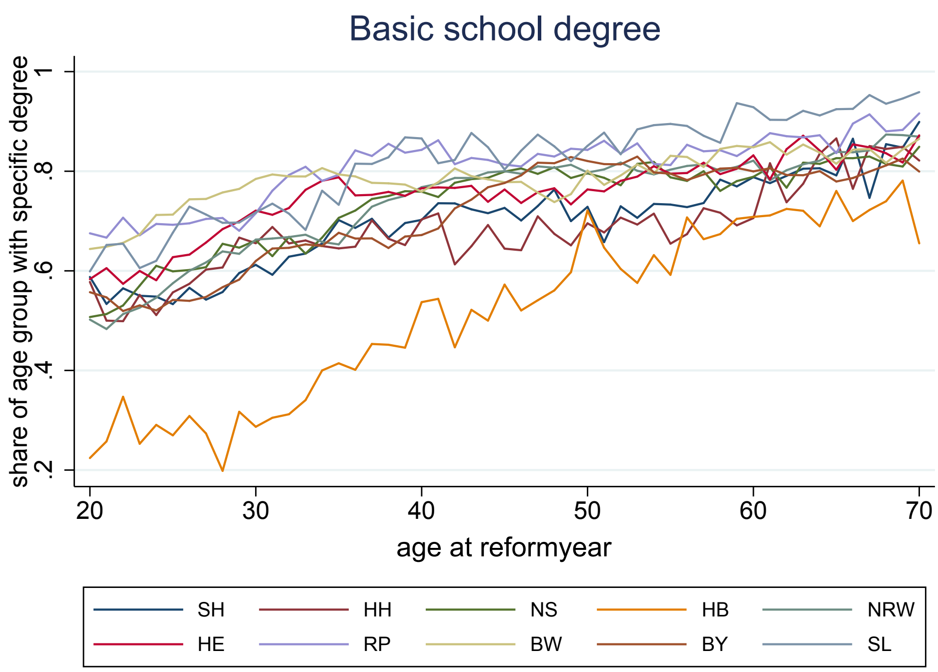
Table 3.11 : Robustness check: leave one state out at a time

Panel A.	Years of schooling			School episodes		
	(1)	(2)	(3)	(4)	(5)	(6)
	no NRW	no Bavaria	no Berlin	no NRW	no Bavaria	no Berlin
post reform	0.672***	0.770***	0.746***	-0.530***	-0.683***	-0.554***
	(0.187)	(0.156)	(0.124)	(0.133)	(0.151)	(0.111)
state FE	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes
observations	941	1095	1406	941	1095	1406
R^2	0.088	0.121	0.111	0.088	0.129	0.093
mean outcome	12.243	12.291	12.303	2.589	2.536	2.568
SD outcome	1.130	1.077	1.092	0.881	0.810	0.857
Panel B.	Downgrading			high track school degree		
	(1)	(2)	(3)	(4)	(5)	(6)
	no NRW	no Bavaria	no Berlin	no NRW	no Bavaria	no Berlin
post reform	-0.287***	-0.287***	-0.291***	0.238***	0.287***	0.237***
	(0.061)	(0.061)	(0.041)	(0.044)	(0.046)	(0.032)
state FE	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes
observations	939	1094	1404	941	1095	1406
R^2	0.105	0.084	0.096	0.088	0.083	0.077
mean outcome	0.140	0.103	0.130	0.815	0.853	0.828
SD outcome	0.347	0.304	0.337	0.388	0.354	0.378

Notes: The table shows robustness checks excluding one state at a time from our estimation equation 3.1. The outcome variables are years of schooling, school episodes, downgrading, and holding a high track school degree. The variables are described in detail in section 3.3. Columns 1 and 4 show results for leaving out Northrhine-Westphalia, columns 2 and 5 show results for leaving out Bavaria, and column 3 and 6 show results for leaving out Berlin. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

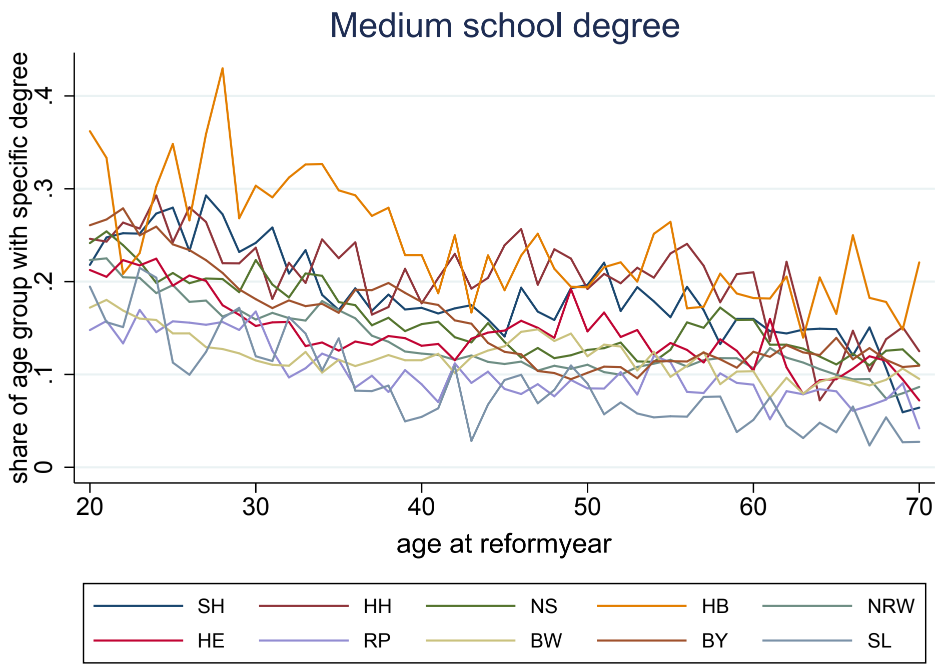
Appendix B

Figure B-1 : Common trend assumption: share of people with basic school degree by state pre-reform



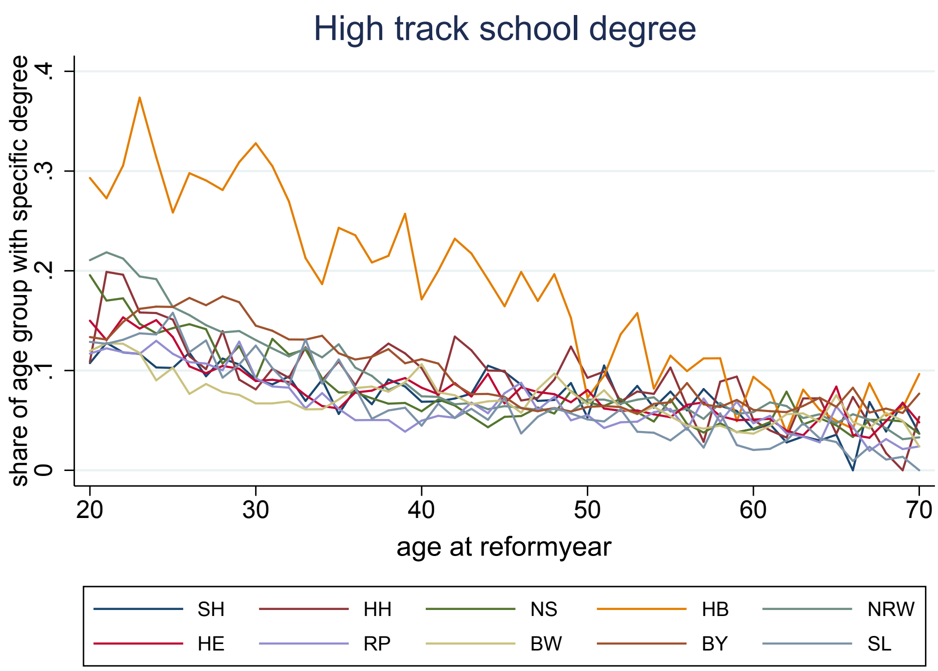
Notes: The figure shows the share of people who hold a basic school degree by birth cohort and state. Data source: Microcensus 1991, 1996, 2001, 2011. Own calculations.

Figure B-2 : Common trend assumption: share of people with middle school degree by state pre-reform



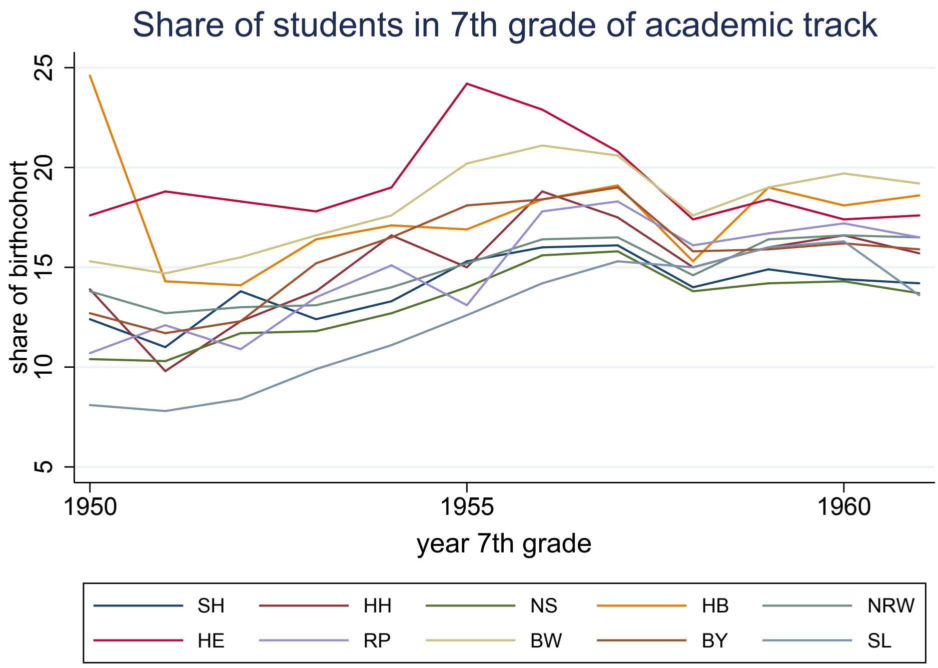
Notes: The figure shows the share of people who hold a middle school degree by birth cohort and state. Data source: Microcensus 1991, 1996, 2001, 2011. Own calculations.

Figure B-3 : Common trend assumption: share of people with high school degree by state pre-reform



Notes: The figure shows the share of people who hold an high school degree by birth cohort and state. Data source: Microcensus 1991, 1996, 2001, 2011. Own calculations.

Figure B-4 : Common trend assumption: share of children in high track by state 1950-1962 (pre-reform)



Notes: The figure shows for the years 1950 to 1962 the percentage of the specific birth cohorts who attend the high track in 7th grade by state. Source: (Wissenschaftsrat, 1964). Own calculations.

Table B-1 : Reform effect on school careers by age group

	(1) Years of schooling	(2) Excess time in education	(3) Age at leaving school system	(4) School episodes	(5) Down-grading	(6) Up-grading
11-14	0.560*	0.054	0.387**	-0.657***	-0.274***	-0.121***
years post reform	(0.270)	(0.071)	(0.142)	(0.152)	(0.061)	(0.036)
15-17	0.680***	0.037	0.599***	-0.636***	-0.293***	-0.076*
years post reform	(0.153)	(0.053)	(0.092)	(0.124)	(0.049)	(0.039)
18+	0.796***	0.072	0.649***	-0.444***	-0.295***	0.024
years post reform	(0.102)	(0.052)	(0.060)	(0.131)	(0.030)	(0.044)
state FE	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes
observations	1462	1461	1460	1462	1460	1460
R^2	0.108	0.066	0.093	0.097	0.098	0.131
mean outcome	12.296	0.313	18.547	2.563	0.128	0.120
SD outcome	1.088	0.464	0.895	0.851	0.334	0.325

Notes: In the table we estimate the reform effect on school careers for three different age groups of high track school attendees. The first age group is 11-14 years old in the year of the reform, the second 15-17, and the third 18 and older. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table B-2 : Additional reform effect on school careers by selection

	(1) Years of schooling	(2) Excess time in education	(3) Age at leaving school system	(4) School episodes	(5) Downgrading	(6) Upgrading
post reform	0.297**	0.027	0.268***	-0.274***	-0.145***	-0.051*
	(0.125)	(0.022)	(0.084)	(0.083)	(0.038)	(0.025)
post reform selected	0.058	0.035	0.053	0.326***	0.027**	0.205***
	(0.058)	(0.026)	(0.051)	(0.095)	(0.011)	(0.061)
state FE	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes
observations	3723	3721	3719	3723	3719	3719
R^2	0.095	0.033	0.074	0.071	0.036	0.089
mean outcome	12.497	0.292	18.675	2.656	0.134	0.169
SD outcome	0.957	0.455	0.761	0.908	0.341	0.375

Notes: In this table we increase the sample to the possibly selected treated individuals from later birth cohorts. The table shows the results of estimating equation 3.1 for years of schooling, excess time in school, age at leaving the school system, the number of school episodes, downgrading, and upgrading described in detail in section 3.3. Additionally to the coefficient of *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform, we report the treatment effect for the selected treatment group *additional selection effect*. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table B-3 : Additional reform effect on school degrees by selection

	degree in last school year				final school degree 2010			
	(1) none	(2) basic	(3) middle	(4) high track	(5) none	(6) basic	(7) middle	(8) high track
post reform	-0.014	-0.018	-0.142***	0.185***	-0.001	-0.003	-0.104***	0.107***
	(0.011)	(0.011)	(0.038)	(0.048)	(0.001)	(0.006)	(0.030)	(0.030)
post reform selected	0.016*	-0.005	0.004	-0.053*	0.002	0.001	-0.014	0.011
	(0.008)	(0.006)	(0.017)	(0.026)	(0.001)	(0.006)	(0.020)	(0.018)
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
observations	3719	3719	3719	3719	3723	3723	3723	3723
R^2	0.027	0.016	0.053	0.059	0.007	0.012	0.049	0.054
mean outcome	0.039	0.016	0.151	0.737	0.001	0.010	0.119	0.871
SD outcome	0.194	0.126	0.358	0.440	0.023	0.098	0.324	0.335

Notes: In this table we increase the sample to the possibly selected treated individuals from later birth cohorts. The table shows the results of estimating equation 3.1 for degrees obtained in the last school year (no degree, basic, middle, and high track school degree) and degrees held in 2010 (no degree, basic, middle, and high track school degree) described in detail in section 3.3. Additionally to the coefficient of *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform, we report the treatment effect for the selected treatment group *additional selection effect*. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

4 Predicting Choice of Math-Intensive Occupations by School Grades: The Role of Ability, (Rank) Signals, and Taste[*†]

4.1 Introduction

Computerization threatens a substantial share of occupations (Frey and Osborne, 2017) and technological change has increased the demand for cognitive skills (Goldin and Katz, 2008). It is thus important to understand the determinants of occupational choice. However, identifying these determinants has not proven easy. While economic factors, such as expected earnings, explain only little, it appears that differences in tastes across individuals—typically not observed in existing studies—are the most important factor in choosing a field of study and, hence, an occupation (Wiswall and Zafar, 2015).[1]

We study determinants of occupational choice in Germany and find that individuals' school grades are significantly associated with chosen occupations. In particular, individuals with good math grades—relative to their German-language grades—tend to choose math-intensive occupations.[2] Because we use the grade difference across two subjects, we implicitly account for any level effects, such as students with good math grades and good German grades choosing occupations that require high skills in both math and reading. The positive association between grade difference and the math intensity of the chosen occupation is robust to including school fixed effects, implying that we are comparing only individuals within the same local labor market. We investigate five different factors that may drive this association: (1) absolute

* This chapter is based on joint work with Marc Piopiunik.

† Financial support by the German Research Foundation (DFG) through Priority Program (SPP) 1646 "Education as a Lifelong Process" is gratefully acknowledged.

1 Arcidiacono (2004), Beffy, Fougère, and Maurel (2012), and Gemici and Wiswall (2014) find similar evidence. Similarly, Zafar (2013) finds that gender differences in tastes for college majors are the most important determinant of the gender gap in major choice, whereas beliefs about ability and major-specific earnings explain only little of this gap.

2 To be precise, we use the difference in the classroom's ordinal rank between math grade and German grade. We prefer grade *ranks* instead of absolute grades because teacher-assigned grades may vary systematically across the subjects math and German. For example, German-language teachers may assign the best grade to the best students in their classroom, whereas math teachers may assign only the second-best grade to their best students (or vice versa). Results are similar, however, when using the simple math-German grade difference instead. In the remainder of the chapter, we use the term "grade difference" to refer to the difference between math grade rank and German grade rank.

achievement in math and reading; (2) parents' occupations; (3) individuals using the grade difference as a signal of their subject-specific ability; (4) employers using the grade difference as a signal of applicants' relative skills; and (5) the role of tastes. We find that the first four factors do *not* explain the positive association between grade difference and math intensity of chosen occupation. However, we find that differences in tastes across individuals—measured immediately *before* entering the labor market—completely explain this association, suggesting that tastes play an important role in occupational choice.

We use panel data from the National Educational Panel Study (NEPS), which follows 9th-grade students into the labor market. These students choose apprenticeship training in an occupation, which typically starts after 9th or 10th grade. Importantly, only a minority of apprenticeship graduates switch occupations after completing the vocational training (Seibert, 2007; Seibert and Wydra-Somaggio, 2017). The NEPS data contain measures of individuals' math and reading achievement, assessed just before choosing an occupation. As we include school fixed effects, we effectively compare only individuals from the same school who experienced the same overall school quality and the same occupations and employers within their (small) local labor market.[3]

We quantify the math intensity of occupations using two complementary measures. First, we compute the average math *skill level* of current workers in each three-digit occupation, using the adult cohort from NEPS. Second, we compute the average math *skill requirements* (no skills, basic, or advanced) in each three-digit occupation. For this measure, we use two recent waves of the Qualification and Career Survey (QaC), in which employees reported the requirements of their current job with respect to various domains (e.g., mathematics). For both types of math intensity measures, in addition to using the math level, we also use the difference between math and reading/language skills to eliminate any level effects, for example, some occupations demanding high skills in both math and reading.

While individuals' absolute achievement in math and reading are related to the math intensity of chosen occupations, they do not explain the positive association between grade difference and math intensity of chosen occupation. Additionally controlling for the level of math grades does not change the coefficient of interest either, suggesting that the association is not simply driven by individuals with (very) good math grades.

[3] Note that apprenticeship trainees tend to stay in the region where they went to school, thus are geographically rather immobile (BMBF, 2015). To some extent, this is likely due to the fact that these individuals are only about 17 years old when starting their vocational education.

Parents' occupation may be an important determinant of occupational choice, given that existing studies find a correlation between parents' and children's occupation for Germany, see Constant and Zimmermann (2004). Furthermore, Zafar (2012, 2013) finds that parental approval is one of the most important factors underlying the choice of majors for college students in the United States. In our setting, parental approval may be even more important since individuals are only about 16 or 17 years old when choosing their occupation.

Since teachers in Germany typically use relative grading to assess students' performance, we exploit quasi-random variation in classmates' average achievement to estimate the causal effect of receiving a math ability signal (i.e., higher/lower math grade rank relative to the German grade rank) on occupational choice.[4] Under relative grading, if a given student's classmates perform poorly in math but well in reading, then she tends to receive a good math grade, but a poor German grade, conditional on her own achievement in math and reading. We therefore exploit the quasi-random variation in classmates' relative achievement (math vs. reading) across two classrooms within the same school as an instrument for the grade difference of a given student. While the first stage is strong (i.e., evidence for relative grading), we find no evidence that random variation in the math ability signal (induced by teachers grading relatively) affects students' occupational choice.[5]

Any observed occupation is the result of the interaction between the supply of job seekers and the demand of firms. Because firms in Germany typically observe the math and German grades on the (written) applications for apprenticeship positions, the positive association between grade difference and math intensity of observed occupation may arise if employers use the grade difference as a signal of applicants' (domain-specific) skills. To rule out this interpretation, and thus provide evidence for a supply-side effect, we use the dream jobs that individuals reported at the start of 9th grade, that is, *before* they started to apply for apprenticeship positions. We find a positive association between grade difference and the math intensity of the dream job, which is of similar magnitude as the association with the math intensity of actual

4 In general, there are two types of grading schemes for assessing students' performance. Under *absolute grading*, grades depend on students' own performance only and are independent of their classmates' performance. Under *relative grading*, also known as "grading on a curve," students' grades depend on their positions, that is, their ordinal rank, in the class's achievement distribution. Relative grading is used, for example, in colleges in the United States and in German schools see, e.g., Piopiunik and Schlotter (2012); Milek, Luedtke, Trautwein, Maaz, and Stubbe (2009).

5 Using quasi-random variation in classmates' average achievement is related to studies that investigate the impact of students' ordinal rank in the class (or cohort) on later educational outcomes (Murphy and Weinhardt, 2014; Elsner and Isphording, 2017). A crucial difference is that classmates' achievement in our setting matters because teachers employ relative grading, whereas in the other settings, ordinal rank matters through its impact on students' perceived ability or confidence in a subject for a review on this so-called Big-Fish-Little-Pond Effect, see Marsh, Trautwein, Lüdtke, Baumert, and Köller (2007).

occupations. This suggests that the association between grade difference and math intensity of chosen occupations is not driven by employers.

Concerning economic determinants of occupational choice, we find a clear association between occupation-specific average hourly wages (unemployment rates) and the *level* of math grades. However, these economic attributes are unrelated to individuals' grade *differences*. This is not surprising since grade differences net out any level effects, such as individuals with good math grades *and* good German grades choosing high-paying occupations or occupations with low unemployment risk.

While existing studies suggest that differences in tastes across individuals are the most important factor in choosing a field of study, they do not observe tastes directly e.g., Wiswall and Zafar (2015). We contribute to the literature by using measures of individuals' tastes elicited just before entering the labor market. In particular, two dimensions of tastes—practical-technical interests and social interests - drive the positive association between grade difference and math intensity of chosen occupation. Individuals with strong practical-technical interests are substantially *more* likely to choose math-intensive occupations, whereas individuals with strong social interests are substantially *less* likely to choose math-intensive occupations.[6] This finding provides direct evidence on the crucial role played by tastes for occupational choice. While we cannot make strong causal claims, we find an effect of tastes on occupational choice when instrumenting own tastes with the average tastes of classmates, conditional on own grades and classmates' mean achievement.[7] While the existing literature finds evidence that individuals tend to choose fields in which they have a comparative advantage (Kirkeboen et al., 2016),[8] we provide direct evidence that tastes play an important role in occupational choice.

The remainder of this chapter is structured as follows. Section 4.2 describes the panel data that follow 9th-grade students into the labor market and the data sets on adult workers used to compute the math intensity of occupations. Section 4.3 lays out our empirical model. Section 4.4 reports the main results on the association between grade difference and math intensity of chosen occupation, followed by the results for the factors that may drive this association. Section 4.5 concludes.

6 Practical-technical interests and social interests explain between 10 and 27 percent of the variation in the math intensity of chosen occupations.
7 However, we cannot rule out that teachers influence both own tastes and classmates' tastes.
8 This is in line with a Roy model of self-selection (Roy, 1951).

4.2 Data

For our analysis we combine rich survey data on individuals in 9th grade who, one or two years later, enter an occupation in the labor market with various measures of the math intensity of occupations, obtained from other data sets. Furthermore, we also merge information on average earnings and unemployment rates from administrative data for each occupation.

4.2.1 NEPS panel data

In 2010, the German National Educational Panel Study (NEPS) started following 9th grade students, that is, students in their last year of compulsory general education.[9] In Germany, after 9th or 10th grade, students either stay in school and continue an academic school career (in Gymnasium) or enter the labor market via an apprenticeship training.[10] The NEPS data include information on students, school environment, and achievement in standardized math and reading tests. Additionally, the data contain information on the type of occupation in which students have apprenticed (based on telephone interviews and online surveys). During 9th grade, NEPS interviews students in the fall and spring to more precisely document the transition into the labor market after compulsory education. Individuals who want to enter the labor market after 9th grade usually start their application process in the spring. Most datasets either lack comparable data on individual achievement and classmates before entering the labor market or lack information on occupations in the labor market. NEPS samples students in a two-step procedure. First, a representative set of German schools is drawn. Within these schools, if 9th grade contains more than one class, two classes are sampled. In general, students can choose not to participate in NEPS; however, if schools support the survey, most students participate (at least either in the survey or test).[11] In starting cohort 4, basic school students are oversampled. This works in favor of our project as basic school students are the most important target group for engaging in an apprenticeship and entering the labor market directly after compulsory schooling. We restrict our sample to students who report an apprenticeship position after spring 2011 and do not distinguish between students who start an apprenticeship in 2011, 2012, or 2013. For grade rank calculations, we use all students who participate in the fall 2010-survey, even though some of these students will continue an academic school career and therefore be dropped from our sample for the analysis. However,

9 This chapter uses data from the National Educational Panel Study (NEPS): Starting Cohort Grade 9, doi:10.5157/NEPS:SC4:9.0.0 (Blossfeld et al., 2011). From 2008 to 2013, NEPS data were collected as part of the Framework Program for the Promotion of Empirical Educational Research funded by the German Federal Ministry of Education and Research (BMBF). As of 2014, NEPS is carried out by the Leibniz Institute for Educational Trajectories (LIfBi) at the University of Bamberg in cooperation with a nationwide network.

10 Basic school ends after 9th grade, middle school after 10th grade. The majority of students enter the labor market after grade 10, that is, after graduating from middle school or after completing an additional, voluntary 'qualification' year in basic school.

11 On average, in each class about 70 percent of students participate.

to minimize measurement error in grade ranks, we use the math and reading achievement of all observed students in the classroom.

4.2.2 Math intensity of occupations

We quantify the math intensity of occupations using two complementary measures. First, we compute the average math *skill level* of current workers in each three-digit occupation, using the adult cohort from NEPS. This sample (starting cohort 6) is representative of adults in Germany who have completed their education and, in 2010, were between 21 and 65 years old. The sample is completely independent from starting cohort 4 and contains about 11,700 adults, whose math and reading achievement was assessed in 2010. For each 3-digit ISCO occupation code, we calculate workers' average math and reading achievement.[12]
Second, we compute the average *skill requirements* in each three-digit occupation. For this measure, we use two recent waves of the Qualification and Career Survey (QaC), a survey of employees conducted by the German Federal Institute for Vocational Training (Bundesinstitut für Berufsbildung, BIBB) and the Research Institute of the Federal Employment Agency (Institut für Arbeitsmarkt- und Berufsforschung, IAB). To increase sample sizes, we pool the most recent waves from 2005/2006 and 2011/2012, each covering about 20,000 employed and self-employed individuals aged 15 and older.[13] The surveys cover a wide range of industries, including services and manufacturing.

Survey participants were asked to indicate, for several domains, the skills required in their current occupation, with the three answer categories being "no skills," "basic skills," and "advanced skills." For math skill requirements, we use the domain "skills in mathematics, specialized calculus, and statistics." For reading skill requirements, we use answers to the domain "skills in German, written expression, and orthography."[14] For the analysis, we use three different measures of occupational requirements at the individual level: (i) the original outcome with the three values 0 for no skills, 1 for basic skills, and 2 for advanced skills; (ii) a dummy variable indicating whether any math skills (either basic or advanced) are required; and (iii) a

12 We cannot use a finer occupation classification (at the four or five digit-level) as occupation cells would become too small. Using median skills instead of mean skills leads to similar results.

13 The first four waves were conducted in 1979, 1985/1986, 1991/1992, and 1998/1999. Individuals in jobs that are part of vocational training, such as apprenticeships, are excluded in the surveys.

14 The question, in the original German version, reads as follows: "Ich lese Ihnen nun verschiedene Kenntnisgebiete vor. Bitte sagen Sie zu jedem Gebiet, ob Sie bei Ihrer derzeitigen Tätigkeit als <Tätigkeit einblenden> diese Kenntnisse benötigen und wenn ja, ob Grundkenntnisse oder Fachkenntnisse. Wenn Fachkenntnisse nur auf einem Teilgebiet benötigt werden, geben Sie bitte trotzdem Fachkenntnisse an." The two items that we use for math and German-language requirements read as follows: "Kenntnisse im Bereich Mathematik, Fachrechnen, Statistik" and "Kenntnisse in Deutsch, schriftlicher Ausdruck, Rechtschreibung," respectively.

dummy variable indicating whether advanced math skills are required. We then aggregate these individual-level measures to the level of two-digit occupations.

For both types of math intensity measures—skill level and skill requirement—in addition to using the math level, we also use the difference between math and reading/language skills to eliminate any level effects, for example, some occupations demanding high skills in both math and reading. For skill requirements, we use the measure (i), which is based on the original three-point scale, to computer this math-reading difference. Additionally, we rank occupations by average earnings and unemployment rates, using administrative data from the German Statistical Office at the 5-digit occupation level. In the next section, we describe summary statistics of the NEPS student panel data and of the occupational characteristics.

4.2.3 Summary statistics

Table 4.1 shows summary statistics for our main explanatory, other student characteristics, and outcome variables. Our sample consists of 5,045 students who transit into an apprenticeship. The main variable of interest is the grade difference, which is the difference between the rank of the math grade in class and the rank of the German-language grade in class. students' grades are averages of the grade received at the end of 8th grade and in the middle of 9th grade. The grade difference varies between -1 and $+1$. A value of $+1$ means that the student has the best math grade in class and the worst German grade in class. At the other extreme, a value of -1 means that the student has the best German grade in class and the worst math grade in class. A value of 0 means that the student has the same grade rank in both subjects (e.g., the best grade in both subjects or the worst grade in both subjects or something in between). Figure C-1 in the appendix shows the distribution of our key explanatory variable. As expected, grade difference is normally distributed, with a mean of roughly zero. Achievement in math and reading are z-standardized within our sample. The sample is 47 percent female and about one-quarter has migration background. On average, we observe 18 students per class, ranging from 6 to 35 students.[15]

The math and reading achievement of workers have been z-standardized within the overall NEPS adult sample. As our estimation sample only includes apprenticeship occupations—but not occupations that require tertiary education—it is not surprising that the average math skill level of workers is below zero in our sample. The highest math skill level, with value 1, are found in engineering (telecommunications engineers and electrical engineers). The difference

[15] Comparing these numbers of observations with categories for class sizes reported by teachers shows that on a weighted average, 70 percent of all students in class participate in NEPS. The minimum is 22 percent. Reported class sizes by the teachers, however, are sometimes missing and some reports of class sizes are lower than the number of participants.

between math and reading level is close to zero on average. The largest (positive) difference is observed for glass and ceramics plant operators and stationary plant and machine operators, who have a math level below zero. For the above-mentioned engineers with the highest math level, the difference is about 0.7. The first measure for skill requirement shows, with a mean of 0.74, that three-quarters of the occupations chosen by our students require basic math skills; however, only one-quarter requires advanced math skills. Occupations that require the highest level of advanced math include agriculture, science occupations, engineering, and machinists. The average difference in math and language requirements is slightly negative, suggesting that language requirements are, on average, higher than math requirements in the chosen occupations.

Average earnings are measured by the gross hourly wage (in Euro) and reported for each 5-digit KldB classification code. The average monthly apprenticeship pay is 790 Euro, and the average unemployment rate in the chosen occupation is 8.4 percent. Finally, individuals report how satisfied they are with their apprenticeship position in 2012/2013, ranging from 0 (not satisfied at all) to 10 (completely satisfied); the average is 8.27 among individuals in our sample.

4.3 Empirical strategy

To estimate the association between grade difference and math intensity of chosen occupation, we run the following OLS model:

$$\begin{aligned} y_{i,c,s} = \alpha + \beta grade\, difference_{i,c,s} + \gamma \boldsymbol{A_{i,c,s}} + \\ \delta \boldsymbol{X_{i,c,s}} + \lambda_s + \kappa Z_{i,c,s} + \epsilon_{i,c,s}, where \end{aligned} \tag{4.1}$$

$y_{i,c,s}$ represents the math intensity of the occupation of individual i who attended class c in school s. The math intensity is measured by the skill level of workers in that occupation and by the occupation's math requirements, respectively. $Grade\, difference_{i,c,s}$ is the difference between the grade rank individual i holds in math class c and in German class. The grade difference is continuous and varies between -1 and $+1$.[16] The coefficient β is the focus of interest. The vector $\boldsymbol{A_{i,c,s}}$ contains flexible measures (third-order polynomials) of individual i's math and reading achievement, which have been assessed in grade 9. The vector $\boldsymbol{X_{i,c,s}}$ contains student characteristics, such as gender, age, and migration background, as well as family background characteristics, such as number of books at home and highest level of parental education. We always include school fixed effects, λ_s, so as to compare only individuals who have been subject to the same school environment and who face the same potential employers

[16] In table C-1, we use the absolute grade difference as well as the math and German grade levels alternatively. Our results are robust to using these alternative measures.

in the same local labor market when choosing an occupation. $\epsilon_{i,c,s}$ is an idiosyncratic error term. Standard errors are clustered at the school level.

Since the grade difference is based on the *difference* between math and German, all factors that similarly affect the grade rank in math and the grade rank in German are already absorbed. Hence, general ability and non-cognitive skills, such as conscientiousness and grit, do not bias the coefficient β, as long as these factors have a similar influence on both the math and German grades. Therefore, any bias in β must be due to subject-specific influences.
As the relative achievement in math versus reading is strongly related to the math intensity of the chosen occupation (see figure C-3), we always control flexibly for both math and reading achievement (except for the very first baseline results; see panel A of table 4.2). Therefore, only subject-specific factors that are unrelated to achievement differences can bias β, our coefficient of interest. Furthermore, by additionally controlling for the math grade level, we show that β is not driven by comparing students with good math grades to students with poor math grades.

Controlling for absolute achievement with $\boldsymbol{A_{i,c,s}}$, the term $Z_{i,c,s}$ captures varying control variables that we successively include to investigate two further factors that may drive the positive association between grade difference and math intensity of occupation: parents' occupation (see section 4.4.2) and tastes (see section 4.4.5).

4.4 Grade difference and math-intensity of occupation

We first present baseline results, without and with controls for absolute achievement (section 4.4.1). Then, we investigate the role of parents' occupation in section 4.4.2. Using an IV approach, we assess whether individuals use their grade difference as a signal of subject-specific ability in section 4.4.3. To investigate whether employers use the grade difference as a signal of applicants' relative skills, we use individuals' dream jobs instead of observed occupations (section 4.4.4. Finally, section 4.4.5 investigates the importance of individuals' tastes.

4.4.1 Baseline results

Individuals who rank higher in math than in German at the end of school tend to choose occupations in which workers have higher math skills (see figure 4.1) and occupations that require higher math skills (see figure C-2 in the appendix). In general, this association is not surprising if the grade difference reflects differences in individual's math and reading achievement. Indeed, there is also a strong positive correlation between relative math achievement (vs. reading) and the math intensity of chosen occupations (figure C-3 in the appendix). A match between a more math-intensive occupation and an individual who performs better in math is likely efficient for the economy. However, the correlations between grade difference and math intensity of occupations shown in figures 4.1 and C-2 are conditional on math and reading achievement. This indicates that the grade difference incorporates factors beyond absolute achievement that drive occupational choice.

Table 4.2 shows that there is a strong correlation between the grade difference and the five measures of occupations' math intensity (panel A). These correlations are robust to including individuals' achievement in math and reading, which are flexibly controlled for with third-order polynomials (panel B). The associations are highly statistically significant regardless of how math intensity is measured. The magnitude of the effect is also comparable across the five measures and varies between 17 and 25 percent of a standard deviation in math intensity.

Rather than using grade ranks (our preferred measure), students may view themselves as being talented in math by receiving a (very) good math grade, irrespective of their German grade and independent of their grade rank in the classroom. To rule out the possibility that our grade difference is simply capturing pure math grade effects, panel C of table 4.2 controls for the level of math grade. The effect of grade difference is somewhat reduced, but statistically significant for most outcomes, particularly when math intensity is measured by the difference between math and reading skills and skill requirements, respectively (columns 2 and 6).

To assess whether our findings are driven by the specific construction of our grade-difference measure, we use alternative measures. Importantly, teachers in Germany do not have to use the entire grade spectrum when assessing their students' performance. Therefore, the worst-performing student in math might receive a "5," whereas the worst-performing student in German class may receive a "3." Using the absolute difference in grades would suggest that this student has a quite large absolute grade difference. Based on our preferred rank-based measure, the grade difference would be zero as she receives the worst grades in both subjects. To rule this out, we prefer using the rank-based measure for grade difference. However, panel A in table C-1 shows that our finding is robust to measuring the difference in absolute grades and not in ranks. For the grade levels we find a positive correlation with math grade (panel B) and a smaller but negative correlation with the German grade (panel C). Including both absolute grades simultaneously shows that there is a positive and significant correlation of the occupation's math intensity with the math grade and a negative and significant one with the German grade (panel D). The overall association observed in table 4.2 therefore comes from both the positive correlation with math grades and the negative correlation with German grades.

One question arising from this correlation is whether other characteristics of math-intensive occupations are driving this relationship. For example, it is possible that math-intensive occupations (among all occupations offered in the apprenticeship system, i.e., excluding occupations of college graduates) pay higher wages or offer more job security. In this case, individuals may simply choose higher-paying or safer occupations, rather than choosing occupations based on their skill requirements. In panel A of table C-2 in the appendix we use apprenticeship pay, hourly wages (based on 5-digit occupations), and unemployment rates of occupations as outcomes (columns 1-3). We find no significant relationship between grade difference and these occupational characteristics. However, as expected, we find a clear positive association with average earnings in the occupation, apprenticeship pay, and a negative correlation with unemployment risk when using the math grade *level* (similarly for German grade level; see panel B), indicating that better students do choose better paid and safer occupations. In contrast, these level effects are eliminated when looking at the grade difference between math and German. Finally, we find that individuals with a higher math grade rank are significantly more satisfied with their apprenticeship occupation (column 4).[17]

4.4.2 Parental occupation

The positive association between grade difference and math intensity of chosen occupation might arise if omitted factors both affect the grade difference and the probability of choosing a math-intensive occupation. Importantly, research shows that parents' occupations are related to their children's occupation (Constant and Zimmermann, 2004; Hellerstein and Morrill, 2011). Parents with math-intensive occupations might use their professional network to find an apprenticeship position in a math-intensive occupation for their child and simultaneously support their children more in math than in German. Such subject-specific support might not be completely reflected in the child's math achievement, but may affect grades, which also reflect active participation in class, among other things. To assess the role of parents' occupations, we control for parents' occupation by using the occupation of the mother or father that minimizes the difference in the two-digit ISCO classification from the occupation of the child (regardless of one-digit classification).[18] Table 4.3 shows that our coefficient of interest does not change when we include parents' occupation, indicating that parental occupation does not drive this association.

[17] Individuals are asked about satisfaction with their apprenticeship position on a yearly basis. From 2011 onward, the relationship with grade difference is positive and statistically significant in four out of five years and has about the same magnitude as in column 4.

[18] Alternative measures, such as using the higher-ranked parental occupation, lead to similar findings. We have no information of parents' occupation for about 25 percent of the individuals in our sample. We include a dummy for missing parents' occupation so as to retain keep all individuals in our sample. However, results do not change if we exclude individuals with missing parental occupation information.

4.4.3 Grade difference as signal of own subject-specific ability

Since we compare individuals who attended the same school and we control for the absolute achievement level, variation in grade difference (i.e., differences in grade ranks between math and German) may arise via two channels. First, differences in class participation might drive differences in grade ranks because more intensive class participation typically leads to a better grade, conditional on achievement. Second, conditional on own achievement, differences in classmates' achievement leads to different grades because teachers use relative grading.[19] That is, higher-achieving classmates in math, ceteris paribus, lead to a lower achievement rank in math, and therefore to a worse math grade and lower math grade rank.

We exploit idiosyncratic differences in classroom composition within the same school to use classmates' average achievement (more precisely, the difference between math and reading achievement) as an instrument for a given student's grade difference. To illustrate: we compare student 1 to student 2, who are in two different classrooms, A and B, in the same school. Student 1 and student 2 have the same achievement in math and reading. Compared to class B, class A, on average, performs worse in math than in reading. Therefore, student 1 (in class A) tends to receive a better math grade and therefore a signal that she is talented in math as she obtains a higher grade rank in math than in German.

Figure C-4 in the appendix depicts the first-stage relationship. As expected, the grade difference becomes smaller, that is, the own math grade rank lower, when classmates perform better in math. Table 4.5, column 1, presents the regression results of the first stage. Grade difference and the difference in classmates' achievement in math and reading are strongly negatively correlated and the F-statistic of the excluded instrument is large (about 27). Despite the fact that classmates' relative achievement is a strong predictor of a given student's own grade difference, the instrument is completely unrelated to the math intensity of chosen occupations, with coefficients being close to zero; see the reduced-form results in columns 2-7. Since there is no reduced-form relationship, the IV results are also close to zero and statistically insignificant (table C-4 in the appendix).

To ensure that this finding is not due to the fact that students simply do not react to their grade difference at all, we use the same instrument and investigate whether a student's grade difference affects her academic self-concept . Table C-3 in the appendix shows that there is a positive effect of a student's grade difference on the level of her academic self-concept in math (column 1) and on the difference in academic self-concept of math and German. This finding

19 Under *relative grading*, also known as "grading on a curve," students' grades depend on their positions, that is, their ordinal rank, in the class's achievement distribution.

suggests that students do react to grade differences: students with better math grades ranks than German grade ranks are more confident in their math skills. However, the former finding suggests that students do not use their grade difference as a signal of own subject-specific ability when choosing an occupation.

4.4.4 The role of employers

When investigating determinants of occupations, one issue is that observed occupations depend on both firms and job seekers. While job seekers choose occupations, firms must also hire them for the desired occupation. To understand the underlying mechanism, it is important to disentangle supply-side from demand-side effects. Since our focus is on the determinants of individuals' occupation choices, we want to isolate the supply-side effect. The NEPS dataset enables doing so since students in 9th grade, before applying to apprenticeship positions, also report their dream jobs, which should be unaffected by firms.[20]

Table 4.4 shows that the coefficient on the grade difference is almost the same when using dream jobs instead of actual occupations. (Because the dream job sample is considerably smaller as many students did not report a dream job, we replicated the baseline results for the reduced sample in panel B.) This finding indicates that the relationship between grade difference and math intensity of observed occupation is driven by individuals' choices, not by employers reacting to the grade differences on individuals' resumes when they apply for a job.

4.4.5 Differences in tastes

Since research suggests that differences in tastes across individuals are an important determinant of occupational choice—which prior studies have only indirectly inferred based on residual components (Altonji et al., 2012; Stinebrickner and Stinebrickner, 2012, 2014) — we directly control for tastes. We observe measures of subject interest in math and German and general measures of individuals' social interests as well as practical-technical interests, which were reported before applying for jobs. The subject-specific interests come from three to four statements on math and German activities that relate to what students do and like in their *leisure time* (not in school).[21] General interests are reported for the domains of conventional, intellectual/researching, artistic/language, practical-technical, social, and entrepreneurial

[20] At the 3-digit ISCO level, 30 percent of individuals start an apprenticeship in their stated dream job occupation; at the 2-digit level, this is true for 34 percent and at the 1-digit level for 57 percent of individuals.

[21] The statements for math interest are: "I enjoy figuring out a mathematical problem"; "When I am working on a mathematical problem, it may happen that I do not notice how time flies"; "If I can learn new things in mathematics, I am willing to spend my leisure time on it"; and "Mathematics is one of the most important things to me." The statements for German interest are: "It means a lot to me to get more familiar with the German language and literature"; "I very much enjoy learning new things about myself and the world when

interests. We use the indices on practical-technical interests and on social interests.[22] These measures allow us to directly investigate the importance of (stated) tastes for occupational choice, and in particular whether heterogeneity in tastes across individuals explains the positive association between grade difference and math-intensity of chosen occupation.

In panel A of table 4.6 we include three dummies for math interest, three dummies for German-language interest, and four dummies, respectively, for each dimension of practical-technical and social interests. Controlling for individuals' tastes completely eliminates the positive and significant coefficient on grade difference (while not affecting standard errors). The coefficients on the different levels of practical-technical interests and on social interests are plotted in figure 4.2 and 4.3, respectively. Figure 4.2 shows that individuals with higher practical-technical interests choose more math-intensive occupations. (The omitted category is "very little interest.") The opposite is true for social interests: the higher the level of social interests, the lower the math intensity of the chosen occupation (figure 4.3).

In contrast to existing studies that do not observe tastes, these results provide direct evidence on the important role of tastes for occupational choice. Since the coefficient on grade difference essentially becomes zero, the association between grade difference and math intensity of chosen occupation largely reflects differences in individual tastes for certain fields (e.g., math). Interestingly, these interests are only weakly correlated with individuals' achievement. For example, the math-specific interest and math achievement are only correlated with $r = 0.11$ and practical-technical interests and math achievement with $r = 0.17$.
However, these findings do not tell us whether the grade difference affects interests or whether interests affect the grade difference. Unfortunately, we cannot answer this question with the data at hand since we observe grade difference and interests at the same time and only once. However, we provide some evidence that tastes are influenced by a student's environment. Assuming that classes within schools are formed quasi-randomly,[23] we can exploit

reading books"; and "I am prepared to use part of my leisure time to get to know the German language and literature better." For each statement, students can choose between "doesn't apply at all" (1), "hardly applies" (2), "partly applies" (3), and "completely applies" (4).

22 The activities for these interests are: *Practical-technical*: "setting up or putting things together"; "building something according to a plan/sketch"; and "working with/making something out of metal/wood." *Social interests* include the following three items: "supporting the matters of concern of others"; "helping sick people"; and "looking after children or adults in need of help". For each activity, students can choose between "I have very little interest in that; I do not like to do that at all" (1), "I have little interest in that" (2), "I am somewhat interested in that" (3), "I am rather interested in that" (4), or "I am very interested in that; I like to do that a lot" (5). Note that results are very similar if we additionally include the indices for the other interests: conventional, entrepreneurial, artistic/language, and scientific/analytic interest.

23 In future research, we will investigate whether classes are formed quasi-randomly within schools with respect to students' practical-technical interests and social interests.

classmates' (average) interests as an instrument for a given student's interests. We find that a student's practical-technical interests are strongly related to her classmates' average practical-technical interests (F-statistic on the excluded instrument is about 25). Using classmates' average practical-technical interests as an instrument, we find a significant effect of practical-technical interests on occupational choice (table 4.7). This effect is robust to including classmates' average math achievement (panel B).[24] Note, however, that this result may also arise if great (or lousy) math teachers influence the tastes of all students in the classroom. In this case, we would also observe an association between a given student's interests and her classmates' interests. But most importantly, in both cases—classmates' influence or teachers affecting all students in the class—this finding suggests that interests can be shaped by the environment and that tastes have an effect on occupational choice.

4.5 Conclusion

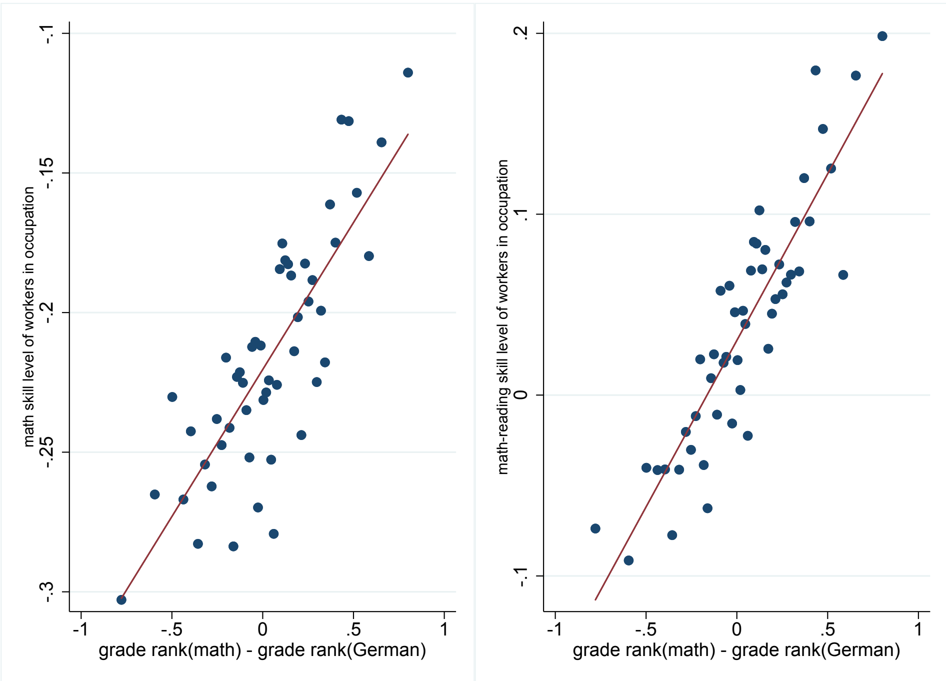
We study the determinants of occupational choice among apprenticeship trainees in Germany. We find that the difference between math and German grade is significantly associated with the math intensity of the chosen occupation. Comparing only individuals who graduated from the same school, we find that this robust association cannot be explained by any of the following potential factors: individuals' absolute achievement in math and reading; parents' occupations; individuals using the grade difference as a signal of their subject-specific ability; or employers using the grade difference as a signal of an applicant's relative skills. However, we find that differences in tastes across individuals, measured before entering the labor market, completely explain this association.

While the literature on occupational choice, which does not observe tastes directly, suggests that individuals' differences in tastes are the most important factor in choice of field of study, we observe various measures of individuals' tastes. We find that practical-technical interests and social interests are strongly associated with occupational choice: individuals with strong practical-technical interests are substantially *more* likely to choose math-intensive occupations, whereas individuals with strong social interests are substantially *less* likely to choose math-intensive occupations. This finding provides direct evidence on the crucial role of tastes for occupational choice.

[24] We do not find a causal relationship for social interests, even though the instrument is even stronger in this case.

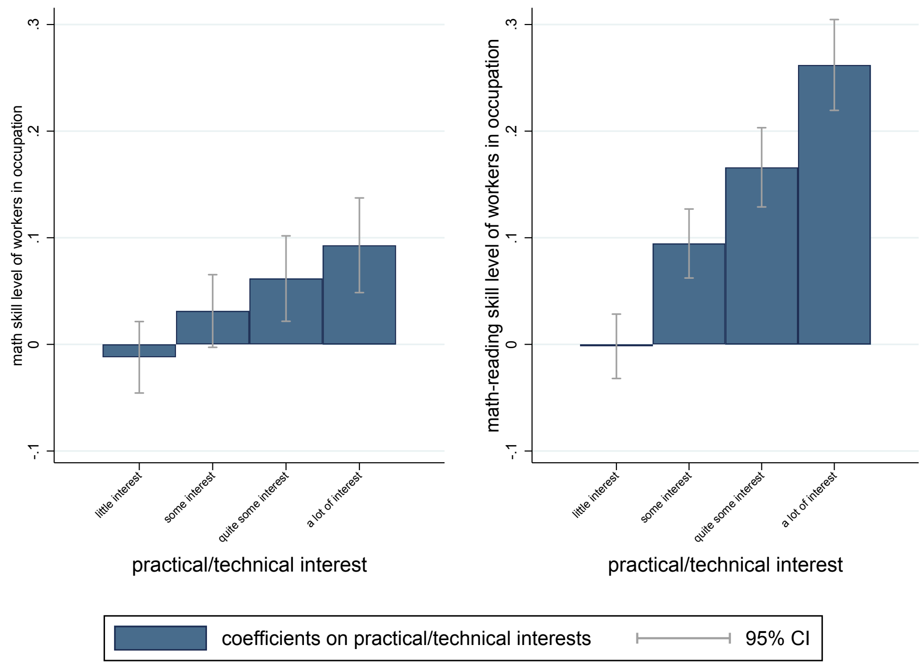
4.6 Figures and tables

Figure 4.1 : Grade difference and math skill level of occupation



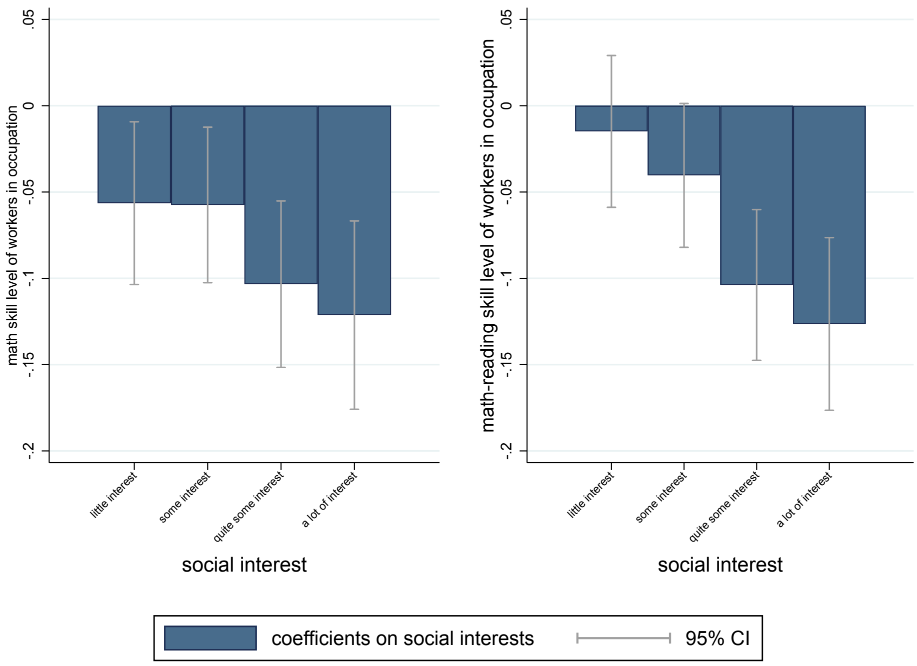
Notes: The binscatter plot shows the correlation of math intensity of occupations measured in levels and differences of skills of workers in an occupation and grade difference. The average skills of workers are calculated in 3-digit ISCO classified occupations from the NEPS SC6 adult sample. grade difference is grade rank math - grade rank German. The left figure shows the correlation for math skill level, the right figure the correlation for the difference in skill levels (math-reading). The correlation is conditional on school fixed effects and individual achievement in math and reading. *Data source:* NEPS SC4 9.0.0.

Figure 4.2 : Relationship between practical/technical interests and math skill level of occupation



Notes: The figure shows the beta coefficients of the four dimensions of practical/technical interest (compared to no practical/technical interest). The coefficients stem from estimating equation 4.1 including measures for subject interest and four dummies for practical/technical and social interest in our basic specification. The left figure shows the effects on math skill level of workers in occupations, the right figure the effects on the math-reading skill levels of workers in occupations. *Data source:* NEPS SC4 9.0.0.

Figure 4.3 : Relationship between social interests and math skill level of occupation



Notes: The figure shows the beta coefficients of the four dimensions of social interest (compared to no social interest). The coefficients stem from estimating equation 4.1 including measures for subject interest and four dummies for practical/technical and social interest in our basic specification. The left figure shows the effects on math skill level of workers in occupations, the right figure the effects on the math-reading skill levels of workers in occupations. *Data source:* NEPS SC4 9.0.0.

Table 4.1 : Summary statistics

	Mean	SD	Min	Max
Student-level characteristics				
Grade difference	0.04	0.35	-1	1
Math achievement	-0.00	1.00	-4	4.9
Reading achievement	-0.00	1.00	-3.7	3.2
Female	0.47	0.50	0	1
Migration background	0.23	0.42	0	1
Class size	17.82	5.72	6	35
Practical/technical interest	2.91	1.18	1	5
Social interest	3.06	1.07	1	5
Occupation characteristics (outcomes)				
Math skill level of workers in occupation	-0.22	0.32	-1.4	1
Skill level difference of workers in occupation	0.03	0.35	-.69	.94
Math requirements in occupation: basic/advanced	0.98	0.26	.15	1.9
Math requirements in occupation	0.74	0.13	.15	1
Advanced math requirements in occupation	0.24	0.15	0	.88
Difference in math requirements in occupation	-0.31	0.39	-1.5	.67
Gross hourly wage	17.02	3.71	8.3	41
Monthly apprenticeship pay	790.01	155.29	269	1,125
Unemployment rate in percent	8.43	5.99	.4	39
Satisfaction with apprenticeship position 2012/13	8.27	1.64	0	10
Observations	5045			

Notes: The table shows summary statistics for our control and outcome variables. The math skill level and the skill level difference of workers stem from calculations with the NEPS Adult cohort. The math requirements of an occupation stem from the Qualification and Career Survey. Section 4.2 describes these variables in detail. *Data source:* NEPS SC4 9.0.0.

Table 4.2 : Grade difference and math intensity of occupations: Baseline results

	Skills of workers in occupation		Requirements in occupation			
	(1) Math	(2) Math-reading	(3) Math	(4) Math (binary)	(5) Advanced math	(6) Math-language
Panel A. Baseline Results - without controls for achievement						
Grade difference	0.0616***	0.0938***	0.0667***	0.0292***	0.0376***	0.0990***
	(0.0136)	(0.0129)	(0.0113)	(0.00566)	(0.00634)	(0.0156)
Observations	4923	4915	4923	4923	4923	4923
R^2	0.259	0.387	0.267	0.219	0.288	0.355
Panel B. Baseline Results - with achievement controls						
Grade difference	0.0547***	0.0871***	0.0579***	0.0257***	0.0322***	0.0871***
	(0.0141)	(0.0130)	(0.0116)	(0.00580)	(0.00657)	(0.0160)
Math, reading (squared, cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4923	4915	4923	4923	4923	4923
R^2	0.266	0.389	0.270	0.221	0.291	0.359
Panel C. Baseline Results - incl. math grade levels						
Grade difference	-0.0144	0.0716***	0.0296**	0.0137*	0.0159**	0.0926***
	(0.0165)	(0.0153)	(0.0144)	(0.00728)	(0.00797)	(0.0193)
Math, reading (squared, cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4923	4915	4923	4923	4923	4923
R^2	0.280	0.390	0.274	0.224	0.295	0.361
Controls	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family
School FE	Yes	Yes	Yes	Yes	Yes	Yes
F-test p-value math	0.043	0.547	0.008	0.018	0.008	0.040
F-test p-value reading	0.012	0.554	0.639	0.938	0.473	0.006
Mean outcome	-0.223	0.027	0.977	0.737	0.240	-0.313
SD outcome	0.316	0.345	0.262	0.126	0.148	0.388

Notes: The table shows the estimated coefficient of grade difference obtained from regressing equation 4.1. In panel A achievement is not controlled for. Panel B shows results for our main specification which controls for math and reading achievement linearly, squared, and cubic. In panel C dummies for the math grade level are added. All regressions include school fixed effects, individual (year of birth, quarter of birth, being female, migration background, class size) and family (number of books at home, highest parental education) controls. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC4 9.0.0.

Table 4.3 : Grade difference and math intensity of occupation: controlling for parents' occupation

	Skills of workers in occupation		Requirements in occupation			
	(1) Math	(2) Math-reading	(3) Math	(4) Math (binary)	(5) Advanced math	(6) Math-language
Grade difference	0.0556***	0.0847***	0.0549***	0.0245***	0.0304***	0.0814***
	(0.0139)	(0.0130)	(0.0117)	(0.00585)	(0.00661)	(0.0161)
Observations	4923	4915	4923	4923	4923	4923
R^2	0.276	0.396	0.280	0.231	0.301	0.369
Math, reading (squared, cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Controls	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	-0.223	0.027	0.977	0.737	0.240	-0.313
SD outcome	0.316	0.345	0.262	0.126	0.148	0.388

Notes: The table shows the estimated coefficient of grade difference controlling in equation 4.1 additionally for the occupation of parents. We use the occupation (if both parents report an occupation) with the minimal distance to the occupation of the child on the 2-digit level (regardless of the 1-digit level). All regressions include school fixed effects, individual (year of birth, quarter of birth, being female, migration background, class size) and family (number of books at home, highest parental education) controls. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC4 9.0.0.

Table 4.4 : Grade difference and math intensity of occupation: using individuals' dream jobs

	Skills of workers in occupation		Requirements in occupation			
	(1) Math	(2) Math-reading	(3) Math	(4) Math (binary)	(5) Advanced math	(6) Math-language
Panel A. Dream job						
Math talent signal	0.0629**	0.0798***	0.0536***	0.0225***	0.0312***	0.0830***
	(0.0244)	(0.0209)	(0.0154)	(0.00787)	(0.00878)	(0.0223)
Observations	2615	2612	2549	2549	2549	2549
R^2	0.345	0.415	0.354	0.303	0.375	0.409
Panel B. Actual occupation using dream job sample						
Math talent signal	0.0649***	0.0891***	0.0513***	0.0248***	0.0265***	0.0810***
	(0.0190)	(0.0193)	(0.0162)	(0.00807)	(0.00901)	(0.0237)
Observations	2615	2611	2615	2615	2615	2615
R^2	0.346	0.456	0.346	0.301	0.366	0.430
Math, reading (squared, cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Controls	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	-0.139	0.038	0.969	0.732	0.237	-0.350
SD outcome	0.384	0.348	0.252	0.121	0.143	0.382

Notes: The table estimates equation 4.1 for the dream job occupation characteristics of the individuals reported in 9th grade (panel A). Panel B re-estimates the results of table 4.2, panel B, in the reduced sample of individuals who report a dream job. All regressions include school fixed effects, individual (year of birth, quarter of birth, being female, migration background, class size) and family (number of books at home, highest parental education) controls. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC4 9.0.0.

Table 4.5 : Exploiting quasi-random variation in grade difference (first stage and reduced form)

	First stage		Skills of workers in occupation				
	(1) Grade diff.	(2) Math	(3) Math-reading	(4) Math	(5) Math (binary)	(6) Advanced math	(7) Math-language
average peer achievement: math-reading	-0.129*** (0.025)	-0.003 (0.024)	-0.005 (0.026)	0.006 (0.020)	0.003 (0.010)	0.004 (0.011)	0.020 (0.031)
Math, reading (squared, cubic)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4923	4923	4915	4923	4923	4923	4923
R^2	0.202	0.263	0.383	0.266	0.217	0.287	0.354
Mean outcome	0.036	-0.223	0.027	0.977	0.737	0.240	-0.313
SD outcome	0.347	0.316	0.345	0.262	0.126	0.148	0.388

Notes: The table shows in column 1 results of the first stage regressing the difference in the grade rank in math and reading on the difference of peer achievement in math and reading. Column 2-7 show the reduced form of regressing the instrument *average peer achievement difference* on math intensity of occupations. The F-statistic of the instrument is about 27. All regressions include school fixed effects, individual (year of birth, quarter of birth, being female, migration background, class size) and family (number of books at home, highest parental education) controls. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC4 9.0.0.

Table 4.6 : Grade difference and math intensity of occupation: controlling for interests

	Skills of workers in occupation		Requirements in occupation			
	(1) Math	(2) Math-reading	(3) Math	(4) Math (binary)	(5) Advanced math	(6) Math-language
Panel A. Including measures for interest						
Grade difference	0.00542	0.0274*	0.0205	0.0112*	0.00931	0.0297
	(0.0169)	(0.0154)	(0.0134)	(0.00659)	(0.00765)	(0.0186)
Observations	4417	4409	4417	4417	4417	4417
R^2	0.297	0.450	0.332	0.275	0.351	0.422
Panel B. R^2 from regressions including only interest measures						
Observations	4487	4479	4487	4487	4487	4487
R^2	0.105	0.289	0.194	0.141	0.210	0.270
Panel C. R^2 from regressions including interest measures and school FE						
Observations	4487	4479	4487	4487	4487	4487
R^2	0.258	0.383	0.301	0.255	0.313	0.370
Math, reading (squared, cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Controls	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	-0.221	0.024	0.976	0.737	0.239	-0.319
SD outcome	0.316	0.344	0.261	0.126	0.148	0.389

Notes: The table shows the coefficient of *grade difference* if subject interests, the dimensions of practical/technical interest and social interest are controlled for (panel A). Panel B and panel C show with R^2 the explanatory power of interests for math intensity of chosen occupations. Panel A and panel C include school fixed effects. In panel A we control for individual (year of birth, quarter of birth, being female, migration background, class size) and family (number of books at home, highest parental education) characteristics. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC4 9.0.0.

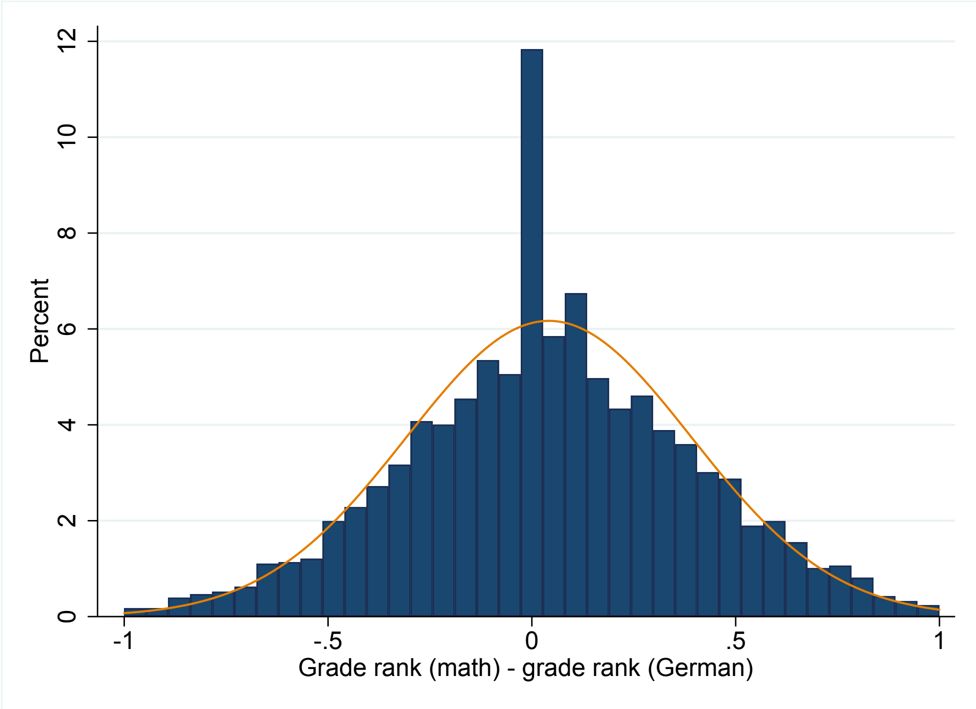
Table 4.7 : Effect of practical/technical interests on occupational choice: instrumenting own interest with peer interests

	Skills of workers in occupation		Requirements in occupation			
	(1)	(2)	(3)	(4)	(5)	(6)
	Math	Math-reading	Math	Math (binary)	Advanced math	Math-language
Panel A. Baseline results						
practical/technical interest	0.0440*	0.0692***	0.0400*	0.0171	0.0229*	0.0486*
	(0.0230)	(0.0236)	(0.0212)	(0.0104)	(0.0120)	(0.0276)
Observations	4710	4702	4710	4710	4710	4710
F-statistic	25.283	25.144	25.283	25.283	25.283	25.283
R^2	0.130	0.330	0.181	0.122	0.206	0.285
Panel B. Controlling for average peer achievement math						
practical/technical interest	0.0480**	0.0636***	0.0407**	0.0170*	0.0237**	0.0404
	(0.0226)	(0.0231)	(0.0199)	(0.00978)	(0.0112)	(0.0267)
Observations	4710	4702	4710	4710	4710	4710
F-statistic	30.433	30.388	30.433	30.433	30.433	30.433
R^2	0.129	0.330	0.181	0.122	0.206	0.281
Math, reading (squared, cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Controls	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	-0.223	0.024	0.976	0.736	0.239	-0.316
SD outcome	0.317	0.344	0.262	0.127	0.148	0.388

Notes: The table shows the relation between math intensity of occupations and own interests with an IV approach. Own interests (practical/technical) are instrumented by the average level of interest of all peers. The first-stage F-statistic is reported in the third row of each panel. Panel A shows the baseline results. Panel B controls in the IV setting for average peer achievement in math. Panel C controls for own grade levels in math. Panel D controls for the grade difference. All regressions include school fixed effects, individual (year of birth, quarter of birth, being female, migration background, class size) and family (number of books at home, highest parental education) controls. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC4 9.0.0.

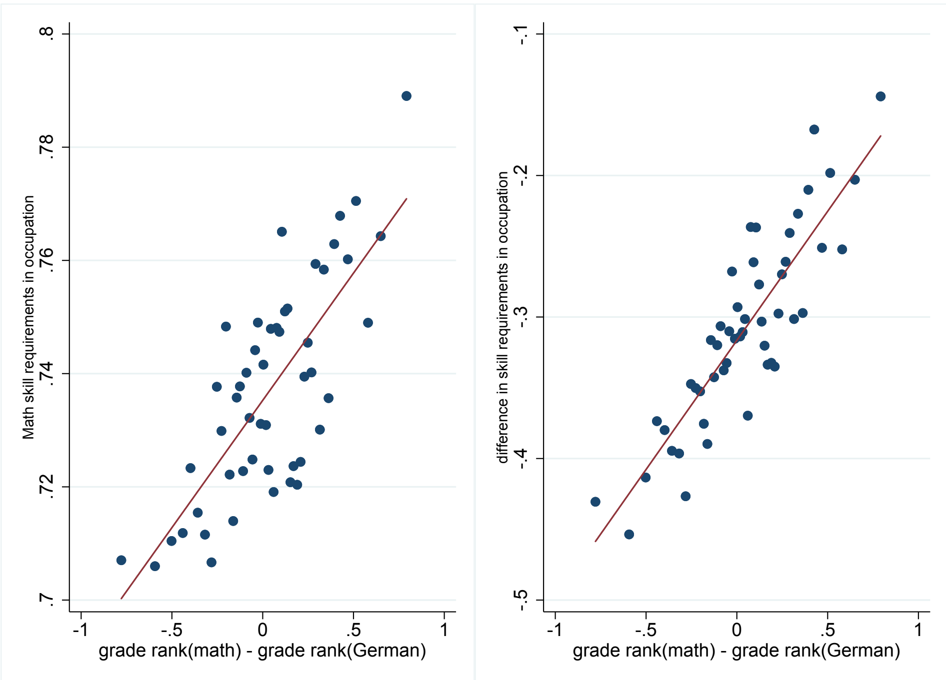
4.7 Appendix C

Figure C-1 : Histogram of grade difference



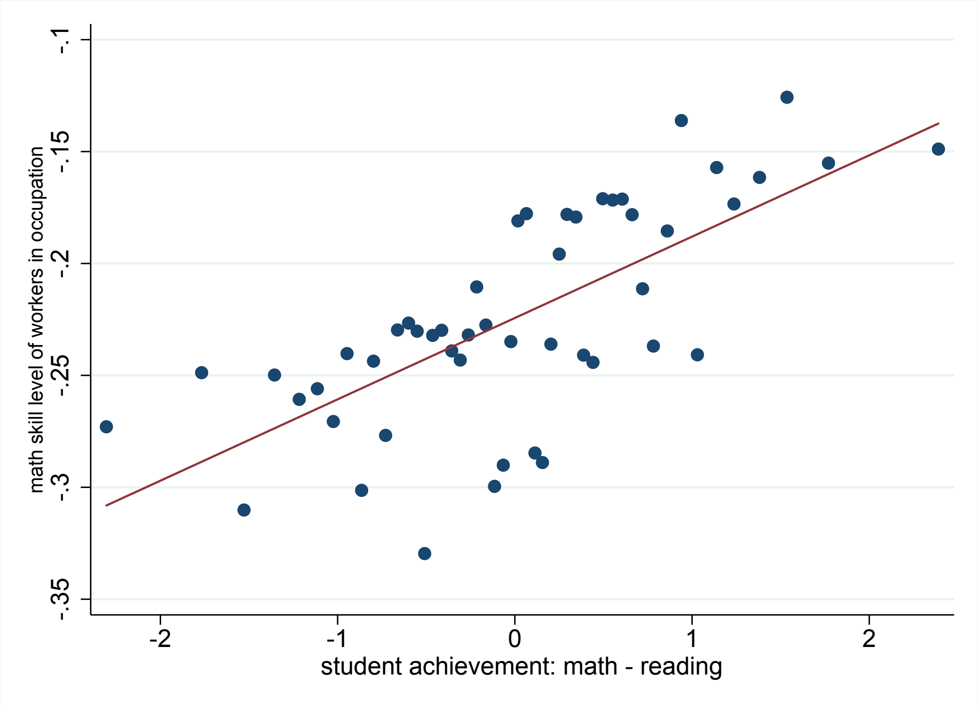
Notes: The figure shows in a histogram the distribution of our main explanatory variable: grade difference=grade rank math - grade rank German. The dark orange line gives a normal distribution. The value -1 is reached if the student is the best student in German but worst in math. The value +1 is reached if the student is the best in math but the worst in German. The value 0 means that the student has the same rank in math and German. *Data source:* NEPS SC4 9.0.0.

Figure C-2 : Grade difference and math skill requirements in occupation



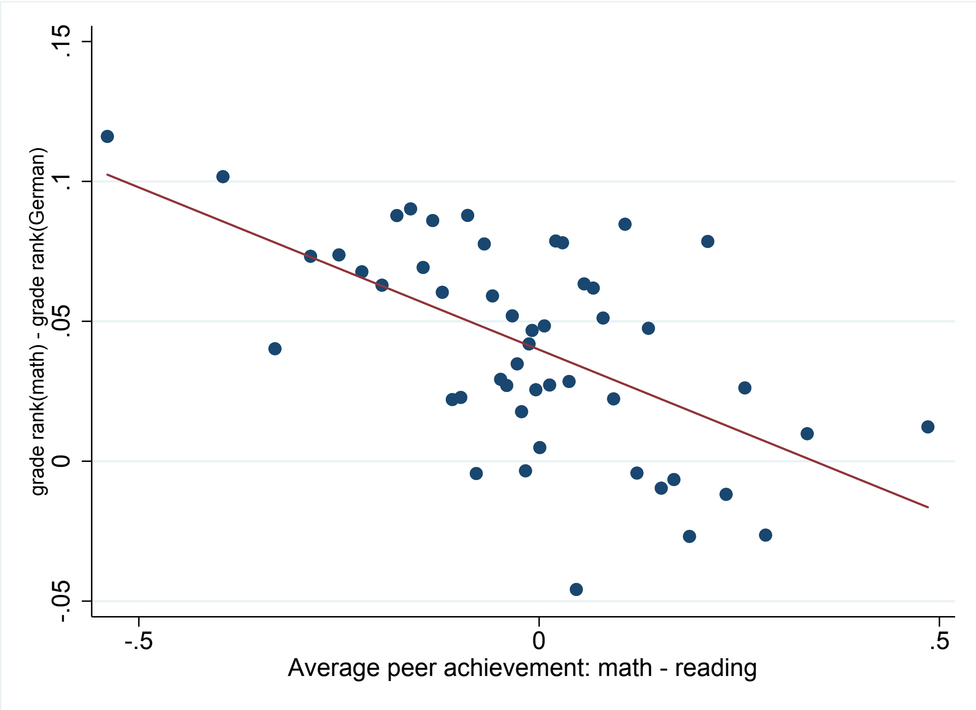
Notes: The binscatter plot shows the correlation of grade difference and math intensity of occupations measured in levels and differences of skill requirements in an occupation. The skill requirements are calculated in 3-digit ISCO classified occupations from the Qualification and Career Survey (QaC). The left figure shows the correlation with math skill requirements, the right figure the correlation with the difference in skill requirements (math-German). The correlation is conditional on school fixed effects and individual achievement in math and reading. *Data source:* NEPS SC4 9.0.0.

Figure C-3 : Achievement difference and math skill level of occupation



Notes: The bin-scatter plot shows the correlation of math skill levels of workers in 3-digit ISCO classified occupations from the NEPS SC6 adult sample and the difference in math and reading achievement. The correlation is conditional on school fixed effects. *Data source:* NEPS SC4 9.0.0.

Figure C-4 : Difference in average peer achievement and grade difference (first stage)



Notes: The bin-scatter plot shows the correlation of grade difference and the difference in the average peer achievement (math-reading). This is the correlation we exploit in the first stage of the IV setting in table 4.5. The correlation is conditional on own achievement in math and reading and contains school fixed effects. *Data source:* NEPS SC4 9.0.0.

Table C-1 : Relation between grades and math intensity of occupation

	Skills of workers in occupation		Requirements in occupation			
	(1) Math	(2) Math-reading	(3) Math	(4) Math dummy	(5) Advanced math	(6) Math-language
Panel A. Difference in grades						
Grade difference (absolute)	0.0309***	0.0386***	0.0283***	0.0127***	0.0156***	0.0385***
	(0.00573)	(0.00557)	(0.00446)	(0.00223)	(0.00255)	(0.00643)
Observations	4923	4915	4923	4923	4923	4923
R^2	0.269	0.390	0.273	0.223	0.293	0.360
Panel B. Math grade						
Grade math	0.137***	0.0759***	0.0720***	0.0303***	0.0417***	0.0346*
	(0.0161)	(0.0158)	(0.0130)	(0.00644)	(0.00736)	(0.0188)
Observations	4923	4915	4923	4923	4923	4923
R^2	0.276	0.386	0.271	0.221	0.292	0.355
Panel C. German grade						
Grade German	0.0576***	-0.0461***	-0.00958	-0.00584	-0.00373	-0.0863***
	(0.0163)	(0.0157)	(0.0147)	(0.00737)	(0.00813)	(0.0194)
Observations	4923	4915	4923	4923	4923	4923
R^2	0.266	0.384	0.266	0.217	0.287	0.358
Panel D. Math and German grade						
Grade German	0.0186	-0.0756***	-0.0342**	-0.0164**	-0.0178**	-0.106***
	(0.0171)	(0.0163)	(0.0154)	(0.00769)	(0.00852)	(0.0203)
Grade math	0.131***	0.0990***	0.0825***	0.0354***	0.0471***	0.0671***
	(0.0171)	(0.0164)	(0.0135)	(0.00671)	(0.00774)	(0.0194)
Observations	4923	4915	4923	4923	4923	4923
R^2	0.276	0.389	0.272	0.222	0.293	0.359
Math, reading (squared, cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Controls	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean outcome	-0.223	0.027	0.977	0.737	0.240	-0.313
SD outcome	0.316	0.345	0.262	0.126	0.148	0.388

Notes: The table explores the general relation between math intensity of chosen occupations and grades or grade differences. Panel A uses the raw difference of math and German grades - no ranks. Panel B includes the math grade linearly. Panel C includes the German grade linearly. Panel D includes the math and the German grade linearly. All regressions include school fixed effects, individual (year of birth, quarter of birth, being female, migration background, class size) and family (number of books at home, highest parental education) controls. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC4 9.0.0.

Table C-2 : Grade difference and other occupational characteristics

	(1) Gross hourly wage	(2) Monthly apprenticeship pay	(3) Unemployment rate	(4) Satisfaction apprenticeship 2012/13
Panel A. Grade difference				
Grade difference	0.136	9.891	-0.213	0.287***
	(0.164)	(8.389)	(0.283)	(0.108)
Observations	4844	4083	4917	2800
R^2	0.222	0.181	0.185	0.182
Panel B. Math and German grades				
German grade	0.406***	6.663	-0.383**	-0.0666
	(0.0963)	(4.522)	(0.163)	(0.0633)
Math grade	0.456***	15.17***	-0.540***	0.165***
	(0.0746)	(3.697)	(0.122)	(0.0501)
Observations	4844	4083	4917	2800
R^2	0.237	0.187	0.192	0.184
Math, reading (squared, cubic)	Yes	Yes	Yes	Yes
Controls	indiv.+family	indiv.+family	indiv.+family	indiv.+family
School FE	Yes	Yes	Yes	Yes
Mean outcome	17.039	791.360	8.410	8.274
SD outcome	3.716	155.434	5.999	1.640

Notes: The table shows the relation of grade difference and other occupational characteristics: the gross hourly wage of an occupation, the monthly apprenticeship pay, the unemployment rate (these variables stem from official sources - see section 4.2 for details), and the satisfaction level with the apprenticeship position (i.e. the occupation) reported in NEPS. Panel A shows coefficients for grade difference, panel B for the math and German grade. All regressions include school fixed effects, individual (year of birth, quarter of birth, being female, migration background, class size) and family (number of books at home, highest parental education) controls. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC4 9.0.0.

Table C-3 : Effect of grade difference on academic self concept - IV: average peer achievement math-reading

	(1) Math	(2) Math-German
Grade difference	1.594***	1.409**
	(0.521)	(0.573)
Math, reading (squared, cubic)	Yes	Yes
F-statistic	27.426	27.426
Controls	indiv.+family	indiv.+family
School FE	Yes	Yes
Observations	4923	4923
R^2	0.325	0.313
Mean outcome	2.326	-0.020
SD outcome	0.990	1.038

Notes: The table reports coefficients for the causal effect of grade difference on an index of academic self concept in math (column 1) and the difference of academic self concept (math-German) (column 2). The coefficients stem from an IV regression using the average peer achievement difference (math-reading) as instrument for the own grade difference (as in table 4.5/C-4). All regressions include school fixed effects, individual (year of birth, quarter of birth, being female, migration background, class size) and family (number of books at home, highest parental education) controls. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC4 9.0.0.

Table C-4 : Exploiting quasi-random variation in grade difference (IV)

	Skills of workers in occupation		Requirements in occupation			
	(1) Math	(2) Math-reading	(3) Math	(4) Math dummy	(5) Advanced math	(6) Math-language
Grade difference	0.024	0.037	-0.050	-0.021	-0.029	-0.155
	(0.178)	(0.196)	(0.148)	(0.073)	(0.083)	(0.224)
Math, reading (squared, cubic)	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic	27.426	26.616	27.426	27.426	27.426	27.426
Controls	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family	indiv.+family
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4923	4915	4923	4923	4923	4923
R^2	0.125	0.291	0.145	0.095	0.169	0.211
Mean outcome	-0.223	0.027	0.977	0.737	0.240	-0.313
SD outcome	0.316	0.345	0.262	0.126	0.148	0.388

Notes: The table shows the results of the IV regression of table 4.5. The F-statistic of the first stage is reported in the fourth row. Grade difference is instrumented by average peer achievement difference (math-reading). All regressions include school fixed effects, individual (year of birth, quarter of birth, being female, migration background, class size) and family (number of books at home, highest parental education) controls. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC4 9.0.0.

Table C-5 : Effect of interests on grades from pre-observational period

	Grade difference		Grade levels	
	(1) Math	(2) German	(3) Math	(4) German
math interests leisure	0.305***	0.020	0.194***	-0.019
	(0.015)	(0.015)	(0.013)	(0.014)
German interests leisure	-0.070***	0.092***	-0.055***	0.067***
	(0.015)	(0.013)	(0.013)	(0.011)
practical/technical interests	-0.025**	-0.002	-0.017	0.000
	(0.012)	(0.011)	(0.011)	(0.009)
social interests	-0.020	0.007	-0.006	0.011
	(0.013)	(0.011)	(0.011)	(0.010)
Grade difference	0.772***	-0.685***		
	(0.039)	(0.035)		
Math, reading (squared, cubic)	Yes	Yes	Yes	Yes
Controls	indiv.+family	indiv.+family	indiv.+family	indiv.+family
School FE	Yes	Yes	Yes	Yes
Observations	4379	4361	4379	4361
R^2	0.480	0.324	0.603	0.447
Mean outcome	2.690	2.756	2.690	2.756
SD outcome	0.880	0.702	0.880	0.702

Notes: The table explores the relation between grade difference and grade levels and interests. The outcome variable are an intensity of approval that the individual always had good grades in math resp. German. All regression control for either the actual grade difference or the actual grade levels such that the outcome should capture previous grades from an unobserved period. The subject specific interests are: *math interests leisure* and *German interests leisure*. All regressions include school fixed effects, individual (year of birth, quarter of birth, being female, migration background, class size) and family (number of books at home, highest parental education) controls. Standard errors are clustered on school level. * = p<0.1, **=p<0.05, ***=p<0.01. *Data source:* NEPS SC4 9.0.0.

5 Long-Term Consequences of Inequality: Evidence from Historical Inheritance Rules*

5.1 Introduction

One of the oldest debates in economics concerns the effect of inequality on growth and development see, e.g. Kuznets (1955). How would growth prospects change if income or wealth were counterfactually distributed more evenly? The answer to this question has become particularly relevant in recent decades due to rising levels of inequality, especially wealth inequality (Alvaredo, Atkinson, Piketty, Saez, and Zucman, 2015-2017; Piketty and Saez, 2014; Alvaredo, Atkinson, Piketty, and Saez, 2013). Yet, the debate has remained largely unresolved due to the scarcity of suitable data and credible research designs allowing for an estimation of causally interpretable effects, as variations in inequality likely correlate with drivers of growth (Banerjee and Duflo, 2003).

We contribute to the resolution of this debate by leveraging sharp geographic variation in institutions that govern how resources are passed from parents to children. Specifically, we analyze the relationship between historical inheritance rules for land, inequality, and development in Germany from before the Industrial Revolution until today. Historical inheritance rules for agricultural land varied sharply within Germany and prescribed either equal or unequal division of land among a decendent's children. In unequal division areas agricultural property was considered indivisible and had to be passed on to a single heir. In contrast, agricultural land had to be divided equally among all children in equal division areas. The canonical economic theories of inheritance rules (Stiglitz, 1969; Menchik, 1980; Chu, 1991) make a strong prediction that wealth would be more evenly distributed under an equal division regime, therefore implying that differences in inheritance rules lead to variation in the distribution of wealth.

For our study, we analyze the relationship between inheritance rules, inequality, and development in a geographic regression discontinuity (RD) design, exploiting variation in inheritance rules that is uncorrelated with other drivers of growth. We analyze data based on fine-grained historical surveys on the local prevalence of inheritance rules see e.g., Sering (1897) that we digitized and geocoded for the historical county-level of Germany. Broadly speaking, equal division of agricultural land was prevalent in parts of Southern and Western Germany and the

* This chapter is based on joint work with Simon Jäger and Johannes Eigner. We thank Oliver Falck who was a co-author at an earlier stage of this project for ideas, comments, and suggestions.

boundary between the two inheritance rule regimes traverses political, linguistic, geological, and religious borders. The geographic prevalence of the different inheritance rule regimes dates back to the Middle Ages (Rösener, 2012) and is plausibly connected to the sphere of influence of the Salian Frankish civil law code (507 AD), which prescribed an equal split of assets among male offspring (Behrend, 1897).[1] To estimate the relationship between inheritance rules and longer-term outcomes, we hone in on counties close to the boundary between equal and unequal division areas in an RD design following Dell (2010). We show that predictors of long-term development and also of a particular inheritance rule regime are smooth at the boundary, thereby suggesting that the variation in inheritance rules that we analyze is idiosyncratic and not systematically related to other drivers of growth.

We document that ownership of land–the key store of wealth in an agricultural society–was more concentrated in unequal division areas, showing that inheritance rules indeed affect the inequality of landed wealth. This finding is non-trivial as a Coasean argument would suggest that inter vivos land transactions may lead to concentrated land ownership in equal division areas if transaction costs were low and concentrated ownership were optimal. Drawing on county-level Prussian data from before the onset of the Industrial Revolution see Becker, Cinnirella, Hornung, and Woessmann (2012), we show that landholding inequality in the early 1800s was lower in equal division areas, which featured roughly a quarter SDs more landholdings per population. Towards the end of the 19th century when detailed landownership data for the entire German empire were measured (Ziblatt, 2008), we find even more pronounced effects with equal division areas featuring 0.8 to 0.9 SDs more small landholdings and 0.6 SDs fewer large landholdings.[2] We further find that over time landholding inequality increased more in unequal division areas compared to equal division areas. All in all, our results on inequality in the 19th century paint a picture of differences in inheritance rules leading to substantial differences in inequality. However, using modern tax data we do not find significant differences in the distribution of earnings anymore. This suggests that the higher level of equality did not persist until today.

We next investigate whether differences in inequality between equal and unequal division areas are associated with differences in long-term growth and find that modern income and GDP levels are 6 to 14 percent higher in equal division areas. The robust positive relationship between equal division inheritance and modern growth outcomes is particularly remarkable

1 In section 2, we describe other theories on the origin of equal and unequal division rules in greater detail.

2 The stronger association between landholding inequality and inheritance rules towards the end of the 19th century compared to the beginning of the century could indicate that the rapid population growth in between contributed to stronger effects. To illustrate, in a society with only one child per family equal or unequal division would lead to the same distribution of wealth while differences would be much more pronounced in large families.

as observable determinants of long-term growth are smooth at the boundary between the two inheritance regimes while actual growth outcomes differ discontinuously. Using a variety of data sources on growth and economic development before the Industrial Revolution, we further find no evidence that equal division areas had more advantageous starting conditions. Agricultural productivity measured directly was not significantly higher. Similarly, urban population and population density in general did not differ until the beginning of the 20th century. Early measures for technology and industrialization are, if anything, lower in equal division areas. Last but not least, general education levels hardly differed at the beginning of the 19th century. At a county level, the data therefore show a robust correlation between lower levels of wealth inequality in the 19th century–associated with an equal division inheritance regime–and favorable long-term outcomes and allow us to rule out a variety of potential confounders.

Why might a more even wealth distribution spur long-term growth? An influential body of literature hypothesizes that the distribution of wealth affects long-term growth through its effect on investment decisions and the occupational choice of individuals see, e.g., Galor and Zeira (1993); Banerjee and Newman (1993); Ghatak and Nien-Huei Jiang (2002); Galor and Moav (2004). In such models, individuals have the choice between, e.g., subsistence farming and becoming a skilled worker or entrepreneur. Compared to a situation in which a large part of the population has essentially no wealth, a more even distribution of wealth can alleviate credit constraints in parts of the population or provide a buffer to absorb the potential risks of innovating, investing in human capital, or becoming an entrepreneur, all of which have favorable consequences for growth. Consistent with inheritance rules affecting long-term growth through such mechanisms, Ferdinand von Weckherlin, the Finance Minister in the Kingdom of Wuerttemberg, argued that Wuerttemberg's economic strength at the time was "*the unconditionally permissible division of landed property. On property of paltry size, the industriousness, thrift and ingenuity of the owner blossoms. He nourishes himself in the character of a businessman [Gewerbsmann], indeed, he becomes [...] a business man. [...] No matter where one looks, one finds everywhere industrious artisans, highly skilled manufacturers and thoughtful merchants. That is the character of industry in this land. [...] Supported by their small farms they are at least able to salvage a meager existence until luck or genius brings to them better times*" see Herrigel (2000). The quote lends support to the mechanisms hypothesized in the literature and suggests that occupational choice and entrepreneurship could be mechanisms through which a more even distribution of landed wealth contributed to long-term growth.

We investigate the hypothesized mechanisms empirically drawing on our RD design and find that occupational choice indeed differed between equal and unequal division areas as predicted by the models. Specifically, we find that higher shares of the population in equal division areas–with a more even land distribution–worked in manufacturing as well as in trade and services from industrialization on. The additional employment in manufacturing is fully accounted for by employment in particularly innovative sectors with high patenting activity defined as in Streb, Baten, and Yin (2006), which command a 36 percent higher employment share in equal division areas. We also find that self-employment out of agriculture is slightly higher in equal division areas as well as the density of middle schools which addressed students who want to become an apprentice. Finally, patenting activity was higher in equal division areas. Overall, the data suggest that a more even distribution of wealth enabled workers to take up new opportunities in a changing economy.

Our findings are in line with the literature on landholding inequality and economic growth. Galor, Moav, and Vollrath (2009) formalize in a theoretical model that a more equal land distribution supports the rise of a new entrepreneurial elite during industrialization. The entrepreneurial elite then supports education of the former unskilled labor force. Data on education spending during the high-school movement in the US delivers evidence for that model. Similarly, Cinnirella and Hornung (2016) find for Prussia a negative cross-sectional relationship between large landholdings and primary school enrollment rates throughout the 19th century. With our findings that equal division counties are characterized by higher innovative activity and more self-employed people out of agriculture, we deliver evidence that a more equal land distribution fosters the rise of a new entrepreneurial elite and with that economic growth.

More broadly, our study builds on several additional strands of the literature. We add causal evidence to the literature on the long-term effects of historical conditions on the economic development see, e.g., La Porta, Lopez-de Silanes, Shleifer, and Vishny (1998); Acemoglu, Johnson, and Robinson (2001); Glaeser, La Porta, Lopez-de Silanes, and Shleifer (2004); Nunn (2009); Alesina, Giuliano, and Nunn (2013) as well as to a body of literature studying the determinants of growth at the regional level see, e.g., Glaeser, Kallal, Scheinkman, and Shleifer (1992); Long and Shleifer (1993); Gennaioli, La Porta, Lopez-de Silanes, and Shleifer (2013) and to the literature on the economic and political consequences of landholding inequality see, e.g., Gerschenkron (1966); Banerjee, Iyer, and Somanathan (2005); Acemoglu, Bautista, Querubín, and Robinson (2007); Ziblatt (2008); Becker, Cinnirella, and Woessmann (2010); Bleakley and Ferrie (2013). In addition, our study builds on previous research on industrialization in Germany see, e.g., Gerschenkron (1989); Tilly (1969); Herrigel (2000). Related to our work, Duranton, Rodríguez-Pose, and Sandall (2009) correlate the regional prevalence of particular medieval family types with long-run outcomes across European regions. Contrary to our findings, they observe lower GDP levels in what we would classify as equal division areas. Similarly, the authors find higher levels of manufacturing and lower levels of service sector employment in what we would classify as equal division areas. These differences in the results might arise from differences in methodology as their analysis relies on a broader, cross-regional comparison. Habakkuk (1955) analyzes the role of family structures focusing primarily on population growth. He hypothesizes that single-heir systems slow population growth whereas equal sharing rules foster it. With regard to mobility, Habakkuk (1955) argues that unequal division areas faced more out-migration as non-inheriting children had fewer ties to the parental home. Analyzing micro-data from 19th century Hesse-Cassel, Wegge (1998) finds some support for this hypothesis. Finally, our study speaks to the historical origins of entrepreneurial activity.

This chapter proceeds as follows: Section 5.2 discusses the history and the hypothesized origins of agricultural inheritance rules in Germany. Section 5.3 gives an overview of the different data sets we use. We present and discuss our empirical strategy in Section 5.4. Section 5.5 shows the estimated effects of equal division on historical and modern inequality measures. Section 5.6 discusses the effects of equal division on economic development and well-being - today, before the industrial revolution and from the onset of industrialization on. Section 5.7 provides evidence for mechanisms through which equal division might have affected modern outcomes. Section 5.8 shows robustness checks for our inheritance rule variable and different samples and Section 5.9 concludes.

5.2 Historical background of inheritance rules

Historically, two main rules of inheritance for farms and agricultural land existed in Germany, prescribing equal division ('Realteilung') and unequal division of land ('Anerbenrecht') (Rösener, 2012).[3] Under unequal division inheritance, agricultural property is considered indivisible and has to be passed on to a single heir. The most common unequal division rule prescribed "primogeniture", thus making the oldest son the designated heir. Historically, daughters and last-borns did not have a claim to the parental land and received little or no compensation in most unequal division areas. As a consequence, the non-inheriting children typically became landless and worked as farmhands on the brother's farm (Cole and Wolf, 1995) or as factory workers (Becker, 1998), unless they married into a landed family. Under equal division inheritance, land holding is split equally among all children including daughters.[4]

[3] These are also referred to as partible and impartible inheritance, respectively. Variations in inheritance rules are also present in other parts of Europe. In England for example traditionally non-partible inheritance is applied (Alston and Schapiro, 1984) while in France (Crouch, 2005) as well as the Netherlands (Alston and Schapiro, 1984) people split farms equally. Spain applies partible inheritance in the South (Andalusia) and non-partible inheritance in the other parts (Tur-Prats, 2013).

[4] Rouette (2003) stresses that agricultural inheritance rules are not fixed. She points to two changes in the 19th century: The industrialized Ruhr area switched from unequal to equal division while the area around Paderborn increasingly practiced unequal division. We stick to the maps provided by the sources in the late 19th century.

In the 19th century, equal division led to the parcellation of arable land (Becker, 1998). The threat of harming the productivity of agriculture and the existence of farmers puts eliminating equal division on the political agenda (Rouette, 2003; Eheberg, 1883). The Nazis implemented such a reform through the 'Reichserbhofgesetz' (State Hereditary Farm Law) of 1933 which introduces unequal division all over Germany (Rouette, 2003). Röhm (1957) and Röhm (1961) later finds evidence that equal division rules persisted regionally until the 1950s. Still, inheritance of farms is locally regulated by different state-specific regulations today.[5] The state-specific rules suspend equal division of the inheritance among a community of heirs as prescribed by the German Civil Law Code (BGB §§1922). Bavaria, Saarland, Thuringia, Saxony, Saxony-Anhalt, Mecklenburg-Vorpommern, Brandenburg, and Berlin follow BGB §2049 and §2312 (Landgüterrecht) which in general prescribes equal division but regulates that farms are assessed at a lower value than other property. The aim remains to secure productivity of agriculture.

Since we use geographic variation in the historical prevalence of inheritance rules, the question of which factors lead to the adoption of a particular inheritance rule in a locality arises naturally. This question remains debated among historians, who concur that the rules had been in place since at least the Middle Ages: Two of the first written codices, the Lex Salica of 507 AD and the Sachsenspiegel of 1220 AD, regulated agricultural inheritance. The Lex Salica prescribed equal division among male offspring in Frankish lands (South-Western Germany) (Behrend, 1897), whereas the Sachsenspiegel prescribed a single heir in parts of the North-East (Blanckmeister, 1913).[6]

5.3 Data

This section provides a detailed description of our data sources and shows summary statistics of how equal and unequal division counties differed. The unit of observation throughout is a rural county in Germany at different points in time.[7] The counties' locations are indicated by historical and modern maps of German counties provided by MPIDR [Max Planck Institute for

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5 Hamburg, North Rhine-Westphalia, Lower Saxony, and Schleswig-Holstein introduced unequal division inheritance by the the Höfeordnung (HöfeO from 26 July 1976, BGBl. 1, S. 1933). Baden-Wuerttemberg applies the Badisches Hofgütergesetz and Württembergisches Anerbengesetz. Hesse follows the Hessische Landgüterordnung, Rhineland-Palatinate the Rheinland-Pfälzische Höfeordnung, and Bremen the Bremisches Höfegesetz.

6 A hypothesis dating back to at least Weber (1924) is that unequal division was established where local feudal lords or the state had the power and incentives to prohibit the division of land as it was thought that larger land plots could be taxed more easily (Rösener, 2012). Other scholars have suggested that features of the terrain or the soil were conducive to the adoption of one inheritance rule over the other (Schröder, 1979). Following Boserup (1965) and Fastenmayer (2009) soil quality in combination with crops that support plough use would give an advantage to unequal division rules.

7 Sample restrictions exclude free cities from our analysis for several reasons. First, agriculture played a minor role in cities. Second, urbanization triggered migration into cities at a large scale and brings people

Demographic Research] and CGG [Chair for Geodesy and Geoinformatics, University of Rostock] (2011) and the German Federal Institute for Research on Building, Urban Affairs, and Spatial Development (BBSR). We link our historical sample, consisting of 900 rural German counties in 1895, either spatially, via official county codes, or via county names to our modern sample of 397 counties in 2014.

5.3.1 Inheritance rule

Figure 5.1 shows the spatial distribution of different inheritance rules in 19th century Germany. We distinguish equal division (green) from unequal division areas (blue). Unequal division is prevalent in a majority of regions. With few exceptions, equal division is found in the southwest of Germany. The map is a combination of various sources in form of maps and texts from the late 19th century: The first comprehensive overview of the geographic distribution of inheritance rules in Prussia was created in 1894 when the Prussian government conducted a survey among judges and county administrators to inquire into the nature and history of inheritance rules in their jurisdiction (Rouette, 2003). The survey results were later published in Sering (1897). Around the same time, similar surveys were conducted in the other kingdoms of the German Empire by Verein für Socialpolitik (1883), Grossherzogliches Ministerium des Inneren (1883), Miaskowski (1882-84), Fick (1895), and Krafft (1930). Several decades later, the geographers Hartke and Westermann (1940) created an overview map that depicted the local prevalence of particular inheritance rules based on the results published by Sering (1897) and others. These surveys allow a very fine-grained categorization of inheritance rules by locality, typically at the village level.[8] In our samples the inheritance rule of each county is classified by the inheritance rule of the majority of the area of a county.

5.3.2 Data sources

Our main outcome variables stem from the INKAR 2014 data set provided by the BBSR and from censuses of the Federal Statistical Office of Germany or its predecessors from the late 19th century on. The INKAR 2014 data includes official aggregated information on income, education, and industry structure on the county level. For finer measures of income, we incorporate county-level data on income taxes provided by the German Federal Statistical Office. These data provide information on income bins, including the mean income and the number of people within each bin. We use that data to calculate within-county inequality measures. His-

with unequal division origin into areas of equal division and vice versa. Migrants' behavior influences the outcome variables of cities and we cannot distinguish if this is driven by people with equal or unequal division background.

8 We follow the original sources as Hartke and Westermann (1940) published their map under the Nazi regime and might have been influenced by the propaganda similar to Huppertz (1939). For counties for which we could not identify the prevalent inheritance rule from the original sources we filled the gaps from the comprehensive map of Hartke and Westermann (1940).

torical data on farm sizes and occupations stem from official German censuses on agriculture (Kaiserliches Statisisches Amt, 1912) and censuses on occupations and businesses (Kaiserliches Statisisches Amt, 1897, 1910) that we digitized. These statistics allow us to calculate average farm sizes, the share of farms in a specific size category, the population per county and employment shares in agriculture, manufacturing, and trade and services. Within occupations we can identify innovative branches in manufacturing as specified in (Streb et al., 2006). We enrich agricultural information by data on landholding inequality from 1895 provided by Ziblatt (2008). The distribution of height in Bavarian conscripts in the 19th century serves as individual data on inequality (Baten, 1999, 2000). Further measures of economic development come from the census on occupations and businesses from 1925 which allows us to distinguish between employees and laborers (Fritsch and Wyrwich, 2016). [9] For agricultural productivity before and after 1500 we use data on caloric output of land by Galor and Özak (2016). Another measure of pre-industrial economic development is population data which we obtain from Bairoch, Batou, and Chevre (1988) and the Statistical Office of the German Empire which published the population of 1375 German towns and cities in 1867, 1871, and 1875 (Kaiserliches Statistisches Amt, 1877). Additionally, we use data from the iPEHd on Prussia provided by Becker et al. (2012) to evaluate pre-industrial development. Patent data from Streb et al. (2006) which includes all valuable patents filed in Germany between 1877 and 1914 serves as a second measure for innovative activity.[10] Table D-1 in the Appendix provides a brief overview of the variables we use from these data sets.

We use three types of control variables: geographical variables, cultural and institutional variables, and controls for the location. The geographic control variables come from GIS raster data depicting current information on soil, climate, elevation, and navigable waterways. We rely on data from the European Soil Data Base, the free climate data from WorldClim.org and navigable waterways from Kunz (2004). The calculation of average elevation of a county is based on data from (Jarvis, Reuter, Nelson, and Guevara, 2006). Cultural and institutional control variables come from various sources: The share of protestants in a county stems from Meyers Grosses Konversationslexikon (1905) and the general law type stems from a map by Schröder (1870). Hanseatic involvement is inferred from a map by Helmolt (1902) as is a dummy for belonging to the Frankish territory from Shepherd (1911). We include controls for location in the form of longitude and latitude of the centroid of a county as well as a dummy for the historical state the county belongs to.

[9] We thank Michael Wyrwich for kindly sharing their data.
[10] We thank Jochen Streb for kindly sharing his data.

5.3.3 Summary statistics

Table 5.1 shows summary statistics for equal and unequal division counties. These statistics assess to what extent the two groups differed in their control characteristics. Panel A lists the geographic controls. In equal division areas, the average temperature and elevation are slightly higher. Unequal division areas have a significantly higher share of sand, silt, and loam in the soil, while the share of loess, which is favorable for agriculture, does not differ significantly between the two groups. Panel B shows the summary statistics of the cultural and institutional control variables. While Frankish territory and Code Napoleon mainly appear in equal division areas and the Hanseatic League and Prussian Law in unequal division areas, the share of protestants does not differ significantly between inheritance rules. We conclude that equal and unequal division areas are not completely balanced but differ in some aspects which is why we include the geographic and cultural control variables later on in our analyses and use geographic regression discontinuity models with counties close to the boundary as our preferred specification.

5.4 Empirical strategy

We apply two empirical strategies to estimate the effect of equal division on inequality and economic development. First, we estimate OLS regressions with a rich set of control variables including flexible controls for the location of the county. Second, we view the location where unequal division changes to equal division as a boundary and discontinuous jump in inheritance rules which is determined by longitude and latitude. In this framework we apply a multidimensional, semi-parametric regression discontinuity (RD) approach similar to Dell (2010) to identify the effect of equal division. Our estimation model is:

$$Y_{cs} = \alpha + \gamma \cdot Equal\ Division_c + X_{cs}^{'}\beta + f(Geographic\ Location_c) + \phi_s + \epsilon_{cs}. \tag{5.1}$$

The outcome Y_{cs} is a specific outcome measure of county c in state s. $Equal\ division$ is an indicator variable for equal division inheritance in county c. The coefficient of interest γ measures the effect of equal division on the outcome variable Y_{cs}. The matrix X_{cs} contains control variables for county c. In the OLS specification, the RD polynomial $f(Geographic\ Location_c)$ is a linear function of longitude and latitude.

Our regression discontinuity specifications include geographic controls for longitude, latitude, and distance to the boundary. The main specifications contain these variables as linear terms, however, our results are qualitatively and quantitatively robust to using a quadratic polynomial instead. The term ϕ_s determines the state in which county c is located. Counties of one state are clustered locally. Therefore, the state dummies divide the border of inheritance rule into nine different segments.[11] For the OLS specification we use the full sample of all rural counties in Germany in a specific year. For the RD specification we reduce the sample to counties with a centroid in a 35 km radius of the border as figure 5.2 shows. Standard errors are clustered on the district level which is one aggregation level above the county level.[12] Counties are weighted by the number of their inhabitants in order to allow a population-related interpretation of our results.

The RD approach relies on the identification assumption that the characteristics between the two groups - i.e. across the border - vary smoothly. Usually, this assumption is tested by plotting that all controls in an RD setting do not change discontinuously at the border. This does not hold in our case. However, the identification assumption is satisfied if the controls do not change discontinuously *conditional* on the other controls. In order to check if this assumption holds, we test whether predicted inheritance rules (by our controls) differ discontinuously at the boundary between the two regimes. To test whether counties have selected themselves by unobserved factors which are positively correlated with the outcome variables into a specific inheritance rule, we check if long-term development measures differ discontinuously at the boundary.

Figure 5.3 plots the predicted equal division, based on all control variables, against the distance of a county to the border of inheritance rule. No jump or discontinuity in the outcome variable at the boundary can be detected. Moreover, figure 5.3 reveals that the relationship between controls and inheritance rule in a range of 35 km left and right of the border can be well approximated with a linear specification. Taken together, this evidence supports the identification assumption and suggests that, close to boundary, equal and unequal division areas did not differ discontinuously in the characteristics that determine particular inheritance rules in the cross-section.

One might also be concerned about counties at the border sorting into equal or unequal division due to unobserved characteristics which are positively correlated with economic growth and income today. We check the continuity of county characteristics following Card, Chetty, and Weber (2007) and plot a measure of predicted long-term development, based on all control variables, against distance to the border in figures 5.4 and 5.5. If counties sorted into equal division based on unobserved characteristics, which are positively correlated with income today, figure 5.4 and 5.5 should reveal a positive discontinuity with respect to GDP per capita and household income per household member, respectively. The evidence does not support

11 The segments including both inheritance rules are: Prussia, Bavaria, Baden, Wuerttemberg, Hessia, Schwarzburg Rudolstadt, Sachsen Weimar Eisenach, Sachsen Meinigen Gotha, Sachsen Coburg Gotha.

12 There are 51 districts i.e. clusters in 35 km radius to the inheritance rule border.

such a conclusion. For a measure of predicted GDP per capita, no large positive discontinuity at the boundary is discernible. For predicted household income, we find an economically small change at the boundary, with equal division areas having features that are associated with, if anything, slightly lower long-term GDP per capita levels. Taken together, the results in figures 5.3, 5.4, and 5.5 lend support to a geographic discontinuity strategy.

5.5 Inequality

The first step of our empirical analysis is to assess the relationship between equal division inheritance and historical measures of inequality and inequality today.

5.5.1 Historical inequality

We start with assessing the relationship between equal division inheritance and measures of landholding inequality within counties in 19th century Germany. Data of the first comprehensive agricultural census for the German Empire in 1895 shows substantially lower inequalities in the distribution of the production factor land in equal division counties. Landholding Gini coefficients based on calculations by Ziblatt (2008) are lower and the distribution of farms sizes is shifted to the left. Table 5.2 presents that counties which featured equal division have a significantly lower Gini coefficient with a magnitude of about a third of a standard deviation (SD) in our RD approach. The data allow us to show the percentage of farms in size categories below 2 hectares, between 2 and 5 hectares, between 5 and 20 hectares, and between 20 and 100 hectares.[13] In equal division counties the prevalence of small farms is significantly higher while there are lower shares of large farms. The effects are robust to including geographic and cultural controls (Panel B) and to the restrictions of the RD approach (Panel C). Additionally, in equal division counties the number of farms per inhabitant are higher (positive coefficient of equal division, however, not significantly (not shown here). A similar distribution of farm sizes is already visible in pre-industrial Prussian census data of 1816. Table D-2 in the Appendix shows that the share of small landholdings was significantly higher in equal sharing counties while the share of large landholdings was significantly lower.

Historically, agricultural land was more equally distributed in equal division counties, as evidenced by a higher number of small and fewer large farms. The snapshot in 1895 does not reveal whether equal division was still performed at the end of the 19th century. Differences might have emerged hundreds of years ago and may not have faded away by 1895. Table D-3

[13] The census also includes a category for farms above 100 hectares. There are no significant differences between equal and unequal division counties in the share of farms above 100 ha. It is likely that church land and feudal estates which existed in both inheritance regimes fall in that category and were not affected by inheritance rules.

shows that this is not the case: a difference-in-differences analysis between 1895 and 1907 shows that within 12 years farms in equal division areas became significantly smaller and the share of small farms increased even more.

Overall, the results document two core facts: First, until the end of the 19th century, equal division affected the distribution of land. Second, although people could have sold their inherited land and moved to cities or abroad, this was not common practice. A Coasean argument would suggest that inter vivos land transactions should have concentrated land ownership in equal division areas if transaction costs were low and if concentrated land ownership were optimal for agricultural productivity. Yet, the evidence shows persistent differences in landholding inequality at the boundary between the two inheritance regimes.

5.5.2 Inequality today

If there are persistent effects of historical inequality, the most prominent mechanism would be via inequality itself.[14] Then, inequality levels today should also be lower in equal division areas. Although equal division is negatively correlated with historical measures for inequality, the significant lower inequality does not persist until today. Table D-4 shows that if at all, equal division counties have a higher inequality level in income today. This holds particularly for the Gini coefficient in column 1. Column 2 and 3 show that as well the 10th as the 90th income percentile is slightly higher in equal division counties. These results suggest that the income distribution is shifted to the right and everybody is better-off while the distribution is close to the distribution of unequal division counties.

5.6 Economic development

We next assess the long-term relationship between equal division inheritance rule and measures of economic development today. To ensure that this relationship is not spurious and driven by other long-term differences between equal and unequal division counties.

5.6.1 Level of economic development today

We use several measures of development and income in 2014 and show that today's income and GDP per capita is, on average, higher in equal division counties than in unequal division counties. Table 5.3 shows that the correlation between average income and equal division inheritance is positive, highly statistically significant, and robust across all specifications. The magnitudes for all income measures are around 45 percent of a SD (except for median income

[14] A lower inequality could be transmitted via institutions like local taxes or via culture for example manifested in civic engagement, opinions towards inequality, local NPOs, etc.

at about 27 percent). Equal division counties have 6-14 percent higher modern income and GDP levels.

Other measures of wealth like educational outcomes and industry structure (Table 5.4) provide a first hint as to why these large differences in income might have emerged. In equal division counties, the percentage of the population with a college degree is about 50 percent of a SD higher (Table 5.4 in the Appendix). Simultaneously, the share of people with a vocational degree is lower, while the share of people without degree does not differ (not shown). Additionally, equal division counties have higher employment in the trade and service sector and particularly in creative industries. There are also more firms present (particularly small firms) in equal division counties. These differences suggest that human capital and the industry structure might contribute to the large income differences today. In the following we will discuss these and further potential channels and, in particular, shed light on the importance of industry structure in dimensions of innovation and structural change.

5.6.2 Development before industrialization

We now explore if equal division counties had advantages in economic development before industrialization started. The large income gaps between equal and unequal division counties today might not be driven by a more equal distribution of the production factor land in the past but by unobserved characteristics which made equal division counties better off ever since. As measures for pre-industrial wealth and development are rare and hardly available at a geographically disaggregated level for the whole German territory[15], we draw on different data sets and subsamples. Our analysis includes four observable determinants of long-term growth: First, we discuss agricultural productivity measured directly by potential caloric output per hectare per year, average farm sizes, and Prussian grain yields. Second, we use population density as a long-run measure of development. Third, we analyze early economic progress which is examined by Prussian census data on early industrialization, employment, and wages. Fourth, we inspect human capital development as covered by Prussian educational censuses.

Agricultural Productivity We test whether equal division counties had more favorable conditions for agriculture that might have contributed to different long-term development trajectories. An index of caloric output before the year 1500 per hectare per year constructed by Galor and Özak (2016) is the outcome variable in column 1 of table 5.5. Although the coefficient is positive, it is not robust to the inclusion of controls and vanishes in the RD specification. In addition to differences in land, there might be discrepancies between potential productivity of land and realized output. In column 2 we draw on data on grain yields from Prussia in

15 Germany was split into independent kingdoms and principalities until German unification in 1871.

1878 which show slightly lower yields in equal division counties, although the difference is far from being economically or statistically significant. These results provide evidence for the hypothesis that agricultural productivity was similar in equal division and unequal division counties.

Population Density Long-term data with direct evidence on levels of economic development is scarce so we draw on urban population data from 1500 onwards based on (Bairoch et al., 1988) to assess measures of development before the Industrial Revolution in equal and unequal division areas. We find that the density of urban population developed similarly in equal and unequal division areas (Figure 5.6). If anything, population density is slightly lower throughout in equal division areas. A potential objection to the use of urban population data in the context of our study might be that rural population density could be a better measure for development in the context of agricultural inheritance rules. As shown in table 5.6, the population density in Prussia in 1816 was not higher in equal division counties. For the sample of the whole German territory we show that the density is still not significantly higher in equal division counties by 1895 but we can detect relative increases by 1907. This increase in the population density from 1895 to 1907 is significantly higher in equal division counties than in unequal division counties which is, however, driven by counties which surround free cities. Excluding these counties from the sample eliminates statistical significance.

Early Industrialization, Employment, Income, and Wages Drawing on Prussian data, we can assess the relationship between inheritance regimes and measures of early industrialization. Prussian censuses are available from 1816 on and cover a lot of disaggregated data on historical counties.[16] Although Prussia had only very few equal division counties, table D-6 show that our main results on inequality also hold in a subsample of (historical) Prussia. Data on the number of factories, mills, and looms in 1821 as well as self-employment out of agriculture, and employment in manufacturing in 1882 shed light on the economic situation of equal division counties before industrialization started. Income and county taxes in 1878 and daily wages in 1892 show if industrialization had a positive effect in equal division counties very quickly.

Table 5.7 shows that there are hardly any significant differences between equal and unequal division counties in Prussia. If at all, coefficients are negative for the density of factories, mills, and looms suggesting that adoption of new technologies must have started later than 1821. In 1882, there are also no differences in the percentage of people who are self-employed (outside of agriculture) and in the percentage of people working in manufacturing.

[16] More information about Prussian census data is given in Becker et al. (2012).

For income and wages, table 5.8 shows that there are no significant positive effects of equal division on various measures of revenues from income and local county taxes. The difference in daily wages for males is also very small and insignificant.

Human Capital We rely on Prussian educational censuses which document the number of schools, students, and literacy rates early on to shed light on the human capital stock and development in equal and unequal division counties. Table 5.9 reveals that the percentage of people who could read and write and the percentage of illiterate people were not significantly different between equal and unequal division counties in 1871 when including our controls or RD approach. There are no differences between equal and unequal division counties in school density or pupils in pre-industrial 1816 or at the onset of industrialization in 1886.

Bringing the results on agricultural productivity, population, early industrialization, income, and education together, reveals that equal division counties did not have more advantageous starting conditions than unequal division counties before the Industrial Revolution. This allows us to rule out a broad class of potential confounders that could have contributed to the large differences in outcomes we observe in 2014.

5.6.3 Development from industrialization onward

To shed light on the potential pathways through which inheritance rules may have affected long-term outcomes, we investigate the industry structure in equal and unequal division areas. Table 5.10 reveals that in equal division counties agricultural employment was lower, while employment in manufacturing and in trade and services was slightly higher than in unequal division counties in 1895 and 1907. The magnitude of the coefficient is larger for manufacturing and amounts to about a quarter of a SD. It increased from 1895 to 1907 by 35 percent of a SD. Simultaneously, the coefficient of equal division for employment in agriculture decreased almost by the same magnitude from 1895 to 1907. To get an idea when equal division counties started off to industrialize quicker than unequal division counties we use Prussian census data from 1882. Table D-7 shows that there are no significant differences between the areas in agriculture and manufacturing in 1882. We interpret these findings as evidence that equal splitting counties industrialized quicker from the late 19th century on.

5.7 Mechanism: innovation and occupational choice

Why were equal division counties more successful in industrializing and featured better long-term outcomes? A class of models hypothesizes that a more equal distribution of wealth may increase long-term growth by giving broader parts of the population the opportunity to become a skilled worker or entrepreneur see, e.g., Galor and Zeira (1993); Banerjee and Newman (1993); Ghatak and Nien-Huei Jiang (2002); Galor and Moav (2004). Compared to a situation in which a large part of the population has essentially no wealth, a more even distribution of wealth can alleviate credit constraints in parts of the population or provide a buffer to absorb the potential risks of innovating, investing in human capital, or becoming an entrepreneur, all of which have favorable consequences for growth. We take this hypothesis to the data and test the relationship between inheritance rules and occupational choice and innovative activity.

Distinguishing between 163 occupations in manufacturing in 1907, we find that employment in innovative branches of manufacturing was higher in equal division areas. We follow Streb et al. (2006) who categorize metal working, industry of machines and instruments, chemical industry, printing, and photography as innovative branches based on the amount of patents between 1877 and 1914. Table 5.11 shows the effect of equal division on employment in innovative manufacturing occupations. The coefficient is almost the same as in table 5.10, which leads us to the conclusion that the additional employment in manufacturing comes almost entirely from occupations in innovative branches.

Using patent data of Streb et al. (2006) directly in columns 3 to 5, we find that innovative activity was higher in equal division counties from 1877 to 1914. The positive correlation holds when using a dummy variable for having filed a patent in that time, using a log of the total number of patents[17] to include only counties with patenting activity, and when using the log total number of patents including the counties with no patenting activity as zeros. The magnitude is quite large at about a third of a SD. As presented in table 5.12, there are further, suggestive results that are consistent with an occupational choice mechanism, through which inequality may have affected long-term outcomes, even though a lack of precision in our estimates prohibits a definitive interpretation of these findings. Considering a rough proxy for entrepreneurial activity, we find that equal division countries have a higher percentage of self-employed people out of agriculture in 1925. Additionally, there is some weak evidence that the density of middle schools and the share of middle-school pupils is slightly higher in equal division counties. These schools were primarily attended by students wo wanted to become an apprentice in a particular trade. These additional results are not robust across specifications and the effects are only imprecisely estimated but the results are broadly consistent with landholding inequality affecting longer-term outcomes through occupational choice.

[17] Using the log is necessary as there are some counties which are extrem outliers in patenting activity compared to the other counties. While the 50th percentile of filed patents is 1, the 99th percentile lies at 123 patents and the maximum is 913 filed patents between 1877 and 1914.

5.8 Robustness checks

In the following we provide some robustness checks on our indicator for equal division and on our RD specification.

5.8.1 Inheritance rule

As our main explanatory variable – the equal division indicator – stems from sources from varying times and quality, we re-estimated the coefficients for our main findings in restricted samples. First, we restrict the sample to Prussia, Bavaria, Baden, and Wuerttemberg, to rely completely on pre-Nazi area maps and not on the 1940 map of Hartke and Westermann (1940). As these states were the larger states, they have already had a high quality statistical office in the 19th century. They provided maps or lists of their territory determining the predominant inheritance rule there. Table D-8 shows that our main results are robust to restricting the sample to the states with the best sources for inheritance rule. Second, we exclude all counties which could not be clearly defined as belonging to the equal or unequal division regime. We define as equal division county a county where the majority of the area is coded as *equal division*. In this analysis all counties which are not completely equal sharing or unequal sharing are dropped. Table D-9 shows that the coefficients go into the same direction, however, as sample size decreases, the coefficients cannot be estimated that precisely anymore.

5.8.2 RD sample restriction

For the RD specification the sample restriction parameter of a 35-km distance to the border is set arbitrarily. In the following we play around with the distance radius and show that results do not change if we restrict the sample to a 20, 50, or 80 km distance to the border. A larger distance than 80 km would only include more unequal division counties as the maximum distance to the border in the equal division regime is 80.4 km. Table D-10 shows in panel A the results for the 20 km distance, in panel B the results for the 50 km distance and in panel C the results for the 80 km distance. Overall, our results are robust to the border sample restriction.

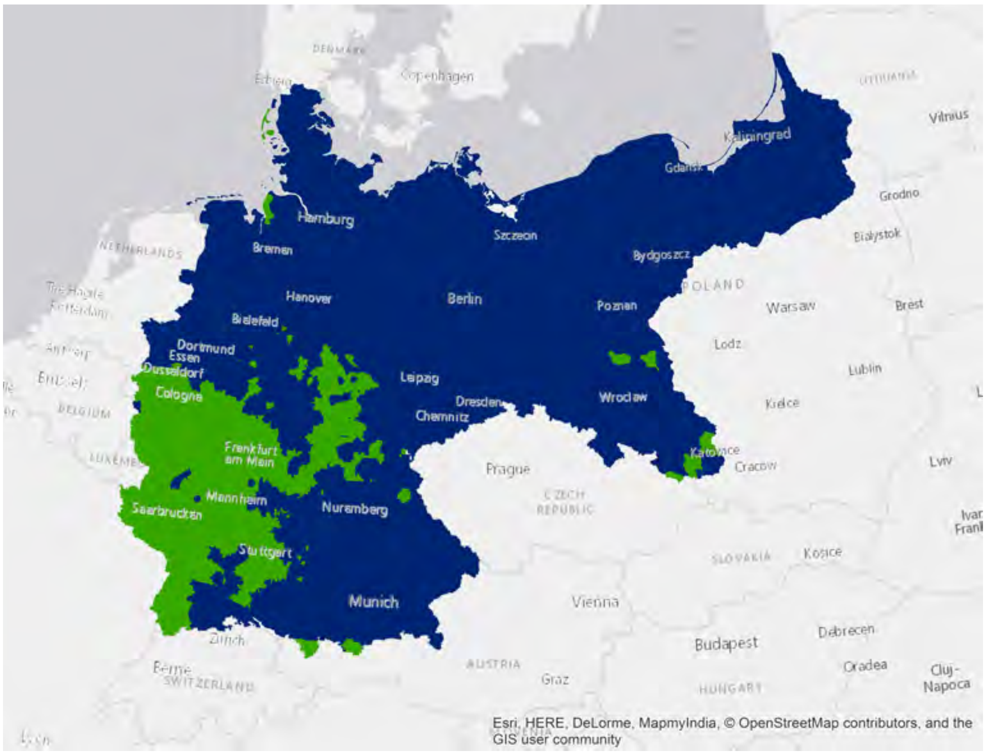
5.9 Conclusion

A long-lasting and ongoing debate in economics concerns the association between wealth inequality and economic growth. Increasing wealth inequality in the US and Europe in the past 30 years lends renewed relevance to the resolution of the debate. However, data on inequality at the local level are rare and exogenous shifts of inequality scarce which makes it hard to identify causal effects. We exploit variation in a historical institution, namely agricultural inheritance rules, which regulated the distribution of land since the Middle Ages in Germany.

Agricultural inheritance rules vary between unequal division where land is indivisible and is passed on to a single heir and equal division of land among all siblings. Our identification strategy relies on a RD approach with longitude, latitude, and distance to the inheritance-rule border as running variables. We add a rich set of controls for geographical, cultural, and institutional characteristics of counties. We find that counties which divided land equally among siblings had historically lower landholding inequality. Today, counties which historically divided land equally have a higher GDP per capita and higher individual income. We show that equal division counties did not have better starting conditions before the industrialization by testing for differences in agricultural productivity, urban population, population density, early technological progress, or human capital stock. Instead, we propose the mechanism that equal division counties started to industrialize quicker in the late 19th century than unequal division counties focusing on occupations in particularly innovative branches. Additionally, we find evidence for higher entrepreneurial activity in equal division counties. We conclude that a more equal distribution of wealth enables people to take up new occupational opportunities in a changing economy, which ultimately contributed to long-term growth.

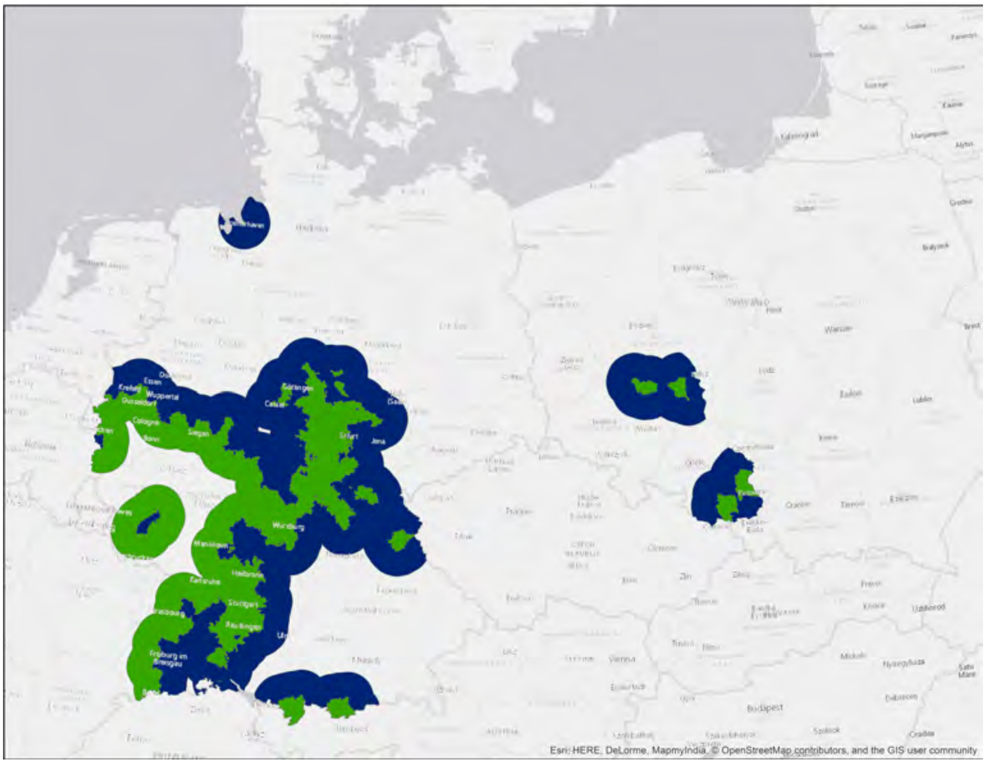
5.10 Figures and tables

Figure 5.1 : Map inheritance rules - equal and unequal division



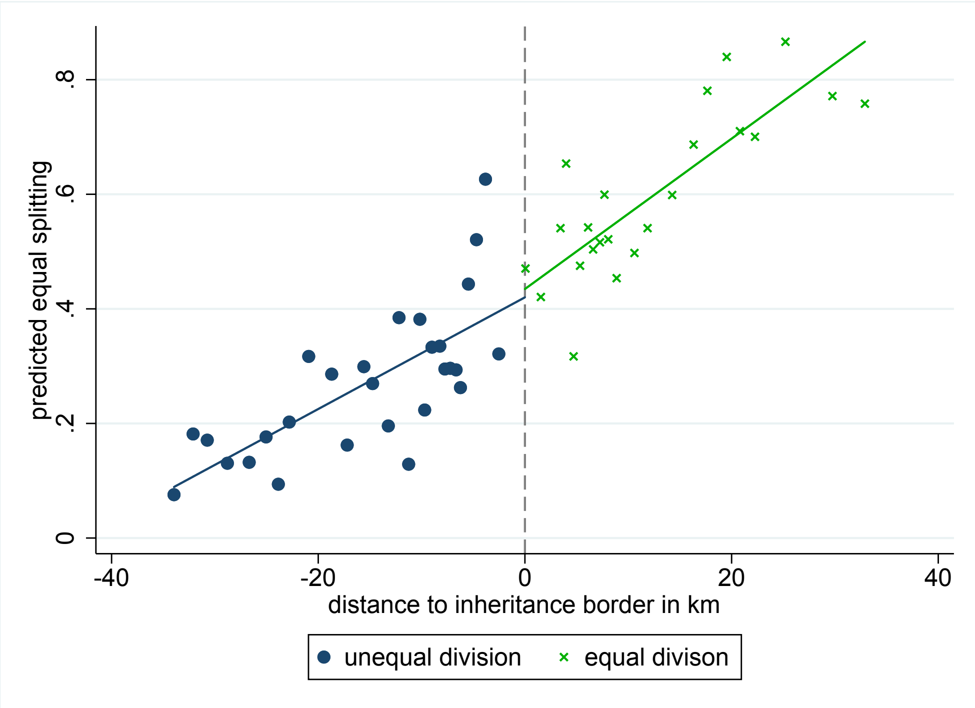
Notes: The map shows the distribution of inheritance rules in Germany with its borders of the 19th century. The map distinguishes two types of inheritance rule: unequal division (dark blue) and equal division (green). The areas of milder forms of these two types are categorized as unequal division.

Figure 5.2 : Map of inheritance rules in the 35-km-border sample



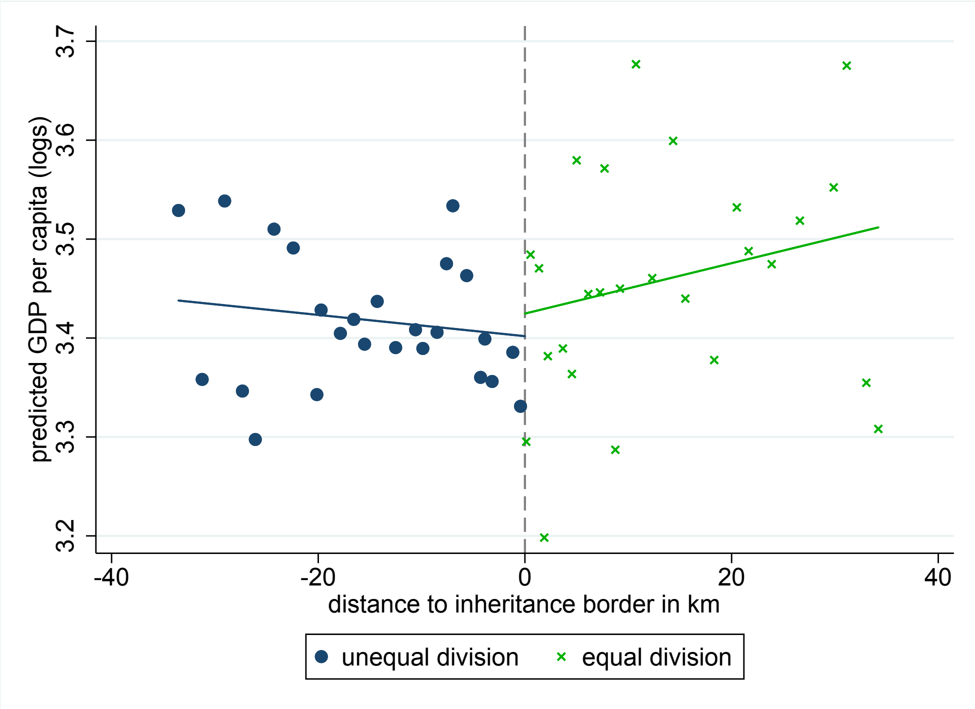
Notes: The map shows the distribution of inheritance rules in Germany with its borders of the 19th century. The sample is reduced to a 35 km radius around the border of the inheritance rule regimes. This sample restriction is used in the RD approach. The map distinguishes two types of inheritance rule: unequal division (dark blue) and equal division (green). The areas of milder forms of these two types are categorized as unequal division.

Figure 5.3 : Predicted equal division



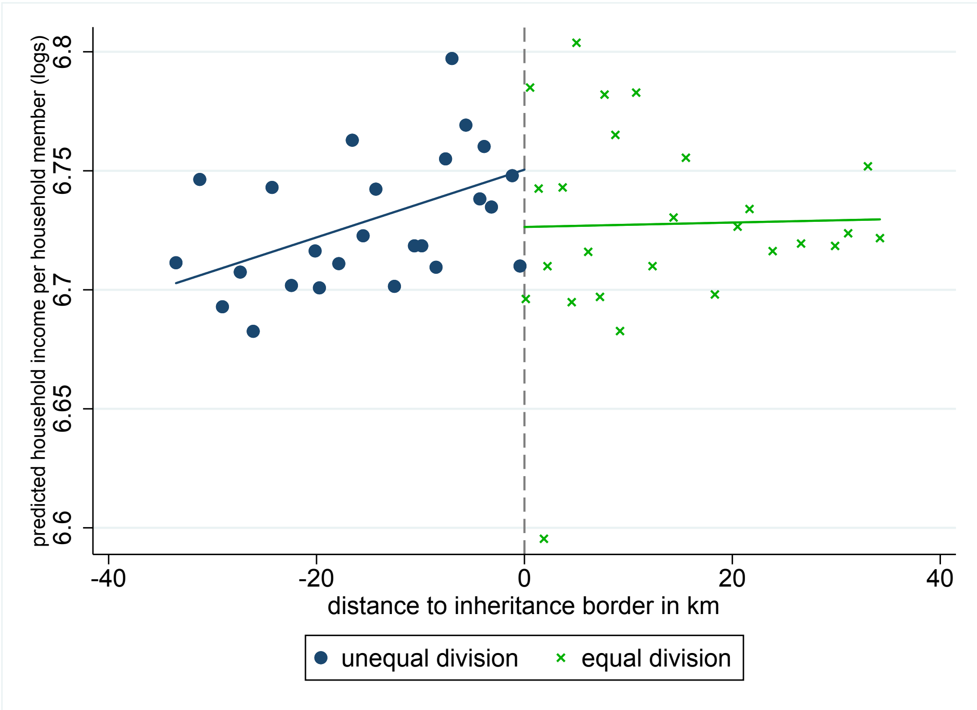
Notes: The figure depicts our identification assumption for the RD approach. The y-axis shows predicted equal division by our control variables. The x-axis shows the distance to the inheritance-rule border (at zero). The left-hand side of the graph shows the evolution of the predicted equal division in the unequal division area. The right-hand side shows the evolution in the equal division area. Section 5.4 provides details on the identification assumption and the prediction of equal division.

Figure 5.4 : GDP per capita (logs)



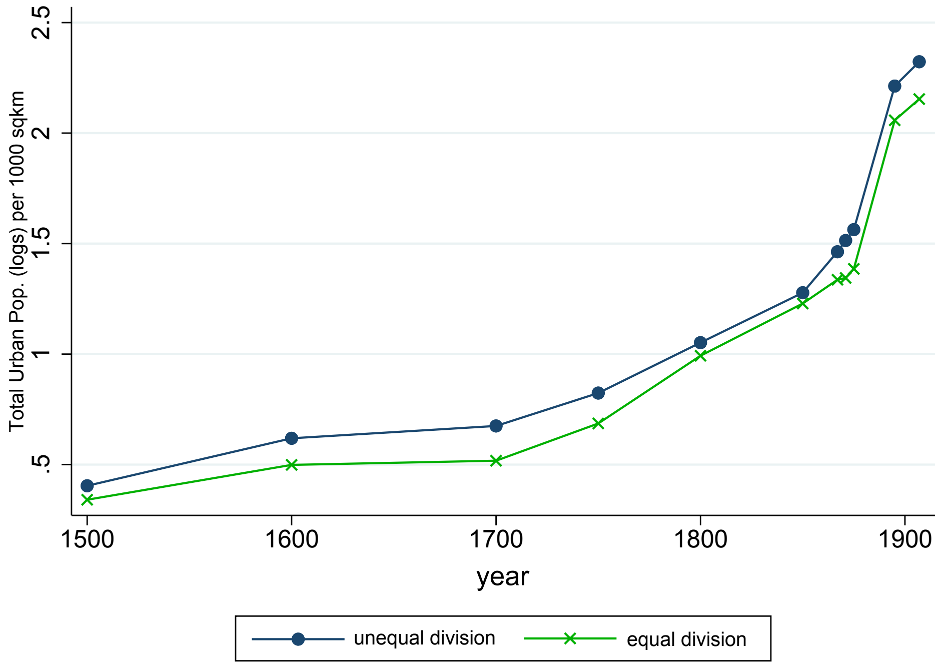
Notes: The figure depicts the RD approach we use in a binscatter plot. The outcome variable on the y-axis is GDP per capita in logs. The x-axis shows the distance to the inheritance-rule border (at zero). The left-hand side of the graph shows the evolution of GDP per capita in the unequal division area. The right-hand side shows the evolution in the equal division area. Section 5.3.2 and table D-1 provide details on the data sources.

Figure 5.5 : Household income per household member (logs)



Notes: The figure depicts the RD approach we use in a binscatter plot. The outcome variable on the y-axis is log household income per household member. The x-axis shows the distance to the inheritance-rule border (at zero). The left-hand side of the graph shows the evolution of household income in the unequal division area. The right-hand side shows the evolution in the equal division area. Section 5.3.2 and table D-1 provide details on the data sources.

Figure 5.6 : Urban population density per 1000 sqkm: 1500-1907



Notes: The graph shows the evolution of urban population for equal (green) and unequal (blue) division areas separately from 1500 until 1907. The y-axis shows the logs of the urban population weighted by the area of the specific inheritance-rule regime in 1000 sqkm. The data stems from Bairoch et al. (1988) and the Statistical Office of the German Empire which published the population of 1375 German towns and cities in 1867, 1871, and 1875 (Kaiserliches Statistisches Amt, 1877).

Table 5.1 : Summary statistics

		Summary statistics		T-test	
		unequal division	equal division	difference	se
Geographic Controls					
temperature in °C	mean	8.125	8.820	0.241	0.142*
	sd	(0.784)	(0.903)		
precipitation in mm	mean	59.769	61.940	-3.594	2.313
	sd	(12.345)	(8.543)		
elevation in m	mean	249.349	313.408	-45.241	23.185*
	sd	(223.019)	(159.704)		
roughness: difference in elevation	mean	3.820	6.347	0.643	0.469
	sd	(3.172)	(3.352)		
distance to navigable waterway in km	mean	25.840	20.704	-3.930	2.912
	sd	(21.606)	(16.831)		
soil characteristics					
soil: share of sand	mean	0.219	0.012	-0.096	0.033***
	sd	(0.274)	(0.074)		
soil: share of loam, sand, silt	mean	0.609	0.300	-0.145	0.053***
	sd	(0.340)	(0.269)		
soil: share of loess	mean	0.098	0.130	-0.014	0.042
	sd	(0.189)	(0.202)		
Cultural Controls					
Frankish territory in 507 AD	mean	0.093	0.473	0.275	0.075***
	sd	(0.291)	(0.500)		
protentants in %	mean	65.272	47.368	-8.317	6.709
	sd	(38.008)	(33.602)		
Hanseatic league	mean	0.404	0.103	-0.249	0.074***
	sd	(0.491)	(0.304)		
general law					
common law	mean	0.449	0.433	-0.100	0.120
	sd	(0.498)	(0.497)		
Prussian	mean	0.453	0.076	-0.236	0.085***
	sd	(0.498)	(0.265)		
Saxonian	mean	0.040	0.000		
	sd	(0.196)	(0.000)		
Code Napoleon	mean	0.015	0.371	0.329	0.095***
	sd	(0.121)	(0.484)		
Badish	mean	0.043	0.121	0.007	0.005
	sd	(0.203)	(0.326)		
observations	obs	676	224		

Notes: The table shows summary statistics for our control variables in rural German counties in 1895. Column 1 gives the mean and standard deviation of the control variables in unequal division counties while column 2 shows means and standard deviation for equal division counties. Column 3 and 4 show the difference between these groups and test if the difference is equal to zero. The difference and the standard errors stem from a regression which includes longitude, latitude, state-fixed effects, clusters standard errors on the district (Regierungsbezirk) level and weighs observations by population. * = p<0.1, **=p<0.05, ***=p<0.01

Table 5.2 : The effect of equal division on landholding inequality 1895

	landholding Gini 1895		% of farms in size category				Farm size	Number of farms
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	linear	quadratic	<2	2-5	5-20	20-100	in ha	per 1000
	RD poly.		ha					inhabitants
Panel A. OLS								
equal division	-0.0382**	-0.0353*	7.280***	11.87***	-1.863	-17.89***	-1.866***	4.787
	(0.0181)	(0.0198)	(1.346)	(1.502)	(2.125)	(2.168)	(0.545)	(7.004)
Observations	931	931	930	930	930	929	927	931
Panel B. With controls								
equal division	-0.0537***	-0.0505***	6.835***	11.07***	-3.830*	-13.36***	-1.573***	8.756
	(0.0124)	(0.0126)	(1.019)	(1.546)	(2.104)	(1.691)	(0.269)	(6.618)
Observations	931	931	930	930	930	929	927	931
Panel C. Distance to border								
equal division	-0.0459***	-0.0500***	5.798***	9.512***	-5.046***	-10.29***	-1.246***	5.874
	(0.00939)	(0.00986)	(1.021)	(1.592)	(1.683)	(1.618)	(0.164)	(5.750)
Observations	397	397	394	394	394	393	391	397
mean outcome	0.716	0.716	8.242	13.34	33.84	27.08	5.997	127.7
SD outcome	0.123	0.123	7.031	10.69	15.04	16.06	3.318	45.65

Notes: The Gini coefficient stems from Ziblatt (2008), the share of farms in 5 size categories are as stated in 'Statistik des Deutschen Reichs' Vol. 109. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Free cities are excluded. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table 5.3 : The effect of equal division on income measures 2014

	(1) log household income	(2) log taxable income	(3) log median income	(4) log GDP p.c.
Panel A. OLS				
equal splitting	0.0690***	0.0726***	0.0499**	0.133**
	(0.0236)	(0.0205)	(0.0186)	(0.0562)
Observations	397	374	397	397
Panel B. With controls				
equal splitting	0.0617***	0.0637***	0.0446***	0.144***
	(0.0193)	(0.0195)	(0.0103)	(0.0379)
Observations	397	374	397	397
Panel C. Distance to border				
equal splitting	0.0572***	0.0663***	0.0438***	0.143***
	(0.0167)	(0.0168)	(0.00986)	(0.0481)
Observations	198	178	198	198
mean outcome	6.719	3.461	7.956	3.447
SD outcome	0.115	0.146	0.162	0.336

Notes: Data on income and GDP per capita stem from the Federal Statistical Office of Germany and INKAR of 2013/14. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. counties with more than 100.000 inhabitants are excluded. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = $p<0.1$, **= $p<0.05$, ***= $p<0.01$

Table 5.4 : The effect of equal division on education and industry structure 2014

	Education		Employment				Firms	
	(1) College Degree	(2) Vocational Degree	(3) Agric.	(4) Manuf.	(5) Trade and Services	(6) Creative Ind.	(7) Per Pop	(8) Size
Panel A. OLS								
equal splitting	3.343***	-3.819***	-0.259***	-4.613***	4.882***	1.266***	0.00127*	-0.387
	(0.734)	(0.940)	(0.0913)	(1.365)	(1.381)	(0.343)	(0.000715)	(0.387)
Observations	397	397	397	397	397	397	380	380
Panel B. With controls								
equal splitting	2.789***	-2.759***	-0.397**	-2.902*	3.300*	1.187***	0.00205***	-0.907**
	(0.696)	(0.608)	(0.155)	(1.608)	(1.630)	(0.357)	(0.000594)	(0.401)
Observations	397	397	397	397	397	397	380	380
Panel C. Distance to border								
equal splitting	2.388***	-2.371***	-0.326***	-1.539	1.870	1.088***	0.00195***	-0.706*
	(0.729)	(0.634)	(0.104)	(1.970)	(1.976)	(0.388)	(0.000612)	(0.385)
Observations	198	198	198	198	198	198	183	183
mean outcome	11.19	64.51	1.052	32.40	66.55	2.645	0.0261	14.41
SD outcome	4.873	6.396	1.260	10.49	10.66	2.090	0.00355	1.983

Notes: Data on education and industry structure stem from INKAR data of 2013/14. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. counties with more than 100.000 inhabitants are excluded. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table 5.5 : The effect of equal division on agricultural productivity

	(1) Mean caloric output pre 1500	(2) Prussia: Grain yields kg/ha
Panel A. OLS		
equal division	68.12***	-3.112
	(25.39)	(7.277)
Observations	935	415
Panel B. With controls		
equal division	3.323	-3.392
	(16.63)	(5.004)
Observations	935	415
Panel C. Distance to border		
equal division	10.50	-2.065
	(9.863)	(4.951)
Observations	396	190
mean outcome	2211.4	74.77
SD outcome	152.1	24.44

Notes: The table uses in column 1 an index of caloric output per hectare per year before the year 1500 as outcome variable which is constructed by Galor and Özak (2016). In column 1 we use the whole sample of the German Empire in 1895. In column 2 grain yields in kilogram per hectare from Prussia in 1878 is the outcome variable. The sample is reduced to the 415 Prussian counties. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table 5.6 : The effect of equal division on population

	Population density			
	(1) Prussia 1816	(2) 1895	(3) 1907	(4) DID
Panel A. OLS				
equal division	1.507	63.73	117.5	75.86
	(9.257)	(88.75)	(115.1)	(95.07)
equal division x 1907				31.33
				(22.75)
Observations	318	937	948	1885
Panel B. With controls				
equal division	-14.65**	76.68	131.9*	91.42
	(6.754)	(58.79)	(70.90)	(58.54)
equal division x 1907				28.08
				(21.03)
Observations	318	937	948	1885
Panel C. Distance to border				
equal division	2.608	55.48	101.8*	56.08
	(4.080)	(44.05)	(51.91)	(42.45)
equal division x 1907				47.76**
				(20.85)
Observations	95	398	406	804
mean outcome	58.80	169.2	220.5	196.3
SD outcome	35.89	283.9	369.3	332.6

Notes: The tables uses population density as outcome measure. In column 1 the sample stems from Prussian counties in 1816. In column 2 and 3 from the whole sample of the German Empire in 1895 and 1907, respectively. Column 4 shows a DID approach estimating the change in population density in rural German counties between 1895 and 1907. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table 5.7 : The effect of equal division on technological progress 1821/82

	Technological Progress 1821			Employment 1882	
	(1) Factories	(2) Mills (per 1000 People)	(3) Looms	(4) Self Empl. out of Agric.	(5) Manuf.
Panel A. OLS					
equal splitting	-0.0895	0.105	-5.217*	-0.143	-1.237
	(0.0878)	(0.286)	(2.926)	(0.553)	(1.626)
Observations	318	318	318	415	415
Panel B. With controls					
equal splitting	0.00689	0.231	-7.419*	-0.640	0.237
	(0.0731)	(0.320)	(4.044)	(0.728)	(1.675)
Observations	318	318	318	415	415
Panel C. Distance to border					
equal splitting	-0.0703*	-0.207	-0.633	0.0254	0.778
	(0.0329)	(0.185)	(4.690)	(0.532)	(1.353)
Observations	95	95	95	190	190
mean outcome	0.254	1.111	5.492	5.557	8.227
SD outcome	0.238	0.986	12.83	2.385	5.591

Notes: The tables uses a sample of Prussian counties in 1821 and in 1882 with different proxies for technological progress. Column 1 uses factories column 2 mills and column 3 looms per 1000 people as outcome variable. In column 4 self-employed people out of agriculture as percent of total population is the outcome variable. In column 5 percent of population in the manufacturing sector is the outcome variable. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table 5.8 : The effect of equal division on income and wages 1878/95

	Income and county taxes 1878				Daily Wages
	(1) Federal Class Tax	(2) Federal Income Tax	(3) Local County Tax	(4) Total County Income	(5) Males 1892
		(per Capita)			
Panel A. OLS					
equal splitting	-0.141*	-0.0815	0.0449	-0.420	0.100
	(0.0802)	(0.129)	(0.172)	(0.341)	(0.0761)
Observations	415	415	415	415	413
Panel B. With controls					
equal splitting	-0.125*	0.0958	0.0885	0.0465	0.0596
	(0.0682)	(0.110)	(0.122)	(0.275)	(0.0780)
Observations	415	415	415	415	413
Panel C. Distance to border					
equal splitting	-0.157***	0.0946	0.118	-0.00813	0.0317
	(0.0496)	(0.116)	(0.121)	(0.242)	(0.0667)
Observations	190	190	190	190	189
mean outcome	1.376	0.624	1.010	2.068	1.487
SD outcome	0.390	0.441	0.792	2.777	0.356

Notes: The table uses various measures of tax income per person in each Prussian county in 1878 as outcome variables (column 1-4). The outcome variables in column 1 and 2 come close to an income tax while taxes in column 3 and 4 are local community taxes. In column 5 it uses daily wages for males in 1892 in Prussia as outcome variable. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table 5.9 : The effect of equal division on education 1886

	1871		1816		1886	
	(1) Percent Able to Read and Write	(2) Percent Illiterate	(3) Schools	(4) Pupils (per 1000 People)	(5) Schools	(6) Pupils
Panel A. OLS						
equal division	-6.103**	4.564**	0.0609	-32.82***	0.203	1.808
	(2.528)	(1.737)	(0.227)	(9.807)	(0.160)	(13.04)
Observations	415	415	308	305	415	415
Panel B. With controls						
equal division	-1.135	-0.102	0.414	-3.967	0.0144	-10.59
	(1.824)	(1.035)	(0.247)	(10.55)	(0.168)	(13.52)
Observations	415	415	308	305	415	415
Panel C. Distance to border						
equal division	-0.0543	-0.574	0.0395	-4.428	-0.114	-15.70
	(1.847)	(0.629)	(0.111)	(8.813)	(0.150)	(15.63)
Observations	190	190	90	87	190	190
mean outcome	62.57	9.544	1.928	110.7	1.328	169.8
SD outcome	12.03	9.305	0.923	43.72	0.533	43.13

Notes: The table uses data on education levels in Prussia as outcome variables. In column 1 and 2 the data stems from 1871 and shows the percent of the population which is able to read and write. In column 2 the percent of illiterate people is used. Earliest measures of education come from the documented number of schools and pupils in Prussian counties in 1816 (column 3 and 4). In 1886 the same measures are documented for Prussia again. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table 5.10 : The effect of equal division on sectoral employment 1895 and 1907

	Employment 1895			Employment 1907		
	(1) Agric.	(2) Manuf.	(3) Trade and Services	(4) Agric.	(5) Manuf.	(6) Trade and Services
Panel A. OLS						
equal division	-2.489	0.304	0.165	-3.998	1.130	0.277
	(2.085)	(1.427)	(0.174)	(2.729)	(1.507)	(0.214)
Observations	889	889	889	900	900	900
Panel B. With controls						
equal division	-2.578*	1.040	0.220*	-4.104**	2.253**	0.309*
	(1.296)	(1.036)	(0.130)	(1.694)	(1.067)	(0.184)
Observations	886	886	886	897	897	897
Panel C. Border sample						
equal division	-2.109**	1.577**	0.273**	-3.832***	2.624***	0.312**
	(1.001)	(0.767)	(0.120)	(1.308)	(0.810)	(0.154)
Observations	382	382	382	390	390	390
mean outcome	19.190	14.560	3.428	20.359	16.471	4.208
SD outcome	8.941	6.783	2.174	11.893	7.376	2.721

Notes: Employment in sectors and occupations as stated in 'Statistik des Deutschen Reichs' Vol. 109 for 1895 and 209 for 1907 as percent of total population in each district. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Free cities are excluded. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table 5.11 : The effect of equal division on measures for innovative occupations

	Employment 1907: innovative manufacturing		Patents 1877 to 1914		
	(1)	(2)	(3)	(4)	(5)
		in % of			
	total pop.	manuf. pop.	Dummy	log patents	log patents w/ zero
Panel A. OLS					
equal division	2.125**	2.861**	0.0837	0.646***	0.739**
	(0.938)	(1.376)	(0.0744)	(0.231)	(0.317)
Observations	900	900	899	499	899
Panel B. With controls					
equal division	2.358***	3.179**	0.138	0.561**	0.888**
	(0.766)	(1.398)	(0.0837)	(0.226)	(0.344)
Observations	897	897	899	499	899
Panel C. Border sample					
equal division	2.493***	3.083**	0.105*	0.472**	0.623**
	(0.843)	(1.509)	(0.0582)	(0.224)	(0.246)
Observations	390	390	390	228	390
mean outcome	6.874	16.807	0.669	1.979	1.994
SD outcome	5.664	8.712	0.471	1.542	1.886

Notes: Unit of observation is a county. The outcome in column 1 is a dummy which is 1 if a patent was filed in county i between 1877 and 1914 and 0 otherwise. The outcome in column 2 is the log of the number of patents filed in county i between 1877 and 1914 turning all 0 to missings. The outcome in column 3 is 0 if no patent was filed in county i between 1877 and 1914 and the log of the number of patents +1 otherwise. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Column 3 reduces the sample to counties in 35 km distance to the border of the inheritance rule. Free cities are excluded. Regressions are weighted by population in 1907. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

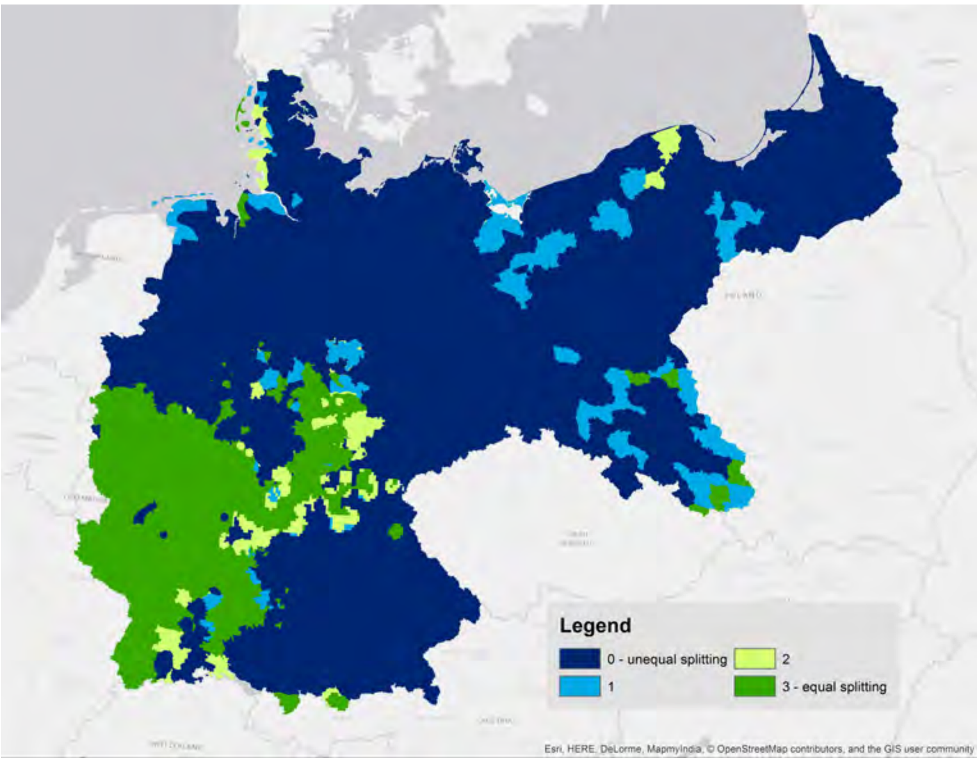
Table 5.12 : The effect of equal division on measures for innovation and entrepreneurship

	Employment 1925:	Prussia 1816:	
	(1) Self Empl. out of Agric.	(2) Middle Schools	(3) Middle School Pupils
		(per 1000 People)	
Panel A. OLS			
equal division	0.0656	-0.0113	-0.338
	(0.0848)	(0.0195)	(1.210)
Observations	779	311	311
Panel B. With controls			
equal division	0.151	0.0180	1.943**
	(0.118)	(0.0129)	(0.803)
Observations	763	311	311
Panel C. Border sample			
equal division	0.309*	0.0347**	1.934
	(0.162)	(0.0158)	(1.450)
Observations	329	90	90
mean outcome	4.415	0.0430	2.305
SD outcome	1.540	0.0804	4.503

Notes: In column 1 the percent of people in self-employment out of agriculture (manufacturing and trade and services) is used as outcome variable for the sample of the whole German Empire in 1925. In column 2 and 3 the sample is reduced to Prussian counties. The number of middle schools (column 2) and the number of pupils in middle schools (column 3) per 1000 people are used as outcome variables. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

5.11 Appendix D

Figure D-1 : Map inheritance rules in four categories



Notes: The map shows the distribution of inheritance rules in Germany with its borders of the 19th century. The map distinguishes four types of inheritance rule: unequal division (dark blue), mild form of unequal division (light blue), mild form of equal division (yellow), and equal division (green).

Table D-1 : Overview main outcome variables

Outcome		mean (sd)	min	max	Explanation	Source
income	log household income	6.719 0.115	6.448	7.46	Log of household income	Bundesamt für Bauwesen und Raumforschung, INKAR Indikatoren und Karten zur Raum- , und Stadtentwicklung, from www.inkar.de
	household income	833.531 103.02	631.6	1737.281	The average monthly household income in Euro in 2013 divided by the average household size in 2012	
	log taxable income	3.46 0.146	3.107	4.022	Log of average taxable income	Federal Statistical Office of Germany, from https://www.destatis.de
	taxable income	32.181 4.809	22.352	55.808	Average taxable income in thousand Euro	Bundesamt für Bauwesen und Raumforschung, INKAR Indikatoren und Karten zur Raum- und Stadtentwicklung, from www.inkar.de
	log median income	7.956 0.162	7.555	8.383	Log of median income	
	median income	2889.439 455.414	1910	4371	Median monthly income in Euro in 2013	
	log GDP p.c	3.447 0.336	2.674	4.964	Log of GDP p.c.	
	GDP p.c	33.494 14.404	14.5	143.1	GDP p.c. in 2013 in thousand Euro	
inequality measures	gini	0.64 0.238	0.576	0.808	Calculated with information on the number of individuals in a county in 10 income bins and the average taxable income in each bin	Federal Statistical Office of Germany, from www.destatis.de
	log 10th income pctile	0.746 0.348	0.622	0.874	Log of 10th income pctile	
	10th income pctile	1.951 0.561	0	2.396	Average taxable income in thousand Euro of the income bin which includes the 10th percentile of the population	
	log 90th income pctile	4.255 0.087	3.724	4.341	Log of 90th income pctile	
	90th income pctile	70.66 4.877	41.42	76.76	Average taxable income in thousand Euro of the income bin which includes the 90th pctile of the population	
	90th-10th pctile income gap	4.719 0.317	3.507	5.84	Difference between 90th and 10th log income percentile	
landholding inequality	landholding gini 1895	0.716 0.123	0.426	0.948	For more information about this measure see Ziblatt (2008).	Ziblatt (2008)
distribution of farm sizes	<2 ha	8.242 7.031	0.65	41.86	Percentage of farms below 2 ha number stated in source	Kaiserliches Statistisches Amt (1898)
	2-5 ha	13.336 10.692	0.76	61.71	Percentage of farms between 2-5 ha number stated in source	
	5-20 ha	33.843 15.044	3.03	70.54	Percentage of farms between 5-20 ha number stated in source	
	20 -100 ha	27.081 16.055	0.3	82.17	Percentage of farms between 20-100 ha number stated in source	
	> 100 ha	17.483 19.319	0	80.8	Percentage of farms above 100 ha number stated in source	

Notes: This table gives an overview of the outcome variables used in table 2, 3, and 4 which are our main tables. Column 3 shows means and standard deviations in parentheses. Column 4 and 5 show the minimum and the maximum of the variable. Description about how the variables are prepared are listed in column 6 and the sources are stated in column 7. The exact reference for some of the sources can be found in the bibliography.

Table D-2 : The effect of equal division on farm sizes in Prussia 1816

	(1) Small Landholdings	(2) Medium Landholdings (per 1000 People)	(3) Large Landholdings
Panel A. OLS			
equal splitting	26.32***	13.47	-0.291
	(7.614)	(7.969)	(0.264)
Observations	305	305	305
Panel B. With controls			
equal splitting	21.33***	7.484	0.0333
	(5.706)	(5.465)	(0.178)
Observations	305	305	305
Panel C. Distance to border			
equal splitting	8.472*	2.327	0.0550
	(4.666)	(6.067)	(0.166)
Observations	123	123	123
mean outcome	50.10	40.42	1.340
SD outcome	37.12	21.96	1.496

Notes: This table uses the earliest available measure of the distribution of farm sizes of German lands from Prussia in 1816. Column 1 shows the percent of small landholdings per 1000 people. Column 2 the percent of medium landholdings and column 3 the percent of large landholdings per 1000 people. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = $p<0.1$, **=$p<0.05$, ***=$p<0.01$

Table D-3 : The effect of equal division on change in farm sizes and their distribution

	(1) average farmsize ha	(2) < 2 ha	(3) 2-5 ha	(4) 5-20 ha	(5) 20-100 ha	(6) > 100 ha
Panel A. OLS						
equal splitting x 1907	-0.288***	1.018**	0.212	-3.068***	0.869	0.935
	(0.105)	(0.434)	(0.351)	(0.538)	(0.777)	(0.566)
Observations	1870	1873	1873	1873	1871	1739
Panel B. With controls						
equal splitting x 1907	-0.279***	1.027**	0.275	-2.987***	0.716	0.850
	(0.103)	(0.423)	(0.310)	(0.524)	(0.710)	(0.596)
Observations	1870	1873	1873	1873	1871	1739
Panel C. Distance to border						
equal splitting x 1907	-0.221**	1.513***	0.155	-3.112***	1.624**	0.0294
	(0.0959)	(0.511)	(0.354)	(0.665)	(0.793)	(0.644)
Observations	795	798	798	798	796	715
mean outcome	5.818	8.607	13.60	35.05	26.49	17.46
SD outcome	3.403	7.636	10.95	15.07	16.02	18.68

Notes: Average farm size and shares of farms in 5 size categories as stated in 'Statistik des Deutschen Reichs' Vol. 109(1895) and Vol. 209(1907). The regressor is the interaction term of 'equal division' (compared to 'unequal division') and year 1907 (compared to 1895). The results show the change in farm sizes between 1895 and 1907. Panel A includes besides main effects longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Free cities are excluded. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table D-4 : The effect of equal division on inequality measures 2014

	(1) Gini	(2) log 10^{th} income pctile	(3) log 90^{th} income pctile	(4) $90^{th} - 10^{th}$ pctile income gap
Panel A. OLS				
equal splitting	0.00248	0.000185	0.0146**	0.0200
	(0.00426)	(0.00561)	(0.00552)	(0.0432)
Observations	374	344	374	344
Panel B. With controls				
equal splitting	0.00335	0.0105*	0.00169	-0.0740
	(0.00368)	(0.00574)	(0.0115)	(0.0489)
Observations	374	344	374	344
Panel C. Distance to border				
equal splitting	0.00677**	0.0130**	0.0153	-0.0793
	(0.00318)	(0.00566)	(0.00972)	(0.0492)
Observations	178	151	178	151
mean outcome	0.640	0.746	4.255	4.719
SD outcome	0.0238	0.0348	0.0866	0.317

Notes: Income percentiles calculated from 2014 income tax statistics of the Federal Statistical Office of Germany. Income bins are available with mean income per bin and number of people in that bin. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. counties with more than 100.000 inhabitants are excluded. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table D-5 : The effect of equal division on income measures 2014 - quadratic RD polynomial

	(1) log household income	(2) log taxable income	(3) log median income	(4) log GDP p.c.
Panel A. OLS				
equal splitting	0.0722***	0.0960***	0.0475***	0.142**
	(0.0222)	(0.0171)	(0.0173)	(0.0525)
Observations	397	374	397	397
Panel B. With controls				
equal splitting	0.0593***	0.0613***	0.0437***	0.140***
	(0.0177)	(0.0168)	(0.0104)	(0.0353)
Observations	397	374	397	397
Panel C. Distance to border				
equal splitting	0.0488**	0.0506***	0.0434***	0.146**
	(0.0187)	(0.0159)	(0.0123)	(0.0539)
Observations	198	178	198	198
mean outcome	6.719	3.461	7.956	3.447
SD outcome	0.115	0.146	0.162	0.336

Notes: Data on income and GDP per capita stem from the Federal Statistical Office of Germany and INKAR of 2013/14. In comparison to table 5.3 this table includes a quadratic RD polynomial in all panels. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. counties with more than 100.000 inhabitants are excluded. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = $p<0.1$, **=$p<0.05$, ***=$p<0.01$

Table D-6 : The effect of equal division on landholding inequality 1895 in Prussia

	landholding Gini coeff. 1895		% farms in size category...			
	(1)	(2)	(3)	(4)	(5)	(6)
	linear RD polyn.	quad. RD polyn.	< 2 ha	2-5 ha	5-20 ha	20-100 ha
Panel A. OLS						
equal splitting	-0.0612**	-0.0562*	5.268***	9.776***	1.841	-14.61***
	(0.0290)	(0.0314)	(1.856)	(2.065)	(2.465)	(2.781)
Observations	490	490	488	488	488	488
Panel B. With controls						
equal splitting	-0.0490***	-0.0471***	5.970***	9.381***	-2.524	-13.10***
	(0.0169)	(0.0170)	(1.273)	(1.573)	(2.754)	(2.380)
Observations	490	490	488	488	488	488
Panel C. Distance to border						
equal splitting	-0.0423***	-0.0450***	5.169***	8.055***	-3.138	-8.303***
	(0.0114)	(0.0134)	(1.289)	(1.608)	(1.973)	(2.486)
Observations	157	157	155	155	155	155
mean outcome	0.770	0.770	7.627	10.45	27.97	29.23
SD outcome	0.0975	0.0975	6.727	8.458	12.56	15.92

Notes: This table shows the results of table 2 for Prussia only. Share of farms in 5 size categories as stated in 'Statistik des Deutschen Reichs' Vol. 109. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Free cities are excluded. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table D-7 : The effect of equal division on sectoral employment 1882 in Prussia

	(1) Agric.	(2) Manuf.	(3) Trade and Services
Panel A. OLS			
equal division	0.720	-1.481	0.364
	(1.872)	(1.791)	(0.383)
Observations	415	415	415
Panel B. With controls			
equal division	-0.820	-0.686	0.753**
	(1.801)	(2.057)	(0.346)
Observations	415	415	415
Panel C. Distance to border			
equal division	-0.765	0.489	0.724***
	(1.411)	(1.578)	(0.229)
Observations	190	190	190
mean outcome	20.68	12.40	2.636
SD outcome	6.907	6.584	1.481

Notes: This table uses the percentage of people in the three main economic sectors: agriculture, manufacturing, and trade and services as outcome variables. The sample stems from 1882 Prussia. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table D-8 : Robustness check: main findings in Prussia, Bavaria, Baden, and Wuerttemberg

	historical outcomes			modern outcomes	
	(1) Gini 1895	(2) Agric.	(3) Manuf.	(4) log household income	(5) log GDP p.c.
Panel A. OLS					
equal division	-0.0407**	-4.080	1.233	0.0737***	0.152**
	(0.0191)	(2.879)	(1.551)	(0.0264)	(0.0660)
Observations	794	808	808	327	327
Panel B. With controls					
equal division	-0.0575***	-3.832**	2.115**	0.0730***	0.184***
	(0.0131)	(1.767)	(1.044)	(0.0199)	(0.0410)
Observations	794	808	808	327	327
Panel C. Distance to border					
equal division	-0.0575***	-4.521***	3.059***	0.0675***	0.178***
	(0.0131)	(1.420)	(0.748)	(0.0189)	(0.0481)
Observations	794	355	355	166	166
mean outcome	0.712	23.53	15.14	6.726	3.469
SD outcome	0.129	11.74	6.879	0.120	0.342

Notes: The table reduces the sample to the states of Prussia, Bavaria, Baden, and Wuerttemberg. The Gini coefficient stems from Ziblatt (2008), the share of farms is as stated in 'Statistik des Deutschen Reichs' Vol. 109. Data on income and GDP per capita stem from the Federal Statistical Office of Germany and INKAR of 2013/14. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Free cities or counties with more than 100.000 inhabitants are excluded. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table D-9 : Robustness check: excluding all mixed-inheritance-rule counties

	historical outcomes			modern outcomes	
	(1) Gini 1895	(2) Agric.	(3) Manuf.	(4) log household income	(5) log GDP p.c.
Panel A. OLS					
equal division	-0.0452	-2.462	-0.239	0.0958**	0.232***
	(0.0313)	(4.312)	(2.289)	(0.0394)	(0.0697)
Observations	508	521	521	227	227
Panel B. With controls					
equal division	-0.0701**	-7.315***	4.160**	0.146**	0.347***
	(0.0271)	(2.410)	(1.606)	(0.0637)	(0.119)
Observations	508	521	521	227	227
Panel C. Distance to border					
equal division	-0.0343	-4.565*	3.783**	0.0980	0.0535
	(0.0248)	(2.643)	(1.642)	(0.152)	(0.255)
Observations	107	112	112	65	65
mean outcome	0.718	19.77	17.24	6.717	3.468
SD outcome	0.123	13.17	7.796	0.122	0.375

Notes: The table reduces the sample to counties with a 100 percent area of unequal or equal division. The Gini coefficient stems from Ziblatt (2008), the share of farms is as stated in 'Statistik des Deutschen Reichs' Vol. 109. Data on income and GDP per capita stem from the Federal Statistical Office of Germany and INKAR of 2013/14. Panel A includes longitude, latitude, and state-fixed effects. Panel B includes additionally geographic and cultural controls as specified in summary statistics. Panel C reduces the sample to counties in 35 km distance to the border of the inheritance rule. Free cities or counties with more than 100.000 inhabitants are excluded. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

Table D-10 : Robustness check: changing radius around the border for RD specification

	historical outcomes			modern outcomes	
	(1) Gini 1895	(2) Agric.	(3) Manuf.	(4) log household income	(5) log GDP p.c.
Distance to border <=20					
equal division	-0.0415***	-4.111***	2.966***	0.0605***	0.168***
	(0.00866)	(1.360)	(0.910)	(0.0189)	(0.0509)
Observations	289	292	292	137	137
Panel B. Distance to border <=50					
equal division	-0.0444***	-4.752***	2.954***	0.0776***	0.164***
	(0.00938)	(1.490)	(0.870)	(0.0176)	(0.0435)
Observations	489	499	499	241	241
Panel C. Distance to border <=80					
equal division	-0.0464***	-4.647***	3.280***	0.0691***	0.154***
	(0.00975)	(1.568)	(0.936)	(0.0163)	(0.0394)
Observations	614	629	629	299	299
mean outcome	0.657	19.64	18.33	6.746	3.478
SD outcome	0.119	12.52	6.888	0.120	0.289

Notes: The table shows results for the RD specification with varying distances to the border. The Gini coefficient stems from Ziblatt (2008), the share of farms is as stated in 'Statistik des Deutschen Reichs' Vol. 109. Data on income and GDP per capita stem from the Federal Statistical Office of Germany and INKAR of 2013/14. Panel A reduces the sample to counties in 20 km distance to the border of the inheritance rule. Panel B reduces the sample to counties in 50 km distance to the border of the inheritance rule. Panel C reduces the sample to counties in 80 km distance to the border of the inheritance rule. Free cities or counties with more than 100.000 inhabitants are excluded. Regressions are weighted by population. Standard errors clustered on district (Regierungsbezirk) level. * = p<0.1, **=p<0.05, ***=p<0.01

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Econometric Society European Meeting (EEA-ESEM), Geneva, Switzerland; EBE Summer Meeting, Freising, Germany; IZA European Summer School, Buch-Ammersee, Germany; European Society of Population Economics (ESPE) (Annual Conference), Berlin, Germany
2015
European Historical Economics Society (EHES) Conference, Pisa, Italy; Spring Meeting of Young Economists (SMYE), Ghent, Belgium; Economic History Society (EHS), Annual Conference, Telford, UK; German Economic History Conference, Munster, Germany

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