Essays on the economics of education, fertility, and well-being

Rechts- und Wirtschaftswissenschaftliche Fakultät

des Fachbereichs Wirtschaftswissenschaften

der Friedrich-Alexander-Universität Erlangen-Nürnberg

zur Erlangung des akademischen Grades eines Doktors

(Dr. rer. pol.)

vorgelegt von

Dipl.-Kffr. Miriam Gesa Mäder

aus Würzburg

Als Dissertation genehmigt
von der Rechts- und Wirtschaftswissenschaftlichen Fakultät
dem Fachbereich Wirtschaftswissenschaften
der Friedrich-Alexander-Universität Erlangen-Nürnberg

Tag der mündlichen Prüfung: 17. April 2014

Vorsitzende des Promotionsorgans: Prof. Dr. Nadine Gatzert

Gutachter/in: Prof. Regina T. Riphahn, Ph.D.

Prof. Dr. Guido Heineck

Contents

List of Figures

List of Tables

List of acronyms

BIS ..... Department for Business Innovation & Skills London

BMFSFJ ..... Bundesministerium für Familie, Senioren, Frauen und Jugend (Federal Ministry of Family Affairs, Senior Citizens, Women and Youth)

DemoDiff ..... Demographic differences in life course dynamics in Eastern and Western Germany

Diff-in-diff ..... Differences-in-differences

EGGE ..... European Commission's Expert Group on Gender and Employment Issues

EPA ..... Einheitliche Prüfungsanforderungen für das Abitur (Uniform test requirements for high school exit exams)

EUR ..... Euro

EVS ..... Einkommens- und Verbrauchsstichprobe (Income and consumption survey)

G8 ..... 8-stufiges Gymnasium (high school in 8 years)

G9 ..... 9-stufiges Gymnasium (high school in 9 years)

GDP ..... Gross domestic product

IV ..... Instrumental variable

KMK ..... Ständigen Konferenz der Kultusminister der Länder in der Bundesrepublik Deutschland (Standing conference of the ministers of education and cultural affairs of the Länder in the Federal Republic of Germany)

LATE ..... Local average treatment effect

LSAY ..... Longitudinal Surveys of Australia Youth

MZ ..... Mikrozensus (German Microcensus)

OECD ..... Organization for economic co-operation and development

OLS ..... Ordinary least squares

Pairfam ..... Panel analysis of intimate relationships and family dynamics

PIRLS ..... Progress in international reading literacy study

PISA ..... Programme for international student assessment

QaC ..... Qualification and career survey (Qualifikation und Berufsverlauf)

SMSA ..... Standard metropolitan statistical area

SOEP ..... German Socio-Economic Panel

TFR ..... Total fertility rate

TIMMS ..... Trends in international mathematics and science study

U.K. ..... United Kingdom

U.S. ..... United States

1 Introduction

Higher education is strongly associated with higher employment rates, better labor market opportunities and higher wages (see, e.g., OECD; 2013a). Consequently, individuals and governments have the incentive to promote the level of educational attainment. However, increasing educational attainment has consequences not only for the labor market. Other outcomes affected by education comprise health, marriage behavior, or fertility.

Developments such as the educational expansion, technological change and the emerging 'knowledge economies' characterize the last decades. These trends impose challenges for the education system. In the OECD countries, the average share of the population holding a tertiary degree differs for individuals aged 55-64 years and those aged 25-34 years by more than 14 percentage points (OECD; 2013a). This difference across age groups is largely driven by women, for whom it amounts to 23 percentage points.

The increasing educational attainment of women and their steadily growing labor market participation imposes new challenges on social policies. In the 1990s, many countries reacted to the increasing labor market participation of women either by supporting the compatibility of work and family or by making parental leave more favorable for women (Ziefle; 2009). As for Germany, related literature underpins that established family policy instruments hamper the compatibility of work and family life and do not counteract declining fertility rates (Cygan-Rehm and Maeder; 2013).

A consequence of the incompatibility of work and family life in Germany is a low maternal labor market participation rate. The employment rate sharply decreases with the number of children (see OECD; 2013b). In 2007, the German government introduced a reform to promote labor market participation of mothers. The introduction of a wage replacement - parents money (*Elterngeld*) - was a step away from the encouragement of successive work-family periods to compatibility of labor market participation and child-rearing. However, also traditional role models are still supported by transfers such as the cash-for-care benefit (*Betreuungsgeld*) which discourages the use of formal child care in

favor of home care. Also, long-established regulations such as income splitting still favor traditional role models.[1]

This dissertation presents three studies that address determinants of regional variation in returns to schooling, the causal effect of education on completed fertility, and the effect of the introduction of earnings-related parental leave benefits on the subjective well-being of young mothers. The next Sections 1.1 and 1.2 give summaries of the German school system and its regulations, as well as the German family policy in comparison to that of other countries.

1.1 The German school system and the role of the federal states

1.1.1 The German school system

Figure 1.1 illustrates the German school system.[2] Children are enrolled at the age of six in a comprehensive primary school. After grade four, the system sorts the 10 year old students in one out of three secondary school tracks (secondary level I):[3] basic school (*Hauptschule*), middle school (*Realschule*), or high school (*Gymnasium*). These three school tracks differ in ability requirements, duration, and school leaving certificate, and prepare for different occupations and career paths. Basic school lasts 5 years and middle schools lasts 6 years.[4] While basic schools prepare largely for subsequent vocational education, middle schools also prepare for education tracks that qualify for tertiary education.

From age 15 onwards, students can attend secondary level II if they attained a basic school degree (*Qualifizierender Hauptschulabschluss*) or a middle school degree (*Mittlere Reife*). Secondary level II splits in an educational path (*Allgemeinbildende Schulen*) and a vocational path (*Berufliche Schulen*), including the so called dual system. The educational

[1] Income splitting instead of individual taxation provides substantial tax benefits for traditional family forms. See Section 1.2.1 for a detailed description.

[2] Compare the official description of the German school system of the *Standing Conference of the Ministers of Education and Cultural Affairs of the Länder in the Federal Republic of Germany* (KMK; 2013). For simplification and clarity I do not consider all regulations, which differ by state and school type (e.g., special need school equivalents to high schools).

[3] In Berlin and Brandenburg primary school lasts 6 years (until age 12).

[4] Some federal states introduced an additional 10th grade in basic school: Berlin, Bremen, Brandenburg, North Rhine-Westphalia.

path comprises the attendance of high schools (until grade 12 or 13) and the attainment of a general high school degree (*Allgemeine Hochschulreife*). High school degree gives access to university. The most common path of reaching a general high school degree is the attendance of high school from grade 5 to grade 12 or 13 without interruptions.[5]

The vocational path of secondary education is diverse. Numerous different types of vocational schools exist, that offer different degrees of general education alongside the vocational education itself. Vocational schools (*Berufsschule* and *Berufsfachschule*) prepare for a specific occupation. At a number of high school equivalents an entrance certificate for tertiary education can be obtained, too. Vocational high schools (*Fachoberschule*, *Berufsoberschule*, and *Fachgymnasium*) usually require a middle school degree, rather than a basic school degree, and a finished apprenticeship training or equivalent professional experience.[6]

The German system comprises a number of additional school choices varying by federal state. An alternative to the tracked system after age 10 are comprehensive schools. Instead of tracking, students get support according to their ability. However, comprehensive schools are not very common in Germany (see, e.g., Ertl and Phillips; 2000). Further, students can choose private schools with alternative educational ideologies if the school has a state permission. Another option is the so called second educational path (*Zweiter Bildungsweg*) where students of at least 19 years attend evening classes (*Abendschulen*) or so called *Kollegs* to attain a secondary school degree.

Table 1.1 shows the relative importance of different school tracks and paths in secondary level I and II. The table displays total numbers and shares of students for school year 2011/12 in secondary level I (panel A), educational path of secondary level II (panel

[5] In recent years all federal states shortened high school from 9 grades (G9) to 8 grades (G8).

[6] At technical high schools (*Fachoberschule*) students can obtain an entrance certificate to technical colleges (*Fachhochschule*) after grade 12. At *Berufsoberschule* students can acquire a subject-linked high school degree (*Fachgebundene Hochschulreife*) or a general high school degree if they master a second foreign language. The provision of vocational high schools strongly differ by federal state. Also, terminology, detailed organization, and requirements are not always consistent across federal states.

B), and vocational path of secondary level II (panel C). Panel A shows the relative importance of the school tracks basic school, middle school, and high school.[7] Students in these tracks constitute about 70% of students in secondary level I. Approximately 10% of students in the educational path of secondary level I and II attend comprehensive schools. In panel B, 89% of students in the educational path of secondary level II attend high school. This share corresponds to about 870,000 students. For attaining an entrance certificate for university, high school in the educational path is more common than vocational high schools in the vocational path (panel C). The sum of students in these schools reaches only about 330,000. Among the vocational path of secondary level II, most students attend the dual system as 64.3% attend vocational schools. Attending a vocational school is always connected to an apprenticeship training position.

In sum, Table 1.1 shows the importance of basic, middle, and high schools on the one hand, and the dual system on the other. The number of students attending, e.g., evening classes or vocational tracks that finish with high school degree are negligible. Although the German school system claims permeability, raw statistics in Table 1.1 support strong dependence on the track a student is sorted in at age of 10. Also, previous literature supports that students rarely change tracks and downward changes are more frequent than upward changes (see, e.g., Dustmann; 2004; Pischke; 2007).

1.1.2 The role of the federal states

The German schools are governed by the federal states. In the constitution, established after World War II, Article 70 gave almost all legislation powers regarding the educational system to the federal states (Wolf; 2006; Guckelberger; 2012). The federal organization was motivated by the support of the state governments as well as by the fact that a number of states began to establish their own legislation representing the state's political orientations already in 1946 (Guckelberger; 2012). The fact that schools are governed at the

[7] Some federal states abolished the separation of middle and basic schools and implemented schools for secondary level I with multiple tracks. In these states, students can reach both, middle and basic degree, at these schools.

state level had long-lasting effects, and induced considerable differences, for example, in the duration of primary schooling, the organization of secondary schools, and the permeability of the systems across federal states (Avenarius et al.; 2003).

In order to harmonize the different systems to some degree, the federal government launched the *standing conference of the ministers of education and cultural affairs of the Länder in the Federal Republic of Germany* (KMK) in 1948. The KMK is the most important organ for the coordination between states in concerns of the education system. Several agreements such as the *Düsseldorfer Abkommen* in 1955 and the *Hamburger Abkommen* in 1964 aimed at a partial standardization of regulations between states. The *Düsseldorfer Abkommen*, for example, sets the Latin word Gymnasium as the general term for high schools. Large parts of the *Hamburger Abkommen* are still important foundations of today's school system. The states agreed, among other things, on the duration of compulsory schooling, consistent terminology, and consistency in the recognition of certificates (see, e.g., Wolf; 2006; van Ackeren and Klemm; 2009; Guckelberger; 2012).

Governing schools at the state level has advantages as well as disadvantages (see Guckelberger (2012) for a more comprehensive summary). One disadvantage may be that it causes inequity in children's occupational and educational opportunities. Braun et al. (2010) find that the chance of being accepted at a German university varies considerably with the federal state where an applicant graduated. The reason for such a selective admission policy is that different examination modes (centralized exams vs. exams set by the school) and differences in curricula cause differences in the (perceived or factual) average quality of high school certificates from different states. In 1979, a KMK agreement set uniform test requirements for the final high school exams to tackle the problem (*Einheitliche Prüfungsanforderungen für das Abitur, EPA*). However, differences are still considerable, because EPA settles curricula contents only at the smallest common denominator.[8]

[8] A study of Kaun (2006) exemplifies differences between states for the subject stochastic at secondary school. The author compares curricula contents in the federal states and finds considerable differences even though the KMK set uniform requirements in 2003 (e.g., subject in some states compulsory, in others elective).

Another aspect that affects educational and occupational opportunities of students are differences in the training of teachers. Since schooling differs across states, and the training of teachers is matched to the states' school systems, the training itself differs (KMK; 2013). In 1999, the state governments agreed on uniform guidelines for teachers' training, and on uniform requirements for teacher admission. However, although teachers' university degrees are generally recognized in every state, they might not be able to teach the same subjects in every state (KMK; 2009).

Overall, differences in curricula and in the training of teachers affect children's educational opportunities. A further disadvantage of the school system being governed at the state level, is that it generates barriers to regional mobility. If students move to another federal state, they might have to repeat a year in order to catch up on curricula contents. Also, teachers face restrictions if their teaching certificate is not fully recognized in every state.

An advantage of governing schools at the state level is that it might improve efficiency and quality, as it generates competition between the states. Moreover, since parents are free to change locations and to choose between different school systems, proponents interpret the state-level governance as an increase in choices. However, such improved efficiency and quality can only occur if state governments are willing and able to learn from each other (Guckelberger; 2012). Since the publication of first international and national comparisons of student performance, there has been considerable progress in the evaluation of the school system and educational standards. After 2000 and the first publication of the results of the *Programme of international student assessment* (PISA), where Germany ranked below the OECD average, a discussion about the quality of the school system began. Furthermore, a comparison of the PISA results across states revealed considerable differences in students' assessment scores within the country (PISA-Konsortium Deutschland; 2002). Many differences between states may contribute to variation in student performance. Since 2006, there is a strategy for the monitoring of the education system (*Gesamtstrategie der Kultusministerkonferenz zum Bildungsmonitoring*), which sets

key goals for efficiency and quality of schooling (KMK; 2006). Important instruments comprise, for example, national and international assessments such as the *Programme for International Student Assessment* (PISA), *Progress in International Reading Literacy Study* (PIRLS), and *Trends in International Mathematics and Science Study* (TIMMS).

1.2 Family policy

One of the key purposes of family policy is the establishment of legislation for the protection of families and their shared living arrangements (Bäcker et al.; 2010). Furthermore, policies aim at economic support for families, improvement of their living conditions, and compatibility of work and family life. The following Section 1.2.1 gives an overview of the most prominent regulations in family policy in Germany. Section 1.2.2 provides an international comparison of family policies.

1.2.1 Family policy in Germany

In Germany, family policy focuses on income security for families rather than on provision of public or financial support for private services for families, such as child care facilities (see, e.g., Ziefle; 2009). Monetary transfers either aim to compensate for the costs of caring for children, or aim to secure parents' income during times of parental leave. In addition, the tax system grants transfers for married couples which are not conditional on having children (see Bäcker et al.; 2010, for a more comprehensive description).

Child allowances (*Kinderfreibetrag*) and child benefits (*Kindergeld*) are the core of family allowances (*Familienleistungsausgleich* or *Kinderlastenausgleich*) and were implemented already in West Germany in the 1950s (Kreyenfeld; 2004). Both are monetary transfers that aim to secure parents' income, and compensate them for any income losses that are due to their caring for children (Bäcker et al.; 2010). Since 2002, the tax authorities grant either child allowances or child benefits, depending on what is more beneficial for the family (Bäcker et al.; 2010). Child allowance is a tax reduction, which reduces

the annual taxable income by a fixed amount. In contrast, child benefit is a flat-rate transfer, paid on a monthly basis. Additional transfers for the support of parents include, for example, free health insurance for children.

In order to support parents during periods of raising children, and compensate them for their foregone labor market income, there are child-rearing-benefits available (Erziehungsgeld) (replaced by parents money (*Elterngeld*) in 2007), and pension entitlements are earned during periods of child-rearing for up to three years per child (Bäcker et al.; 2010). A means-tested child-rearing benefit was first implemented in 1986. If the net household income was below a certain threshold, parents were eligible to receive either EUR 300 per month for a period of 24 months, or EUR 450 per month for a period of 12 months. In 2007, the new parents money replaced the old means-tested system. Parents can now receive a wage-replacement for 12 month, equivalent to 67% of their pre-birth net earnings. The new system is more generous, and supports the economic independence of women. Additionally, the new system explicitly promotes fathers' involvement in child-rearing (see Section 4 for a more detailed description of the two systems).

Married couples can receive additional transfers through the tax system (Bäcker et al.; 2010). Instead of taxing each of the partners individually, they are taxed together: their combined income is divided by two, and both spouses are taxed as if they had earned fifty per cent of the sum. This system generates considerable financial advantages, and the tax concession is the higher, the higher the difference in earnings between the spouses. The maximum tax concession is granted if one spouse has no income at all. In addition to tax benefits, married couples benefit, for example, from free health insurance for one spouse.

Another important part of family policies are maternity leave regulations and parental leave regulations. In Germany, the underlying assumption for all leave schemes is that the care by mothers is the best for the child (BIS; 2009). German parental leave consists of maternity leave and parental leave. Maternity leave is restricted to 6 weeks before and 8 weeks after the birth. In this period, a mother is not allowed to work, her job is protected and she receives her full previous wage, usually paid for by her health insurer

(BIS; 2009). After the maternity leave ends, parents (mother or father) can take parental leave. Since parental leave was first introduced in the 1970s, the duration of job protection was extended several times. Initially, it was restricted to 6 months. Since 1992, parents can take leave for up to 36 months with job protection (Kreyenfeld; 2004).

1.2.2 International comparison of family policies

Following the classification of Ziefle (2009), the different orientation of policy interventions splits countries in three rough groups: conservative systems, liberal systems, and welfare-supporting systems (for similar classifications see, e.g., Esping-Andersen; 1990; Morgan and Zippel; 2003; Hantrais; 2004).

Examples for a liberal system are the U.S. and the U.K., which are both similar in their support of families. In both countries, policy interventions aim to prevent poverty, rather than to promote labor market participation or fertility (Ziefle; 2009). The generosity of parental leave schemes is relatively low: in the U.K., maternity leave lasts for 13 months but wage replacement is only for 6 weeks available, the U.S. introduced an unpaid maternity leave scheme in large firms in 1993 (BIS; 2009). The U.K. and the U.S. grant monetary transfers mostly through the tax system. Since tax credits depend on parents earning income, this system of income support sets incentives for labor market participation. In the U.S., a negative income tax also supports families with low taxable income. Public child care coverage is relatively low in these countries and child care costs are high (Gornick et al.; 1997; Waldfogel; 2001; Immervoll and Barber; 2006). Although the U.K. subsidized formal child care coverage considerably during the last decade, there are still shortages especially in full time places (EGGE; 2009).

In sharp contrast to family policies in the liberal system stand the welfare-supporting systems, e.g., the Nordic countries and France. Their family policies have one main goal: to support parents in reconciling their family life with both partners' careers (Ziefle; 2009; Salles et al.; 2010). In the Scandinavian countries, flexible working time regulations, nearly universal child care coverage for all age groups, generous wage replacement

during parental leave, and individual taxation aim at making work and family life compatible. Family policy in France originally focused the promotion of fertility rates, e.g., by family tax splitting and extra transfers for higher order births (Ziefle; 2009). Since the 1980s, also compatibility of family and work life came into focus. Parental leave regulations and benefits, flexible weekly working hours, and the support of external child care by tax transfers, facilitates and gives incentives for an early return to work of mothers (e.g., by conditioning parental leave allowances on pre-birth labor market participation) (Salles et al.; 2010).

Family policy in Germany, Netherlands, and Italy constitute conservative systems. Here, social policies encourage women to drop out of the labor force until children reach school age (Morgan and Zippel; 2003; BIS; 2009; Salles et al.; 2010). Policies follow traditional role models and support successive work and child-rearing periods rather than compatibility (Ziefle; 2009). Parental leave is generous, but requires the parent to not work, parental leave benefits are not sufficient to cover reasonable costs of living, and single breadwinner families are granted financial advantages; these policies are the core of German family policy since the 1950s. These transfers induce a high dependence of mothers on partners' income, and explicitly encourage women to drop out of the labor force for family reasons.

For an international comparison of transfers on family benefits, Figure 1.2 displays the family expenditures as percentage share of the GDP at market prices in national currency for five countries: U.K., France, Norway, Germany, and the U.S. For each country, the expenditures are given for the years 2001, 2005, and 2009. On average, France, Norway, and the U.K. spend the most on family benefits. Here, the sharp increase in U.K. transfers since 2001 marks a considerable progress towards a more generous support of families.[9] The U.S. spends the smallest share of GDP on the support of families. Besides differences in the total expenditures, also the allocation of expenditures on different measures varies widely across countries. For example, while expenditures on cash benefits are comparable

[9] Note that the U.K. GPD continuously increased during that period. Thus, the growing share of GDP is not caused by a GDP decrease.

across France, Norway and Germany, Germany spends less on services and more on tax breaks (e.g., child allowances) than the other two countries.[10] One example for variety in responses to changing social norms is the reaction on increasing labor market attachment of women in the 1990s. Many countries promoted child care coverage in order to make work and family life more compatible for women. Germany however, reacted by the extension of unpaid parental leave for non-working mothers and encouraged women to drop out of the labor force (Ziefle; 2009; BIS; 2009).

The different family policies in the different regimes have consequences, e.g., for fertility rates, labor market participation of women, and child care coverage. The costs of child-rearing, i.e., direct costs (e.g., child care) and forgone earnings, affect women's decisions for family formation (Becker; 1964). Figure 1.3 gives total fertility rates and female labor market participation for selected countries: Germany, U.S., Norway, and the OECD average. Here, the upper graph depicts that the decline in fertility rates since the 1960s in Germany was the one of the fastest worldwide. Also, since the mid 1970s, total fertility rates in Germany stagnate at one of the lowest levels in the OECD at about 1.4 children per woman. Female labor market participation in Germany is at the OECD average; however, it is much lower than in Norway or the U.S. where fertility and labor market participation rates are higher.

Overall, countries with lowest levels of family support (e.g., the U.S.) and countries with high levels of support for the compatibility of work and family life (e.g., Norway) show the highest labor market participation and high fertility rates. The low support of families in the U.S. may contribute to one of the highest poverty rates of children among OECD countries. The U.K. lowered child poverty rates considerably since the 1990s, when the rate was on relatively high levels as well (OECD; 2013b). Possibly as a response to generous support of families with children in Scandinavia and France, child poverty is low and female labor market participation and fertility rates are the highest among OECD countries (see also Salles et al.; 2010; OECD; 2013b).

[10] Note however, that tax breaks in Figure 1.2 do not include tax benefits through income splitting as these benefits are not related to children and therefore, they are not family benefits per se.

In sum, German public transfers for family support as a percentage share of the GDP are comparable to other European countries. However, the allocation of transfers is very different to the U.K., France, and Scandinavia. German policies focus on direct cash benefits (e.g., parental leave benefits) and tax benefits (e.g., child allowances). Transfers on services, e.g., direct subsidies for child care facilities, are low. Thus, in line with the traditional model of home care provision, German family policies encourage mothers' career interruptions and do not support a combination work and family life, e.g., by subsidizing child care (see also BIS; 2009).

1.3 Organization of this dissertation

This dissertation comprises three articles that analyze i) state-level heterogeneity in returns to secondary school in Germany, ii) the effect of education on fertility, and iii) the effect of a parental leave benefit wage replacement scheme on the subjective well-being of young mothers.

Section 2 focuses on the determinants of regional heterogeneity in returns to schooling. School quality, labor market characteristics, and amenities are potential determinants of regional differences. In contrast to previous research, this study jointly evaluates the contribution of the different mechanisms. I find that returns to schooling differ substantially across the West German states, and correlate mainly with institutional features of the school system. A strong positive association between qualitative differences (e.g., curricula contents or teachers' training) and returns to schooling shows that these qualitative aspects may be powerful policy instruments.

Section 3 is co-authored by Dr. Kamila Cygan-Rehm and published in *Labor Economics* (see Cygan-Rehm and Maeder; 2013). The paper investigates the effect of education on fertility under inflexible labor market conditions. We exploit exogenous variation from a German compulsory schooling reform to deal with the endogeneity of education. By using data from two complementary data sets, we examine different fertility outcomes over the life cycle. In contrast to evidence for other developed countries, we find that

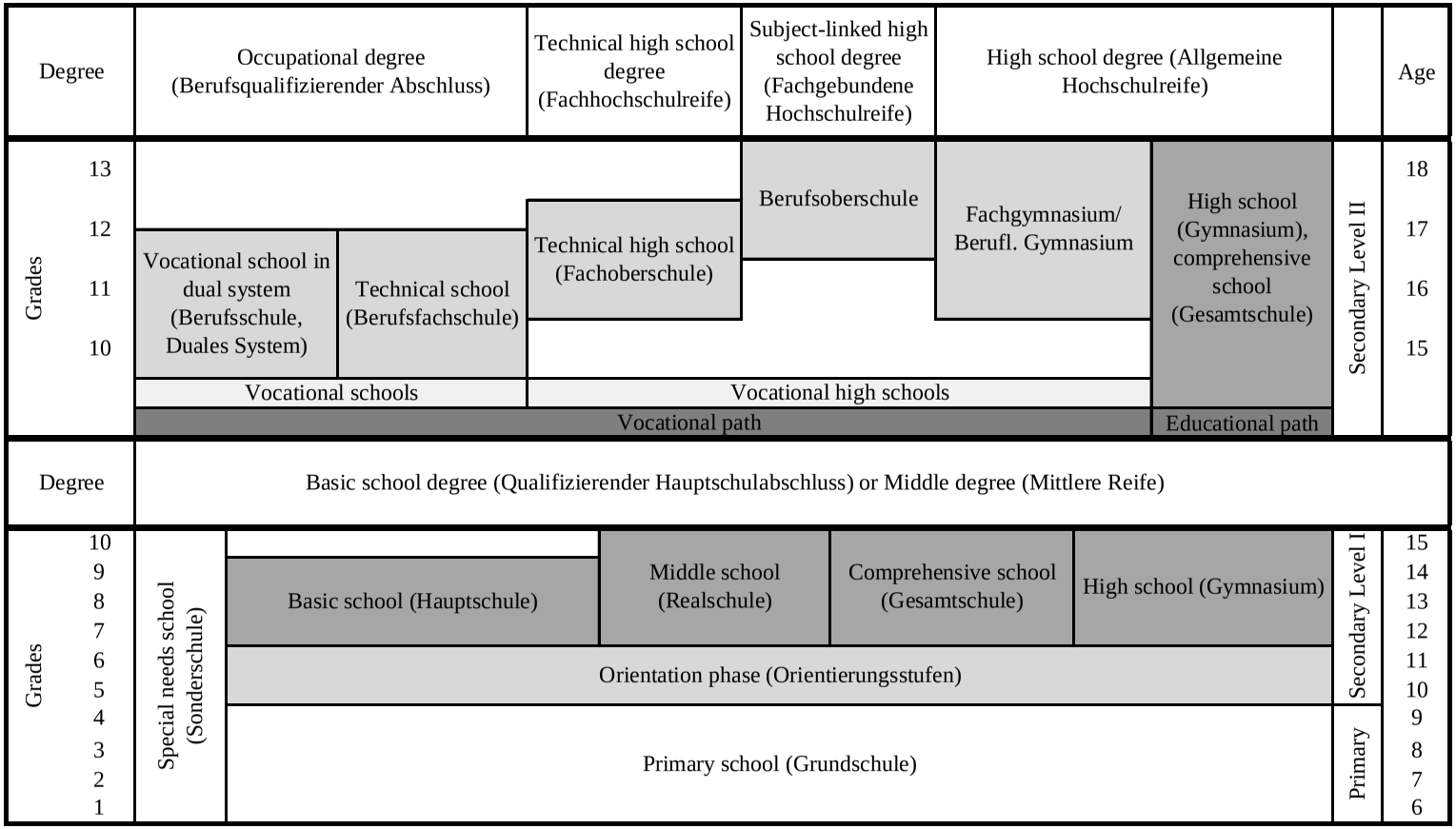
increased education causally reduces completed fertility. This negative effect operates through a postponement of first births away from the teenage years, and no catch-up later in life. We attribute these findings to the particularly high opportunity costs of child-rearing in Germany.

The study in Section 4 investigates the causal effect of earnings-related parental leave benefits (Elterngeld) on subjective well-being of young mothers. The new subsidy was introduced in 2007, and replaced a former means-tested benefit (Erziehungsgeld). The reform changed the total amount of benefits as well as the duration of pay. By construction of the reform, the change in benefits differs across population subgroups, depending on their eligibility for the former means-tested benefit. The reform also introduced incentives for paternal leave taking. Income effects, fathers' involvement, and social norms constitute potential channels through which the reform affects well-being. Using a regression discontinuity design, I find remarkable heterogeneities in the response to the reform. While subjective well-being of West German mothers increases, East German mothers experience decreasing life satisfaction.

Finally, Section 5 summarizes and concludes.

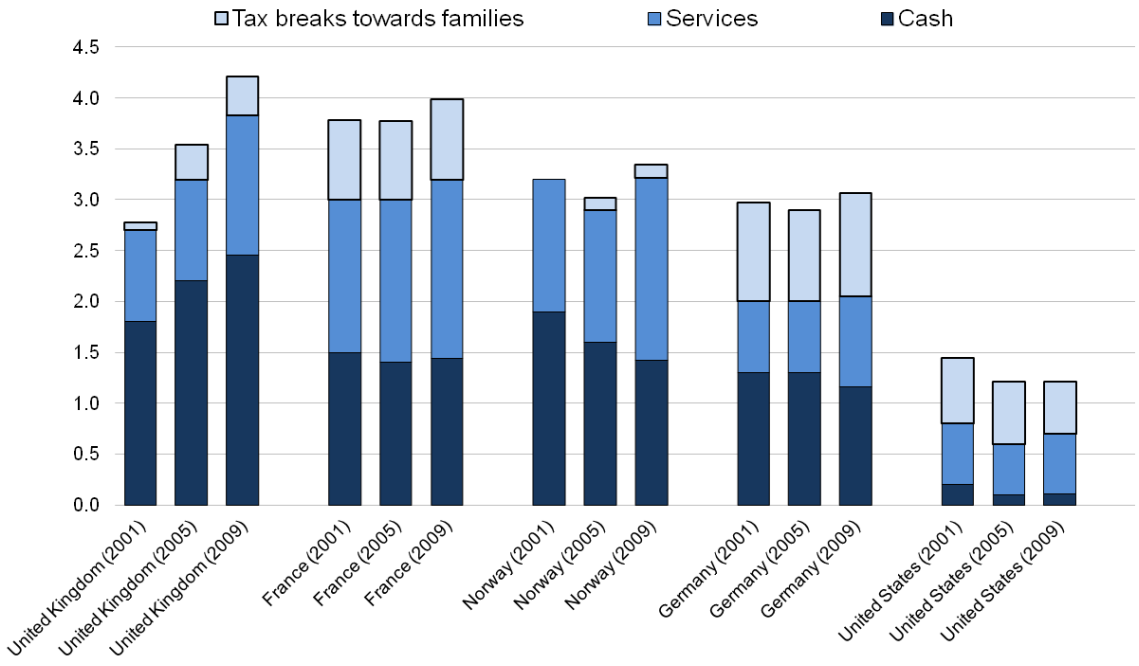
1.4 Figures and tables

Figure 1.1: The German school system



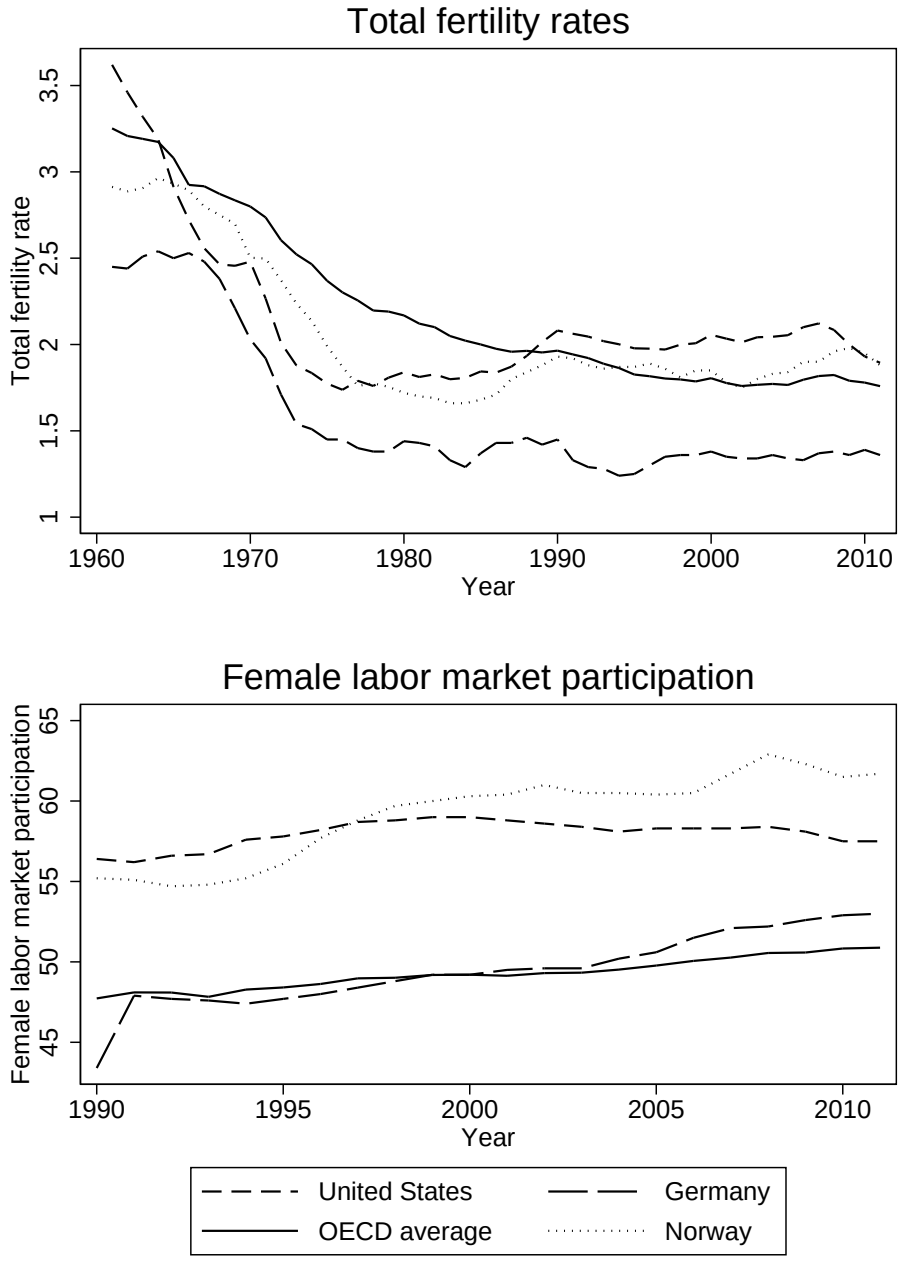
Source: KMK (2013); own illustration.

Figure 1.2: Public spending on family benefits in cash, services and tax measures, in per cent of GDP



Note: Child related transfers include child allowances, payments during parental leave, and income support for families and sole parents. Public spending on services consist of direct financing and subsidized child care and early education facilities. Tax benefits include tax exemptions, child tax allowances (only amounts that are deducted from the taxable income), child tax credits, and other amounts that are deducted from the tax liability.
Source: OECD (2013b), OECD (2009); own calculation and illustration.

Figure 1.3: Development of total fertility rates and labor market participation in selected countries



Source: World Bank (2013); own illustration.

Table 1.1: Students by secondary level and school track in 2011/12

	Number of Students	Share of students in percent
Panel A: Secondary level I		
Orientation phase	101,135	2.1
Basic school (*Hauptschule*)	656,754	13.6
School with multiple tracks (Grades 5-10)	389,316	8.1
Middle school (*Realschule*)	1,129,994	23.4
High school (*Gymnasium*)	1,514,612	31.4
Comprehensive school	540,280	11.2
Waldorf school	67,041	1.4
Special needs school	364,563	7.6
Evening schools and Kollegs	58,927	1.2
Total	4,822,622	100
Panel B: Educational path secondary level II		
High school (*Gymnasium*)	879,168	89.1
Comprehensive school	92,395	9.4
Waldorf school	14,534	1.5
Special needs school	1,151	0.1
Total	987,248	100
Panel C: Vocational path secondary level II		
Vocational schools (*Berufsschule*)	1,558,964	64.3
Technical schools (*Berufsfachschule*)	455,212	18.8
Technical high schools (*Fachoberschule*)	137,447	5.7
Berufsoberschule	24,768	1.0
Fachgymnasium	168,359	6.9
Other	79,044	3.3
Total	2,423,794	100

Source: Federal Bureau of Statistics (2014), own calculation

2 State-level heterogeneity in returns to secondary schooling in West Germany[11]

2.1 Introduction

The return to education is the percentage wage increase resulting from investments in education (Becker; 1964; Mincer; 1974). International comparisons show that the rate of return to schooling correlates, e.g., with the level of economic development, average income, and the supply of education (see, e.g., Psacharopoulos and Patrinos; 2004). Consequently, returns to schooling are important indicators in international comparisons of school systems, education policies, and labor market characteristics (see, e.g., OECD; 2012). Also, returns to schooling are subject to remarkable regional heterogeneity: in 2008, the rate of the return to upper secondary education for men was 20.2% in the U.S. and 12.2% in the U.K.. In Germany, the rate was one of the lowest among OECD countries with 6.8% (OECD; 2012).

Interestingly, determinants of regional heterogeneity in returns to schooling are hardly studied even though they are of major economic relevance. Understanding mechanisms behind regional differences enhances transparency and thereby promotes government productivity, for example, by more targeted programs. A number of studies compare returns to schooling across countries, states or regions and draw conclusions about potential determinants of regional differences (see, e.g., Psacharopoulos and Patrinos; 2004; OECD; 2012). Previous literature finds that returns to education correlate, e.g., with school quality (Card and Krueger; 1992), unemployment rates (Ammermueller et al.; 2009), and labor market characteristics and regional amenities (Beeson; 1991). Overall, the literature distinguishes the effect of school quality, labor market characteristics, and regional amenities on returns to education. However, so far no study evaluates these potential determinants

[11] I thank Regina T. Riphahn, Kamila Cygan-Rehm, Martina Eschelbach, Daniel Kuehnle, Robert Orlowski, and the participants of the EEA in Oslo and the EALE in Cyprus for very valuable comments on earlier versions of this paper. The usual disclaimer applies.

jointly and no study focuses on their relative contribution to regional differences in returns to schooling.

This paper contributes to the literature in two important ways: first, it is the first study that evaluates the relative contribution of different determinants (labor market characteristics, regional amenities, institutional features of school systems) to regional heterogeneity in returns to secondary schooling in Germany. Second, so far no other study investigates the school quality-earnings relationship in Germany. I exploit variation in returns to schooling, institutional features of school systems, and labor market characteristics across federal states but within the country. This mitigates the influence of confounding unobserved regional characteristics. I use five representative cross sections of the German work force for the empirical analysis.

The empirical strategy extends the two stage approach of Card and Krueger (1992): the first stage represents a Mincer wage equation estimated by ordinary least squares (OLS). The second stage of the estimation strategy uses estimated returns to schooling clustered by state, year of birth, and survey year as dependent variable. Independent variables comprise labor market characteristics, amenities and institutional features of the school system. Here, OLS estimates give correlations between returns to schooling and state characteristics. Cross-state variation in returns to schooling identifies the estimates, which are likely to understate the underlying relationship.

I find substantial variation in state-level returns to schooling. Estimates range from 6.7% in North Rhine-Westphalia to 8.5% in Bavaria. Also, mainly institutional features of the school system contribute to these differences (e.g., pupil-teacher ratios), whereas labor market characteristics and regional amenities correlate weakly with returns to schooling. Traditional school quality measures such as pupil-teacher ratios or spending per pupil measure financial differences between states. However, the states' school systems also differ in qualitative aspect (e.g., curricula contents, training of teachers). PISA test scores are likely to capture these qualitative differences. I consider individuals born between 1940 and 1980 in the main sample and relate their returns to schooling to PISA results

of pupils born in the late 1980s. A robust positive relationship between PISA 2003 test scores and returns to schooling affirms an association between school quality and returns to schooling. The results show that the design of school systems (e.g., curricula contents) may be an even more powerful instrument to guide incentives for educational investments and enhance students achievement than are financial investments. Thus, the evaluation of state differences in returns to schooling and potential determinants provides insights to the efficient allocation of resources. However, the analysis also reveals the lack of high quality information on school performance and school quality in Germany.

2.2 Theory and literature

2.2.1 Potential mechanisms

At the individual level, education represents an investment in human capital and the human capital approach views those investments as enhancements of individual productivity (Becker; 1964; Mincer; 1974). In perfect labor markets improved productivity will result in higher wages.[12] In fact, most empirical studies show a strong positive correlation between formal education and wages.[13] Overall, correlation studies by, for example, Lauer and Steiner (2001) and Boockmann and Steiner (2006) show that the average return to an additional year of education ranges between 6 and 8% for Germany.

Regional heterogeneity in returns to schooling may arise through different mechanisms. One potential determinant is school quality. The individual stock of human capital comprises educational attainment, its quality, and other components (Heckman et al.;

[12] This holds true in a labor market with perfect competition and without barriers. In contrast to human capital theory, the theories of signaling (Spence; 1973) and screening (Thurow; 1970) both assume that education does not necessarily increase individual productivity. Following this theory, formal education is a signal for inherent ability and productivity. Bedard (2001), Layard and Psacharopoulos (1974) or Jaeger and Page (1996) suggest that both theories are relevant. In line with the main literature on returns to education the human capital approach is the basis for interpretation in my study.

[13] The main issue discussed in the international literature is whether education itself causes the observed positive correlation between education and wages, or whether instead individuals with greater ability choose to acquire higher levels of education. The main focus lies on the estimation of the causal effect of education on individual wages. Ashenfelter et al. (1999) review the literature on (causal) returns to education. The authors find in line with economic theory that controlling for ability lowers the OLS estimates for the returns to education for studies in the United States. However, an upward bias of OLS results does not hold for studies using non-US data (Ashenfelter et al.; 1999).

1996b). If educational attainment itself affects the productivity of individuals, we expect that the quality of the acquired education affects earnings as well (Burtless; 1996). If higher school quality raises productivity of workers we expect higher returns to schooling. Thus, high school quality in the region where individuals were educated in the past, translates into high returns to schooling.

Present characteristics of the region of residence affect returns to schooling as well. Here, the literature differentiates between a labor demand effect (e.g., high unemployment) and the effect of regional amenities (e.g., crime rate, child care institutions) (Roback; 1982).

The impacts of labor demand and regional amenities are closely related. Both work through the labor supply of skill groups. One can interpret the return to education as the price individuals get for their qualification. The higher the excess labor supply of a certain skill group, the lower the price this skill group receives (holding labor demand constant). Unfavorable labor market conditions such as high unemployment, i.e., excess labor supply, translate into lower skill prices (labor demand effect). Amenities affect the labor supply of skill groups in a certain region. Favorable conditions such as good infrastructure affect labor supply through a pull effect (see, e.g., Beeson; 1991). Especially the well educated, with high disposable incomes move to these regions and increase labor supply of this skill group and thereby lower skill prices for this skill group if labor demand remains constant (see, e.g., Beeson; 1991; Graves et al.; 1999; Black et al.; 2009; Krupka; 2009; Lee; 2010). In sum, we expect that state-level returns to schooling correlate with local labor market conditions. We expect that returns to schooling are low in desirable regions and in regions with excess labor supply.

2.2.2 Previous empirical findings

Empirical evidence on determinants of differences in returns to schooling by country, state, or region mainly concentrates on the U.S. with a few exceptions.

Findings for the school quality-earnings relationship are mixed (see Heckman et al.; 1996a; Card and Krueger; 1996). Omitted variables, misspecification of the underlying relationship, or the level of aggregation bias the results (Hanushek; 1997).[14] Card and Krueger (1992) study U.S. data and find a positive association of school quality (measured by pupil-teacher ratio, term length, relative teacher wage) and returns to schooling. The authors identify the correlations based on variation between the state of school attendance and the state of residence. Contradictory, Betts (1995) finds hardly any impact of school quality measures (e.g., percentage of teachers with master's degree) on returns to schooling using a similar identification strategy (see also Hanushek; 1997). Heckman et al. (1996b) affirm the impact of school quality (pupil-teacher ratio, term length, relative teacher wage) mainly for the return to college education. In line, Altonji and Dunn (1996) show a positive association of school quality (e.g., pupil-teacher ratio, expenditures per student) and wages in the U.S. using siblings correlation for identification. Harmon and Walker (2000) exploit a schooling reform in England and Wales and find only small effects of school quality on wages. For Germany, no study so far analyses the classical school quality measures. A related study of Baumgartner (2004) focuses on the relationship between class size and early career earnings and uses German district data. The author finds only a weak association.

Research on the relationship between labor market characteristics and returns to schooling usually finds the expected patterns. Beeson (1991) shows, based on OLS results, that amenities and labor demand are important but cannot fully account for regional differences in returns to schooling across U.S. metropolitan statistical areas (SMSA). Ammermueller et al. (2009) find a significant negative association between returns to schooling and aggregated unemployment rates in Germany. In contrast, Reilich (2013) finds predominantly homogeneous returns to schooling by German state which correlate positively with unemployment rates. Also, results of Reilich (2013) underpin the role of regional amenities as she finds a negative relationship between returns to schooling and land costs.

[14] See Card and Krueger (1996) for a detailed summary of previous findings.

Psacharopoulos and Patrinos (2004) survey estimated returns to schooling for about 70 countries and investigate potential patterns. The authors show that returns to schooling decrease with increasing average income. Overall, measures of labor demand conditions comprise, e.g., unemployment rates, shares of workers in a specific industry, or population density (see Hanushek; 1981). Some indicators, e.g., population density, alternatively constitute indicators for amenities (Beeson; 1991). Crime rates, recreational and health facilities, or the number of heating or cloudy days exemplify other typical measures for local amenities (see, e.g., Roback; 1982; Beeson; 1991).

2.3 Institutional background and school quality

The German secondary school system is a tripartite system that sorts students at the age of 10 into three school tracks: basic school (*Hauptschule*), middle school (*Realschule*), and high school (*Gymnasium*). These tracks prepare for different occupational careers. Basic school lasts for 5-6 years (depending on the state) and prepares for apprenticeship training and vocational schools. Middle school lasts 6 years and prepares for training in white-collar jobs. High school lasts 8-9 years and prepares for university attendance and academic careers.[15]

Because each German state governs its school system independently, substantial differences between state school systems arise and contribute to differences, for example, in school quality (Pluennecke et al.; 2007). Examination modes in high school (centralized exams at state level vs. school level exams)(van Ackeren and Klemm; 2009), the supply of comprehensive schools (Woessmann; 2010), and curricula contents (Rösner; 1999) exemplify differences between school systems. Here, qualitative differences such as curricula contents or the training of teachers may be of importance for labor market outcomes, but are difficult to quantify.

[15] Additionally, students in all states can attend comprehensive schools without any tracking. Comprehensive schools are not very common in West Germany. In fact, in 1996/97 the share of students attending comprehensive schools reached only 8.7% in West Germany (Rösner; 1999). See Ertl and Phillips (2000) for further discussion.

Another important difference between school systems is the organization of tracking. In general, student ability determines tracking after primary school. However, the organization of the transition to secondary school tracks differs between states (KMK; 2010a). Some states (e.g., Bavaria) rest on primary school grades to screen students' ability and require specific grade averages for the access to middle and high school. In other states, regulations are more flexible and parents decide on track choice (e.g., in Bremen) or the decision lies with the parents but teachers give a recommendation (KMK; 2010b). Overall, different requirements for the transition to secondary school tracks may have different potential consequences for the distribution of students across and within states: first, selective states may exhibit low shares of high school students if we assume that ability is uniformly distributed across states. Second, high school students in selective states may have higher average ability. Third, labor supply of high school graduates may be lower in selective states if individuals work in the state where they are educated.

2.4 Empirical approach

2.4.1 Method

I examine potential determinants of regional heterogeneity in returns to schooling following the two step procedure of Card and Krueger (1992). In the first step, the authors estimate returns to schooling by U.S. state and birth cohort by OLS. In a second OLS estimation the authors explain differences in returns to education by school quality measures. Thus, instead of including interactions of schooling and state characteristics in one equation, they prefer two steps. Card and Krueger (1992) argue that the two step procedure has important advantages: first, it simplifies the interpretation of patterns in returns to schooling. Second, the two step estimation allows them to exploit the contribution of state characteristics to differences in state-level returns to schooling (see also Beeson; 1991). In the approach of Card and Krueger (1992), individuals who are born in a specific state and

moved to another state identify the impact of school quality. I cannot follow their strategy because of low regional mobility among German workers (Harhoff and Kane; 1997; Huber; 2004; Machin et al.; 2012). Instead, I assume that individuals still live where they went to school. Thus, cross-state variation in returns to schooling identifies the determinants of regional differences. Section 2.4.2 discusses potential consequences of the modification of the approach of Card and Krueger (1992).

The first stage estimates returns to schooling by OLS and uses the following earnings equation:

$$\ln w_{ijtc} = \delta_1 \, \textit{state}_j + \delta_2 \, \textit{cohort}_c + \beta_{jtc} \, S_i(\textit{state}_j, \textit{cohort}_c, \textit{year}_t) + \delta_3 Z_i + \epsilon_{ijtc}. \quad (2.1)$$

The dependent variable is the logarithm of the gross hourly wage w_{ijtc} of individual *i*, living in state *j*, surveyed at time *t*, and born in year *c*. Variables $\textit{state}_j$ and $\textit{cohort}_c$ represent state and cohort fixed effects. S_{ijtc} measures the individual's secondary schooling, for example, years of secondary schooling.[16] I assume the return to schooling β_{jtc} to consist of three components: a state effect, year of birth effect, and a survey year effect.[17] Z_i represents additional controls for age and gender. ϵ_{ijtc} is an error term.

From OLS results of equation (2.1) I calculate the marginal effect of schooling on wages for clusters of state, cohort, and survey year. Thus, I construct returns to schooling β_{jtc} for each group of individuals born in year *c*, living in state *j* at time *t*. In the second step of the analysis, I relate returns to schooling β_{jtc} to characteristics of German states:

$$\beta_{jtc} = \alpha_1 q_{jc} + \alpha_2 a_{jt} + \alpha_3 d_{jt} + \alpha_4 \, \textit{year}_j + \alpha_5 \, \textit{cohort}_c + u_{jtc}. \quad (2.2)$$

The vector q_{jc} contains controls for institutional features of the school system and school quality that affected birth cohort *c* in state *j*. The vector a_{jt} controls for amenities

[16] Because linearity may be an inappropriate assumption in a tracked school system, I also use a specification with indicator variables for school degrees.

[17] Using three components contrasts to Card and Krueger (1992) who concentrate only on state and cohorts effects. An additional survey year effect accounts, for example, for labor market conditions.

and d_{jt} for labor market conditions in state *j* at time *t*. The estimates of α_1, α_2, and α_3 measure the correlations between school quality, labor market characteristics, amenities, and returns to schooling. u_{jtc} is an error term. Returns to schooling β_{jtc} vary by state, birth cohort, and survey year. As I aim to explain differences across states, I include cohort and survey year fixed effects in the second equation. These fixed effects capture cohort and time trends which may also correlate with state characteristics. Thus, the estimates α_1, α_2, and α_3 are identified by cross-state variation and show the contribution of state characteristics to state differences in returns to schooling.

2.4.2 Potential Biases

As I estimate equation (2.1) by OLS unobserved characteristics such as ability are likely to violate the conditional mean independence assumption and bias returns to schooling upwards.[18] Ability bias affects the estimates α_1, α_2, and α_3 in equation (2.2) if it varies systematically by state and correlates with state characteristics. As some states are more restrictive in the transition to secondary school tracks it is rather strong to assume that ability bias is regionally homogeneous. Further, control variables such as PISA scores may reflect regional differences in ability especially if I use track specific scores. However, an important advantage of the two step procedure is that I am able to control for selectivity of school tracks in the second stage of the estimation. The share of high school graduates potentially reflect average ability in high school and controlling for it mitigates biases because of omitted ability.

In equation (2.1), I assume that individuals are regionally immobile. Regional mobility may affect the results if mobility varies across states and mobility is correlated with state characteristics. Mobility across states is likely to be higher for smaller city states. Also, regional labor market characteristics are likely to be correlated with mobility and should even equalize regional returns to schooling in the long run. Consequently, results

[18] Card (1999) shows that returns to schooling are upward biased by omitting ability and individual heterogeneity. Nonetheless, no other method is applicable here: instrumental variable estimations require valid instruments, which are not available in my data.

for regional amenities and labor market characteristics might be systematically biased. However, school quality characteristics are potentially not or only weakly correlated with mobility after graduation and lead to an attenuation bias.

Overall, regional mobility is on a such a low level in Germany that it potentially imposes minor consequences. In fact, Harhoff and Kane (1997) finds that 80% of West German workers never move within or across state during their working life. Also, Huber (2004) reports a gross mobility rate of 1.32. Thus, only 1.32% of the German population moves across states within a year. Furthermore, the author finds that the rate decreased during the 1980s.[19] Given this low regional mobility, the mentioned biases should reveal minor consequences for the estimates. As a robustness check, I restrict the sample to men who are usually less regionally mobile, excluded all individuals who ever moved during their working life, and excluded city states with potentially higher mobility (Hunt; 2004).[20] The results underpin that regional mobility does not affect the results and main conclusions hold.

I am able to control for differences in ability bias across states and regional mobility is likely to lead to an attenuation bias. Thus, consequences from regional mobility and ability bias are very limited and estimates may represent a lower bound of the causal effect. Unfortunately, a threat to internal validity is the scarcity of high-quality state-level data and multicollinearity in the second stage of the estimation. Consequently, aggregated control variables such as unemployment rates may capture a whole set of business cycle characteristics and estimates represent correlations. However, as the main aim of the paper is the evaluation of the contribution of variable sets (labor demand, regional amenities, school quality) omitted variables do not weaken the main conclusions.

[19] I also analyzed official statistics on across state migration from the federal statistical office. The statistics support that since the 1950s constantly only about 3% of individuals move across states.

[20] Results without individuals who ever moved during working life and results without city states are available upon request.

2.5 Data and variables

This analysis exploits five repeated cross sections of the Qualification and Career Survey (QaC), which provides large sample sizes in total and for each federal (West) German state. Each wave is a 0.1% representative cross section of the German labor force (i.e., blue and white-collar workers, civil servants, self employed) and covers the entire income distribution (Dostal and Jansen; 2002). The data provide detailed information about individuals' socio economic background and formal education. Each of the five waves of years 1979, 1985/86, 1991/92, 1998/99, and 2005/06 contains up to 35,000 observations. I pool all cross sections and restrict the sample to employed West German natives born between 1940 and 1980 and aged between 16 and 60 years at the time of the interview.[21] I drop observations with missing values in the schooling or in the income variable (13%).[22] The final sample comprises 70,474 observations.

I construct the hourly wage using information about monthly labor earnings.[23] The QaC provides detailed information about the acquired secondary and post-secondary degrees. I build the variable "Years of Schooling" using standard durations for graduation in secondary school tracks (Krueger and Pischke; 1995). In a tracked system years of schooling may differently affect wages depending on the attended track. In a second, more flexible specification, I replace the variable "Years of Schooling" by indicators for the highest secondary degree achieved. The three categories are: basic degree (reference)[24], middle degree, and high school degree.

The data used for control variables in the second stage are provided mainly by the Federal Bureau of Statistics (1952-2006). Table 2.1 describes control variables included in the second stage of the estimation. The first part shows controls capturing institutional

[21] East Germany is of interest as well. However, the East German school system differed during the communist regime and one cannot easily compare school degrees (Riphahn and Trübswetter; 2013).

[22] In the distribution of qualifications of the dropped individuals, high and low educated individuals are over represented. Missing values are uniformly distributed across the federal states.

[23] The earnings variable is measured in classes. Following Pischke and von Wachter (2008) I set earnings to the class midpoints (see details in their discussion paper version).

[24] The reference category also entails drop-outs (1.5% of individuals in the data set). Excluding them from the analysis gives the same results.

features of the school system. One of the *traditional* measures for school quality is the pupil-teacher ratio by state and *year of birth+10* (see, e.g., Card and Krueger; 1992).[25] Thus, I assume that the pupil-teacher ratio matters for the quality of education of 10 years old pupils. Additionally, I use spending per pupil (see, e.g., Altonji and Dunn; 1996) and include the share of high school students to control for the selectivity of transition mechanisms to secondary school tracks. All school quality measures are aggregated at the state and year of birth level and capture mainly financial differences between states. As pointed out in Section 2.3, other factors such as the training of teachers or curricula contents differ across states. As these are difficult to quantify I include test results of the *Programme for International Student Assessment* (PISA) by state in a second specification.[26] I use PISA math scores from 2003, so the underlying population of the PISA tests and the main sample is different with respect to year of birth. The main sample entails individuals born 1940-1980. Pupils participating in PISA 2003 are born in the late 1980s.[27] PISA test scores capture qualitative differences in school systems across states and reflect school quality for individuals in the main sample if older cohorts faced similar conditions. As PISA results may also be an outcome of returns to education of the students' parents or a measure of average state ability, PISA scores suffer from reverse causality. However, no better measure for qualitative differences in school quality is available so far.

The second part of Table 2.1 displays means of variables capturing cultural and labor market conditions in the state of residence at the time of the interview. Whereas crime rate and GDP per capita control for local amenities, population density reflects both, labor demand conditions and amenities (see, e.g., Beeson; 1991). The share of workers in the manufacturing sector and the unemployment rate control for labor demand differences between states (see, e.g., Hanushek; 1981).

[25] Pupil-teacher ratios entail primary and secondary school pupils and teacher.

[26] The OECD conducts PISA and provides representative and comparable data about basic competences of students from the OECD countries (PISA-Konsortium Deutschland; 2002). An extension of the international survey, the PISA-E survey, ensures representative sampling also within each German state.

[27] PISA tests always put special attention on one subject (reading, science, or mathematics). Whereas PISA 2000 focused on reading scores, the main focus of PISA 2003 was mathematics (PISA-Konsortium Deutschland; 2005).

2.6 Empirical results

2.6.1 Descriptive evidence and first stage results

Table 2.2 shows state averages and standard deviations of log wage and years of secondary schooling. Additionally, I calculate the share of graduates by school leaving certificate, unemployment rates, and pupil-teacher ratios. Shaded cells represent minimum and maximum values within one column. The differences in years of schooling and shares of graduates are most striking. Differences in years of schooling (column 2) reach 0.57 years with 10.5 years (highest) in Hamburg and 9.73 years (lowest) in Bavaria. The share of basic school graduates varies between 36% (Hamburg) and 55% (Saarland); the shares of middle school (column 4) and high school graduates (column 5) vary as well. However, variation in schooling and differences in log wages are not obviously related (column 1). As previously mentioned, selective transitions to high school translate into low shares of high school graduate and vise versa. The descriptive evidence supports this pattern as Bavaria is most restrictive in the transition to secondary school tracks and has lowest shares of high school graduates. The last columns provide average unemployment rates, population densities, and pupil-teacher ratios.

Table A.1 in the Appendix presents the results of two first stage estimations where I regress log wages on interactions between state and schooling and a rich set of covariates. To measure schooling, I use years of schooling in the first estimation (column 1) and more flexible indicators for school leaving certificates in the second estimation (column 2). The statistical significance of the state and schooling interactions in Table A.1 already show state-level heterogeneity in returns to schooling. Based on the first stage regression I calculate returns to schooling for 1,685 cluster of *state* $\times$ *year of birth* $\times$ *year of survey*. Table 2.3 shows state-level average returns to schooling of these clusters and the corresponding ranks.[28] The average return to an additional year of schooling in West Germany (column 1) ranges between 6.7 (North Rhine-Westphalia) and 8.5% (Bavaria)

[28] In order to save space I do not present average returns to schooling by state, cohort, and year of survey. Results are available upon request.

and is comparable to previous findings (e.g., Lauer and Steiner; 2001; Boockmann and Steiner; 2006). As the assumption of a linear relationship between schooling and wages is restrictive, especially in a tripartite school system, the model specification in columns 2 and 3 use an indicator-based definition of schooling. Table 2.3 gives the percentage wage increase after graduation from middle (column 2) or high school (column 3) compared to basic school graduates. The average return to middle degree compared to basic degree (column 2) ranges between 11.4 and 21.1% and the return to high school degree (column 3) lies between 33.1 and 43.6% (see Dustmann (2004) for comparable results). The last columns gives the number of clusters of survey year and cohort in each state and again the share of high school graduates.

The estimates vary across German states. Whereas Bavaria is in the upper third of the ranking regardless of the education variable, Saarland consistently ranks 9th. For the other states the results are mixed. If the return to one of the degrees is high, also returns to years of schooling are in an upper range (see, e.g., Bremen). Interestingly, the return to high school degree and the corresponding shares of graduates are related. I observe high shares of high school graduates in Hamburg and low returns to high school degrees. In Bavaria I find the reversed pattern. Apparently, the share of high school graduates captures differences in transition mechanisms to high school and reflects the selectivity of school systems and ability in high school. The lower the share of high school students, the higher is average ability, and ability bias in returns to schooling. Thus, the negative relationship of returns to schooling and the share of graduates underpins an upward bias in returns to schooling. Also, it supports that the share of graduates captures ability bias. Consequently, controlling for the share of high school graduates in the second stage of the estimation limits potential consequences of ability bias for second stage estimates.

2.6.2 Second stage results

The second stage of the estimation relates returns to schooling to measures for the states' labor market conditions and institutional features. Table 2.4 presents the key results for

the second stage of the estimation. The columns present results from regressions where the dependent variables are returns to years of schooling (columns 1 and 2), middle degree with reference basic degree (columns 3 and 4), and high school degree with reference basic degree (columns 5 and 6). For each dependent variable, I estimate two specifications: one controlling for labor market characteristics (estimates given in panel A) only and one where I also include institutional features (panel B). Each specification also comprises cohort fixed effects and year of survey fixed effects to capture cohort trends (e.g., educational expansion) and year effects (e.g., inflation). Thus, only cross-state variation in returns to schooling identifies estimates in the second stage. As the estimation procedure involves two estimation steps, I calculate standard errors by a bootstrap algorithm that estimates first and second stage jointly (200 replications).

Panel A shows correlations between labor market conditions and returns to schooling. The estimates for the share of workers in manufacturing and population density show the expected negative sign. Only for the population density the estimates are precisely estimated. The higher the population density, the lower the returns to schooling. This result underpins the hypothesis that higher labor supply leads to lower skill prices.[29] Surprisingly, I find a positive relationship between crime rates and returns to high school degree. However, one may attribute this finding to reverse causality. The other labor market characteristics are only weakly correlated with returns to schooling. Estimates are imprecise because state characteristics are highly collinear.[30]

Panel B in Table 2.4 gives the results when I add controls for institutional features of the school system. As expected, the number of pupils per teacher correlates negatively with returns to schooling. However, this relationship is significant only for returns to years of schooling. The size of the estimate is comparable to that in studies for the U.S. (e.g., Card and Krueger; 1992). Log spending per pupil is not significantly related to returns to schooling.

[29] Population density is especially high in the city states Hamburg and Bremen. The results are robust to the exclusion of the city states. Results are available upon request.

[30] Here, I use crime rate as a measure for regional amenities. In other specifications I also used spending for recreation and average rent. I did not find a significant relationship. Results are available upon request.

The share of high school students reflects regional labor supply of high qualified individuals and average ability in school tracks. The correlation between shares of high school graduates and returns to high school degree is significantly negative. This negative relationship suggests that omitted ability biases OLS estimates of returns to schooling upwards. Also, the negative relationship suggests that returns to high school degree are low where the labor supply of high qualified individuals is high.

In sum, state characteristics measuring labor supply (unemployment, share of workers in manufacturing) are negatively correlated with returns to schooling. This negative relationship supports the labor demand hypothesis. The relationship between returns to schooling and regional amenities is inconclusive, for example, crime rates are positively related to returns to schooling. However, the correlation is hardly significant and reverse causality potentially causes the positive correlation. When I include measures for institutional features of the school system, I find evidence for a negative correlation of the pupil-teacher ratio (support for school quality hypothesis) and a negative relationship between the share of high school graduates and returns to schooling.

2.6.3 Qualitative differences across state school systems

The results in Table 2.4 show that especially institutional features of the school system correlate with state-level returns to schooling. As discussed earlier, the German school system differs also in qualitative aspects.

PISA results of German students vary substantially by state, correlate with institutional features of the school system (Woessmann; 2010), and may reflect differences in school quality (Pluennecke et al.; 2007). In Table 2.5 I include state-level PISA math results from 2003 in the second step regression (equation 2.2) to capture qualitative differences across state school systems.

The last line in panel B of Table 2.5 gives the relationship between PISA scores in math in 2003 and returns to schooling. I find a highly significant positive relationship especially between returns to high school degree and PISA. The significant relationship

between the share of high school students and returns to schooling remains unchanged. The inclusion of PISA test scores decreases the coefficient of pupil-teacher ratios in magnitude and precision. Thus, PISA scores correlate with both, returns to schooling and traditional school quality measures.

The correlation of PISA scores and returns to schooling on the one hand, and the correlation with pupil-teacher ratio on the other support the argument that PISA scores measure school quality. However, the question remains whether PISA scores correlate with returns to schooling because of reverse causality (e.g., because students in PISA show more effort in certain states because they expect higher returns to schooling such as their parents). Unfortunately, because of a lack of detailed data I cannot test which mechanism lies behind the results.

2.7 Robustness

The basic sample uses men and women born between 1940 and 1980. The changing role of women in education and the labor market in this period may affect my results. Additionally, women are typically more regionally mobile then men (Hunt; 2006). As regional mobility potentially leads to an attenuation bias, I expect more pronounced estimates in a male subsample.

Table 2.6 gives results for men only. Here, I give one specification with and one without including PISA scores. The sample size reduces to 40,775 men in the first stage and remains at 1685 clusters in the second stage of the estimation. The results underpin my baseline results. Here, estimates for the relationship of amenities and labor demand and returns to schooling are no longer significant though similar in signs. The findings for institutional features are robust to the change in sample selection criteria. The correlation between pupil-teacher ratio and returns to schooling is significant at least at the 10% level for all schooling variables in a specification without PISA scores. The positive and significant correlation supports the school quality hypothesis. The inclusion of PISA test scores also affects the results in this subsample. The correlation with PISA scores is again

especially pronounced for high school. The pupil-teacher ratio coefficient decreases and is no longer significant once I control for PISA scores.

Table 2.7 gives results when I use PISA 2000 reading scores instead of the 2003 math scores. Before PISA 2003 states had the opportunity to compare their performance in the 2000 tests to each other and react accordingly. Before 2000 such detailed information on student performance by state was not available. Thus, my results with 2003 scores might be downward biased if states with low school quality reacted to their bad results. For the 2000 scores we expect a more pronounced relationship between PISA and returns to schooling. Unfortunately, for 2000 PISA scores are not available for Hamburg and I have to exclude the state and reduce the sample size. However, Table 2.7 shows a more pronounced relationship between PISA scores and returns to schooling. Thus, the main results are robust to that change and the robustness check supports the hypothesis that PISA reflects school quality and that school quality affects individuals' labor market outcomes.

2.8 Conclusions

Returns to schooling constitute important indicators in the comparison of school systems and labor markets; they are systematically related to economic development, average income, and supply of education (Psacharopoulos and Patrinos; 2004). Many studies focus on cross-country differences in returns to education (see, e.g., OECD; 2012). However, because of unobserved country characteristics the analysis of potential determinants of regional heterogeneity in returns to schooling, for example, labor market characteristics and institutional features, is difficult. Following the strategy of Woessmann (2010) this paper uses Germany as a microcosm for regional differences in an international context. This paper studies determinants of state-level heterogeneity in returns to secondary schooling in Germany.

Institutional features of the school system, labor market characteristics such as unemployment rates, or regional amenities constitute potential determinants of differences

in returns to schooling. Whereas institutions might affect returns to schooling through the quality of education (see, e.g., Card and Krueger; 1992), characteristics of the region of residence affect returns to schooling through regional amenities or through the labor supply/demand of/for certain skill groups (see, e.g., Beeson; 1991).

To evaluate potential determinants of state-level heterogeneity in returns to schooling, my empirical strategy extends the two step procedure of Card and Krueger (1992). In the first step, I estimate a Mincer wage regression where I assume the return to schooling to consist of a state, cohort, and a year of survey component. On the basis of this first regression I calculate returns to schooling for 1,685 clusters of state, survey year, and year of birth. In the second step I regress clustered returns to schooling on state characteristics.

My findings imply that institutional features of the school systems are more important in the explanation of differences in returns to schooling than labor market conditions. I find a positive relationship between school quality (measured by pupil-teacher ratios) and the selectivity of school systems and returns to schooling. Furthermore, returns to schooling of individuals born 1940-1980 are strongly positively related to PISA test scores of pupils born in the late 1980s. Although reverse causality might generate the positive correlation, it is also plausible that qualitative differences between school systems (e.g., curricula contents) affect returns to schooling.

Educational policy, education spending, and the design of the school system are important policy instruments. My findings show that characteristics of school systems are highly correlated with individuals' later labor market outcomes and suggest that qualitative inputs to the education system such as curricula contents may be more important than financial inputs.

2.9 Figures and tables

Table 2.1: Description, aggregation level, and source of aggregated control variables

	Variable	Mean (Sd.)	Min.	Max.	Description	Aggregation level	Source
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Institutional Features	Pupil-teacher ratio	0.28 (0.09)	0.11	0.61	Pupil-teacher Ratio ÷ 100	State × Birth cohort	Federal statistical office, *Statistische Jahrbücher*
	Log Spending / pupil	0.57 (0.87)	-1.55	2.33	Logarithm of educational spending per pupil	state × Birth cohort	Federal statistical office, *Statistische Jahrbücher*
	Share high school	0.23 (0.17)	0.00	1.00	Share of high school graduates	Survey year × state × Birth cohort	QaC
	PISA math 2003	4.96 (0.16)	4.71	5.33	State averages of PISA 2003 math scores ÷ 100	State	PISA-Konsortium Deutschland (2005)
	PISA reading 2000	4.82 (0.16)	4.48	5.10	State averages of PISA 2000 reading scores ÷ 100	State	PISA-Konsortium Deutschland (2002)
Amenities and Demand	Population Density	0.60 (0.69)	0.15	2.31	Number of inhabitants per square meter ÷ 1000	Survey year × state	Federal statistical office, *Statistische Jahrbücher*
	Share in manufacturing	0.32 (0.09)	0.15	0.52	Share of workers in sector manufacturing	Survey year × state	Federal statistical office, *Statistische Jahrbücher*
	Unemployment	9.12 (3.45)	2.10	16.60	Unemployment rate in percent	Survey year × state	Federal Employment Agency (2009)
	Crime rate	11.21 (1.54)	7.66	15.19	Number of convictions per capita × 1000	Survey year × state	Federal statistical office, *Statistische Jahrbücher*
	Log GDP	11.43 (1.04)	9.23	13.13	Logarithm of GDP per capita in EUR	Survey year × state	Federal statistical office, *Statistische Jahrbücher*

Note: Standard deviations in parentheses. Own calculations.

Table 2.2: Descriptive statistics for the German states

State	Log wage	Years of schooling	Share of graduates by degree: Basic degree	Middle degree	High school degree	Unemployment rate	Population Density	Pupil-teacher ratio /100	Observations
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Schleswig-Holstein	2.820	10.040	0.472	0.339	0.190	8.768	0.170	0.288	3160
	(0.563)	(1.434)	(0.499)	(0.473)	(0.392)	(2.763)	(0.006)	(0.083)	
Hamburg	2.839	10.517	0.366	0.325	0.309	9.923	2.206	0.256	1912
	(0.571)	(1.632)	(0.482)	(0.469)	(0.462)	(3.775)	(0.070)	(0.068)	
Lower Saxony	2.828	9.966	0.476	0.321	0.203	9.628	0.159	0.298	8623
	(0.585)	(1.577)	(0.499)	(0.467)	(0.402)	(2.982)	(0.007)	(0.101)	
Bremen	2.789	10.159	0.441	0.318	0.241	12.013	1.673	0.259	1031
	(0.551)	(1.573)	(0.497)	(0.466)	(0.428)	(4.838)	(0.036)	(0.069)	
North Rhine-Westphalia	2.877	10.039	0.468	0.265	0.266	9.689	0.511	0.322	20295
	(0.560)	(1.784)	(0.499)	(0.441)	(0.442)	(2.755)	(0.016)	(0.089)	
Hesse	2.856	9.970	0.443	0.327	0.229	6.859	0.274	0.295	6422
	(0.569)	(1.727)	(0.497)	(0.469)	(0.420)	(2.837)	(0.011)	(0.080)	
Rhineland-Palatinate	2.815	9.747	0.542	0.267	0.191	7.203	0.192	0.307	4464
	(0.593)	(1.646)	(0.498)	(0.442)	(0.393)	(2.270)	(0.009)	(0.082)	
Baden-Wurttemberg	2.898	9.950	0.473	0.301	0.226	6.035	0.278	0.289	10258
	(0.552)	(1.705)	(0.499)	(0.459)	(0.418)	(1.719)	(0.017)	(0.085)	
Bavaria	2.811	9.734	0.533	0.281	0.186	8.148	0.164	0.287	12937
	(0.580)	(1.638)	(0.499)	(0.450)	(0.389)	(2.900)	(0.009)	(0.069)	
Saarland	2.775	9.903	0.551	0.231	0.218	7.561	0.414	0.305	1347
	(0.609)	(1.614)	(0.498)	(0.422)	(0.413)	(3.954)	(0.005)	(0.071)	
West Germany	2.849	9.948	0.483	0.291	0.226	8.409	0.374	0.300	70449
	(0.571)	(1.689)	(0.500)	(0.454)	(0.418)	(3.122)	(0.376)	(0.086)	

Note: State specific means and standard deviations in parentheses. Hamburg is excluded, because average PISA scores in 2000 are not available. Shaded cells represent maximum and minimum values by column.
Source: Qualification and Career Survey waves 1979, 1985/86, 1991/92, 1998/99, 2005/06. Own calculation.

Table 2.3: OLS results: state averages of returns to years of schooling, middle degree, and high school degree

	Returns to							
	Years of schooling		Middle degree		High school degree		Number of Clusters	Share high school
	ME	Rank	ME	Rank	ME	Rank		
	(1)		(2)		(3)			
Schleswig-Holstein	8.456 ***	3	21.086 ***	1	38.273 ***	6	168	0.190
	(0.103)		(0.988)		(0.045)			
Hamburg	7.308 ***	8	11.443 ***	10	33.129 ***	10	169	0.309
	(0.663)		(2.111)		(3.699)			
Lower Saxony	7.930 ***	4	16.532 ***	7	38.336 ***	5	170	0.203
	(0.182)		(1.863)		(1.288)			
Bremen	8.504 ***	2	15.027 ***	8	40.140 ***	2	158	0.241
	(0.085)		(1.911)		(0.679)			
North Rhine-Westphalia	6.696 ***	10	17.520 ***	4	34.051 ***	8	172	0.266
	(0.085)		(0.405)		(0.338)			
Hesse	7.485 ***	6	17.955 ***	3	38.732 ***	4	172	0.229
	(0.067)		(1.224)		(0.059)			
Rhineland-Palatinate	7.693 ***	5	17.320 ***	5	39.526 ***	3	169	0.191
	(0.574)		(1.792)		(3.718)			
Baden-Wurttemberg	7.476 ***	7	16.917 ***	6	37.527 ***	7	172	0.226
	(0.211)		(0.074)		(1.155)			
Bavaria	8.505 ***	1	20.487 ***	2	43.659 ***	1	171	0.186
	(0.583)		(1.112)		(3.654)			
Saarland	6.964 ***	9	14.871 ***	9	33.336 ***	9	164	0.218
	(0.041)		(2.963)		(0.950)			

Notes: Returns to schooling by state (ME= marginal effect) calculated from year of birth, year of survey, and state specific returns to schooling. Standard errors are bootstrapped (200 replications). ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.
Source: QaC 1979, 1985/86, 1991/92, 1998/99, 2005/06; own calculations.

Table 2.4: Relationship between state characteristics and returns to schooling

	Dependent variable: Returns to					
	Years of schooling		Middle degree		High school degree	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Amenities and labor demand						
Unemployment rate	0.02	0.025	0.123	0.129	0.079	0.106
	(0.062)	(0.062)	(0.223)	(0.224)	(0.320)	(0.321)
Share in manufacturing	-2.881	-2.227	-10.349	-9.184	0.689	4.564
	(4.143)	(4.045)	(13.869)	(13.415)	(20.279)	(19.516)
Population density	-0.899 **	-0.922 **	-4.151 ***	-4.155 ***	-4.546 **	-4.503 **
	(0.390)	(0.381)	(1.261)	(1.201)	(1.886)	(1.808)
Crime rate	0.167 *	0.15	0.011	-0.017	0.85	0.757
	(0.098)	(0.092)	(0.341)	(0.313)	(0.521)	(0.485)
Log of GDP per Capita	-0.274	-0.249	0.042	0.079	-0.573	-0.446
	(0.207)	(0.209)	(0.871)	(0.887)	(1.062)	(1.072)
Panel B: Institutional features and school quality						
Pupil-teacher ratio	-	-5.384 *	-	-8.527	-	-27.169
		(3.020)		(12.469)		(16.711)
Log of spending per pupil	-	-0.09	-	-0.294	-	-0.847
		(0.208)		(0.893)		(1.428)
Share of high school graduates	-	-0.281 ***	-	-0.443	-	-2.708 ***
		(0.085)		(0.336)		(0.512)
Number of clusters	1685	1685	1685	1685	1685	1685
Year of birth fixed effects	YES	YES	YES	YES	YES	YES
Survey year fixed effects	YES	YES	YES	YES	YES	YES

Notes: Correlations of state characteristics and returns to years of schooling, middle degree, and high school degree. Dependent variable: estimated returns to schooling (see Table A.1). First step of estimation based on 70449 observations. Each column represents a separate linear regression. Standard errors are bootstrapped (200 replications, first and second stage jointly). ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.
Source: QaC 1979, 1985/86, 1991/92, 1998/99, 2005/06; own calculations.

Table 2.5: Qualitative differences in school systems

	Dependent variable: Returns to		
	Years of schooling (1)	Middle degree (2)	High school degree (3)
Panel A: Amenities and labor demand			
Unemployment rate	0.068	0.222	0.306
	(0.056)	(0.203)	(0.291)
Share in manufacturing	-3.289	-11.454	-0.347
	(4.110)	(13.352)	(19.821)
Population density	-0.579	-3.421 **	-2.915
	(0.422)	(1.399)	(2.004)
Crime rate	0.107	-0.11	0.556
	(0.100)	(0.342)	(0.526)
Log of GDP per Capita	-0.358 *	-0.153	-0.948
	(0.215)	(0.921)	(1.102)
Panel B: Institutional features and school quality			
Pupil-teacher ratio	-1.659	-0.572	-9.954
	(3.679)	(14.722)	(19.884)
Log of spending per pupil	-0.144	-0.41	-1.1
	(0.197)	(0.850)	(1.372)
Share of high school graduates	-0.215 **	-0.303	-2.405 ***
	(0.090)	(0.363)	(0.544)
PISA 2003 Math	2.678 ***	5.721	12.38 **
	(0.912)	(3.683)	(4.872)
Number of clusters	1685	1685	1685
Year of birth fixed effects	YES	YES	YES
Survey year fixed effects	YES	YES	YES

Notes: State characteristics and returns to years of schooling, middle degree, and high school degree only for men. Dependent variable: estimated returns to schooling (see Table A.1). First step of estimation based on 70449 observations. Each column represents a separate linear regression. Standard errors are bootstrapped (200 replications, first and second stage jointly). ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.
Source: QaC 1979, 1985/86, 1991/92, 1998/99, 2005/06; own calculations.

Table 2.6: Robustness: relationship between state characteristics and returns to schooling - only men

	Dependent variable: Returns to					
	Years of schooling		Middle degree		High school degree	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Amenities and labor demand						
Unemployment rate	0.061	0.099	0.333	0.375	0.294	0.515
	(0.079)	(0.072)	(0.269)	(0.254)	(0.385)	(0.360)
Share in manufacturing	1.524	0.567	9.852	8.793	23.406	17.866
	(5.285)	(5.341)	(18.864)	(18.833)	(24.251)	(24.425)
Population density	-0.74	-0.428	-1.734	-1.39	-3.075	-1.274
	(0.598)	(0.657)	(1.485)	(1.655)	(2.658)	(2.893)
Crime rate	0.161	0.12	0.365	0.321	0.816	0.583
	(0.113)	(0.121)	(0.393)	(0.415)	(0.574)	(0.607)
Log of GDP per Capita	-0.217	-0.316	-0.251	-0.361	-0.404	-0.975
	(0.252)	(0.259)	(1.201)	(1.245)	(1.226)	(1.270)
Panel B: Institutional features and school quality						
Pupil-teacher ratio	-6.596 *	-3.086	-28.437 *	-24.555	-37.68 **	-17.379
	(3.543)	(4.209)	(14.530)	(16.815)	(18.986)	(21.629)
Log of spending per pupil	-0.11	-0.15	-0.886	-0.929	-0.394	-0.623
	(0.226)	(0.217)	(1.168)	(1.137)	(1.479)	(1.437)
Share of high school graduates	-0.211 **	-0.191 **	-0.882 **	-0.86 **	-1.977 ***	-1.861 ***
	(0.094)	(0.096)	(0.348)	(0.353)	(0.484)	(0.495)
PISA 2003 Math	-	2.377 **	-	2.629	-	13.749 **
		(1.115)		(3.993)		(5.803)
Number of clusters	1685	1685	1685	1685	1685	1685
Year of birth fixed effects	YES	YES	YES	YES	YES	YES
Survey year fixed effects	YES	YES	YES	YES	YES	YES

Notes: Correlations of state characteristics and returns to years of schooling, middle degree, and high school degree only for men. Dependent variables: estimated returns to schooling. First step of estimation based on 40775 observations. Each column represents a separate linear regression. Standard errors are bootstrapped (200 replications, first and second stage jointly). ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.
Source: QaC 1979, 1985/86, 1991/92, 1998/99, 2005/06; own calculations.

Table 2.7: Robustness: qualitative differences in school systems - PISA 2000

	Dependent variable: Returns to		
	Years of schooling (1)	Middle degree (2)	High school degree (3)
Panel A: Amenities and labor demand			
Unemployment rate in percent	0.003	-0.159	-0.146
	(0.044)	(0.166)	(0.238)
Share of workers in manufacturing	-11.725 **	-47.148 ***	-43.053 *
	(5.256)	(18.125)	(25.347)
Population density	0.134	1.517	1.648
	(0.635)	(2.469)	(3.204)
Number of convictions / Inhabitants	0.119	-0.422	0.559
	(0.095)	(0.330)	(0.504)
Logarithm of GDP	-0.032	0.741	0.765
	(0.244)	(0.918)	(1.260)
Panel B: Institutional features and school quality			
Pupil-Teacher Ratio / 100	-8.095 ***	-4.306	-41.513 ***
	(2.509)	(10.857)	(14.601)
Log spending per pupil	-0.354 ***	-0.151	-2.771 ***
	(0.088)	(0.346)	(0.546)
Share of high school graduates	-0.280 *	-0.294	-2.213 *
	(0.151)	(0.772)	(1.151)
PISA 2000 reading	2.998 **	13.081 ***	13.942 *
	(1.401)	(4.640)	(7.294)
Number of clusters	1493	1493	1493
Year of birth fixed effects	YES	YES	YES
Survey year fixed effects	YES	YES	YES

Notes: Correlations of state characteristics and returns to years of schooling, middle degree, and high school degree. Dependent variables: estimated returns to schooling. First step of estimation based on 68537 observations. Each column represents a separate linear regression. State Hamburg is excluded because PISA 2000 scores are not available. Standard errors are bootstrapped (200 replications, first and second stage jointly). ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.
Source: QaC 1979, 1985/86, 1991/92, 1998/99, 2005/06; own calculations.

A Appendix

Table A.1: OLS results: returns to years of schooling, middle degree, and high school degree

	(1)			(2)		
Dependent Variable: Log hourly wage	**coef.**		**SE**	**coef.**		**SE**
Years of secondary schooling	0.099	***	0.009			
Basic degree				Reference		
Middle Degree				0.238	***	0.028
High School degree				0.432	***	0.043
Years of schooling interacted with						
Schleswig-Holstein	Reference					
Hamburg	-0.011		0.009			
Lower Saxony	-0.004		0.007			
Bremen	-0.003		0.01			
North Rhine-Westphalia	-0.013	**	0.006			
Hesse	-0.007		0.007			
Rhineland-Palatinate	-0.004		0.007			
Baden-Wurttemberg	-0.009		0.006			
Bavaria	0.003		0.006			
Saarland	-0.003		0.009			
Survey Year fixed effects						
1979	Reference					
1985/86	0.014	***	0.001			
1991/92	0.033	***	0.002			
1998/99	0.045	***	0.003			
2005	0.052	***	0.004			
Year of birth fixed effects (40)	YES					
Middle degree interacted with						
Schleswig-Holstein				Reference		
Hamburg				-0.088	***	0.026
Lower Saxony				-0.037	**	0.018
Bremen				-0.050	*	0.030
North Rhine-Westphalia				-0.027		0.017
Hesse				-0.023		0.019
Rhineland-Palatinate				-0.030		0.022
Baden-Wurttemberg				-0.037	**	0.018
Bavaria				-0.001		0.017
Saarland				-0.034		0.031
Survey Year fixed effects						
1979				Reference		
1985/86				0.013		0.009
1991/92				0.090	***	0.009
1998/99				0.082	***	0.011
2005				0.027	*	0.014
Year of birth fixed effects (40)				YES		

Continued on next page

Table A.1 - continued

High school degree interacted with						
Schleswig-Holstein				Reference		
Hamburg				-0.041		0.035
Lower Saxony				0.003		0.026
Bremen				0.002		0.038
North Rhine-Westphalia				-0.020		0.023
Hesse				0.011		0.027
Rhineland-Palatinate				0.014		0.028
Baden-Wurttemberg				-0.006		0.025
Bavaria				0.047	*	0.024
Saarland				0.003		0.036
Survey Year fixed effects						
1979				Reference		
1985/86				0.030	**	0.013
1991/92				0.181	***	0.014
1998/99				0.225	***	0.015
2005				0.218	***	0.019
Year of birth fixed effects (40)				YES		
Additional controls						
Female (0/1)	-0.215	***	0.003	-0.221	***	0.003
Age	0.427	***	0.037	0.455	***	0.037
Age^2	-1.431	***	0.157	-1.468	***	0.157
Age^3	2.202	***	0.284	2.262	***	0.285
Age^4	-1.262	***	0.187	-1.302	***	0.188
State fixed effects						
Schleswig-Holstein	Reference			Reference		
Hamburg	0.136		0.092	0.07	***	0.018
Lower Saxony	0.036		0.066	0.004		0.012
Bremen	0.024		0.097	0.011		0.02
North Rhine-Westphalia	0.173	***	0.061	0.045	***	0.011
Hesse	0.124	*	0.066	0.045	***	0.012
Rhineland-Palatinate	0.076		0.069	0.023	*	0.013
Baden-Wurttemberg	0.158	**	0.063	0.071	***	0.011
Bavaria	0.009		0.062	0.016		0.011
Saarland	0.016		0.091	-0.009		0.017
Year of birth fixed effects (40)	YES			YES		
Number of observations	70449			70449		

Note: OLS regressions of equation (2.1) with linear schooling (column 1) and degree indicators (column 2). Dependent variables is log hourly wage. Each column represents a separate linear regression. Standard errors are robust. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level.
Source: QaC 1979, 1985/86, 1991/92, 1998/99, 2005/06; own calculations.

3 The effect of education on fertility: evidence from a compulsory schooling reform[31]

3.1 Introduction

Since the 1970s total fertility rates (TFR) have fallen substantially in most developed countries. The declining birth rates and increasing percentages of childless women accelerate population aging that has become a major demographic problem in the developed world. The causes of these negative fertility trends are not fully understood because various factors such as education, income, marital status, and labor market conditions are likely to influence fertility.

Educational expansion is one of the most disputed determinants of low fertility. Earlier research has shown a significant negative correlation between women's educational attainment and fertility (see, e.g., Björklund; 2006). However, because of potential reverse causality and selection on unobservable factors, a causal link is difficult to establish. More recent studies for several developed countries demonstrate that the relationship between education and fertility disappears or becomes even positive after accounting for the endogeneity of schooling (see, e.g., Monstad et al.; 2008; Fort et al.; 2011; McCrary and Royer; 2011). So far, however, the evidence on the effect of education on fertility is inconsistent and differs across countries.[32]

[31] This study is co-authored by Kamila Cygan-Rehm. The authors gratefully acknowledge the suggestions provided by Regina T. Riphahn. We also thank Joshua Angrist, Anders Björklund, Martina Eschelbach, Barbara Hanel, Boris Hirsch, Daniel Kuehnle, Michael Zibrowius, the participants of the ESPE in Bern, the SOEP conference in Berlin, the annual meeting of the *Verein für Socialpolitik* in Göttingen, the EALE in Bonn, the brown bag seminar of the Melbourne Institute of Applied Economic and Social Research, and the IWQW Seminar at the University of Erlangen-Nuremberg for the useful comments on earlier versions of this paper. We also thank the editor of Labour Economics and two anonymous referees for their very valuable and helpful comments and suggestions.

[32] The causal studies for the U.S. and several European countries usually show that extended education leads to postponement of first births away from teenage motherhood (see, e.g., Black et al.; 2008; Monstad et al.; 2008; Silles; 2011). However, the evidence on the effect on completed fertility is mixed (see, e.g., Monstad et al.; 2008; Fort et al.; 2011; Braakmann; 2011). We give an overview of the previous research in Section 3.3.

The controversy about the empirical evidence on the education-fertility-nexus draws attention to the importance of various institutional conditions such as labor market flexibility, child care availability, and a broad range of policies that vary across countries. This paper focuses on West Germany and thus studies an institutional setting marked by high wage penalties for motherhood, an inflexible labor market, and a very limited supply of public child care (see, e.g., Gangl and Ziefle; 2009; Wrohlich; 2008). Low fertility is currently one of the major political issues in Germany and the debate about the best policies to mitigate the recent demographic changes continues. Fertility developments experienced by Germany over the past decades remain almost unprecedented. For example, the total fertility rate fell below the replacement level of 2.1 children per woman already in 1970 and has since decreased to 1.4 (World Bank; 2013). In 2009, German women gave their first birth on average at age 30. This age at first birth is one of the highest among OECD countries (OECD; 2013b). Furthermore, high numbers of childless women contribute to low birth rates (OECD; 2013b).

This paper traces the effect of extended compulsory schooling on fertility over the life cycle. We examine different outcomes: the number of children, the probability of remaining childless, and the timing of births. We use two complementary data sets, the German Mikrozensus and the German Socio-Economic Panel, to investigate fertility of West German women born between 1937 and 1961. To deal with potential omitted variable bias, we explore a reform that extended mandatory schooling from 8 to 9 years. In particular, our identification strategy takes advantage of exogenous variation in education from the staggered implementation of the reform in the West German states. We identify the causal effect of extended compulsory schooling on fertility by applying an instrumental variables approach.

We find that increased education permanently reduces fertility in Germany. More specifically, one additional year of schooling ultimately reduces the number of children by more than 0.1 and increases the probability of childlessness by about 2-5 percentage points. These results contradict prior evidence for other developed countries (see, e.g.,

Monstad et al.; 2008; Silles; 2011; Geruso et al.; 2011). Our complementary analysis of the timing of births contributes to the understanding of underlying mechanisms and cross-country differences. We confirm the findings from other countries that increased education decreases the probability of teenage motherhood (see, e.g., Black et al.; 2008; Monstad et al.; 2008). However, in contrast to women from other countries, German women do not compensate at later ages for the initial loss in births. Clearly, this lack of a catch-up effect translates to a negative effect of extended compulsory schooling on completed fertility. We argue that contradictory evidence on the effect of education on completed fertility may derive from different cultural and institutional conditions that affect women's opportunity costs over their life course. Our results pass usual specification tests and are robust to different sample restrictions.

3.2 Institutional setting

The German secondary school system is a tripartite system that sorts students at the age of 10 into three school tracks: basic school (*Hauptschule*), middle school (*Realschule*), and high school (*Gymnasium*).[33] These different tracks prepare for different careers. Basic school lasts until grades 8 or 9 and prepares for apprenticeship trainings. Middle school comprises 10 grades and prepares for apprenticeships and training in white collar jobs. A high school certificate after 12 years (*Fachhochschulreife*) gives access to technical colleges (*Fachhochschule*), and after 13 years (*Abitur*) to universities. The sorting of students into the different tracks depends on various criteria that differ by state. Assignment to a particular track strongly affects subsequent careers because students rarely change tracks (Pischke; 2007).

[33] Beyond the dominant tripartite system, there are also comprehensive schools (*Gesamtschule*) and schools that follow various concepts of progressive education (*Reformpädagogik*) in Germany. However, these alternative school types play a minor role in the public educational system. In recent years, some of the German states changed the school system to a two-track system. However, these changes do not affect the birth cohorts under study.

Figure 3.4 gives the percentages of graduates from the three tracks in the birth cohorts 1932-1965. We observe lower percentages of basic school graduates and higher percentages of middle and high school graduates in the younger cohorts. The percentages of basic school graduates in the cohorts born before 1935 accounted for over 80% and fell to less than 40% in the cohorts born after 1960. Accordingly, in the observed period the percentages of high school graduates increased from about 8% to about 23%. Figure 3.4 illustrates that educational expansion in Germany led to a sizable shift of graduates away from basic schools.

The reform that we use for our identification strategy extended compulsory schooling by one year (from 8 to 9 years) between 1946 and 1969. Petzold (1981) specifies two major goals of the reform. First, the reform aimed at improving vocational maturity, the physical and psychological development of children, and the quality of occupational choices because 14 year old pupils were considered to be too immature for the labor market. Second, the reform aimed at directing young workers away from manual to more intellectually demanding jobs. This argument was related to the high level of youth unemployment and a shortfall of apprenticeship training positions in the 1950s.

The German school system is governed at the level of the federal states. This decentralized organization lead to a staggered implementation of the 9th grade by state. Table 3.1 shows the year of implementation and the first affected birth cohort by state. While some states such as Hamburg and Schleswig-Holstein introduced the 9th grade shortly after the World War II, others such as Bavaria postponed the reform until the 1960s.[34] North Rhine-Westphalia, Hesse, Rhineland-Palatinate, and Baden Wurttemberg implemented the 9th grade simultaneously with a shift of the starting date of the school year from spring to fall (see Pischke; 2007). To accommodate the change in schedule, these states introduced two short school years that affected students in all grades at that time, thereby compressing also the duration of the additional 9th grade. Although the short

[34] The timing of the reform at the local level varied also within the states because of shortfalls of teachers and a lack of infrastructure in rural areas (Leschinsky and Roeder; 1980).

school years shortened the instructional time in the attended grade, they did not affect the curriculum (Pischke; 2007).

3.3 Literature and hypotheses

From a theoretical perspective, the predicted effect of an exogenous increase in a woman's education on her fertility is ambiguous because it depends on different substitution and income effects. Economists emphasize several causal channels by which education could affect fertility choices.

One of the most discussed channels is the labor market channel as proposed in the standard microeconomic model of fertility (see Becker; 1981). The model assumes that education increases a woman's permanent wage, but the consequent effect on fertility is unclear. On the one hand, higher earnings raise the opportunity costs of leaving the labor market to rear children. This substitution effect tilts women's optimal fertility choices towards fewer children. On the other hand, higher earnings should be positively related to fertility because families can afford more children. This income effect may be however weaker if parents with higher income prefer children of higher quality. The more parents invest in each child, the fewer children they can afford.

Previous literature emphasizes also the role of several other channels. For example, under positive assortative mating, a woman's education is causally related to her partner's education (Behrman and Rosenzweig; 2002). An exogenous increase in a woman's education affects her permanent income through a spouse-related multiplier effect (McCrary and Royer; 2011). Another causal mechanism works through the effect of education on women's knowledge about contraception or reproductive health (Grossman; 1972; Rosenzweig and Schultz; 1989). Education may also affect fertility through what has been termed a pure "incarceration effect" because enrollment in the educational system itself may be incompatible with motherhood (see, e.g., Black et al.; 2008). However, such birth postponement may be temporary and does not necessarily affect completed fertility (see, e.g., Monstad et al.; 2008).

Although extensive empirical literature documents a negative association between female education and fertility, a causal relationship is difficult to establish because of potential reverse causality and selection on unobservable factors. Some recent studies approach these problems by using exogenous variation from school entry policies (see, e.g., McCrary and Royer; 2011) or changes in compulsory schooling laws (see, e.g., Black et al.; 2008; Monstad et al.; 2008; Silles; 2011) as instruments for education. So far, however, these studies offer mixed findings as we demonstrate in Table 3.2.

Table 3.2 shows that studies for countries and population groups with higher levels of fertility find negative effects of education on fertility (see, e.g., Osili and Long (2008) for Nigeria, Lavy and Zablotsky (2011) for Arabs in Israel). Analyses using compulsory schooling reforms in the U.S. and several European countries usually suggest that increased education leads to a postponement of the first birth away from teenage motherhood (see, e.g., Black et al. (2008) for the U.S. and Norway, Silles (2011) for Great Britain and Northern Ireland). However, increased education does not necessarily affect completed fertility because women can catch up for the initial reduction in births at later ages (see, e.g., Monstad et al. (2008) for Norway, Fort (2009) for Italy, Geruso et al. (2011) for U.K.). McCrary and Royer (2011) explore school entry rules in two U.S. states (California and Texas) and do not find any causal effect of education on fertility; neither on the incidence of motherhood nor on the timing of first births. Recent contributions by Fort et al. (2011) and Braakmann (2011) show contradictory evidence from mandatory schooling reforms in Europe.[35] Their results suggest that extended education significantly increases the number of births because of increased stability of marriages. The estimated effects may differ across studies because education affects fertility through different channels and the importance of these channels may vary across countries, subpopulations, or levels of education.

[35] Closest to ours is the study by Fort et al. (2011) who examine fertility using variation from compulsory schooling reforms in eight European countries. However, this study considers only the four German states that introduced the reform simultaneously in 1967.

As for West Germany, the related literature on mothers' labor market outcomes generally leads to a conclusion that remarkable interrelations in the cultural and institutional framework have for decades hampered the compatibility of work and family life, thereby determining particularly high opportunity costs of child-rearing. For example, cross-national comparisons of fiscal regulations emphasize that the tax system providing a "housewife bonus" favors traditional "male bread-winner" families and creates strong disincentives for married women to supply labor (see, e.g., Sainsbury; 1999; Diprete et al.; 2003; Hanel and Riphahn; 2012). Prior to the legislative changes in 2007, the parental leave regulations (including several extensions of job-protected leave between the 1970s and 1990s) also explicitly aimed at encouraging mothers to stay out of the labor market longer (see, e.g., Hanel and Riphahn; 2012). While, for example, Sweden introduced major extensions in subsidized child care infrastructure between the late 1960s and 1990s (see, e.g., Björklund; 2006), West German mothers still experience particularly low coverage of public child care services (see, e.g., Wrohlich; 2008). A considerable excess demand for subsidized child care is related both to the highly regulated market for child care and to the high cost of private child care alternatives (see, e.g., Wrohlich; 2008). Prevailing social norms including a general skepticism about the quality of public child care (Hank et al.; 2004), strong social attitudes against maternal employment (see, e.g., Lee et al.; 2007), and conservative views of gender roles within a household (Diprete et al.; 2003) exacerbate the problem of low incentives to combine work and child-rearing.

Given the German institutional and cultural framework, we test the hypothesis that education reduces fertility not only temporary, but also permanently. Specifically, we expect that an extension of mandatory schooling generates a postponement of first births because education and child-rearing are difficult to combine. Furthermore, we expect that additional education increases the probability of remaining childless and reduces the number of births because of increased opportunity costs of child-rearing associated with an extra year of schooling.

Although the opportunity cost argument appears to be inconsistent with the results of Pischke and von Wachter (2008), who do not find any significant wage returns to schooling for Germany, extended education may still increase women's costs of leaving the labor market to rear children for several reasons: first, while Pischke and von Wachter (2008) do not explore potential heterogeneous effects for men and women, previous studies for Germany strongly suggest higher wage returns to education for women than for men (see, e.g., Boockmann and Steiner; 2006). Compared to women in other European countries, German women experience higher wage penalties for motherhood (Gangl and Ziefle; 2009), so that the smallest effect of extended education on wages is augmented through a multiplier effect. Second, the opportunity costs of child-rearing are not necessarily limited to losses in hourly wages. The previously discussed distinctive features of the institutional and cultural environment in Germany also justify other parallel mechanisms. For example, the highly inflexible system of child care, the tax system providing a "housewife bonus", and the strong social attitudes against maternal work may exacerbate mothers' costs that result from reduced labor supply.[36] Finally, extended schooling may have important non-pecuniary returns in the labor market (Oreopoulos and Salvanes; 2011). Because the German reform aimed primarily at improving the quality of occupational choices (Petzold; 1981), it could have had long-term effects along dimensions such as occupational prestige, subjective job satisfaction, and sense of accomplishment, thereby permanently increasing women's costs of leaving the labor market to have children.

[36] In addition to wages, Pischke and von Wachter (2008) examine potential effects of the German schooling reform on labor supply at the extensive margin. Although they again find a positive, but very small effect in the pooled sample, the "effects are smaller or negative when the sample is restricted to men". This result suggests positive effects for women and supports the argument that the reform differently affected men and women.

3.4 Identification strategy

Our identification strategy considers the following equations:

$$y_i = \alpha\, educ_i + state_i'\theta + cohort_i'\xi + statetrend_i'\mu + z_i'\pi + \epsilon_i \tag{3.1}$$

$$educ_i = \phi\, reform_i + state_i'\beta + cohort_i'\gamma + statetrend_i'\delta + z_i'\lambda + \nu_i \tag{3.2}$$

where the dependent variable y_i in (3.1) represents different measures for woman *i*'s fertility outcomes. We consider three fertility outcomes: the number of children ever born, the probability of remaining childless, and the age-specific probability of births. These outcomes are a function of years of education ($educ_i$), state fixed effects ($state_i$), cohort fixed effects ($cohort_i$), state-specific cohort trends ($statetrend_i$), and socio-demographic background variables z_i such as marital status and community size.[37] α, θ, ξ, μ, π, ϕ, β,γ, δ, λ represent coefficients to be estimated, ϵ_i and ν_i are random error terms.

Estimating equation (3.1) by ordinary least squares (OLS) is likely to yield biased estimates of α because the error term ϵ_i may contain unobserved characteristics such as family preferences or childhood experiences. These characteristics are correlated with both education and fertility. However, the direction of the bias in α from an OLS estimation is not clear.

We exploit exogenous changes in education from the schooling reform described in Section 3.2 to identify the causal effect of extended compulsory schooling on fertility outcomes. In particular, we exploit the cross-regional and cross-time variation in education from a staggered implementation in the German states between 1958 and 1969.[38] In a two stage least squares approach we first estimate equation (3.2) that gives years of education of individual *i* ($educ_i$) as a function of an indicator for the reform status ($reform_i$). The reform indicator equals one if a woman was affected by the reform and zero otherwise.

[37] If the data allows, we also control for women's family background, for example, grandmother's age at a woman's birth. We use the state-specific cohort trends both in linear and squared forms.

[38] Because of data limitations, we exclude Hamburg and Schleswig-Holstein from the analysis. Section 3.5 gives more details on our data.

In the second step we estimate equation (3.1). The two equations use exactly the same control variables.

Our identification strategy fails if the timing of the reform is correlated with fertility in the federal states. To mitigate this concern we include linear and squared state-specific cohort trends that should capture any smooth trends in fertility and schooling at the state level. We also include state fixed effects to control for state-specific differences such as religious affiliation or social norms, which may have also influenced the timing of the reform. Given that our estimation strategy relies on a long period, we use cohort fixed effects to account for any changes that took place over time such as introduction of oral contraceptives.[39]

Our identification strategy also fails if other fertility-relevant changes took place and affected the same birth cohorts as the schooling reform did. However, such changes are unlikely because the overall responsibility for family policies lies with the federal government, not with the states. Any important family policy reform in the last 60 years such as the introduction of child benefits (*Kindergeld*) affected women in all states simultaneously.[40]

Another validity threat for our identification strategy is selective regional mobility that may challenge the credibility of our inferences for two reasons: the first threat is that parents of school children would have moved to another state in response to the reform either to avoid the upcoming introduction of the 9th grade in their state of residence or to use the earlier introduction in another state. The second threat is that the reform could have had follow-up effects on mobility, which Machin et al. (2012) show for the extension of compulsory schooling in Norway. Given that our data sets provide little retrospective

[39] We argue that our results are not driven by the set of states and the consequent set of cohorts. Sensitivity tests show that our findings qualitatively do not change if we exclude single states and therefore change the period of analysis. Detailed results are available upon request.

[40] Similarly, the health care system is governed at the federal level, so the timing of the introduction of the contraceptive pill was not state-specific. This argument applies also to the reimbursement of oral contraceptives by health insurances. Some health insurances operate regionally (e.g., company health insurance funds), but the timing of first reimbursements did unlikely vary across health insurances. Nevertheless, we tested whether our results are driven by particular states by excluding single states from the analysis and the results remained qualitatively unchanged. Detailed results are available upon request.

information on a woman's geographic location, our instrument is based on current state of residence and may be therefore partly an outcome of the reform. The data limitation does not allow us to test neither the potential post-reform effects on mobility during adulthood nor the anticipatory behavior during childhood.

However, we exploited official statistics on mobility between the West German states starting from the 1950s and the time series provide compelling evidence that regional mobility should not yield any major consequences for our estimates.[41] First, we do not find any systematic patterns around the introduction of the reform in the single states. Second, the level of regional mobility in West Germany is relatively low and in line with evidence by Machin et al. (2012) who document an annual mobility rate in Germany of 1.32% that is two times lower than the rates in Norway and the U.S.. Finally, while Machin et al. (2012) find clear positive effects of increased education on regional labor mobility in Norway, the relationship for West Germany, if any, is negative because the between states mobility has slightly declined over the recent six decades.

3.5 Data

We use data from two complementary German surveys to examine the relationship between a woman's education and her fertility outcomes such as number of children, childlessness, and the age-specific probability of births.

Our primary data source is the German Mikrozensus (MZ). The MZ is an annual survey of a 1% random sample of German households.[42] We use the 2008 survey, which is the first survey providing information on the number of children ever born to female respondents.[43] The key advantage of the MZ is its large sample size and low unit and item

[41] We calculated annual in- and out-migration ratios for each state by relating the absolute number of in- or out- movements to population size in the previous year. The absolute numbers on between-states movements and population sizes were provided on special request by the German Federal Statistical Office. Detailed results are available upon request

[42] The scientific use file is a 70% sample of the entire data set.

[43] Previous waves report only the number of children living in the household at the time of interview.

non-response rates. However, the survey suffers from the lack of information on children's birth year, the state where an individual went to school, and parental background.

Our second data source is the German Socio-Economic Panel (SOEP). The SOEP is a longitudinal survey of private households conducted annually since 1984 (Wagner et al.; 2007). The data set overcomes three important shortcomings of the MZ. First, the SOEP contains retrospective biographical information on childbearing, thereby permitting our analysis of the timing of births over the life cycle. Second, the SOEP provides family background variables that may affect both education and fertility (e.g., the number of siblings, parental educational attainment, and parents' birth year). Finally, the SOEP provides retrospective information on individuals' educational careers and the state where they went to school. The main shortcoming of the SOEP is its relatively small sample size. To obtain a sufficient number of women for the treatment and control groups, we use observations from the survey years 1984-2010. Here we consider only the first interview that a woman gave after she has turned 40 years old.

We impose similar sample restrictions on both the MZ and SOEP data. Table 3.3 demonstrates the details of our sample selection procedure. We select native German women[44] from eight out of ten (excluding Berlin) West German states. We exclude Schleswig-Holstein and Hamburg from the analysis because the first birth cohorts affected by the reform were 1932 and 1931, respectively, and the MZ 2008 reports fertility only for women born after 1933.[45]

We extract those observations from the MZ who were born up to five years before/after the first birth cohort affected by the reform as illustrated in Figure 3.5. An alternative approach is to include all individuals born between 1938 and 1959, which translates to using an asymmetric 22-year window (see, e.g., Pischke and von Wachter; 2008). We follow Monstad et al. (2008) and Brunello et al. (2009) who argue that a symmetric window guarantees similar sample sizes and characteristics of treatment and control group in each

[44] We omit first and second-generation immigrants.

[45] In addition, the information on the dates of reform implementation in Schleswig-Holstein varies across different sources (compare Leschinsky and Roeder; 1980; Pischke and von Wachter; 2008).

state. Brunello et al. (2009) also argues that the symmetric approach reduces the influences of potentially unaccounted confounding factors, thereby estimating the reform effect on a more local level. Paraphrasing this argument, a sufficiently small window around the first affected birth cohort weakens the effects of any (earlier and later) reforms and changes that may not be captured by our model specification and therefore contaminate our estimates. Because the small sample size of the SOEP does not allow us to use a 5-year time window around the first birth cohort affected by the reform, we use a 7-year window instead.[46]

Ideally, we would need direct information on the state where a woman went to school. However, the geographic location of the attended school is available only for a 50% subsample in the SOEP. For the remaining observations we use the current state of residence as a proxy. Importantly, the limited geographical information leads to an attenuation bias in the reduced form if any pre- or post-reform regional mobility was uncorrelated with the reform.[47] We exclude women who graduated from a school in socialist East Germany.[48]

Neither of the two data sets reports the exact number of completed years of schooling. Instead, we observe a woman's highest secondary school degree, post-secondary education, and training. We construct years of schooling by assigning the usual number of years taken for a particular educational route (see, e.g., Pischke and von Wachter; 2008). We proceed as follows: for the basic track graduates, we use the state, the year of birth, and information on the timing of the reform from Table 3.1 to determine whether an individual should have graduated after 8 or 9 years in the basic track. For the two higher tracks, we

[46] A more sophisticated identification strategy would draw on month of birth to distinguish between women born before and after the exact cut-off date for school enrollment. Unfortunately, we do not have the information on month of birth in the MZ and the variable is missing for one-fourth of our SOEP sample. Note that using month of birth would not guarantee a correct classification because some women may have started school earlier or later than officially scheduled and some may have skipped or repeated grades.

[47] We tested this argument by using the 50% subsample of women for whom the SOEP reports the state of school attendance. If we instead use their current state of residence, then the estimated effects on fertility are slightly lower, thereby supporting the attenuation bias argument. Detailed results are available upon request.

[48] The MZ allows us to identify women with specific school degrees that could have been obtained only in the former East German states (GDR). In the 50% subsample of the SOEP we can identify women who attended school in the GDR.

use the standard duration of a particular track. Finally, for all individuals, we incorporate the information on post-secondary education and training, thereby calculating a measure of total number of years of education (Pischke and von Wachter; 2008).[49] We conclude our sample selection by omitting observations with less than 7 years of education[50] and with missing values on education or fertility variables.

Our final MZ sample contains 17,428 women born 1938-1959; the SOEP sample 2,649 women born 1937-1961. Table 3.4 shows summary statistics for both data sources. The table splits women into those who were affected by the reform and those who were not.

In general, the MZ and SOEP samples show similar patterns for educational attainment and our control variables. However, fertility outcomes differ between the data sets. While in the MZ women who were subject to the reform have on average fewer children and are more likely to remain childless, we observe no fertility differences between the treatment and control groups in the SOEP. Nevertheless, women affected by the reform were more likely to delay their first birth. Fertility variation by reform status may reflect differences in birth cohort and educational attainment. Our sample selection rules determine that women affected by the reform were born later. As expected, they also completed more years of education than women not affected by the reform. In both groups the number of years of education exceeds the mandated 8 or 9 years of schooling because notable percentages of women obtained an additional degree or training.

Figure 3.6 and Figure 3.7 plot the raw data on fertility outcomes by reform status. Figure 3.6 reveals the overall trends of declining fertility and increasing incidence of childlessness, but the differences between women affected and not affected by the reform are small. Figure 3.7 shows that women who were subject to the reform postponed their first birth beyond the early 20s and were more likely to have their first child at older ages.

[49] In Section 3.7 we show that our results are robust to changing the definition of the education variable. For example, we calculate the actual time that a woman remained in the educational system by using information on her graduation year.

[50] This is inconsistent with 8 years (or 9 after the reform) of compulsory schooling. However, inclusion of this small subsample into the analysis does not affect our main results.

Furthermore, they had fewer children up to the age of 36, but then fully caught up, so that the completed fertility at the end of the fertile years is nearly identical in both groups.

3.6 Results

3.6.1 The effect of the reform on education

We first graphically explore whether the formal introduction of the compulsory 9th grade affected education (first stage) and fertility (reduced form). Figure 3.8 plots the average number of years of education for the five birth cohorts before and after the first affected cohort in the MZ.[51] The graph reveals a general increase in education for younger cohorts and a discrete jump of roughly 0.85 years for the first birth cohort affected by the reform. Figure 3.9 illustrates the reduced-form analysis. We plot the average number of children for the five birth cohorts before and after the reform. The adjusted number of children is the residual from a regression of the number of children on the full set of our control variables.[52] The reduced-form graph signals a negative jump in fertility for the first five affected birth cohorts in the different states.

We next estimate equation 3.2 using the MZ sample and show the first-stage estimation results in Table 3.5. Columns 1 and 2 report the results obtained on the full sample and columns 3 and 4 on the subsample of basic track graduates. Additionally, columns 5 and 6 give the effect of the reform on track choice. We show the coefficients obtained from two specifications: one comprises only linear state-specific trends in birth cohort, the other one also includes squared trends. In addition, all regressions include state of residence fixed effects, year of birth fixed effects, and indicators for marital status and municipality

[51] Note that we constructed the education variable by combining the information on a woman's highest completed degree and the total number of years usually taken for this degree. We show the robustness of our results to an alternative measure for education in Section 3.7.

[52] These control variables are state fixed effects, year of birth fixed effects, linear and quadratic state-specific trends in year of birth, marital status, and community size.

size.[53] We estimate standard errors clustered at the state-year of birth level (80 clusters) throughout to deal with potential serial correlation and heteroscedasticity.[54]

The first-stage estimation results in Table 3.5 confirm the graphic impression: the coefficient of the reform indicator obtained on the full sample is significant across all specifications and indicates that the reform increased women's education on average by about 0.65-0.75 years (columns 1 and 2). By way of construction the magnitude of the coefficients obtained from the subsample of basic track graduates is close to one (columns 3 and 4). Because our measure of education comprises years of compulsory schooling and further vocational training, the excess one suggests that the reform could have had some small positive spillover effects on the further vocational path. However, we performed additional tests and their results show that the coefficient is not significantly different from one.

The magnitudes of our first-stage estimates are larger than those reported by other studies that exploit the German school reform (see, e.g., Pischke and von Wachter; 2008; Kemptner et al.; 2011). We argue that these differences result mainly from different sample restrictions. For example, Pischke and von Wachter (2008) report a first-stage estimate of 0.19, but the authors use a different dataset, pool males and females, investigate only salaried workers in the age group 15 to 65, and calculate a different education variable.[55] In an earlier discussion paper, Pischke and von Wachter report a first-stage coefficient from the German Mikrozensus of 0.569. Using our data and model specification, we re-estimated the first stage in a pooled sample of men and women and also separately by

[53] Fort et al. (2011) show that schooling reforms may affect not only a woman's education but also her marriage outcomes. Because such effects would create an identification problem, we explored this issue by testing whether the reform indicator is correlated with a woman's marital status and her partner's education. We find that in Germany the effect of extended compulsory schooling on fertility does not work through the marriage market (see Section 3.6.4). Nevertheless, our main results hold if we drop the control variables from the equation.

[54] Following Angrist and Pischke (2009), we report conservative inference results. We obtain smaller standard errors from alternative methods such as clustering by state or the two-way clustering suggested in Cameron and Miller (2011).

[55] Pischke and von Wachter (2008) calculate years of education by using the information on birth and graduation year. We demonstrate in Section 3.7 that our first-stage coefficients diminish if we use this alternative education variable.

gender. Not only could we nearly replicate the coefficient reported by the previous version of Pischke and von Wachter (2008), but we also found that the schooling reform affected women more strongly than men.[56]

Several sources of measurement error can contribute to an attenuation of the reform indicator coefficient: first, the implementation of the additional grade may not have been immediate and in practice required several years until each school offered the mandatory 9th grade (Leschinsky and Roeder; 1980). Second, because the direct information on the state of school attendance is unavailable, potential random migration may bias our estimates towards zero. Furthermore, year of birth does not perfectly determine whether the reform affected an individual because some women may have started school earlier or later than officially scheduled. However, we draw on previous studies (see, e.g., Pischke and von Wachter; 2008; Kemptner et al.; 2011) and argue that measurement error in the instrument does not bias subsequent estimates unless it is systematically correlated with both the reform introduction and fertility outcomes, which is rather unlikely. The F-statistics of significance tests of the reform dummy in the first stage are greater than ten across specifications and confirm the strength of the instrument (Staiger and Stock; 1997).

Our estimation strategy identifies the *local average treatment effect* (LATE) of extended compulsory education on fertility (see, e.g., Angrist and Pischke; 2009). Compliers here are all women whose education was causally affected by the extension of compulsory schooling, i.e., women who did not leave school after 8 years because of the reform, regardless of the attended track. This group is not necessarily representative for the overall population.

An important assumption for an unbiased estimate of the LATE is *monotonicity*, i.e., all individuals respond to the reform implementation in the same way (Angrist and Pischke; 2009). Therefore, we assume that all women affected by the schooling reform extended their education and that no woman reduced education (e.g., by attending basic track instead of middle track or dropping-out). To test whether the reform affected track

[56] Our estimations yield a first-stage coefficient of 0.647 for women and 0.391 for men. Detailed replication results are available upon request.

choice, we estimate the effect of the reform implementation on the probability of graduating from the basic track (columns 5 and 6 in Table 3.5). The coefficients are very small, negative and insignificant. Therefore, consistently with previous evidence (see, e.g., Pischke and von Wachter; 2008; Kemptner et al.; 2011), we do not find that the reform affected track choice.[57]

3.6.2 The effect of education on completed fertility

Table 3.6 reports our key results on the effect of extended compulsory schooling on the number of children and the probability of remaining childless estimated separately for the full sample (columns 1 and 2) and the subsample of basic track graduates (columns 3 and 4).

The OLS results indicate that an additional year of education is associated with a lower number of children (panel A, first row) and a higher probability of remaining childless (panel B, first row). The magnitude of the coefficients is comparable to results of other studies (see, e.g., Monstad et al.; 2008; Fort et al.; 2011). The OLS estimates are statistically significant at the 1% level across specifications, samples, and fertility outcomes. However, the OLS regressions do not account for possible selection into education and may therefore yield biased results. The direction of the bias is not clear. Because we are interested in a causal relationship, we turn to the instrumental variables (IV) estimates.

The signs of the coefficients obtained by the IV approach are similar to those from the OLS regressions: education reduces the number of children (panel A, second row) and increases the probability of never having children (panel B, second row).[58] However, comparisons of the magnitudes of coefficients obtained from the two estimation methods are not very informative. While OLS estimates the partial correlation between years of education and fertility in the samples, IV estimates a LATE effect of extended compulsory

[57] Kemptner et al. (2011) conclude that changes in the composition of students within tracks should be very small if the reform did not affect the track choice.

[58] We obtain qualitatively similar results from the SOEP. The signs of coefficients obtained using a 5-year window remain the same, but the estimates are imprecise because of small sample size. To prevent inconsistency from a small sample size, we select a 7-year window sample and obtain results similar to those from the MZ. Detailed results are available upon request.

schooling on fertility exclusively for women affected by the reform. Given that the reform aimed at students in 8th grade, primary basic track students and potentially some selected groups in higher tracks, our IV estimates do not necessarily reflect the effect of extended education at other - higher or lower - stages of education.

Our main results are robust to changes in specification of state-specific cohort trends. However, we argue that squared trends better mitigate the concern that the introduction of the reform could be correlated with trends in education and fertility. The coefficient obtained from our preferred specification (panel A, column 2) suggests that, on the margin, an additional year of compulsory education increases the probability of remaining childless by 5.1 percentage points.

However, the effect of extended compulsory schooling on childlessness is smaller and not statistically significant in the basic track subsample (panel A, column 4).[59] The magnitude of the effect on the number of children also varies between the full sample and the basic track subsample (panel B, columns 2 and 4). However, the coefficient is negative and significant throughout and demonstrates that an additional year of compulsory education reduces fertility by at least 0.1 children.

3.6.3 The effect of education on fertility over the life cycle

Table 3.7 gives the results of the analysis for the timing of first births. Using the SOEP, we estimate the effect of extended compulsory schooling on the probability of giving the first birth at a given age, conditional on not already having a child. For example, women who gave their first birth between age 15 and 20 are omitted in the estimation for the age group 21-25, so that the number of observations falls from 2,649 to 2,219. All regressions

[59] The differences between the full sample and the basic track subsample may be a result of different channels (e.g., health related knowledge, assortative mating, or opportunity costs) through which the reform affected fertility in different tracks. We would expect to observe a more pronounced effect in the basic track subsample because those women were directly affected by the reform. However, women attending middle and academic track may have been also affected and comply if , e.g, the reform led them to decide against a drop-out. Monstad et al. (2008) argue that a schooling reform might lead women to choose a different "life track" than they would have chosen without the reform. Furthermore, Lang and Kropp (1986) show that under the signaling and screening hypothesis compulsory attendance laws affect all educational groups. Nevertheless, because of noisy IV estimates, the differences in the effects between the basic track and the full sample are not significant.

control for state of residence, year of birth, linear and quadratic state-specific trends in year of birth, marital status, the grandmother's age at a woman's birth, and municipality size.

The OLS estimations suggest that an increase in education is related to a reduced probability of first birth below the age of 30 and increased probability of first birth at the end of women's fertile period. However, the IV results yield different conclusions. The point coefficients suggest a positive effect of extended compulsory schooling on the probability of first birth only for the age group 26-30, but the coefficient is statistically insignificant. For the remaining age groups the coefficients translate to negative effects of increased compulsory education. As for women aged 15-20, the coefficient is significant at the 5% level and of a large magnitude: each additional year of education reduces the probability of first motherhood as a teenager by 5.7 percentage points. This is a considerable impact relative to the incidence of teenage childbearing in the full sample of 16%. The first-stage F-statistic of 16.83 indicates that a weak instrument is not a concern (Staiger and Stock; 1997).[60]

Furthermore, we find a large and significant effect of extended compulsory schooling on the probability of the first birth at ages 31-35. However, this effect is implausibly large in magnitude and we need to interpret it with caution. Conditioning on not already having a child leads to increasingly selective samples with increasing age. The large coefficient may therefore be a result of specific sample variability. Given that the coefficient is significant at the 10% level and the F-Statistic is relatively low, we are reluctant to assign a specific interpretation to the magnitude of this estimate, but some meaning can be found in the sign. This negative effect at ages 31-35 is accompanied by a zero effect for the subsequent age group and suggests that women in their early 30s are more likely to remain childless than to bear children later in life.

[60] The first-stage coefficients in Table 3.7 are generally higher compared to those from the MZ in Table 3.5. A potential explanation is that the SOEP results are less affected by the attenuation bias from regional mobility that we discuss in footnote 16. Nevertheless, we performed additional tests and their results show that all first-stage coefficients in Table 3.7 are not significantly different from one.

For completeness, we incorporate the information on the timing of subsequent births and estimate the effect of extended compulsory schooling on cumulative fertility at a given age. This cumulative fertility is the number of children born up to a specific age and we run separate IV regressions for each one-year age interval for ages 15 through 45. All of these 31 regressions control for state of residence, year of birth, linear and quadratic state-specific trends in year of birth, marital status, grandmother's age at a woman's birth and municipality size. Figure 3.10 summarizes the results obtained from these regressions.

We plot the coefficients on education and 90% confidence intervals around these point estimates by age. These IV estimates give the effect of extended compulsory schooling on the number of children born up to a given age. Figure 3.10 reveals more heterogeneity in the effect of education on teenage childbearing than Table 3.7. Specifically, for age 18 we observe a positive coefficient that could indicate a catch-up effect after graduation from school. Women may potentially compensate for the earlier loss in births from the "incarceration" by extended schooling, but the effect at age 18 is statistically indistinguishable from zero. Figure 3.10 generally confirms fertility reduction in teenage years. The remaining coefficients indicate a negative effect of extended compulsory schooling on cumulative fertility throughout. Therefore, the effect of education on completed fertility over the life cycle is negative. In contrast to the findings for the U.K. by Geruso et al. (2011) who show an upward trend that implies catch-up effects in births, our point estimates follow a downward trend. Therefore, in Germany, fertility losses from increased education become more severe with increasing age. Although for several age-years after the teenage years the increasing point estimates indicate some catch-up effects, for age 33 and later the effect of extended compulsory schooling on cumulative fertility is clearly negative, thereby rejecting a complete catch-up. The effect at age 45 is identical with the effect on the total number of children from Section 3.6.2 and confirms that additional education leads to a permanent reduction in completed fertility.

3.6.4 Potential mechanisms

Overall, our results suggest that the impact of extended compulsory education is to lower the number of children and to raise the probability of childlessness. Our findings for Germany contradict the evidence for the U.S. and several West European countries (see, e.g., Black et al.; 2008; Silles; 2011; Monstad et al.; 2008). These studies usually find that more education leads to a delay of the first birth beyond teenage years and to a later compensation for the initial loss in births. So far, however, there is no evidence that education causally reduces completed fertility in a developed country. While birth postponement away from early motherhood may reflect a pure "incarceration" effect, reduced childbearing over the entire life-cycle and lower completed fertility must work through other channels.

The conventional explanation for a negative effect of education on fertility is higher opportunity costs of leaving the labor market to rear children (Becker; 1981). Although this argument appears to be inconsistent with the results of Pischke and von Wachter (2008), who do not find any significant wage returns from the German schooling reform, opportunity costs of child-rearing are not necessarily limited to foregone earnings. Because the main intention of the German reform was to improve students' vocational maturity (Petzold; 1981), the additional year of schooling could have led women to make better occupational choices, thereby creating long-term effects along multiple dimensions such as subjective job satisfaction, occupational prestige, and sense of accomplishment. Oreopoulos and Salvanes (2011) argue that such non-pecuniary benefits from schooling may well be quantitatively even more important than wage returns. Distinctive features of the institutional and cultural environment in Germany such as the highly inflexible system of child care, the tax system with its "housewife bonus", and the strong social attitudes against working mothers may exacerbate any effects of extended schooling on women's opportunity costs of leaving the labor market through a multiplier effect.

Our data sets allow us to test selected mechanisms consistent with the opportunity cost explanation. Table 3.8 shows the results of regressions of the reform indicator on

several characteristics of the very last job such as indicators of whether it was a self-employment, white collar, or blue collar job (columns 1 through 4).[61] We also study the Treiman Standard International Occupation Prestige Score (Ganzeboom and Treiman; 1996) that we merged to the MZ by using the International Standard Classification of Occupations (ISCO-88). Table 3.8 suggests that women affected by the reform were more likely to choose self-employment as opposed to employment in both white and blue collar occupations. Furthermore, women with extended compulsory schooling ended up in more prestigious jobs. Overall, the estimations suggest that the German reform affected women's occupational preferences that, even in the absence of meaningful wage returns, may lead to increased opportunity costs of leaving the labor market to rear children.[62]

However, the opportunity cost argument neither exhaustively nor exclusively explains the different results across countries because education may affect fertility also through channels outside the labor market. For example, Fort et al. (2011) argue that compulsory schooling may positively affect fertility through better decisions in the mating market that result in a more stable marriage or higher quality of a partner. Note that positive effects on fertility occur only if the mating effects are sufficiently large to cancel out or dominate any opportunity cost effects. To explore whether the German reform affected marriage behavior, we regress the reform indicator on the indicator of whether she is married and on her partner's years of education. Table 3.8 shows the results of these reduced-form regressions (columns 5 and 6). Because the estimated coefficients are small and statistically insignificant throughout, we conclude that in contrast to other countries (see, e.g., Fort et al.; 2011; Lavy and Zablotsky; 2011), in Germany the effect of education on fertility does not work through the mating market. Education may also affect fertility through the health channel (Grossman; 1972; Rosenzweig and Schultz; 1989). We cannot test whether

[61] Because we focus on completed fertility, many of the sampled women are no longer in the labor force. The MZ reports the job characteristics only for the current or for the very last job. Because there is no further information on labor market history, we cannot study effects of the reform on wages and labor supply. We were only able to construct an indicator of whether a woman has ever held down a paid job, but the incidence in the sample is 98% and does not differ by reform status. Although the SOEP contains detailed labor market histories, the relevant variables are plagued by missing values.

[62] Note that the evidence is more illustrative than causal because the reform reduced fertility and fertility decisions may affect women's labor market choices.

the German reform lead to better knowledge about contraception or reproductive health, but Kemptner et al. (2011) do not find any significant effects of the reform on women's health habits. We therefore conclude that the high persistence of the negative effect of compulsory schooling beyond the teenage ages also traces back to a lack of any offsetting effects (e.g., on health, partnership) or institutions that could facilitate a later catch-up.

3.7 Robustness

In this section we test whether our main results are robust to changes in the definition of the education variable and in sample selection criteria.[63]

Our baseline results use the imputed years of education as a measure for women's educational attainment. We use the typical duration of attaining a specific degree and add post-secondary years of education. This variable has some disadvantages. For example, it does not take into account repeated or skipped classes and the short school years that the states of North Rhine-Westphalia, Hesse, Rhineland-Palatinate, and Baden Wurttemberg performed simultaneously to the extension of compulsory schooling. The short school years affected students in all grades at that time, by shortening their time spent in the attended grade by 26 weeks without changing the curriculum (Pischke; 2007). Pischke (2007) finds that students who were in primary school (grades 1 - 4) during the short school years were more likely to repeat grades and less likely to attend higher secondary school tracks.

To check the robustness of our results we define an alternative variable - "length of education". The MZ asks respondents about the year of obtaining their highest completed degree, including any post-secondary degrees if applicable. We use this self-reported information about the graduation year and the assumption that all women entered primary

[63] We focus here on the results for completed fertility obtained from the MZ, but we applied similar robustness checks to the effects over life cycle, which we estimate by using the SOEP. Although the SOEP sample sizes are generally much smaller, each of these exercises yielded qualitatively similar results. Detailed results are available upon request.

school at age 6 to construct an alternative measure of education. The "length of education" is given by the actual graduation year minus year of birth minus school entry age 6 (graduation year - year of birth - 6) and measures calendar years, not school years.[64] This alternative variable captures skipped and repeated classes and the short school years.[65] However, the "length of education" introduces noise because, for example, some women start school at age 7. Such noise may bias our first-stage results towards zero and lead therefore to higher IV estimates.

Table 3.9 gives the IV results for the effect of compulsory schooling on fertility obtained by using the variable "length of education". Overall, our main results qualitatively do not change, but there are some differences. For example, the first-stage coefficients are below one across all specifications. This result may be driven by measurement error in the alternative education variable, the short school years, or measurement error in the timing of the reform. Furthermore, the values of the first-stage F-statistics are lower than those shown in Table 3.6. Finally, in the full sample (columns 1 and 2) the significance of the results varies with the specification of the cohort trends, but the signs of the coefficients remain unchanged.

To test whether our results depend on our sample selection criteria, we replicate the results by using alternative selection of the analyzed birth cohorts. Our main analysis is based on a 5-year symmetric window around the first cohort affected by the reform, but we repeated the regressions by using a 4 through 7-year window. We also estimated the effects by using a different approach that includes all individuals born between 1938 and 1959, i.e., an asymmetric 22-year window. We found that the choice of the window size

[64] The major problem with this variable is potential measurement error in the self reported graduation year. Our sample size reduces to 12,657 observations if we use only reliable information. We classify information as reliable if the "length of schooling" was not more than up to two years longer or shorter than the standard duration of a certain degree. The first-stage results are slightly affected by our definition of valid information. The wider the window the lower the first-stage coefficient and vice versa. The direction of the effects of education on the number of children and childlessness are robust. More detailed results are available upon request.

[65] An alternative way of testing whether the confounding effects of short school years influence our main results is by exclusion of the four affected states from the estimation sample. Although the sample size reduces to one-third of observations and the estimates are very imprecise, the main findings remain qualitatively unchanged. Detailed results are available upon request.

may affect the magnitude and significance of the coefficients, but their direction remained unchanged throughout. These regressions revealed also a consistent pattern of decreasing effects with increasing window and sample size, thereby underpinning the argument that a symmetric and sufficiently small window around the first affected birth cohort allows to identify the reform effect on a more local level (Brunello et al.; 2009). Detailed results are available upon request.

3.8 Conclusions

We examine the effect of women's education on fertility and track this effect over the life cycle. We exploit a German schooling reform that extended compulsory education from 8 to 9 years, to identify the causal effect of one additional year of compulsory schooling on the number of children, the probability of ultimate childlessness, and the timing of births. Our empirical approach takes advantage of the exogenous variation in the reform's implementation across federal states and time.

Consistent with causal evidence for other developed countries (see, e.g., Monstad et al.; 2008; Silles; 2011), we show that extended compulsory schooling leads to a birth postponement away from early motherhood. However, in contrast to previous studies, we also find a significantly lower fertility later in life (see, e.g., Monstad et al.; 2008; Fort et al.; 2011). In particular, the additional year of compulsory education raised the probability of childlessness by at least 2 percentage points. This effect is considerable compared to the average incidence of childlessness of almost 20% in the analyzed cohorts of women. The effect of extended compulsory schooling on the total number of children is also pronounced because the additional year of education reduced the number of births per woman by more than 0.1 children. This decrease accounts for about 6% of average cohort-specific fertility. We show that our main findings are robust to changes in sample selection criteria and alternative definition of the education variable.

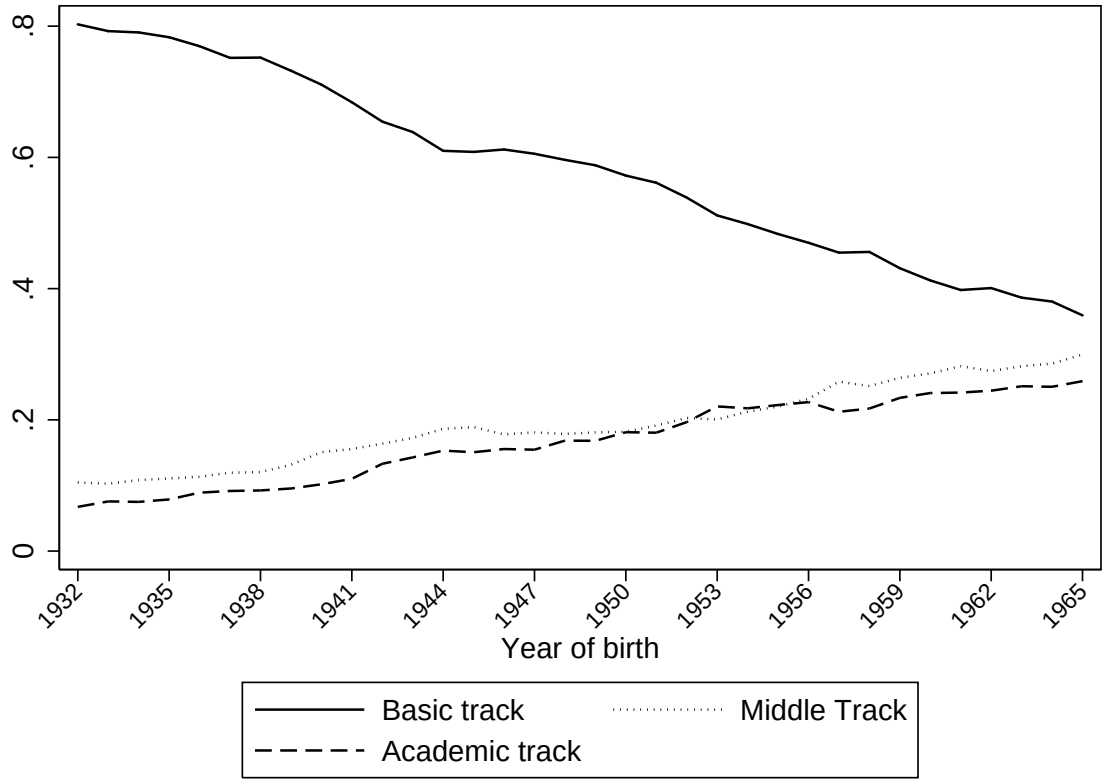
Our findings sharply contrast with the recent evidence for other European countries by Fort et al. (2011) and Braakmann (2011) who conclude that extended compulsory

education significantly increases the number of births. We interpret our findings mainly as a result of particularly high opportunity costs of child-rearing in Germany. Although this argument may appear inconsistent with the findings of Pischke and von Wachter (2008), who determine no wage returns from the same reform, we demonstrate that the reform could have lead to important non-pecuniary returns in the labor market. Specifically, we find that affected women were more likely to choose self-employment and ended up in more prestigious jobs. Oreopoulos and Salvanes (2011) argue that such non-pecuniary benefits may well be quantitatively even more important than wage returns. Furthermore, in contrast to findings from other countries (Fort et al.; 2011; Lavy and Zablotsky; 2011), our results suggest that the German reform had no offsetting effects in the mating market that would facilitate a fertility catch-up later in life.

Several distinctive features of the institutional and cultural environment in Germany may exacerbate any effects of extended schooling on women's opportunity costs of leaving the labor market to rear children. Most striking is the highly inflexible system of child care that translates to a considerable excess demand for subsidized child care (see, e.g., Wrohlich; 2008). Furthermore, extensive parental leave regulations and the tax system with its "housewife bonus" favor traditional family types and reinforce attitudes in favor of mothers' non-involvement in the labor force (Hanel and Riphahn; 2012). Our results are also consistent with previous evidence suggesting that German mothers experience the highest wage penalty for motherhood in the Western world (Gangl and Ziefle; 2009). Gangl and Ziefle (2009) find that the high permanent wage losses appear to be related to statistical discrimination against mothers in the German labor market. We conclude that remarkable interrelations between cultural and institutional conditions may hamper the compatibility of work and family life, thereby determining particularly high opportunity costs of child-rearing.

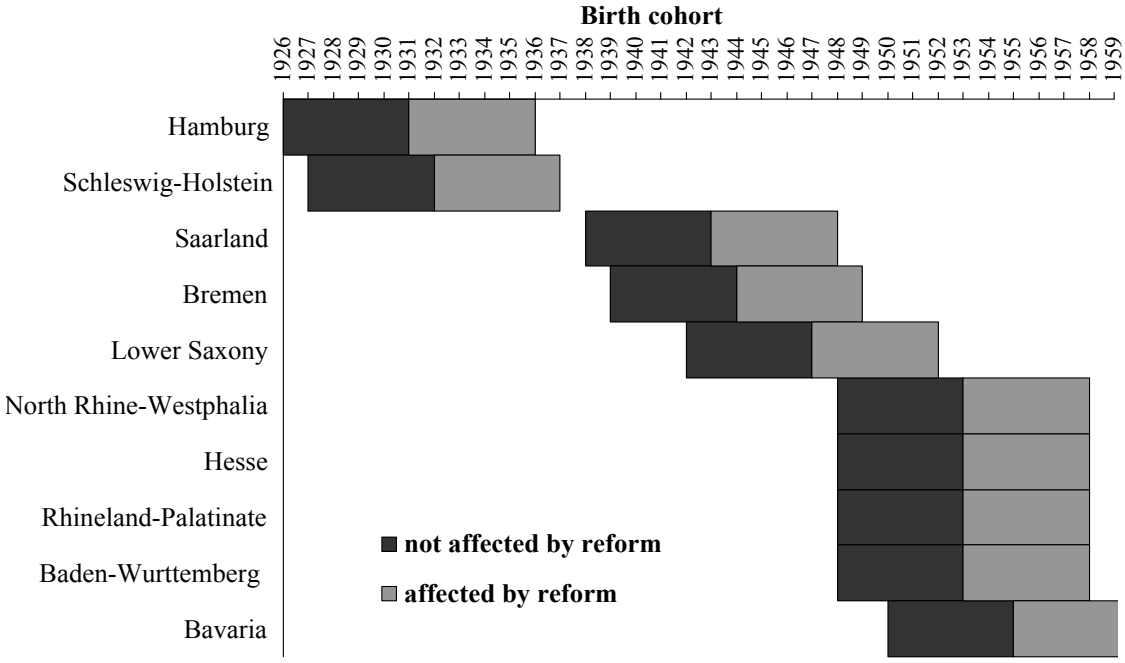
3.9 Figures and tables

Figure 3.4: Percentage of graduates by school degree and birth year



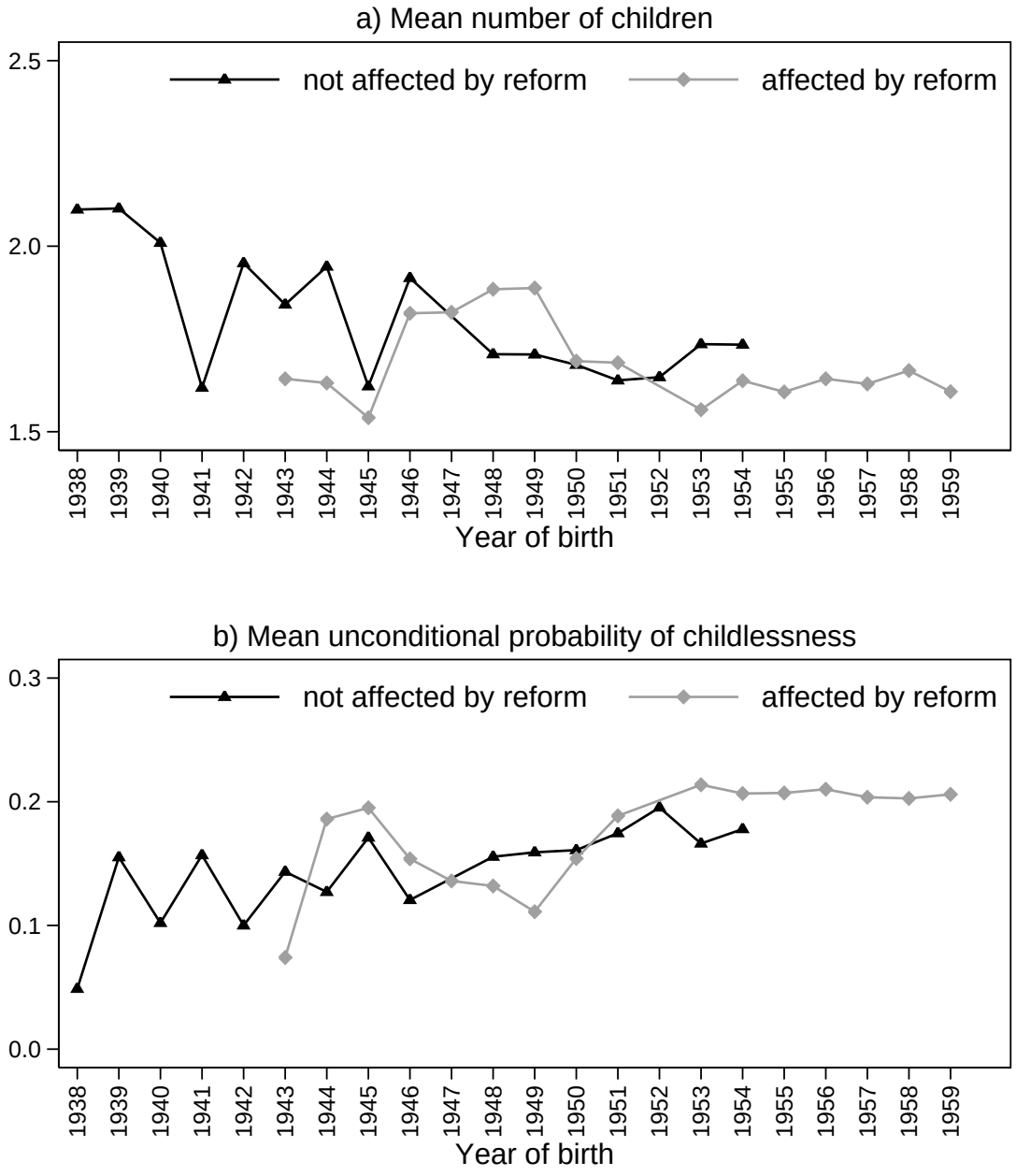
Source: German Mikrozensus (MZ) 2008; own calculations.

Figure 3.5: Analyzed birth cohorts by federal state and reform status



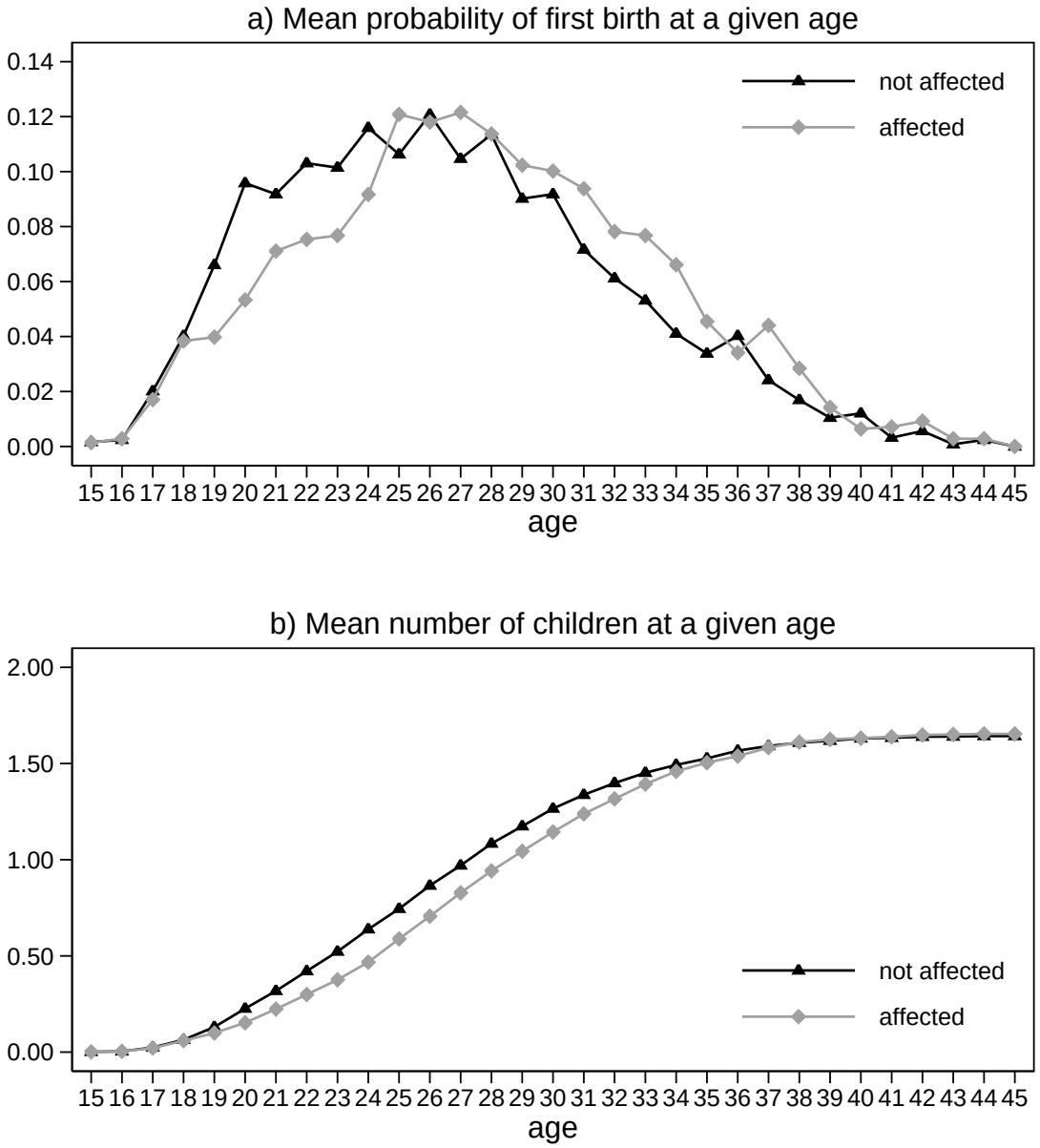
Note: We exclude Schleswig-Holstein and Hamburg from the analysis because the MZ 2008 reports fertility only for women born after 1933.
Source: Leschinsky and Roeder (1980); own illustration.

Figure 3.6: Fertility outcomes by birth cohort and reform status



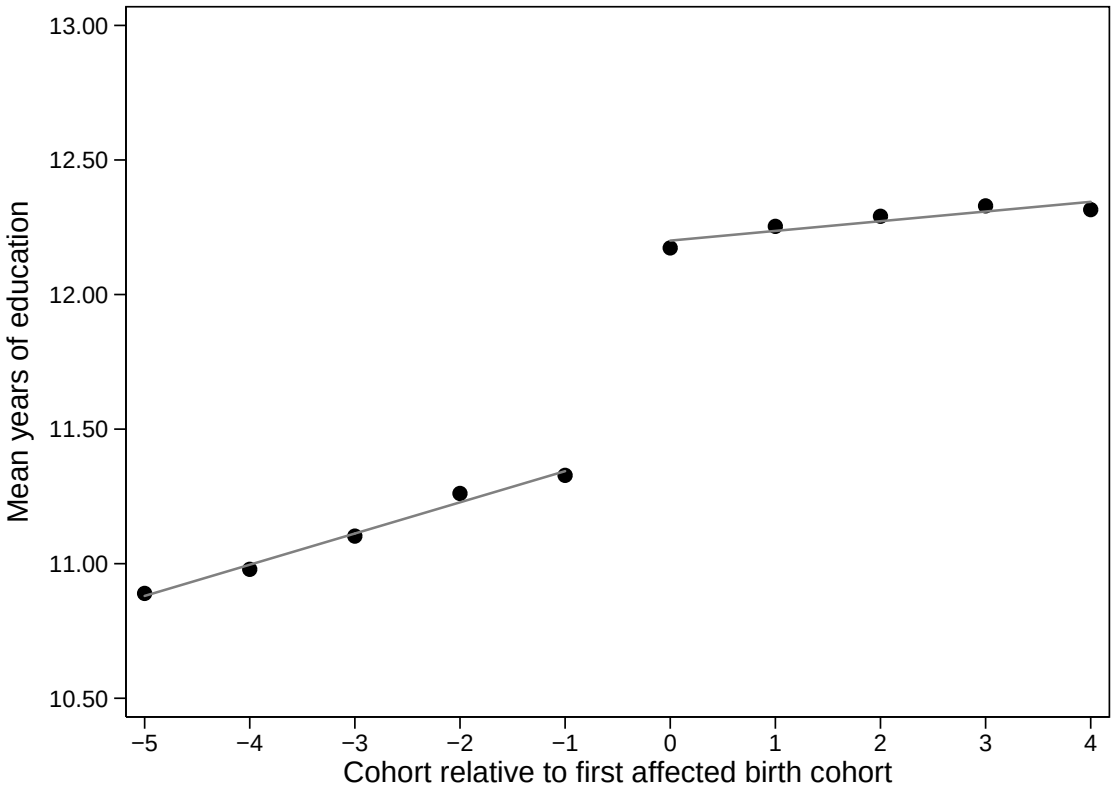
Note: The plots show unweighted raw data.
Source: German Mikrozensus (MZ) 2008; own calculations.

Figure 3.7: Timing of births by age and reform status



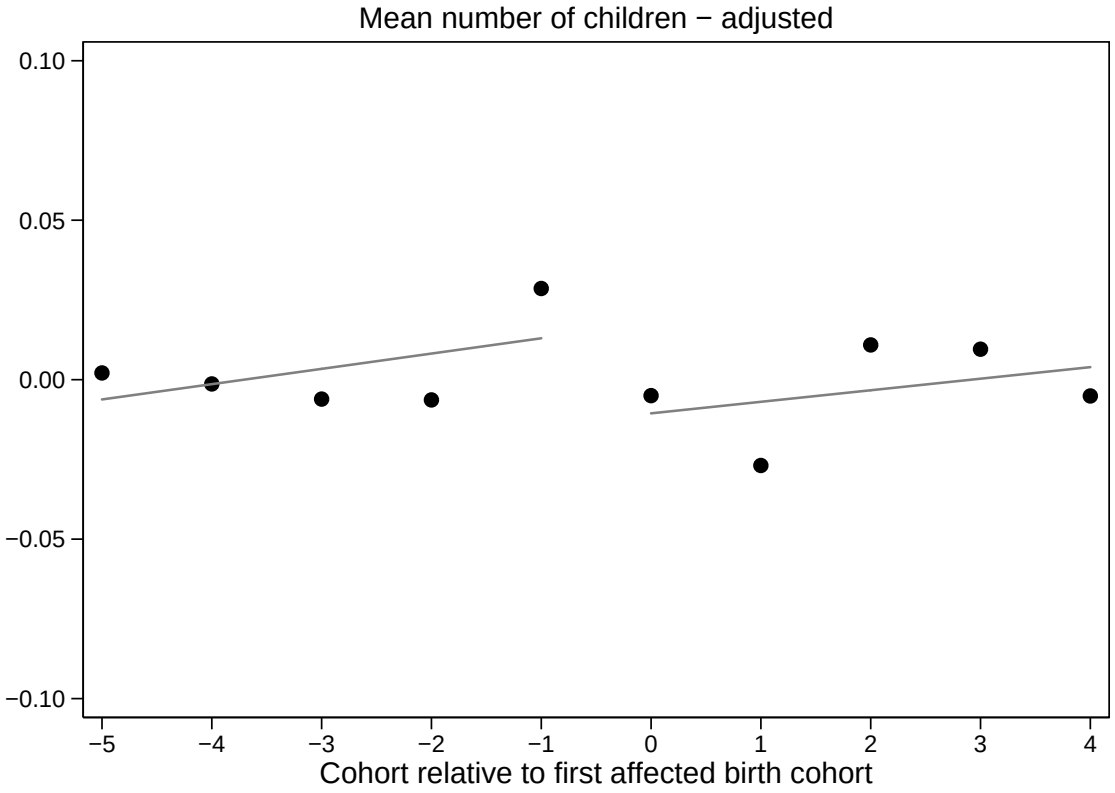
Note: The plots show unweighted raw data.
Source: SOEP 1984-2010; own calculations.

Figure 3.8: First stage: effect of the reform on years of education



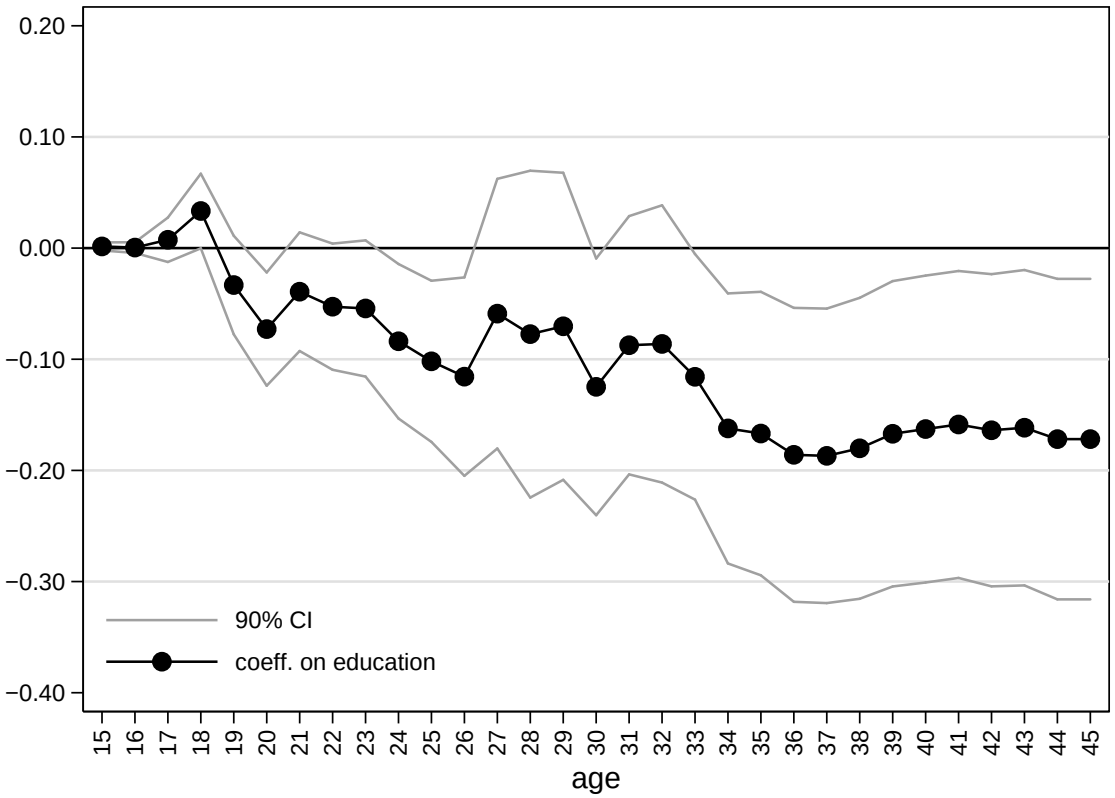
Note: The plot shows unweighted raw data.
Source: German Mikrozensus (MZ) 2008; own calculations.

Figure 3.9: Reduced form: effect of the reform on adjusted number of children



Note: The adjusted number of children is the residual from a regression of the number of children on the full set of our control variables. These control variables are state fixed effects, year of birth fixed effects, linear and quadratic state-specific trends in year of birth, municipality size and marital status.
Source: German Mikrozensus (MZ) 2008; own calculations.

Figure 3.10: Effect of education on fertility over the life cycle



Note: Dependent variable is the number of children at a given age. Each dot shows a coefficient on education obtained from a separate linear regression. Gray lines are 90% confidence intervals around the point estimate. All regressions include state of residence fixed effects, year of birth fixed effects, linear and squared state-specific trends in year of birth, indicators for marital status, municipality size, and a grandmother's age at birth.
Source: SOEP 1984-2010; own calculations.

Table 3.1: Introduction of the 9th grade in compulsory schooling

Federal state	First school year with compulsory 9 years	First birth cohort with compulsory 9 years
Hamburg	1946	1931
Schleswig-Holstein	1947	1932
Saarland	1958	1943
Bremen	1959	1944
Lower Saxony	1962	1947
North Rhine-Westphalia	1967	1953
Hesse	1967	1953
Rhineland-Palatinate	1967	1953
Baden-Wurttemberg	1967	1953
Bavaria	1969	1955

Note: Year of the introduction of the 9th grade in secondary schooling in West Germany and the first affected birth cohort.
Source: Leschinsky and Roeder (1980).

Table 3.2: Empirical evidence on the effect of education on fertility

Study	Dependent variable		Country
	Number of children	Age at first birth	
Osili and Long (2008)	negative		Nigeria
Lavy and Zablotsky (2011)	negative		Arabs in Israel
Fort (2009)	no effect		Italy
Monstad et al. (2008)	no effect	positive	Norway
Grönqvist and Hall (2011)	no effect	positive	Sweden
McCrary and Royer (2011)	no effect	no effect	U.S.
Fort et al. (2011)	positive		Europe
Braakmann (2011)	positive		U.K.
Black et al. (2008)		positive	U.S. and Norway
Silles (2011)		positive	U.K. and North Ireland
Geruso et al. (2011)	no effect	positive	U.K.
Humlum et al. (2012)		positive	Denmark

Note: Listed studies determine the effect of education on the number of children and/or the effect of education on the timing of the first birth. All studies address the endogeneity of schooling, for example, by using schooling reforms as an instrument for education.

Table 3.3: Data selection

	Number of observations	
	MZ	SOEP
Women from eight West German states		
born 5 years before/after the first affected birth cohort	20,054	-
born 7 years before/after the first affected birth cohort	-	2,708
Information on the state of school attendance available	-	1,328
Excluded because education in former East German states	251	13
Excluded because education $<$ 7 years or missing	263	45
Excluded because missing fertility information	2,112	1
Sample size	17,428	2,649

Source: German Mikrozensus (MZ) 2008 and SOEP 1984-2010; own calculations.

Table 3.4: Sample means by reform status

	MZ			SOEP		
Variable	not affected	affected	diff.	not affected	affected	diff.
Year of birth	1949.46	1954.53	5.07 ***	1948.54	1955.80	7.26 ***
	(3.12)	(3.08)		(3.36)	(3.09)	
Number of children	1.66	1.59	-0.07 ***	1.64	1.66	0.02
	(1.12)	(1.16)		(1.16)	(1.20)	
Childless (0/1)	0.16	0.20	0.04 ***	0.20	0.20	0.00
	(0.37)	(0.40)		(0.40)	(0.40)	
Probability of first birth at age 15-20 (0/1)				0.19	0.14	-0.05 ***
				(0.39)	(0.35)	
Probability of first birth at age 21-25 (0/1)				0.31	0.27	-0.04 **
				(0.46)	(0.44)	
Probability of first birth at age 26-30 (0/1)				0.21	0.24	0.03 *
				(0.41)	(0.43)	
Probability of first birth at age 31-35 (0/1)				0.06	0.11	0.05 ***
				(0.25)	(0.32)	
Probability of first birth at age $>$ 35 (0/1)				0.02	0.04	0.02 *
				(0.15)	(0.19)	
Years of education	11.11	12.28	1.17 ***	11.63	12.90	1.27 ***
	(2.91)	(2.77)		(3.23)	(2.98)	
Length of education	11.57	12.44	0.87 ***	13.72	15.18	1.46 ***
	(2.66)	(2.78)		(5.54)	(5.68)	
Attended (0/1) basic track: 8th or 9th grade	0.63	0.51	-0.12 ***	0.56	0.41	-0.15 ***
	(0.48)	(0.50)		(0.50)	(0.49)	
Married (0/1)	0.74	0.74	0.00	0.80	0.77	-0.03 **
	(0.44)	(0.44)		(0.40)	(0.42)	
Divorced (0/1)	0.12	0.12	0.00	0.11	0.12	0.01
	(0.32)	(0.33)		(0.31)	(0.33)	
Widowed (0/1)	0.09	0.05	-0.04 ***	0.05	0.02	-0.03 ***
	(0.29)	(0.21)		(0.21)	(0.15)	

Continued on next page...

Table 3.4 - continued

	MZ			SOEP		
Variable	not affected	affected	diff.	not affected	affected	diff.
Single (0/1)	0.06	0.09	0.03 ***	0.03	0.07	0.04 ***
	(0.23)	(0.29)		(0.18)	(0.25)	
Municipality size $<$ 20,000 (0/1)	0.36	0.37	0.01	0.43	0.42	-0.01
	(0.48)	(0.48)		(0.50)	(0.49)	
20,000 $<$ Municipality size $<$ 500,000 (0/1)	0.49	0.48	-0.01	0.48	0.50	0.02
	(0.50)	(0.50)		(0.50)	(0.50)	
Municipality size $>$ 500,000 (0/1)	0.15	0.14	-0.01	0.09	0.09	0.00
	(0.35)	(0.35)		(0.28)	(0.28)	
Grandmother's age at a woman's birth $<$ 19				0.03	0.04	0.01
				(0.18)	(0.20)	
Grandmother's age at a woman's birth 20-24				0.22	0.21	-0.01
				(0.41)	(0.41)	
Grandmother's age at a woman's birth 25-29				0.27	0.28	0.01
				(0.44)	(0.45)	
Grandmother's age at a woman's birth 30-34				0.17	0.21	0.04 **
				(0.38)	(0.41)	
Grandmother's age at a woman's birth 35-39				0.12	0.11	-0.01
				(0.32)	(0.31)	
Grandmother's age at a woman's birth $>$ 40				0.04	0.03	-0.01 *
				(0.20)	(0.16)	
Grandmother's age at a woman's birth missing				0.15	0.12	-0.03 ***
				(0.36)	(0.32)	
Observations	8,399	9,029		1,242	1,407	

Note: Standard deviations in parentheses. Samples are not weighted. The column labeled "diff." indicates the difference between the two subsamples' means. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% level. Variable "length of education" contains missing values in both datasets. Sample sizes for "length of education"
in the MZ are 6,109 for not affected and 6,548 for affected women and in the SOEP 605 and 777 women, respectively.
Source: German Mikrozensus (MZ) 2008 and SOEP 1984-2010; own calculations.

Table 3.5: First-stage results and the effect on track choice

	Dependent variable					
	Years of education				Attends basic track	
	Full sample		Basic track		Full sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform dummy	0.744 ***	0.647 ***	1.040 ***	1.090 ***	-0.027	-0.009
	(0.080)	(0.132)	(0.042)	(0.053)	(0.020)	(0.026)
F-Statistic	85.56	23.89	607.54	414.90	1.46	0.12
State-specific trends in year of birth						
Linear	yes	yes	yes	yes	yes	yes
Squared		yes		yes		yes
Observations	17,428		9,918		17,428	

Note: Each coefficient represents a separate linear regression. The F-Statistic gives the result of a significance test of the reform dummy in corresponding regressions. All regressions include state of residence fixed effects, year of birth fixed effects, indicators for marital status and municipality size. Standard errors in parentheses are adjusted for clustering at the state-birth year level. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% level.
Source: German Mikrozensus (MZ) 2008; own calculations.

Table 3.6: Baseline results: effect of education on completed fertility

	Full sample		Basic track	
	(1)	(2)	(3)	(4)
Panel A: Number of children				
OLS	-0.020 ***	-0.020 ***	-0.134 ***	-0.133 ***
	(0.003)	(0.003)	(0.012)	(0.012)
IV	-0.146 ***	-0.172 ***	-0.117 ***	-0.101 *
	(0.035)	(0.050)	(0.043)	(0.057)
Sample mean dep. variable	1.63	1.63	1.70	1.70
Panel B: Childlessness				
OLS	0.010 ***	0.010 ***	0.016 ***	0.015 ***
	(0.001)	(0.001)	(0.003)	(0.004)
IV	0.060 ***	0.051 ***	0.050 ***	0.020
	(0.013)	(0.018)	(0.012)	(0.015)
Sample mean dep. variable	0.18	0.18	0.15	0.15
State-specific trends in year of birth				
Linear	yes	yes	yes	yes
Squared		yes		yes
Observations	17,428		9,918	

Note: Each coefficient represents a separate linear regression. All regressions include state of residence fixed effects, year of birth fixed effects, indicators for marital status and municipality size. Standard errors in parentheses are adjusted for clustering at the state-birth year level. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% level.
Source: German Mikrozensus (MZ) 2008; own calculations.

Table 3.7: Baseline results: effect of education on the age-specific probability of first birth

Age at first birth	15-20	21-25	26-30	31-35	>35
OLS	-0.028 ***	-0.036 ***	-0.008 **	0.022 ***	0.009 *
	(0.002)	(0.003)	(0.003)	(0.005)	(0.005)
IV	-0.057 **	-0.052	0.048	-0.154 *	-0.045
	(0.022)	(0.034)	(0.045)	(0.079)	(0.081)
Sample mean dep. variable	0.16	0.35	0.42	0.28	0.13
First-stage results					
Reform dummy	1.638 ***	1.396 ***	1.451 ***	1.458 ***	0.989 *
	(0.399)	(0.407)	(0.385)	(0.461)	(0.539)
First-stage F-statistic	16.83	11.76	14.20	10.00	3.38
Observations	2,649	2,219	1,453	849	611

Note: Probability of giving the first birth at a given age, conditioned on not already having a child. Each coefficient represents a separate linear regression. All regressions include state of residence fixed effects, year of birth fixed effects, linear and squared state-specific trends in year of birth, indicators for marital status, municipality size, and a grandmother's age at birth. Standard errors in parentheses are adjusted for clustering at the state-birth year level. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% level.
Source: SOEP 1984-2010; own calculations.

Table 3.8: Reduced-form regressions: effect of the reform on job characteristics and marriage outcomes

	Dependent variable					
	self-employed	employed in white collar job	employed in blue collar job	log of Treimans prestige score	married	partner's years of education
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Full sample						
Reform dummy	0.028 **	-0.020	-0.011	0.025 *	-0.004	-0.167
	(0.012)	(0.028)	(0.021)	(0.015)	(0.015)	(0.134)
Observations	17,025	17,025	17,025	17,025	17,428	13,122
Sample mean dep. variable	0.09	0.72	0.19	3.66	0.74	12.47
Panel B: Basic track						
Reform dummy	0.050 ***	-0.016	-0.038	0.062 ***	0.010	-0.113
	(0.009)	(0.033)	(0.029)	(0.014)	(0.022)	(0.116)
Observations	9,592	9,592	9,592	9,592	9,918	7,674
Sample mean dep. variable	0.08	0.61	0.31	3.53	0.76	11.07
State-specific trends in year of birth						
Linear	yes	yes	yes	yes	yes	yes
Squared	yes	yes	yes	yes	yes	yes

Note: Each coefficient represents a separate linear regression. All regressions include state of residence fixed effects, year of birth fixed effects, and indicators for municipality size. Regressions in columns (1)-(4) additionally include indicators for marital status. Dependent variables in columns (1)-(4) refer to the very last job and estimation samples exclude 2% of women who have never had a paid job. Treimans Standard International Occupation Prestige Score (Ganzeboom and Treiman; 1996) merged to the MZ by using the International Standard Classification of Occupations (ISCO-88). Sample in column (6) excludes women not having a partner at the time of interview. Standard errors in parentheses are adjusted for clustering at the state-birth year level. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% level. Detailed definitions of the dependent variables available upon request.
Source: German Mikrozensus (MZ) 2008; own calculations.

Table 3.9: The effect of education on childlessness and the number of children - alternative definition of the education variable

	Full sample		Basic track	
	(1)	(2)	(3)	(4)
Panel A: Number of children				
OLS	-0.025 ***	-0.025 ***	-0.071 ***	-0.071 ***
	(0.004)	(0.004)	(0.012)	(0.012)
IV	-0.489 ***	-0.910	-0.395 *	-0.323
	(0.160)	(0.564)	(0.204)	(0.203)
Sample mean dep. variable	1.63	1.63	1.70	1.70
Panel B: Childlessness				
OLS	0.010 ***	0.010 ***	0.010 ***	0.010 ***
	(0.001)	(0.001)	(0.002)	(0.002)
IV	0.167 ***	0.270	0.144 **	0.066
	(0.058)	(0.183)	(0.059)	(0.048)
Sample mean dep. variable	0.18	0.18	0.15	0.15
First-stage results				
Reform dummy	0.305 ***	0.165	0.428 ***	0.457 ***
	(0.097)	(0.128)	(0.094)	(0.103)
First stage F-statistic	9.86	1.68	20.50	19.70
State-specific trends in year of birth				
Linear	yes	yes	yes	yes
Squared		yes		yes
Observations	12,657		7,686	

Note: Each coefficient represents a separate linear IV regression. Education variable is "length of education" defined as *graduation year - year of birth - 6*. All regressions include state of residence fixed effects, year of birth fixed effects, indicators for marital status and municipality size. Standard errors in parentheses are adjusted for clustering at the state-birth year level. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% level.
Source: German Mikrozensus (MZ) 2008; own calculations.

4 Earnings-related parental leave benefits and subjective well-being of young mothers: evidence from a German parental leave reform[66]

4.1 Introduction

Low fertility rates and low female labor market participation are major political issues. Declining birth rates accelerate demographic changes such as population aging and shrinking labor forces. Therefore, strengthening the labor market attachment of women is a promising way to mitigate the consequences of the demographic transition. In both dimensions - female labor market participation and fertility rates - Germany exhibits the lowest rates among OECD countries (OECD; 2013b).

In 2007 the German government changed the parental leave benefit system in order to counteract low female labor market participation. The introduction of earnings-related parental leave benefits abolished a means-tested system. Thereby, subsidies became more generous by adapting regulations from the Nordic countries where labor market participation and fertility rates are generally higher (Spiess and Wrohlich; 2008; OECD; 2013b).

Typical reform evaluation concentrates on objective measures of utility such as income or consumption (see, e.g., Meyer and Sullivan; 2004). In the case of the 2007 reform, previous research focuses on key reform goals, e.g., mothers' labor market attachment (e.g., Bergemann and Riphahn; 2011), fertility (Cygan-Rehm; 2013), or fathers' involvement in child-rearing (see also Wrohlich et al.; 2012). However, these objective measures have important shortcomings. A reform of parental leave benefits affects various areas in life, e.g., health and child outcomes, maternal stress, or family circumstances (see, e.g., Ruhm; 2000; Tanaka; 2005; Berger; 2010). A comprehensive policy evaluation should also consider more general outcomes to reveal possible unintended side effects (OECD;

[66] I thank Regina T. Riphahn, Kamila Cygan-Rehm, Robert Orlowski, Caroline Schwientek, Christoph Wunder, and the participants of the IWQW seminar at the University Erlangen-Nuremberg for useful comments on earlier versions of this paper. The usual disclaimer applies.

2011). The analysis of well-being and especially subjective well-being facilitates such a general reform evaluation and gives insights objective measures cannot give (see, e.g., Luechinger; 2009; OECD; 2011).

This study evaluates the effect of the introduction of earnings-related parental leave benefits on the subjective well-being of young mothers. In a first step, I analyze long-run consequences of the reform on mothers' subjective satisfaction about 1.5 to 5.5 years after the birth of their child. Different factors may contribute to long-run effects of the reform, e.g., higher income security because of higher labor market attachment, changes in fertility or family formation. In a second step, I analyze heterogeneity in the responses to the reform and identify potential channels through which the reform affected well-being.

The paper contributes to the literature in three important ways: first, it is the first study evaluating the reform effects on mothers' well-being for Germany. So far, there is also no international study that emphasizes effects of a similar reform on well-being. Second, the analysis of potential channels through which the reform affected well-being reveals important insights in side effects of the reform, e.g., on marriage rates. Third, I use a unique German data set - the *Panel Analysis of Intimate Relationships and Family Dynamics* - for the empirical examination. This survey facilitates the analysis of well-being and contains rich information on family background. Further, compared to other data sets with similar information, for example, the Socio-Economic Panel (SOEP), the data comprise a more sufficient number of births around the reform implementation.

A number of studies already evaluated the reform and usually find positive effects on the intentions to return to work (Bergemann and Riphahn; 2010), on the actual decision for the return to work (Geyer et al.; 2012; Kluve and Tamm; 2013), and on the involvement of fathers in child-rearing (Geisler and Kreyenfeld; 2012; Wrohlich et al.; 2012). Also, the reform affects subgroups of the population very differently depending on region of residence and eligibility for subsidies under the old regime (e.g., Wrohlich et al.; 2012; Cygan-Rehm; 2013).

The empirical strategy applies a combination of a regression discontinuity approach and a differences-in-differences design (see, e.g., Dustmann and Schönberg; 2012; Cygan-Rehm; 2013). The identification benefits from the largely unanticipated reform introduction for children born in the first quarter of 2007 (see, e.g., Kluve and Tamm; 2013). At the timing of conception the parents of these children could not have known about the reform introduction. Therefore, I compare mothers of children born in the last quarter of 2006 (not eligible for the new subsidy) and mothers of children born in the first quarter of 2007 (eligible for the new subsidy). To control for general differences in satisfaction between mothers who gave birth in the first and the last quarter of a year, I additionally include mothers of children born at the turns of years 2003/4 and 2005/6 as control group.

The results show very different responses to the reform in subgroups. The patterns are consistent with a smaller income reduction after child birth under the new regime compared to the old regime. Thus, the results may reflect a reduction in opportunity costs of child-bearing. I also find different effects of the reform on satisfaction in East and West Germany. Whereas East German women are less satisfied under the new regime, the effect is positive for West German mothers. Potential explanations for the contrary responses include differences in social norms and different income changes. Specifically, the reform is related to a reduced marriage probability of East German mothers and may induce unintended long-term financial disadvantages resulting from the German tax system. Results pass the usual robustness checks.

4.2 Institutional setting

This study evaluates the introduction of the German *Elterngeld* (parents' money) for births after January 1, 2007. Wrohlich et al. (2012) state three major goals of the reform: first, income security for families in the year after childbirth; second, helping parents to secure their economic situation on their own; third, promote fathers' involvement in child-rearing. This section describes the main changes with respect to the previous regulations.

Parents of children born before the 1st of January 2007 were supported by a means-tested subsidy called *Erziehungsgeld* (child-rearing benefit). Eligible parents could either receive EUR 300 for 24 months or EUR 450 for 12 months. For each additional child the same amount was added on top. The benefit targeted low income parents. If the joint net household income in the year before birth exceeded certain thresholds, parents were not eligible. If the applying parent was not working during the period of the payment, his/her pre-birth labor income was excluded from the means test and only the partners' income was relevant (BMFSFJ; 2005). One year after the child's birth, parents had to pass a second means test in order to stay eligible. The income thresholds in both tests depended on marital status (higher thresholds for single parents) and the chosen duration and amount of pay (EUR 300 for two years vs. EUR 450 for one year). Although parents could work up to 30 hours per week during the payment period, only mini jobs (below EUR 400 per months) were disregarded in the means test.

On the 1st of January 2007 the means-tested system was abolished and an earnings-related benefit was introduced. The new *Elterngeld* replaces 67% of prior labor earnings up to an upper bound of EUR 1800 per month and a lower bound of EUR 300 per months (BMFSFJ; 2011). All parents are eligible for the new subsidy for up to 12 months after childbirth (14 months for single parents). If parents share parental leave they are free to extend the duration of the transfer to 14 months ("daddy months").[67] Thus, all parents of children born after 01.01.2007 are eligible and both, the total amount of the subsidy and the duration of pay changed. As under the old regime, part time work below 30 hours per week is possible.

Compared to the old regime, the new subsidy is more generous because of the universal eligibility and mostly a higher total amount of benefits (see Kluve and Tamm (2013) for a detailed overview of the changes in total amounts for subgroups). The reform considerably shortens the total duration of pay for those women who were eligible under the old regime. Further, under the old regime earnings from part-time work during the transfer

[67] Additionally, parents can decide to receive only half of the monthly subsidy for a longer period of up to two years. However, only 9.5% of mothers and 2.4% of fathers choose this option (BMFSFJ; 2008).

period were added to the relevant income for the second means test. Thus, part-time work during the first year after birth reduced the likelihood of being eligible in the second year after child birth. Under the new regime, part-time work during the transfer period reduces the amount of subsidies but does not affect eligibility.

4.3 Theory and hypotheses

Different regulations changed through the reform and may contribute to an effect of the reform on well-being: first, the change in the total amount of subsidies may affect women similar as an income effect. Second, the modified duration of the transfer affects incentives for the return to work. Third, the reform gives incentives for fathers' involvement in child-rearing. In addition, other potential outcomes such as mothers' health, child outcomes, marriage rates, and social norms may be relevant for the effect of the reform on women's well-being.

Comparing pre- and post-reform situations, the introduction of earnings-related parental benefits changed the total amount mothers get to compensate financial losses of interrupted labor market participation. However, the change in subsidies varies across subgroups of women since some women were not eligible under the old regime.

To understand why subgroups are differently affected by the reform, consider two cases for a simple differentiation: (1) a high-income family and (2) a low-income family with both parents unemployed. Under the old regime the high-income family was not eligible for benefits because the family did not pass the means test. Under the new regime the family is eligible for a 67% replacement of labor earnings for 12 months (up to EUR 1800 monthly). Consequently, compared to the old regime the high-income family (1) can gain more than EUR 20,000 (see also Kluve and Tamm; 2013). The low income family (2) was eligible for EUR 300 for 24 months under the old regime. Since 2007 the family is still eligible for EUR 300 but only for 12 to 14 months. Thus, the total amount of subsidies halves because of the shorter duration of pay. Overall, if we compare pre- and post-reform

status women experience different changes in subsidies and transfer durations depending on their eligibility for the old subsidy.

The change in the total amount of subsidies between the old and the new regime may affect well-being. The change in subsidies may work similar to an income effect. The related economic literature on the effect of income on happiness predicts a positive effect (Clark et al.; 2008). However, the literature on the Easterlin paradox shows that positive income differences promote well-being only if individuals compare their situation to others or to themselves in the past (see Clark et al. (2008) for a discussion of previous findings).[68] Here, an effect may exist if women compare themselves to a hypothetical situation before the reform, to themselves in the past, or to women who are not eligible for the new subsidy (e.g., women who gave birth shortly before 2007). In sum, women who were not eligible for subsidies under the old regime may experience a positive effect on well-being and women who were eligible under the old regime might face a negative effect.

Another major change is the shorter duration of the transfer. Whereas the old subsidy was paid up to 24 months, the duration under the new regime is only 12 month (or 14 month). For parents who were eligible under the old regime, this increases the incentive to return to work earlier. In fact, Bergemann and Riphahn (2010) and Bergemann and Riphahn (2011) find a higher intention to return to work early after child birth and Kluve and Tamm (2013) find a higher employment probability of mothers after the subsidy expires. The effect of employment on well-being of mothers is not clear ex ante. On the one hand, the literature usually finds that employment relative to (registered) unemployment increases satisfaction (see, e.g., Winkelmann and Winkelmann; 1998). On the other hand, staying at home for family reasons may affect well-being differently than unemployment. Consistent with studies on the consequences of unemployment, Berger (2010) finds that mother in full-time employment report higher subjective well-being than women who

[68] The previous happiness literature stresses the role of adaptation to, for example, income changes (see Deaton; 2008; Di Tella et al.; 2010). Thus, women may adapt to income changes which may rule out a long-run effect of income on well-being.

stay at home for family reasons even if income is fully compensated. Consequently, her results imply that the parental leave reform may affect mothers' well-being positively if they return to work earlier.

Another aim of the reform is to promote fathers' involvement in child-rearing. Parents are free to extend the duration of benefit receipt to 14 months if the father takes up own leave. The system is quite flexible as parents can freely split this duration. Existing evidence on the effect of the reform on fathers' involvement in child care is mixed. Wrohlich et al. (2012) and Geisler and Kreyenfeld (2012) report that fathers use paternal leave more frequently after the reform. This contrasts with the findings of Kluve and Tamm (2013) who find no change in fathers' time devoted to child care. If the reform increased fathers' involvement in child-rearing, mothers might be less stressed because of the father's support. In addition, if the father takes paternal leave, the mothers is able to return to work earlier. Overall, if a higher engagement of fathers affects well-being, I expect a positive effect on mothers' well-being.[69]

Other channels through which the reform might affect well-being include social norms and marriage behavior. Whereas the old regulations or the tax system favor traditional family types (see, e.g., Kreyenfeld; 2004), the new subsidy explicitly promotes female labor market participation. If women under the new regime can decide about their return to work without facing social stigma, I expect their well-being to increase. Further, the reform supports female economic independence of partners' incomes. Literature on marriage behavior finds that marriage rates decrease with women's labor market attachment and economic independence (Konietzka and Kreyenfeld; 2005). Consequently, the reform might affect marriage rates negatively. The literature on the effect of marriage on life satisfaction shows a positive effect at least in the first years after marriage but is ambiguous about long-term effects (see, e.g., Lucas and Clark; 2006). Consequently, if the reform

[69] In the extreme case this channel could also affect satisfaction negatively if this reform generates a new social stigma and mothers feel social pressure to return early to work. However, as other institutions in Germany, for example, long unpaid parental leave and tax splitting, still favor traditional family types (Kreyenfeld; 2004), a negative effect is rather unlikely.

decreases marriage rates, a positive effect on well-being might not be present under the new regime and may affect well-being negatively compared to the old regime.

Related mechanisms include effects of the reform on higher order fertility, health, and child outcomes. If women return to work earlier, they might adjust their higher order births (Cygan-Rehm; 2013) which in turn might also affect well-being. Further, compared to the previous regime, higher income security in the first year after birth and the possibility to stay at home during the first year might affect maternal health, stress, and breastfeeding which potentially affects children's development and mothers' well-being (Baker et al.; 2008; Berger; 2010).

Previous evidence on the reform effect stresses different responses in East and West Germany (see, e.g., Wrohlich et al.; 2012; Cygan-Rehm; 2013). Whereas West German women postpone subsequent births, East German women tend to get subsequent children earlier. Wrohlich et al. (2012) show that East German men use paternal leave more frequently compared to West German men. A number of factors may contribute to these different responses. As average income is lower in East Germany (see, e.g., Wrohlich et al.; 2012), the number of eligible women under the old regime was higher (Fendrich et al.; 2003). Regarding East Germany, labor market attachment of women is generally higher (see, e.g., Krueger and Pischke; 1995), child care availability is higher and more accepted (Hank et al.; 2004), and marriage rates are lower compared to West Germany (Konietzka and Kreyenfeld; 2005). Thus, the change in the total amount of subsidies might on average be lower in East Germany and because of the different social norms and a higher labor market attachment of women, mothers may generally respond differently to the reform.

4.4 Identification strategy

The introduction of the reform provides a largely unanticipated natural experiment. To estimate the causal effect of the reform on subjective well-being, the identification strategy combines a regression discontinuity design and a differences-in-differences approach (see

also Dustmann and Schönberg; 2012; Cygan-Rehm; 2013). A sharp regression discontinuity design compares mothers of children born in the fourth quarter of 2006 to mothers of children born in the first quarter of 2007. Thus, identification rests on the assumption that mothers have similar characteristics in these two groups and that other factors affecting well-being remain constant within this time window. This assumption is rather strong as mothers who gave birth in the first and last quarters of subsequent years might generally differ in well-being. Potential determinants of these general differences include, for example, weather differences. By the inclusion of mothers who gave birth in the years before the introduction of the reform I can capture the effect of general differences between the fourth and the first quarter of the year. This diff-in-diff extension rests on the assumption that these seasonal differences in well-being do not change over the years.

I estimate the following equation:

$$y_i = \beta_1 \textit{reform}_i + \beta_2 \textit{quarter}_i + \textit{cohort}_i' \gamma + \mathbf{Z'}\delta + \epsilon_i \tag{4.1}$$

Here, y_i represents mothers' subjective well-being comprising life satisfaction, satisfaction with job and training, social networks, and family. The variable $\textit{reform}_i$ equals 1 if the mother gave birth after the introduction of the reform, thus after the 1st of January 2007. Additionally, $\textit{quarter}_i$ controls for general differences in the satisfaction of mothers who gave birth in the last and first quarter of subsequent years, and $\textit{cohort}_i$ is a set of dummies for the children's birth years.[70] Further controls such as dummies for children's age in months, mothers' age, mothers' education, partners' education and regional controls enter through $\mathbf{Z}$. ϵ_i is an error term.

The estimate for β_1 gives the causal effect of the *Elterngeld* introduction on mothers' subjective well-being (1) if parents could not anticipate the reform introduction and react accordingly and (2) if differences in satisfaction between mothers of last and first quarter births remain constant over the years.

[70] Here, a cohort is not defined by calendar years but by births from October to March in each year from 10/2003 to 03/2007. Thus, I include four cohorts of children: 10/03-03/04, 10/04-03/05, 10/05-03/06, 10/06-03/07.

The estimate for β_1 is biased if mothers anticipated the reform introduction and changed their behavior. One example are mothers who are highly attached to the labor market and decided to select themselves into motherhood because of the reform introduction. These mothers might in general also differ in life satisfaction. However, the previous literature shows that anticipation is rather unlikely for mothers who gave birth in the first quarter of 2007. For example, Kluve and Tamm (2013) show google search results for *Elterngeld* during the years 2004 to 2008. They find that the public discussion of the reform started in May 2006. Further, parliament passed the reform in September 2006 and until that date it was not clear whether how and when the reform will be introduced. Also, parents cannot exactly plan the timing of conception such that mothers of children conceived between March 2006 and July 2006 are unlikely to have anticipated the reform introduction.

Another threat to the validity of the estimates is the shifting of births. As some women are better off under the old and some under the new regime, women with due dates in December 2006 and January 2007 had the incentive to pre- or postpone the delivery. Neugart and Ohlsson (2013) and Tamm (2013) show that such a shift actually took place. To check the influence of this potentially confounding factor, I exclude January and December births in a sensitivity analysis.

The diff-in-diff strategy fails if seasonal patterns change over the birth cohorts of children. Diff-in-diff assumes that the difference in well-being between mothers who gave birth to a child in the last quarter of the year and mothers who gave birth in the first quarter of the following year do not change over the years. Unfortunately, I cannot test this assumption. However, a graphical inspection supports no changes in seasonal trends. Further, I check the robustness of the results for different cohorts in the control group.

The risk of changing trends over the years should be smaller the fewer cohorts I include in the control group.[71]

4.5 Data

This study exploits four waves from 2008 to 2011 of the *Panel Analysis of Intimate Relationships and Family Dynamics* (Pairfam). The Pairfam data aim at providing an empirical basis for the analysis of family dynamics and collect data of 12,400 participants annually since 2008 (Huinink et al.; 2011). The main advantage of the data is very detailed information on a range of family related characteristics including mothers' subjective well-being. A drawback of the data are small samples sizes. However, for Germany there is no other appropriate data source available comprising more observations. The Pairfam data collection started in 2008 with respondents of three birth cohorts (1991-93, 1981-83, 1971-73). In addition to the Pairfam data I use data called *Demographic Differences in Life Course Dynamics in Eastern and Western Germany* (DemoDiff). The DemoDiff panel data closely follows the design of Pairfam (since 2009) and facilitates East-West comparisons (see Kreyenfeld et al.; 2013).

The Pairfam data ask anchor persons about a wide range of family characteristics.[72] The partner, child, and parents of the anchor person answer a separate questionnaire. I restrict the sample to female anchor persons aged 20 to 31 who gave birth to children at

[71] Another concern might be that effects are driven by a "starting" effect, a Hawthorne effect (see, e.g., Adair; 1984). Women who gave birth in the first quarter of 2007 are the first who receive the new subsidy and we might find positive effects just because something changed. The converse might be true for the last births under the old regime. However, this should impose minor consequences for the estimates: first, I measure well-being 2.5 to 5.5 years after the reform introduction. Second, a Hawthorne effect should be smaller the older children are and I find no significant changes of the reform effects with increasing child age. Also, if mothers who gave birth in the last quarter of 2006 are especially unhappy because they barely missed the reform, satisfaction in the last quarter of 2006 should be lower than in the last quarter of 2005. I rule out this mechanism as a driving factor as I do not find this pattern in the data.

[72] Pairfam started in 2008 with 12,400 randomly drawn individuals from the registration office pool of 310 communities. The population of anchor persons are people living in Germany in private households. Interviews took place face-to-face between September and April of year 2008 to 2011. See Huinink et al. (2011) for further details about survey and sampling.

the turns of 2003/4 to 2006/7.[73] Further, I restrict the sample to births in the first and in the last quarter of these years.[74] I consider only children aged 16 to 63 months (about 1.5 to 5.5 years). These sample restrictions leave me with 119 births under the new regime and 496 births under the old system (615 births in total).[75]

To explore mothers' well-being, I use information on overall life satisfaction. Mothers answer the question "Now I would like to ask about your general satisfaction with life. All in all, how satisfied are you with your life at the moment?". The survey also asks for mothers' satisfaction within specific areas of life (e.g., job and training, or family). As the reform generated different incentives concerning, for example, the return to work or fathers' involvement in child-rearing, these variables might reveal additional insights into the channels through which the reform affects well-being. Mothers evaluate their satisfaction on a Likert scale with minimum zero to maximum ten. The following Section 4.6.1 presents some descriptive statistics.

As discussed in Section 4.3, the reform affected women differently depending on their eligibility for the old subsidy. Thus, I investigate the reform effect for subgroups of women who were and who were not potentially eligible under the old regime. Eligibility depended on partners' income if the mother took leave. Unfortunately, partners' pre-birth income is not available in the data and information on mothers' leave only for a subgroup. To determine whether women were eligible under the old regime, I use information on pre-birth marital status and on partners' education as a proxy for labor income. I consider a

[73] I restrict the analysis to women aged 20 to 31 for different reasons: first, women aged 20-30 are at the beginning of their working and fertile life. Consequently, they are at the core of policy interventions which aim at increasing fertility and labor market participation. Second, the survey design of Pairfam does not facilitate an analysis of women in all fertile ages because of the cohort design of the study. In fact, I exclude only women older than 35. These women potentially differ in various observed and unobserved factors, for example, attitudes towards female employment. I leave a separate investigation of the response of this interesting subgroup for future research.

[74] The control group of the identification includes women who gave birth from 2003 to 2006. In this period also other reforms such as, for example, those of unemployment benefits in 2005 and 2006 took place (see, e.g., Riphahn and Wunder; 2013). However, this is only a concern if these reforms are systematically connected to quarterly births which is not the case. Also, child care availability was strongly promoted after 2005 (Bauernschuster et al.; 2013). To eliminate concerns about potential interactions with the reform, I control for child care availability for under 3 and 3 to 6 year olds in all regressions.

[75] A less restrictive sample uses all children born between 07/2003 and 06/2007. This provides a sample of 1,193 births. I prefer the smaller window as it limits confounding factors. However, baseline findings do not change when I use the less restrictive sample (results available upon request).

woman likely to be eligible if she had no partner or a partner with low education (below intermediate degree) before birth.

4.6 Results

4.6.1 Descriptive analysis

Table 4.1 gives an overview of some characteristics of mothers, partners, and children. The first two columns give averages for births in the last quarter of 2006 and the first quarter of 2007. Columns 3 and 4 give average characteristics for births from 2003/4 to 2005/6, in the last and first quarters of these years respectively. Births in the first quarter of 2007 (column 2) represent the treatment group, columns 1, 3, and 4 control groups. Column 5 presents the difference of column 1 and 2, thus, the regression discontinuity effect in raw data. The difference in average characteristics between first and last quarter of year 2003/4 to 2005/6 represents seasonal effects. The difference is given in column 6. Finally, the difference between columns 5 and 6 give the diff-in-diff results in raw data (column 7). For average life satisfaction we observe an increase after the reform. However, this effect may be completely driven by seasonal effects. Once we consider general differences between the first and the last quarter of subsequent years (column 6), the effect is negative and close to zero (column 7). Overall, in the raw data, there is no clear pattern in differences in satisfaction.

A concern in diff-in-diff estimations are changing seasonal trends in treatment and control groups, i.e., that changes in satisfaction between mothers who gave birth in the first and last quarters of subsequent years vary between 2003 and 2007. I cannot test this assumption, but to investigate this necessary requirement for consistency, Figure 4.11 plots average life satisfaction of women who gave birth in last and first quarters in 2003/4 through 2006/7. Figure 4.11 supports similar trends in treatment and control groups over the years. The changes in satisfaction from last to first quarter births are positive and similar in magnitude in all years. If we compare the change in satisfaction from 2006 to

2007 to changes in satisfaction in the years before, we observe a slightly lower increase. This is consistent with the small negative diff-in-diff effect in Table 4.1. Also, Figure 4.11 underpins that a sharp regression discontinuity design might be misleading as we observe general seasonal trends in satisfaction which cannot be attributed to the reform. Additionally, the different levels of satisfaction show cohort effects which I control for in the following regressions.

4.6.2 Overall effects and results by prior eligibility status

Table 4.2 gives estimation results for the full sample and by eligibility status under the old regime. As the data do neither provide direct information on eligibility nor on pre-birth income, I use pre-birth marital status and partners' education to approximate eligibility for the old subsidy. Panel A gives results for the full sample, panel B for mothers with a highly educated partner (intermediate secondary or higher) and panel C for mothers without partner or with a lowly educated partner. I use four dependent variables: overall life satisfaction, satisfaction with job and training, satisfaction with social networks, and satisfaction with family. As I use panel data, standard errors are clustered at the individual level in all following regressions.

The full sample of 615 births splits in 305 and 310 births in the subsamples (panel A and B). Consequently, precision of estimates is quite low and I'm not willing interpret the magnitude of the effects. However, some meaning can be found in the sign of the effects and the patterns in subgroups. None of the estimated reform effects in the full sample (panel A) in Table 4.2 are statistically significant. This is not surprising as we find offsetting effects in subgroups by partners' education.

Panel B gives the results for women with highly educated partners. These women were potentially not eligible for transfers under the old regime and experience a positive income effect under the new regime. Panel C gives the results for women who would have been eligible for the old subsidy and therefore potentially face a negative income effect or a low positive income effect after the reform.

The estimates show the expected patterns. Whereas the reform effects on life satisfaction are significantly positive for women with highly educated partners, the effect is negative but imprecisely estimated for women without or with a low educated partner. This pattern is consistent with the opposing income effects in the two groups. Further, if we compare the reform effect on the different satisfaction outcomes, we find pronounced effects in column 2 and 4 of panel B. Column 2 gives the reform effect on the satisfaction with job and school. Here, we expect a pronounced effect if, for example, women return earlier to work after child birth as found by Kluve and Tamm (2013). As previous evidence shows that working conditions, for example, training, are related to the duration of parental leave (see, e.g., Puhani and Sonderhof; 2011), a shorter career interruption may contribute to higher work satisfaction.

Column 4 gives the effects on the satisfaction with family. The satisfaction with family may reveal effects of the reform on child development or family dynamics. Women who are newly eligible for paid parental leave (panel B) are more satisfied with their family. Here, a positive effect of the reform on fathers' involvement (see, e.g., Geisler and Kreyenfeld; 2012) may contribute to an overall increase in satisfaction with family. Also, previous literature stresses the positive effect of parental leave on child health and child development (see, e.g., Ruhm; 2000; Berger; 2010). As women who were not eligible under the old regime tend to use parental leave in the first year after birth more frequently (Wrohlich et al.; 2012), we expect the found positive effect on satisfaction in general and a pronounced effect on satisfaction with family.

4.6.3 Heterogeneity - East-West comparison

Table 4.3 gives results for West Germany (panel A) and East Germany (panel B). The results support earlier research as the reform effects in East and West Germany are contrary and offsetting. Again, estimates are imprecisely estimated and I cannot reliably interpret

the magnitude of the estimates. However, I find throughout positive responses to the reform in West Germany and negative or zero effects in East Germany.[76]

Next, I analyze potential determinants of the different responses in East and West Germany.[77] The share of eligible women under the old system was higher in East Germany (Fendrich et al.; 2003). Thus, one important channel may be different income changes in the year after birth. As I have no information on income in the year before and after birth, I cannot test this channel. However, Kluve and Tamm (2013) report negative but insignificant income changes in East Germany between the year before and after birth. Further, Table 4.4 shows the reform effect on satisfaction with the financial situation of the household and on current net household income.[78] Women in East Germany are less satisfied with their financial situation although the effect is not significant. The effect on income is negative and significant at the 10% level for East Germany and small and insignificant for West Germany. Thus, the results underpin a long-run negative income effect in East Germany.

A second channel might be an effect of the reform on marriage behavior. In East Germany marriage rates are generally low because of lower religiousness, a higher labor market attachment of women, and a lower dependence on partners' income (Konietzka and Kreyenfeld; 2005). Table 4.4 shows that the reform affects the probability of a current marriage differently in the two regions. The effect is negative in both East and West Germany, but more pronounced and significant for East German mothers. If marriage has a (short-run) positive effect on satisfaction (Lucas and Clark; 2006), the absence of such an effect might explain the negative effect in East Germany.

[76] Ideally, I would like to separate women in East and West Germany by eligibility under the old regime. Unfortunately, an investigation of the effect for further subgroups by partners' education is not possible because of small sample sizes. However, the opposing effects in subgroups shown in Table 4.2 potentially contribute to the low precision of the estimates in Table 4.3.

[77] Although the literature generally finds differences in, for example, the timing of births in East and West Germany (see, e.g., Kreyenfeld; 2004), descriptive evidence does not support a different selection of women into early motherhood (see Table B.1 in the Appendix). I neither find differences in mothers' age nor in mothers' education. An explanation might be that fertility trends in East and West Germany converge over time and are very similar for birth cohorts after 1970 (Goldstein and Kreyenfeld; 2011).

[78] Unfortunately, some variables are not available in all waves of the Pairfam or comprise missing values. Consequently, for some of the outcomes in Table 4.4 the sample size reduces.

The reform also promoted fathers' involvement in child-rearing. A higher involvement might have positive effects on satisfaction. Table 4.4 gives the effect on satisfaction with the relationship and the effect on the probability that parents share child-rearing. Here, I find significant positive effects in West Germany and zero effects in East Germany. Thus, an absent effect on fathers' involvement might also explain different responses in East and West Germany. An explanation for the absent effect, however, might be a higher level of fathers' involvement in East Germany in general (see Table B.1).

Other potential channels include effects on the return to work after birth and social norms. In East Germany, the former socialist part of Germany, women are traditionally more attached to the labor market (see, e.g., Krueger and Pischke; 1995). The 2007 reform was one of the first reforms explicitly promoting maternal labor market participation. If women feel less social pressure in the decision to return to work, there might be positive reform effects on satisfaction. However, this effect might be especially important in West Germany and explain some of the found differences between East and West Germany. Literature on reform effects on the return to work finds a positive effect in East and West Germany (Bergemann and Riphahn; 2010, 2011; Geyer et al.; 2012; Wrohlich et al.; 2012). Kluve and Tamm (2013) report an increased employment probability for East German mothers 1.5 years after child birth and no change in employment probabilities for West German mothers. Thus, a higher labor market attachment of East German women may relate to an earlier return to work. We generally expect a positive effect of employment on satisfaction. Consequently, an earlier return to work does not explain the negative effect in East Germany in Table 4.3.

In sum, a negative reform effect on satisfaction in East Germany may be attributed to a lower or even negative direct income effect, an absent positive effect through a higher involvement of fathers, or through an absent positive effect of marriage. The negative effect on current net household income indicates long-term financial consequences of

the reform.[79] One explanation for long-term negative income effects might be the reduced marriage probability and a loss of tax benefits. As the German tax system generally favors traditional family types (see, e.g., Kreyenfeld; 2004), unmarried couples face losses in net income.

4.7 Sensitivity analysis

Table 4.5 presents results on two sensitivity analyses with different sample selection criteria. Each coefficient represents a separate linear regression. As earlier results show that estimates for the full sample are difficult to interpret because of offsetting effects in subgroups, I present each sensitivity analysis for four groups: women with a highly educated partner, women with no or a low educated partner, women living in West Germany, and women living in East Germany.

Panel A checks whether the results are affected by pre- and postponed births between years 2006 and 2007. Neugart and Ohlsson (2013) and Tamm (2013) both show that women shifted their births to become eligible for the old or new subsidy. This confounding factor potentially affects the results. Panel A strongly supports my baseline findings. When births from January and February are excluded, the estimates are more precise and similar in direction. There are two changes in the sign of the effect for women with no or low educated partners. However, the effect is statistically indistinguishable from zero.

A critical assumption for diff-in-diff strategies is that general differences between mothers who gave births in the first and last quarters of years are similar for treatment and control group. If these differences in satisfaction change, the results are biased. The more cohorts I select as control group, the more likely the assumption of constant trends is violated. Panel B gives the results for a reduced sample in which I use only births from 2005/6 and 2006/7. The sample size declines considerably and consequently the estimates

[79] Cygan-Rehm (2013) finds that East German mothers reduce birth spacing after the reform. This might also affect the financial situation of the household negatively. However, the negative effect on household income is also present if I exclude mothers with subsequent births. In addition, I am able to control for subsequent births and the estimates are robust (not presented).

loose precision. However, also with fewer cohorts considered in the control group, the estimates show the patterns reported in the baseline results.

4.8 Conclusion

This study analyses the effect of a reform of the German parental leave benefits on the subjective well-being of young mothers. Women who gave birth to children after 1st of January 2007 are eligible for a new earnings-related subsidy which replaces 67% of pre-birth labor earnings. Under the former regime, a means-tested subsidy supported especially low income families. Also, the new subsidy explicitly promoted fathers' involvement in child-rearing.

The reform possibly affects the subjective satisfaction of mothers through different channels. One potential channel might be an income effect. The reform changed the total amount of benefits and the duration of pay. Whereas eligible women under the old regime received the maximum amount of EUR 300 per month for two years, the duration was shortened to one year, the amount is earnings-related but with a minimum transfer of EUR 300 and a maximum of EUR 1800 per month. Consequently, the reform affects women differently depending on the eligibility under the old regime. However, also a higher involvement of fathers in child-rearing and changes in marriage behavior potentially affect well-being.

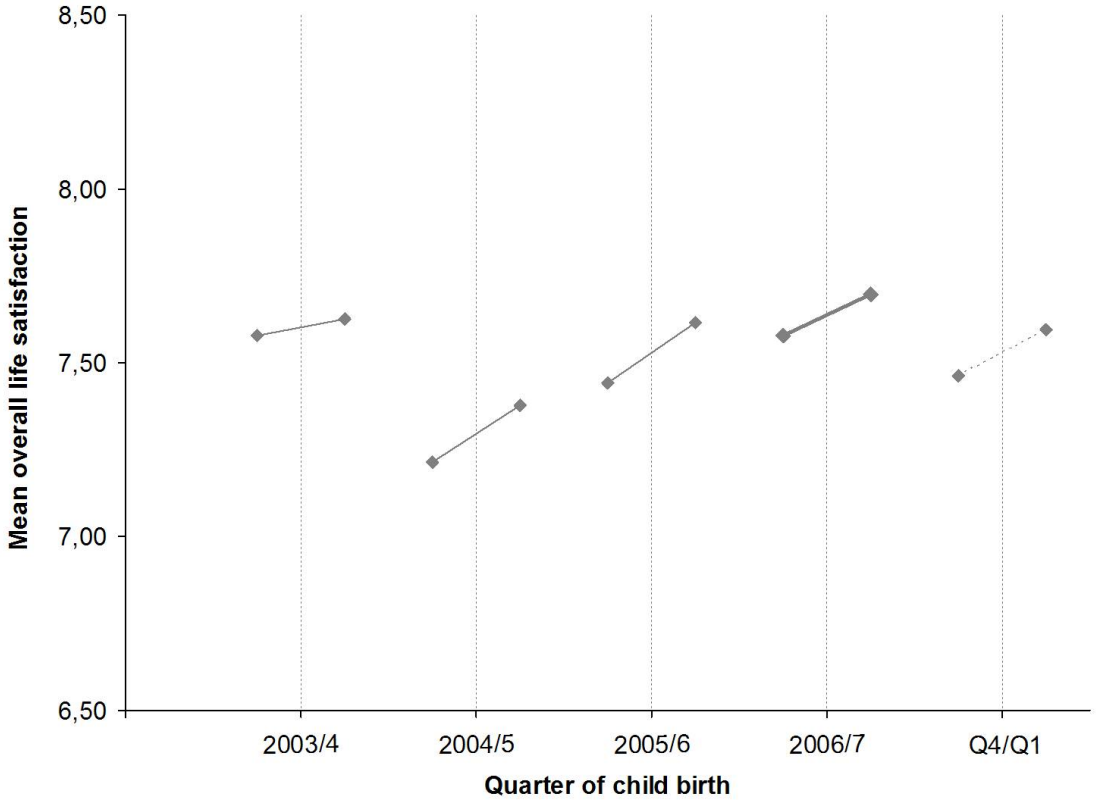
My empirical approach takes advantage of the low anticipation of the reform. At the time of conception of children born in the first quarter of 2007, the parents could not have known about the impending reform. Therefore, I use a regression discontinuity design with a diff-and-diff extension. I compare mothers of children born in the first quarter of 2007 to mothers of children born in the last quarter of 2006. Additionally, I use mothers of children born at the turns of years 2003/4 to 2004/5 as control group to account for seasonal effects in satisfaction, i.e., general differences in the satisfaction of women who gave birth in first and last quarters of subsequent years.

The empirical results show that the reform affected women differently depending on their former eligibility for the means-tested subsidy. Although small samples lead to a low precision of the estimates, women who were potentially eligible for the old subsidy are less satisfied then women who were not eligible. This pattern is in line with a reduction of income losses after child birth. Furthermore, women responded very differently in East and West Germany. Whereas West German mothers are on average more satisfied, East German mothers are less satisfied after the introduction of the new subsidy. As a potential channel, the results show West German fathers to involve more in child-rearing after the reform introduction. This positive effect on fathers' involvement is absent in East Germany. Also, I find that women in East Germany do marry less frequently after the reform and that they experience a reduction in current net household income 1.5 to 5.5 years after child birth. Thus, the results show a long-term financial disadvantage in East Germany after the reform.

One interpretation of the results is that the reform induced unintended side effects. The reform on one hand and the German tax system on the other support very different family types. A recent study from Thévenon (2013) shows that among all OECD countries Germany supports single breadwinner families the most over equal dual earner couples. The support of married couples in combination with the presented negative reform effect on marriage may lead to an unintended financial disadvantage which leads to considerable decrease in mothers' satisfaction.

4.9 Figures and tables

Figure 4.11: Average life satisfaction of mothers by quarter of child birth



Note: The plot shows raw data. Each set of connected dots compares average overall life satisfaction of mothers who gave birth to a child one quarter before and after a particular turn of the year.
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.

Table 4.1: Descriptives

	2006/07		2003/04 - 2005/06		Differences		
	Q4	Q1	Q4	Q1	1st	2nd	Diff-in-Diff
					(2)-(1)	(4)-(3)	(5)-(6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variables: Mothers' satisfaction with							
Life overall	7.579	7.697	7.386	7.530	0.118	0.144	-0.026
School, Training, and Job	6.437	6.555	6.497	6.282	0.118	-0.216	0.334
Friends and social network	7.579	7.328	7.455	7.392	-0.252	-0.063	-0.189
Family	8.270	8.941	8.545	8.834	0.671	0.289	0.382
Mothers' characteristics							
Mothers age	27.794	27.521	26.725	26.895	-0.273	0.170	-0.443*
Lives in East Germany	0.317	0.370	0.344	0.431	0.052	0.087	-0.035
Non-German nationality	0.175	0.101	0.169	0.193	-0.074	0.024	-0.098
Secondary education							
Lower secondary	0.413	0.403	0.360	0.376	-0.009	0.016	-0.025
Intermediate secondary	0.278	0.345	0.397	0.392	0.067	-0.005	0.071
Higher secondary	0.310	0.252	0.243	0.232	-0.057	-0.011	-0.046
Postsecondary education							
Currently enrolled/no degree	0.278	0.210	0.333	0.359	-0.068	0.026	-0.093
Vocational training	0.579	0.664	0.534	0.536	0.085	0.002	0.083
Technical school / civil servant training	0.032	0.025	0.069	0.039	-0.007	-0.030	0.024
College / university	0.111	0.101	0.063	0.066	-0.010	0.003	-0.013
Pre-birth marital status							
Single	0.040	0.042	0.021	0.066	0.002	0.045	-0.043
Married	0.563	0.555	0.524	0.586	-0.009	0.062	-0.071
Divorced	0.016	0.059	0.021	0.017	0.043	-0.005	0.048
Cohabitation	0.365	0.294	0.333	0.254	-0.071	-0.079	0.008
Marital status missing	0.016	0.050	0.101	0.077	0.035	-0.023	0.058
Current partner characteristics							
Partners' age	33.222	33.124	30.981	31.755	-0.098	0.773	-0.872
Secondary education							
Lower secondary	0.492	0.378	0.328	0.337	-0.114	0.009	-0.123
Intermediate secondary	0.310	0.353	0.317	0.376	0.043	0.058	-0.015
Higher secondary	0.119	0.134	0.190	0.166	0.015	-0.025	0.040
Missing information/no partner	0.079	0.134	0.164	0.122	0.055	-0.042	0.098
Children's characteristics							
Child age in months	41.373	39.866	51.751	50.801	-1.507	-0.950	-0.557
Number of observations	126	119	189	181			

Notes: * p<0.1, ** p<0.05, *** p<0.01
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.

Table 4.2: The effect of *Elterngeld* on subjective well-being by partners' education

	Satisfaction with			
	Life overall	School and Job	Social network	Family
PANEL A: Full Sample				
Reform	0.027	0.200	-0.297	0.373
	(0.401)	(0.539)	(0.603)	(0.427)
1st quarter	0.213	-0.374	0.020	0.370
	(0.235)	(0.326)	(0.319)	(0.266)
Year of child birth				
2003/04	Ref.	Ref.	Ref.	Ref.
2004/05	-0.502	0.172	-0.834	-0.248
	(0.429)	(0.623)	(0.559)	(0.439)
2005/06	-0.360	0.682	-0.253	0.146
	(0.642)	(0.969)	(0.898)	(0.686)
2006/07	-0.090	0.330	0.137	0.428
	(0.962)	(1.353)	(1.318)	(1.061)
N	615	615	615	615
PANEL B: Partners' education intermediate or high				
Reform	0.988 *	1.811 **	0.695	1.181 *
	(0.540)	(0.880)	(0.737)	(0.667)
1st quarter	-0.183	-0.560	-0.412	0.367
	(0.317)	(0.410)	(0.396)	(0.372)
Year of child birth				
2003/04	Ref.	Ref.	Ref.	Ref.
2004/05	-0.330	-0.015	-1.283 *	-0.422
	(0.608)	(0.987)	(0.730)	(0.642)
2005/06	-0.593	0.082	-1.759	-1.211
	(1.035)	(1.650)	(1.342)	(1.083)
2006/07	-0.815	-1.839	-2.348	-1.920
	(1.470)	(2.274)	(1.893)	(1.720)
N	305	305	305	305
PANEL C: No partner or low educated partner				
Reform	-0.837	-0.722	-1.161	-0.274
	(0.600)	(0.698)	(0.813)	(0.616)
1st quarter	0.644 *	-0.196	0.577	0.434
	(0.382)	(0.507)	(0.466)	(0.382)
Year of child birth				
2003/04	Ref.	Ref.	Ref.	Ref.
2004/05	-0.816	0.217	-0.329	-0.015
	(0.681)	(0.941)	(0.817)	(0.664)
2005/06	-0.148	1.255	1.370	1.528
	(0.972)	(1.373)	(1.175)	(0.995)
2006/07	0.556	1.968	2.580	2.418 *
	(1.488)	(1.963)	(1.753)	(1.428)
N	310	310	310	310

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at mothers' person number. Additional control variables include child care availability for under 3 and 3-6 year olds by state and year, dummies for child age, survey year, mothers' age, East Germany, mothers' education, mothers' nationality non-German, quartiles of fathers' age, pre-birth marital status.
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.

Table 4.3: The effect of *Elterngeld* on subjective well-being - East-West comparison

	Satisfaction with			
	Life overall	School and Job	Social network	Family
PANEL A: West Germany				
Reform	0.318	1.163 *	0.539	0.982 *
	(0.560)	(0.665)	(0.802)	(0.590)
1st quarter	-0.084	-0.571	-0.352	0.292
	(0.311)	(0.402)	(0.436)	(0.367)
Year of child birth				
2003/04	Ref.	Ref.	Ref.	Ref.
2004/05	0.335	-0.281	-1.454	-0.074
	(0.974)	(0.797)	(1.008)	(1.279)
2005/06	1.156	0.274	-1.512	0.405
	(2.054)	(1.539)	(2.290)	(2.690)
2006/07	1.624	-1.261	-2.274	0.265
	(3.071)	(2.395)	(3.472)	(4.033)
N	388	388	388	388
PANEL B: East Germany				
Reform	-0.680	-1.064	-1.226 *	-0.035
	(0.584)	(0.942)	(0.694)	(0.643)
1st quarter	0.799 **	-0.090	0.614	0.264
	(0.399)	(0.595)	(0.479)	(0.496)
Year of child birth				
2003/04	Ref.	Ref.	Ref.	Ref.
2004/05	-1.048 *	0.441	-1.333	-0.701
	(0.591)	(1.101)	(0.809)	(0.642)
2005/06	-1.092	0.594	-0.380	-0.193
	(0.814)	(1.360)	(1.138)	(0.811)
2006/07	-0.218	1.326	0.117	0.355
	(1.323)	(1.789)	(1.670)	(1.312)
N	227	227	227	227

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at mothers' person number. Additional control variables include child care availability for under 3 and 3-6 year olds by state and year, dummies for child age, survey year, mothers' age, mothers' education, nationality non-German, quartiles of fathers' age, pre-birth marital status.
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.

Table 4.4: Alternative outcomes: East-West differences

		Satisfaction with			
	Current net HH income	Relationship	Financial situation	Currently married	Share Child rearing
PANEL A: Full Sample					
Reform	-130.152	-0.198	-0.139	-0.108	0.215 *
	(245.221)	(0.776)	(0.509)	(0.072)	(0.121)
N	562	304	540	615	493
PANEL B: West Germany					
Reform	-25.962	0.534	0.835	-0.027	0.241 *
	(391.424)	(1.103)	(0.708)	(0.071)	(0.140)
N	347	185	352	388	333
PANEL C: East Germany					
Reform	-515.448 *	-1.937	-0.906	-0.266 *	0.112
	(271.361)	(1.208)	(0.721)	(0.154)	(0.225)
N	215	119	188	227	160

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at mothers' person number. Additional control variables include child care availability for under 3 and 3-6 year olds by state and year, dummies for child age, survey year, mothers' age, mothers' education, mothers' nationality non-German, quartiles of fathers' age, pre-birth marital status.
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.

Table 4.5: Robustness checks - sample selection

	Satisfaction with			
	Life overall	School and Job	Social network	Family
PANEL A: Without December and January births				
High educated partner				
Reform	1.077	2.928 **	1.716 *	1.630 *
	(0.693)	(1.124)	(0.956)	(0.873)
N	201	201	201	201
No or low educated partner				
Reform	-1.762 **	-1.059	-0.017	0.393
	(0.725)	(1.157)	(1.095)	(0.814)
N	188	188	188	188
West Germany				
Reform	0.612	2.156 **	2.192 **	1.575 **
	(0.715)	(0.964)	(0.930)	(0.713)
N	256	256	256	256
East Germany				
Reform	-1.056	-0.327	-1.497 **	0.880
	(0.895)	(1.308)	(0.672)	(0.993)
N	133	133	133	133
PANEL B: Without birth cohort 2003/4 and 2004/5				
High educated partner				
Reform	1.275 *	1.318	0.197	1.175
	(0.677)	(0.936)	(0.834)	(0.842)
N	213	213	213	213
No or low educated partner				
Reform	-1.000	-1.145	-1.404	-0.193
	(0.679)	(0.905)	(0.853)	(0.651)
N	225	225	225	225
West Germany				
Reform	0.185	0.730	0.180	1.042
	(0.665)	(0.802)	(0.941)	(0.788)
N	263	263	263	263
East Germany				
Reform	-0.561	-0.848	-0.661	0.116
	(0.636)	(1.122)	(0.714)	(0.781)
N	175	175	175	175

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at mothers' person number. Additional control variables include child care availability for under 3 and 3-6 year olds by state, dummies for child age, survey year, mothers' age, mothers' education, mothers' nationality non-German, quartiles of fathers' age, pre-birth marital status..
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.

B Appendix

Table B.1: Descriptive evidence: East-West differences

	West Germany	East Germany	Full sample
Current net household income	2122.594	1763.744	1985.311
	(1054.565)	(735.047)	(960.454)
Currently working	0.433	0.449	0.439
	(0.496)	(0.499)	(0.497)
Currently married	0.776	0.463	0.660
	(0.418)	(0.500)	(0.474)
Father is involved in child-rearing	0.372	0.544	0.428
	(0.484)	(0.500)	(0.495)
Current mothers age	27.080	27.264	27.148
	(1.258)	(1.259)	(1.260)
Child age in months	45.881	49.035	47.046
	(12.887)	(10.288)	(12.081)
Mothers' secondary schooling			
Lower secondary	0.412	0.335	0.384
	(0.493)	(0.473)	(0.487)
Intermediate secondary	0.358	0.366	0.361
	(0.480)	(0.483)	(0.481)
Higher secondary	0.229	0.300	0.255
	(0.421)	(0.459)	(0.436)
Social norms: Agrees to statement			
Mothers' should focus family life	0.450	0.253	0.374
	(0.498)	(0.436)	(0.484)
Working mothers' harm children aged < 6	0.407	0.167	0.314
	(0.492)	(0.374)	(0.465)

Notes: * p<0.1, ** p<0.05, *** p<0.01. Table gives cell means, standard deviations (in parentheses).
Source: Pairfam 2008/9-2011/12 and DemoDiff 2009/10-2011/12; own calculations.

5 Summary and conclusive remarks

5.1 Summary and contribution

This dissertation consists of three self-contained studies. To begin with, Section 1.1.2 provides a brief summary of the German school system and German family policy. The three sections analyze state-level returns to schooling, the effect of education on fertility, and the effect of a parental leave benefit reform on the subjective well-being of mothers.

I analyze returns to secondary schooling in West Germany at the federal state level in Section 2. This is the first study that analyzes returns to schooling at the state level in Germany and tests hypotheses concerning the different mechanisms that determine regional heterogeneity, i.e., regional labor market characteristics and institutional features of the school system. Using data from five waves of the Quality and Career Survey, I apply an extension of the method of Card and Krueger (1992). The regional differences in returns to schooling between states amount to 1.8 percentage points. Next, I explore the contribution of regional amenities, regional labor demand, and institutional settings to regional heterogeneity. The results show that institutional settings and school quality contribute to state-level differences. I find significant correlations of selectivity of school systems, pupil-teacher ratios, and PISA 2003 test scores with returns to schooling. The latter two may be interpreted as measures of school quality. Section 2 underpins the relevance of school system characteristics for individuals' earnings in their later working life.

Section 3 is co-authored by Kamila Cygan-Rehm and analyzes the effect of education of completed fertility. OLS results on this relationship might be biased because of endogenous selection in education. We apply IV estimations on two data sets (German Microcensus and SOEP) and use the extension of compulsory schooling from 8 to 9 years as instrument for education. Our study is the first that analyzes this causal relationship for Germany. Additionally, we are the first to find a negative effect of schooling on completed fertility in a developed country. Consistent with findings for other developed countries we find that women experience a decline in teenage births because of the

prolonged education. However, women in other developed countries catch up the initial loss in births at later ages. This catch-up effect translates into a zero or positive effect of education on completed fertility. The analysis of underlying mechanisms supports that German women experience a permanent decline in births because of high opportunity costs of child-bearing.

Section 4 evaluates a German parental leave reform that introduced a parental leave benefits wage replacement scheme in 2007. This reform explicitly aims to reduce opportunity costs of child-rearing and promotes the labor market participation of mothers. I analyze reform effects on the subjective well-being of young mothers and add to the existing literature by utilizing a more comprehensive outcome that reveals also unintended side effects. Using Pairfam and Demodiff panel data, I apply a regression discontinuity approach with a diff-in-diff extension. My results suggest that the reform effect depends on eligibility for transfers in the former regime and differs for mothers in East and West Germany. The two German regions respond differently to the policy intervention. Whereas West German women tend to be more satisfied, East German women are less satisfied. An analysis of potential causes reveals that East German mothers reduce marriages after the reform. The reason for this behavioral change may be increased economic independence. Since the German tax system still supports traditional family types and imposes disadvantages for unmarried couples, East German women may be less satisfied after the reform because of the reduction of marriage rates and long-term reductions in net household income.

5.2 Limitations and future research

The main finding in Section 2 is that state-level differences in school systems reinforce differences in individuals' wages in later working life. The results have implications concerning the importance of the design of school systems (e.g., curricula contents) and potential incentives to invest in education. The study in Section 2 is limited by missing valid instruments and the unavailability of exact measures for state school quality. For a

more comprehensive analysis, more detailed data that facilitate IV estimation of returns to schooling at the state level and the provision of high quality data on school quality are necessary. Although Section 2 shows a relationship between the school systems and returns to education, I do not identify exact mechanisms which are of high political interest. Potential determinants include for example the training of teachers (see Section 1.1.2). Also, the literature typically uses additional school quality measures such as teachers' experience or teachers' salary (see, e.g., Card and Krueger; 1996). The use of KMK documentations or a rather time-consuming investigation of regulations within each federal state may facilitate a further analysis of underlying mechanisms (e.g., civil servant salary, curricula contents).

In Section 2 I find a significant correlation of returns to schooling of birth cohorts 1960 to 1980 and PISA test scores of children born in the late 1980s. I cannot disentangle whether PISA test scores may capture school quality in the federal states or high returns to schooling of parents may work as an incentive for children to perform better in school. More research is needed on the contribution of these two mechanisms to students' PISA scores. A data set that facilitates the analysis of the different channels includes both, information on parents' wages and children's PISA scores. Additionally, information on school quality and the possibility to identify states in the data is necessary. Even if such data is not available for Germany, Australian students who participated in PISA 2003 to 2009 became starting cohorts for the *Longitudinal Surveys of Australia Youth* (LSAY). The data allow an investigation of children's PISA test scores and later labor market outcomes (see, e.g., Thomson and Hillman; 2010). The data entail detailed information on family background and school quality, too. If German states do not provide detailed information on school quality, analyses are limited to other countries.

Section 3 emphasizes that German institutions hamper the compatibility of work and family life and may be a driving factor for the negative effect of education on fertility in Germany. As the promotion of labor market participation and fertility rates are important policy goals, our study shows that lowering the opportunity costs of child-bearing may

mitigate negative trends. As we find remarkable differences between Germany and other developed countries, further research on the driving factors such as non-pecuniary returns to education is necessary. Yet there is little consensus about the effect of education on fertility, a meta-analysis may reveal promising policy implications.

Section 3 and 4 underpin that German family policies hamper the compatibility of work and family life. Although recent reforms aim to reduce opportunity costs of child-rearing they may have unintended side effects which impose long-run financial disadvantages. The interrelation of various family policy regulations is the driving factor as, e.g., income splitting still favors traditional family types whereas new earnings-proportional parental leave benefits support the economic independence of mothers. Further, although the 2007 reform reduced opportunity costs of child-rearing, an additional barrier to a full-time labor market participation of mothers are shortages in child care coverage and relatively high costs of formal child care for 0-3 years old children (Da Roit and Sabatinelli; 2007).

A promising route for further research would be to study how child care costs influence the decision of mothers to return to work. The provision of child care facilities has improved substantially over the last decade in Germany and parents even can claim a child care spot since 2013 (Bauernschuster et al.; 2013). Child care costs have been largely neglected as an influence on mothers' labor supply as child care for over 3 years old children is highly subsidized. However, costs for under 3 years old children are considerably higher and I expect costs to affect mothers labor market participation stronger (see, e.g., Da Roit and Sabatinelli; 2007).

The interrelation of high child care costs, income splitting imposing disincentives for employment, and low coverage of child care potentially reinforces a negative effect on mothers' labor market participation. Bäcker et al. (2010) exemplify the work disincentives through income splitting. In their example, also the potential role of child care costs emerges. The authors calculate that a women with gross income of 1,000 EUR gets a monthly net income of only 590 EUR when a couple uses income splitting (tax bracket

III for the husband, tax bracket V for the wife). Thus, with joint taxation mothers are likely to face above-average income tax burdens and disincentives to work especially in low-wage work (Immervoll and Barber; 2006). Monthly costs of formal child care for 0-3 year olds strongly vary by region and socio demographic background and can be up to 400 EUR per month (Da Roit and Sabatinelli; 2007). Although parents can deduct fees for formal child care from their yearly taxable income, in the above-mentioned example additional child care costs create strong disincentives for a return to work.

A huge restriction in the analysis of child care costs is data availability. Although SOEP and Pairfam ask about hours in child care and satisfaction with child care, the main limitation is a lack of data on costs. The 2005 *child care survey* (Kinderbetreuungsstudie) contains information on costs and labor supply of parents. However, more recent waves of the survey are not available yet. Another possible data source that might at least facilitate an analysis of correlation patterns is the *Income and Consumption Survey* (Einkommens- und Verbrauchsstichprobe, EVS). The survey asks about household members, labor market status, marital status, income, and child care expenses. The EVS data is available for 2008; and soon also for 2013.

References

Adair, J. G. (1984). The Hawthorne effect: a reconsideration of the methodological artifact, *Journal of Applied Psychology* **69**(2): 334–345.

Altonji, J. G. and Dunn, T. A. (1996). Using siblings to estimate the effect of school quality on wages, *The Review of Economics and Statistics* **78**(4): 665–671.

Ammermueller, A., Kuckulenz, A. and Zwick, T. (2009). Aggregate unemployment decreases individual returns to education, *Economics of Education Review* **28**(2): 217–229.

Angrist, J. and Pischke, J. (2009). *Mostly harmless econometrics: an empiricist's companion*, Princeton University Press, Princeton, NJ.

Ashenfelter, O., Harmon, C. and Oosterbeek, H. (1999). A review of estimates of the schooling earnings relationship with tests for publication bias, *Labour Economics* **6**(4): 453–470.

Avenarius, H., Ditton, H., Döbert, H., Klemm, K., Klieme, E., Rürup, M., Tenorth, H.-E., Weishaupt, H. and Weiß, M. (2003). *Bildungsbericht für Deutschland - Erste Befunde*, Leske + Budrich, Opladen.

Bäcker, G., Naegele, G., Bispinck, R., Hofemann, K. and Neubauer, J. (2010). *Sozialpolitik und soziale Lage in Deutschland*, VS Verlag für Sozialwissenschaften, Wiesbaden.

Baker, M., Gruber, J. and Milligan, K. (2008). Universal child care, maternal labor supply, and family well-being, *Journal of Political Economy* **116**(4): 709–745.

Bauernschuster, S., Hener, T. and Rainer, H. (2013). Does expanding public child care encourage fertility? County-level evidence from Germany, *Ifo Working Paper 158*, Ifo Institute - Leibniz Institute for Economic Research at the University of Munich, Munich.

Baumgartner, H. J. (2004). Are there any class size effects on early career earnings in West Germany?, *DIW Discussion papers 417*, Deutsches Institut für Wirtschaftsforschung (DIW), Berlin.

Becker, G. S. (1964). *Human Capital - A theoretical and Empirical Analysis, with special Reference to Education*, National Bureau of Economic Research, New York.

Becker, G. S. (1981). *A Treatise on the Family*, Harvard university press, Cambridge.

Bedard, K. (2001). Human capital versus signaling models: university access and high school drop-outs, *Journal of Political Economy* **109**(4): 749–775.

Beeson, P. E. (1991). Amenities and regional differences in returns to worker characteristics, *Journal of Urban Economics* **30**(2): 224–241.

Behrman, J. R. and Rosenzweig, M. R. (2002). Does increasing women's schooling raise the schooling of the next generation?, *The American Economic Review* **92**(1): 323–334.

Bergemann, A. and Riphahn, R. T. (2010). Female labour supply and parental leave benefits - the causal effect of paying higher transfers for a shorter period of time, *Applied Economics Letters* **18**(1): 17–20.

Bergemann, A. and Riphahn, R. T. (2011). The introduction of a short-term earnings-related parental leave benefit system and differential effects on employment intentions, *Schmollers Jahrbuch: Journal of Applied Social Science Studies / Zeitschrift für Wirtschafts- und Sozialwissenschaften* **131**(2): 315–325.

Berger, E. M. (2010). *Maternal Employment, Life Satisfaction, and Child Developmental Outcomes*, PhD thesis, Freie Universität Berlin, Berlin.

Betts, J. R. (1995). Does school quality matter? Evidence from the national longitudinal survey of youth, *The Review of Economics and Statistics* **77**(2): 231–250.

BIS (2009). International review of leave policies and related research 2009, *Employment relations research series 102*, University of London, Department for Business, Innovation and Skills (BIS), London.

Björklund, A. (2006). Does family policy affect fertility?, *Journal of Population Economics* **19**(1): 3–24.

Black, D., Kolesnikova, N. and Taylor, L. (2009). Earnings functions when wages and prices vary by location, *Journal of Labor Economics* **27**(1): 21–47.

Black, S. E., Devereux, P. J. and Salvanes, K. G. (2008). Staying in the classroom and out of the maternity ward? The effect of compulsory schooling laws on teenage births, *The Economic Journal* **118**(530): 1025–1054.

BMFSFJ (2005). Erziehungsgeld, Elternzeit. Das Bundeserziehungsgeldgesetz, *Broschüre*, Bundesministerium für Familie, Senioren, Frauen und Jugend (BMFSFJ), Berlin.

BMFSFJ (2008). Dossier - Elterngeld als Teil nachhaltiger Familienpolitik, *Broschüre*, Bundesministerium für Familie, Senioren, Frauen und Jugend (BMFSFJ), Berlin.

BMFSFJ (2011). Elterngeld und Elternzeit. Das Bundeselterngeld- und Elternzeitgesetz, *Broschüre*, Bundesministerium für Familie, Senioren, Frauen und Jugend (BMFSFJ), Berlin.

Boockmann, B. and Steiner, V. (2006). Cohort effects and the returns to education in West Germany, *Applied Economics* **38**(10): 1135–1152.

Braakmann, N. (2011). Female education and fertility - evidence from changes in British compulsory schooling laws, *Newcastle Discussion Papers in Economics 2011/05*, Newcastle University Business School, Newcastle.

Braun, S., Dwenger, N. and Oechsle, T. (2010). Chancen auf einen Studienplatz: Regionale Herkunft erheblich, *Kiel Policy Briefs 16*, Kiel Institute for the World Economy, Kiel.

Brunello, G., Fort, M. and Weber, G. (2009). Changes in compulsory schooling, education and the distribution of wages in Europe, *The Economic Journal* **119**(536): 516–539.

Burtless, G. (1996). Introduction and summary, *in* G. Burtless (ed.), *Does Money matter? The Effect of School Resources on Student Achievement and Adult Success*, Brookings Intitutions, Washington D.C., pp. 1–42.

Cameron, A. C. and Miller, D. L. (2011). Robust inference with clustered data, *in* A. Ullah and D. E. Giles (eds), *Handbook of Empirical Economics and Finance*, CRC Press, Boca Raton, FL, pp. 1–28.

Card, D. (1999). The causal effect of education on earnings, *in* O. Ashenfelter and D. Card (eds), *Handbook of Labor Economics*, Vol. 3A, Elsevier Science, Amsterdam, chapter 30, pp. 1801–1863.

Card, D. and Krueger, A. (1996). Labor market effects of school quality: theory and evidence, *in* G. Burtless (ed.), *Does Money matter? The Effect of School Resources on Student Achievement and Adult Success*, Brookings Intitutions, Washington D.C., pp. 97–139.

Card, D. and Krueger, A. B. (1992). Does school quality matter? Returns to education and the characteristics of public schools in the United States, *Journal of Political Economy* **100**(1): 1–40.

Clark, A. E., Frijters, P. and Shields, M. A. (2008). Relative income, happiness, and utility: an explanation for the Easterlin Paradox and other puzzles, *Journal of Economic Literature* **46**(1): 95–144.

Cygan-Rehm, K. (2013). Earnings-dependent parental leave benefit and fertility: evidence from Germany, *BGPE Discussion Paper 142*, Friedrich-Alexander-University Erlangen-Nuremberg, Nuremberg.

Cygan-Rehm, K. and Maeder, M. (2013). The effect of education on fertility: evidence from a compulsory schooling reform, *Labour Economics* **25**(0): 35 – 48.

Da Roit, B. and Sabatinelli, S. (2007). The cost of childcare in EU countries, *Transversal analysis part 1 of 2*, European Parliament - Policy Department Economic and Scientific Policy, Brussels.
URL: *http://www.europarl.europa.eu/document/activities/cont/201107/20110718ATT24321/20110718ATT24321EN.pdf*

Deaton, A. (2008). Income, health and wellbeing around the world: evidence from the gallup world poll, *The journal of economic perspectives: a journal of the American Economic Association* **22**(2): 53–72.

Di Tella, R., Haisken-De New, J. and MacCulloch, R. (2010). Happiness adaptation to income and to status in an individual panel, *Journal of Economic Behavior & Organization* **76**(3): 834–852.

Diprete, T. A., Morgan, S., Engelhardt, H. and Pacalova, H. (2003). Do cross-national differences in the costs of children generate cross-national differences in fertility rates?, *Population Research and Policy Review* **22**(5-6): 439–477.

Dostal, W. and Jansen, R. (2002). Qualifikation und Erwerbssituation in Deutschland - 20 Jahre BIBB/IAB-Erhebungen, *Mitteilungen aus der Arbeitsmarkt- und Berufsforschung* **35**(2): 232–253.

Dustmann, C. (2004). Parental background, secondary school track choice, and wages, *Oxford Economic Papers* **56**(2): 209–230.

Dustmann, C. and Schönberg, U. (2012). Expansions in maternity leave coverage and children's long-term outcomes, *American Economic Journal: Applied Economics* **4**(3): 190–224.

EGGE (2009). *The provision of childcare services - A comparative review of 30 European countries*, European Commission Directorate-General for Employment, Social Affairs and Equal opportunities G1 Unit, Luxembourg. European Commission's Expert Group on Gender and Employment Issues (EGGE).
URL: *http://ec.europa.eu/social/BlobServlet?docId=2803&langId=en*

Ertl, H. and Phillips, D. (2000). The enduring nature of the tripartite system of secondary schooling in Germany: some explanations, *British Journal of Educational Studies* **48**(4): 391–412.

Esping-Andersen, G. (1990). *The three worlds of welfare capitalism*, John Wiley & Sons.

Federal Bureau of Statistics (1952-2006). *Statistical Yearbook*, Federal Statistical Office, Wiesbaden.

Federal Bureau of Statistics (2014). Genesis online-database. [Accessed 31. 01. 2014].
URL: *https://www-genesis.destatis.de*

Federal Employment Agency (2009). Statistik der Bundesagentur für Arbeit - Arbeitlosigkeit im Zeitverlauf. [Accessed 20.04.2011].
URL: *http://statistik.arbeitsagentur.de/Statischer-Content/Statistik-nach-Themen/Zeitreihen/Generische-Publikationen/Arbeitslosigkeit-Deutschland-Zeitreihe.xls*

Fendrich, S., Fischer, J. and Schilling, M. (2003). Erziehungsgeld und Elternzeit, *Bericht des Jahres 2003*, Dortmunder Arbeitsstelle Kinder- und Jugendhilfestatistik, Dortmund. Im Auftrag des Bundesministeriums für Familie, Senioren, Frauen und Jugend.

Fort, M. (2009). New evidence on the causal impact of education on fertility, *EEA-ESEM 2009 Congress WP*.

Fort, M., Schneeweis, N. and Winter-Ebmer, R. (2011). More schooling, more children: compulsory schooling reforms and fertility in Europe, *IZA Discussion Paper 2693*, Institute for the Study of Labor (IZA), Bonn.

Gangl, M. and Ziefle, A. (2009). Motherhood, labor force behavior, and women's careers: an empirical assessment of the wage penalty for motherhood in Britain, Germany, and the United States, *Demography* **46**(2): 341–369.

Ganzeboom, H. B. and Treiman, D. J. (1996). Internationally comparable measures of occupational status for the 1988 international standard classification of occupations, *Social Science Research* **25**(3): 201–239.

Geisler, E. and Kreyenfeld, M. (2012). How policy matters: Germany's parental leave benefit reform and fathers' behavior, *Working Paper 2012-021*, Max Planck Institute for Demographic Research, Rostock.

Geruso, M., Clark, D. and Royer, H. (2011). The impact of education on fertility: quasi-experimental evidence from the UK. *mimeo*, Princeton, Princeton University.

Geyer, J., Hann, P. and Wrohlich, K. (2012). Labor supply of mothers with young children: validating a structural model using a natural experimental, *Conference Proceedings of IZA Workshop: Recent Advances in Labor Supply Modeling*, Institute for the Study of Labor (IZA), Bonn.

Goldstein, J. R. and Kreyenfeld, M. (2011). Has East Germany overtaken West Germany? Recent trends in order-specific fertility, *Population and Development Review* **37**(3): 453–472.

Gornick, J. C., Meyers, M. K. and Ross, K. E. (1997). Supporting the employment of mothers: policy variation across fourteen welfare states, *Journal of European social policy* **7**(1): 45–70.

Graves, P. E., Athur, M. M. and Sexton, R. L. (1999). Amenities and the labor earnings function, *Journal of Labor Research* **20**(3): 367–376.

Grönqvist, H. and Hall, C. (2011). Education policy and early fertility: lessons from an expansion of upper secondary schooling, *Working Paper Series 24*, IFAU - Institute for Labour Market Policy Evaluation, Uppsala.

Grossman, M. (1972). On the concept of health capital and the demand for health, *The Journal of Political Economy* **80**(2): 223–255.

Guckelberger, A. (2012). Bildung und Föderalismus, *in* I. Härtel (ed.), *Handbuch Föderalismus - Föderalismus als demokratische Rechtsordnung und Rechtskultur in Deutschland, Europa und der Welt*, Vol. 3, Springer, Heidelberg, chapter 61, pp. 217–246.

Hanel, B. and Riphahn, R. T. (2012). The employment of mothers - recent developments and their determinants in East and West Germany, *Jahrbücher für Nationalökonomie und Statistik (Journal of Economics and Statistics)* **232**(2): 146–176.

Hank, K., Kreyenfeld, M. and Spieß, C. K. (2004). Kinderbetreuung und Fertilität in Deutschland, *Zeitschrift für Soziologie* **33**(3): 228–244.

Hantrais, L. (2004). *Family Policy Matters: responding to family change in Europe*, The Policy Press, Bristol.

Hanushek, E. A. (1981). Alternative models of earnings determination and labor market structures, *The Journal of Human Resources* **16**(2): 238–259.

Hanushek, E. A. (1997). Assessing the effects of school resources on student performance: an update, *Educational Evaluation and Policy Analysis* **19**(2): 141–164.

Harhoff, D. and Kane, T. J. (1997). Is the German apprenticeship system a panacea for the U.S. labor market?, *Journal of Population Economics* **10**(2): 171–196.

Harmon, C. and Walker, I. (2000). The returns to the quantity and quality of education: evidence for men in England and Wales, *Econometrica* **67**(265): 19–35.

Heckman, J., Layne-Farrar, A. and Todd, P. (1996a). Does measured school quality really matter? An examination of the earnings-quality relationship, *in* G. Burtless (ed.), *Does Money matter? The Effect of School Resources on Student Achievement and Adult Success*, Brookings Intitutions, Washington D.C., pp. 192–289.

Heckman, J., Layne-Farrar, A. and Todd, P. (1996b). Human capital pricing equations with application to estimating the effect of schooling quality on earnings, *The Review of Economics and Statistics* **78**(4): 562–610.

Huber, P. (2004). Inter-regional mobility in Europe: a note on the cross-country evidence, *Applied Economics Letters* **11**(10): 619 – 624.

Huinink, J., Brüderl, J., Nauck, B., Walper, S., Castiglioni, L. and Feldhaus, M. (2011). Panel analysis of intimate relationships and family dynamics (pairfam): conceptual framework and design, *Zeitschrift für Familienforschung* **23**(1): 77–101.

Humlum, M. K., Kristoffersen, J. H. and Vejlin, R. (2012). Timing of college enrollment and family formation decisions, *Economics working papers*, School of Economics and Management, University of Aarhus, Aarhus.

Hunt, J. (2004). Are migrants more skilled than non-migrants? Repeat, return, and same-employer migrants, *Canadian Journal of Economics* **37**(4): 830–849.

Hunt, J. (2006). Staunching emigration from East Germany: age and the determinants of migration, *Journal of the European Economic Association* **4**(5): 1014–1037.

Immervoll, H. and Barber, D. (2006). Can parents afford to work? Childcare costs, tax-benefit policies and work incentives, *IZA Discussion Papers 1932*, Institute for the Study of Labor (IZA), Bonn.

Jaeger, D. A. and Page, M. E. (1996). Degrees matter: new evidence on sheepskin effects in the return to education, *The Review of Economics and Statistics* **78**(4): 733–740.

Kaun, A. (2006). Stochastik in deutschen Lehrplänen allgemeinbildender Schulen, *Stochastik in der Schule* **26**(3): 11–15.

Kemptner, D., Jürges, H. and Reinhold, S. (2011). Changes in compulsory schooling and the causal effect of education on health: evidence from Germany, *Journal of Health Economics* **30**(2): 340 – 354.

Kluve, J. and Tamm, M. (2013). Parental leave regulations, mothers' labor force attachment and fathers' childcare involvement: evidence from a natural experiment, *Journal of Population Economics* **26**(3): 983–1005.

KMK (2006). Gesamtstrategie der Kultusministerkonferenz zum Bildungsmonitoring, *Broschüre*, The Standing Conference of the Ministers of Education and Cultural Affairs of the Länder in the Federal Republic of Germany (KMK), Bonn.

KMK (2009). Gegenseitige Anerkennung von Lehramtsprüfungen und Lehramtsbefähigungen, *Information des Sekretariats über die Regelungen des KMK-Beschlusses vom 22.10.1999*, The Standing Conference of the Ministers of Education and Cultural Affairs of the Länder in the Federal Republic of Germany (KMK), Bonn. [Accessed: 06.02.2014].
URL: *http://www.kmk.org/fileadmin/pdf/Bildung/AllgBildung/2009-Informationsschrift-Gegenseitige_Anerkennung.pdf*

KMK (2010a). *The Education System in the Federal Republic of Germany 1999. A Description of Responsibilities, Structures and Developments in Education Policy for the Exchange of Information in Europe*, The Standing Conference of the Ministers of Education and Cultural Affairs of the Länder in the Federal Republic of Germany (KMK), Bonn.

KMK (2010b). Übergang von der Grundschule in Schulen des Sekundarbereichs I und Förderung, Beobachtung und Orientierung in den Jahrgangsstufen 5 und 6 (sog. Orientierungsstufe), *Informationsschrift des Sekretariats der Kultusministerkonferenz, Stand: 18.10.2010* , The Standing Conference of the Ministers of Education and Cultural Affairs of the Länder in the Federal Republic of Germany (KMK), Bonn. [Accessed 02.05.2013].
URL: *http://www.kmk.org/fileadmin/veroeffentlichungen_beschluesse/2010/2010_10_18-Uebergang-Grundschule-S_eI1-Orientierungsstufe.pdf*

KMK (2013). *Das Bildungswesen in der Bundesrepublik Deutschland 2011/2012 - Darstellung der Kompetenzen, Strukturen und bildungspolitischen Entwicklungen für den Informationsaustausch in Europa*, The Standing Conference of the Ministers of Education and Cultural Affairs of the Länder in the Federal Republic of Germany (KMK), Bonn.

Konietzka, D. and Kreyenfeld, M. (2005). Nichteheliche Mutterschaft und soziale Ungleichheit: zur sozioökonomischen Differenzierung der Familienformen in Ost- und Westdeutschland, *Kölner Zeitschrift für Soziologie und Sozialpsychologie* **57**(1): 32–61.

Kreyenfeld, M. (2004). Fertility decisions in the FRG and GDR: an analysis with data from the German Fertility and Family Survey, *Demographic Research* **S3**(11): 275–318.

Kreyenfeld, M., Walke, R., Salzburger, V., Lenke, R., Hensel, T. and Mousavi, B. M. (2013). Demodiff supplement to the pairfam data manual. [Accessed 05.02.2014]. **URL:** *http://www.pairfam.de/fileadmin/user_upload/redakteur/publis/Dokumentation/demodiff/Data-Manual_en_DemoDiff3.0.pdf*

Krueger, A. and Pischke, J.-S. (1995). A comparison of East and West German labor markets before and after unification, *in* L. F. H. Freeman, R. B. & Katz (ed.), *Differences and Changes in Wage Structures*, University of Chicago Press, Chicago, pp. 405–445.

Krupka, D. J. (2009). Location-specific human capital, location choice and amenity demand, *Journal of Regional Science* **49**(5): 833–854.

Lang, K. and Kropp, D. (1986). Human capital versus sorting: the effects of compulsory attendance laws, *The Quarterly Journal of Economics* **101**(3): 609–624.

Lauer, C. and Steiner, V. (2001). Germany, *in* C. Harmon, I. Walker and N. Westergaard-Nielsen (eds), *Education and earnings in Europe: a cross country analysis of the returns to education*, Elgar, Cheltenham, pp. 102–127.

Lavy, V. and Zablotsky, A. (2011). Mother's schooling, fertility, and children's education: evidence from a natural experiment, *NBER Working Paper 16856*, National Bureau of Economic Research (NBER), Cambridge.

Layard, R. and Psacharopoulos, G. (1974). The screening hypothesis and the returns to education, *The Journal of Political Economy* **82**(5): 985–998.

Lee, K. S., Alwin, D. F. and Tufiş, P. A. (2007). Beliefs about women's labour in the reunified Germany, 1991-2004, *European Sociological Review* **23**(4): 487–503.

Lee, S. (2010). Ability sorting and the consumer city, *Journal of Urban Economics* **68**(1): 20–33.

Leschinsky, A. and Roeder, P. M. (1980). Didaktik und Unterricht in der Sekundarschule I seit 1950 - Entwicklung der Rahmenbedingungen, *in* P. B. Max-Planck-Institut für Bildungsforschung (ed.), *Bildung in der Bundesrepublik Deutschland - Daten und Analysen, Band 1: Entwicklungen seit 1950*, Klett-Cotta, Stuttgart, chapter 4, pp. 283–392.

Lucas, R. E. and Clark, A. E. (2006). Do people really adapt to marriage?, *Journal of Happiness Studies* **7**(4): 405–426.

Luechinger, S. (2009). Valuing air quality using the life satisfaction approach, *The Economic Journal* **119**(536): 482–515.

Machin, S., Salvanes, K. G. and Pelkonen, P. (2012). Education and mobility, *Journal of the European Economic Association* **10**(2): 417–450.

McCrary, J. and Royer, H. (2011). The effect of female education on fertility and infant health: evidence from school entry policies using exact date of birth, *American Economic Review* **101**(1): 158–195.

Meyer, B. D. and Sullivan, J. X. (2004). The effects of welfare and tax reform: the material well-being of single mothers in the 1980s and 1990s, *Journal of Public Economics* **88**(7): 1387–1420.

Mincer, J. A. (1974). *Schooling, Experience, and Earnings*, National Bureau of Economic Research, New York.

Monstad, K., Propper, C. and Salvanes, K. G. (2008). Education and fertility: evidence from a natural experiment, *Scandinavian Journal of Economics* **110**(4): 827–852.

Morgan, K. J. and Zippel, K. (2003). Paid to care: the origins and effects of care leave policies in Western Europe, *Social Politics: International Studies in Gender, State & Society* **10**(1): 49–85.

Neugart, M. and Ohlsson, H. (2013). Economic incentives and the timing of births: evidence from the German parental benefit reform of 2007, *Journal of Population Economics* **26**(1): 87–108.

OECD (2009). Pf1.1: Public spending on family benefits. OECD - Social Policy Division - Directorate of Employment, Labour and Social Affairs, Paris.

OECD (2011). *How's Life?: Measuring well-being*, OECD Publishing, Paris.

OECD (2012). *Education at a Glance 2012: OECD Indicators*, OECD Publishing, Paris. **URL:** *http://dx.doi.org/10.1787/eag-2012-en*

OECD (2013a). *Education at a Glance 2013: OECD Indicators*, OECD Publishing, Paris. **URL:** *http://dx.doi.org/10.1787/eag-2012-en*

OECD (2013b). OECD Family Database, Organisation for Economic Co-operation and Development (OECD), Paris. [Accessed: 11.12.2013].
URL: *http://www.oecd.org/els/soc/oecdfamilydatabase.htm*

Oreopoulos, P. and Salvanes, K. G. (2011). Priceless: the nonpecuniary benefits of schooling, *Journal of Economic Perspectives* **25**(1): 159–184.

Osili, U. O. and Long, B. T. (2008). Does female schooling reduce fertility? Evidence from Nigeria, *Journal of Development Economics* **87**(1): 57–75.

Petzold, H.-J. (1981). *Schulzeitverlängerung: Parkplatz oder Bildungschance? Die Funktion des 9. und 10. Schuljahres*, Päd.-Extra-Buchverlag, Bensheim.

PISA-Konsortium Deutschland (ed.) (2002). *PISA 2000 Ein differenzierter Blick auf die Länder der Bundesrepublik Deutschland*, Leske + Budrich, Opladen.

PISA-Konsortium Deutschland (ed.) (2005). *PISA 2003: Der zweite Vergleich der Länder in Deutschland - Was wissen und können Jugendliche?*, Waxmann, Münster.

Pischke, J.-S. (2007). The impact of length of the school year on student performance and earnings: evidence from the German short school years, *The Economic Journal* **117**(523): 1216–1242.

Pischke, J.-S. and von Wachter, T. (2008). Zero returns to compulsory schooling in Germany: evidence and interpretation, *The Review of Economics and Statistics* **90**(3): 592–598.

Pluennecke, A., Riesen, I. and Stettes, O. (2007). *Bildungsmonitor - Forschungsbericht*, Deutscher Instituts-Verlag, Köln.

Psacharopoulos, G. and Patrinos, H. A. (2004). Returns to investment in education: a further update, *Education Economics* **12**(2): 111–134.

Puhani, P. A. and Sonderhof, K. (2011). The effects of parental leave extension on training for young women, *Journal of Population Economics* **24**(2): 731–760.

Reilich, J. (2013). Regionale Bildungsrenditen und weitere Disparitäten in Deutschland, *Jahrbuch für Regionalwissenschaft* **33**(1): 49–72.

Riphahn, R. T. and Trübswetter, P. (2013). The intergenerational transmission of educational attainment in East and West Germany, *Applied Economics* **45**(22): 3183–3196.

Riphahn, R. T. and Wunder, C. (2013). Patterns of welfare dependence before and after a reform: evidence from first generation immigrants and natives in Germany, *Review of Income and Wealth* **59**(3): 437–459.

Roback, J. (1982). Wages, rents, and the quality of life, *The Journal of Political Economy* **90**(6): 1257–1278.

Rosenzweig, M. R. and Schultz, T. P. (1989). Schooling, information and nonmarket productivity: contraceptive use and its effectiveness, *International Economic Review* **30**(2): 457–477.

Rösner, E. (1999). *Das Schulsystem in Deutschland - Kritische Befunde zur Schulstruktur der Sekundarstufen*, Bergmann + Helbig Verlag, Hamburg.

Ruhm, C. J. (2000). Parental leave and child health, *Journal of Health Economics* **19**(6): 931–960.

Sainsbury, D. (1999). *Gender and welfare state regimes*, Oxford University Press, USA, New York.

Salles, A., Rossier, C. and Brachet, S. (2010). Understanding the long term effects of family policies on fertility: the diffusion of different family models in France and Germany, *Demographic Research* **22**(34): 1057–1096.

Silles, M. A. (2011). The effect of schooling on teenage childbearing: evidence using changes in compulsory education laws, *Journal of Population Economics* **24**(2): 761–777.

Spence, M. (1973). Job market signaling, *Quarterly Journal of Economics* **87**(3): 355–374.

Spiess, C. and Wrohlich, K. (2008). The parental leave benefit reform in Germany: costs and labour market outcomes of moving towards the Nordic model, *Population Research and Policy Review* **27**(5): 575–591.

Staiger, D. and Stock, J. H. (1997). Instrumental variables regression with weak instruments, *Econometrica* **65**(3): 557–586.

Tamm, M. (2013). The impact of a large parental leave benefit reform on the timing of birth around the day of implementation, *Oxford Bulletin of Economics and Statistics* **75**(4): 585–601.

Tanaka, S. (2005). Parental leave and child health across OECD countries, *Economic Journal* **115**(501): F7–F28.

Thévenon, O. (2013). Drivers of female labour force participation in the OECD, *Employment and Migration Working Papers 145*, Organisation for Economic Co-operation and Development (OECD), Paris.

Thomson, S. and Hillman, K. (2010). *Against the Odds: Influences on the Post-School Success of "Low Performers"*, National Centre for Vocational Education Research (NCVER), Adelaide.

Thurow, L. (1970). *Investment in Human Capital*, Wadsworth, Belmont.

van Ackeren, I. and Klemm, K. (2009). *Entstehung, Struktur und Steuerung des deutschen Schulsystems - Eine Einführung*, VS Verlag, Wiesbaden.

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

Waldfogel, J. (2001). International policies toward parental leave and child care, *The Future of Children* **11**(1): 98–111.

Winkelmann, L. and Winkelmann, R. (1998). Why are the unemployed so unhappy? Evidence from panel data, *Economica* **65**(257): 1–15.

Woessmann, L. (2010). Fundamental determinants of school efficiency and equity: German states as a microcosm for OECD countries, *Jahrbücher für Nationalökonomie und Statistik* **230**(2): 234–270.

Wolf, F. (2006). Bildungspolitik: Föderale Vielfalt und gesamtstaatliche Vermittlung, *in* R. Zohlnhöfer (ed.), *Regieren in der Bundesrepublik Deutschland: Innen- und Aussenpolitik seit 1949*, Springer, Wiesbaden, pp. 221–241.

World Bank (2013). World Development Indicators. [Accessed: 03.08.2013].
URL: *http://data.worldbank.org/indicator*

Wrohlich, K. (2008). The excess demand for subsidized child care in Germany, *Applied Economics* **40**(10): 1217–1228.

Wrohlich, K., Berger, E., Geyer, J., Haan, P., Sengül, D., Spieß, C. K. and Thiemann, A. (2012). Elterngeld Monitor, *Politikberatung kompakt 61*, Deutsches Institut für Wirtschaftsforschung (DIW), Berlin.

Ziefle, A. (2009). *Familienpolitik als Determinante weiblicher Lebensverläufe? Die Auswirkungen des Erziehungsurlaubs auf Familien- und Erwerbsbiographien in Deutschland*, VS Verlag für Sozialwissenschaften, Wiesbaden.