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The length of schooling and the timing of family formation[☆]

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Abstract

Individuals typically traverse several life phases before forming a family. We analyse whether changing the duration of one of these phases, the education phase, affects the timing of marriage and childbearing. For this purpose, we exploit the introduction of short school years in Germany in 1966-67, which compressed the education phase without affecting the curriculum. Based on difference-in-differences regressions and German Micro Census data, we find that short school year exposure affects the timing of marriage for individuals in all secondary school tracks and shifts forward the birth of the first child mainly for academic-track graduates. This highlights that education policies might not only affect family formation through human capital accumulation, but also through changing the duration of earlier life phases. This is important as not only age at marriage and first birth increases in many countries, but also the duration of the education phase.

Keywords: family formation, instruction time, fertility, marriage

JEL: I26, J12, J13, J24

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1. Introduction

Almost all industrialized countries see a secular trend toward postponing family formation to later ages (OECD, 2019) – with important economic consequences. Postponed childbearing decreases – all else equal – the number of children born per year, putting the funding of social security systems under pressure (e.g. Billari et al., 2006). The timing of marriages and first births also have large impacts on other economic decisions, including on savings and the spacing of subsequent births (Díaz-Giménez and Giolito, 2013; Hodsdon and Marini, 2019). Additionally, medical costs increase with later child births due to adverse health effects (e.g. Gustafsson, 2001; Myrskylä and Fenelon, 2012) and because fecundity declines with age (Larsen and Vaupel, 1993) making fertility treatments and involuntary childlessness more likely.

This paper contributes to our understanding of whether and how policies that alter the duration of specific life phases can act as drivers of marriage and fertility timing. In most developed countries, individuals traverse several life phases in a rather strict order before they form a family (Blossfeld and Rose, 1992; Billari et al., 2000; Lutz and Skirbekk, 2005; Huinink and Kohli, 2014). Primary and secondary schooling precede tertiary education, education phases typically precede labor market entry and labor market entry typically precedes family formation. Given the sequencing of life phases, there is surprisingly little research on the consequences of extending or reducing one of these phases. However, analyzing the duration of earlier life phases is not only relevant in explaining later childbearing and decreasing fertility rates, but it may also offer a potential lever for public policies. We study a policy change in Germany that reduced the length of the education phase. This policy allowed entire cohorts to graduate from secondary school about eight months earlier – with the same degree and the same curriculum. Before the reform, the school year started in spring in some German states and in fall in other states. After a policy change to harmonize the education system across states, school started in fall in all states. States achieved the shift of the school year start from spring to fall by the introduction of so-called short school years (*Kurzschuljahre*), in which two school years were put in about 16 calendar months in 1966/67. Pischke (2007) analyses this reform as well. He finds that the short school years did not have any negative impacts on human capital acquisition as it reduced neither labor income nor employment prospects; a finding that we replicate in this paper. Further, Braakmann (2010) shows that there are no effects of the reform on health outcomes. Similar to Pischke (2007) and Braakmann (2010), we exploit this reduction of the length of schooling in a difference-in-differences framework, where we compare cohorts before and after the reform in affected states with the same cohorts in states that did not introduce short school years. We pool

several cross-sections of the German Micro Census, a one percent sample of the German population, to study the effect of this reform on the timing of marriage and fertility.

We find that short school year exposure affects the timing of marriage and child birth. More specifically, we show that the short school years increase the probability to be married eight years after the normal graduation age by about 3.7 percentage points (pp) for individuals in the middle track and by about 4.1 pp for individuals in the academic track (compared to sample means of 43 and 37 percent, respectively). Moreover, individuals in the academic track are also 3.2 pp more likely to have a first child eight years after graduation (compared to a sample mean of 22 percent). These effects do not only hold eight years after graduation but also five years after graduation and up to ten years after graduation. However, the effects fade over time, indicating that the short school years affect the timing of marriage and parenthood, but not the probability to ever marry or become a parent. We further show that the obtained effects are driven by both males and females. Additionally, we find suggestive evidence that the reform also affects subsequent births and completed fertility for individuals in the academic track. Our findings provide evidence that policies that change the duration of specific life phases can affect family formation.

Our study contributes to the literature on the relationship between education and family formation. There is ample evidence that higher levels of education are associated with later childbearing (see, e.g., Skirbekk, 2008) and marriage (see, e.g., Jejeebhoy, 1995; Oppenheimer, 1997) in various countries and time periods. The literature discusses mainly lock-in and human capital effects as mechanisms why education can causally affect family formation. The lock-in effect means that individuals are less likely to marry and give birth while in school (Black et al., 2008), e.g. due to a high degree of economic dependence on the parents or the incompatibility of child rearing and acquiring education (Blossfeld and Rose, 1992). The human capital effect relates to the idea that education increases labor market opportunities and, thereby, the opportunity costs of children (Becker, 1981).¹ While the former mechanism relates only to family formation during education, the latter mechanism looks at family formation after the education phase.

In order to empirically test whether the negative education-fertility relationship is causal, many empirical studies capitalize on unintended fertility consequences induced by educational reforms. Most studies use exogenous variation from laws changing age at school entry or compulsory schooling reforms. The effect of education using school entry rules is found

¹Becker's theoretical approach targets marriage and fertility behavior alike, as his approach regards child production and rearing as the main purpose of marriage. He formulates the argument of sex-specific division of labor as an incentive to enter into marriage and also with respect to the decision to have children.

to be more profound with respect to teenage pregnancies (Black et al., 2011; Tan, 2017), while McCrary and Royer (2011) find little evidence for school entry policies affecting age at first birth. Almost all studies on compulsory schooling reforms provide evidence that longer educational attainment leads to postponement of first births (see, e.g., Black et al., 2008; Monstad et al., 2008; Silles, 2011; Cygan-Rehm and Maeder, 2013; Grönqvist and Hall, 2013). The empirical evidence regarding the effect on completed fertility is more mixed. While some studies find that education decreases completed fertility (see Cygan-Rehm and Maeder (2013); Fort et al. (2016) for England), other studies show that education has no effect on completed fertility (Monstad et al. (2008); Fort et al. (2016) for Continental Europe). Furthermore, Devereux and Tripathi (2009) find that increasing the length of compulsory schooling also leads to higher ages at first marriage. There are also some studies that exploit institutional changes at higher levels of the educational system. Currie and Moretti (2003) and Kamhöfer and Westphal (2019) use college expansions in the U.S. and Germany, respectively, as an instrument for education. Currie and Moretti (2003) find that higher educational attainment reduces completed fertility and Kamhöfer and Westphal (2019) find that increasing education affects the timing of childbirth and reduces the probability of becoming a mother.

We contribute to the literature on the relationship between education and family formation by proposing a third causal mechanism, a duration effect. Education might affect family formation since it affects the timing of subsequent life phases (in particular, labor market entry), which individuals typically traverse before forming families. It is very difficult to separate this duration effect from the human capital effect and, actually, all studies that rely on post-education effects of compulsory schooling reforms and college expansions look at the combined human capital and duration effect. The short school years are, therefore, a particular policy reform as (i) this reform allowed for earlier graduation from school without affecting the curriculum; and (ii) previous empirical studies find no evidence that this reform had adverse effects for human capital acquisition (Pischke, 2007; Braakmann, 2010). The idea of the duration effect of education is also in line with the finding of Humlum et al. (2017) that delayed college enrollment leads to the postponement of marriages and childbearing.

Our study also contributes to the literature on policies that affect family formation by highlighting the importance of unintended consequences of policies that reduce or extend specific life phases. While pro-natalist and pro-marriage policies are highly controversial (see, e.g., Cherlin, 2003), it is important to know whether and how existing policies affect family formation, irrespective of the normative standpoint. Many empirical fertility studies focus on the impact of specific family policies including direct financial transfers like child

allowances and fiscal incentives (e.g., Björklund, 2006) as well as work-related family policies like parental leave benefits (e.g., Lalive and Zweimüller, 2009; Cygan-Rehm, 2016; Kluge and Schmitz, 2018; Raute, 2019) and child care availability (e.g., Rindfuss et al., 2010; Mörk et al., 2013; Bauernschuster et al., 2016). In her literature review, Gauthier (2007) concludes that several family policies are found to increase fertility but that the magnitude of these effects is small. There are also several studies that deal with the effect of specific policies on the marital status. These policies almost exclusively focus on financial incentives; for instance, tax penalties (Alm and Whittington, 1997; Baker et al., 2004) and benefits (Fink, 2020), welfare expansion (Halla et al., 2016), the elimination of survivors insurance (Persson, 2020), and cash-on-hand marriage subsidies (Frimmel et al., 2014). While the majority of these studies provide evidence that financial incentives affect the timing of marriage, only some find effects on the probability to ever marry.

Our study emphasizes that family formation is not only affected by pro-natalist or pro-marriage policies, but also as a side effect of other policies and institutional features. Related to the idea that couples prefer to achieve a certain level of financial security before childbirth, Auer and Danzer (2016) show that for women in Germany, starting a career with a fixed-term contract is associated with postponement of first births and a lower number of children, even after ten years. Similarly, policies that alter the duration of the education phase might affect family formation. This is not only important for policymakers to keep in mind when discussing education reforms, but it also might offer a tool for those who would like to change the timing of family formation. Further, our study points out that the consequences for family formation should be considered when discussing policies that affect the duration of specific life phases. This is important, for instance, in current debates about the European Bologna reform shortening the time to a first university degree (Hahm and Kluge, 2019), the suspension (and re-introduction) of compulsory military service in several countries (Imbens and van der Klaauw, 1995; Bauer et al., 2012), the German G8 reform shortening the schooling phase (Huebener and Marcus, 2017; Marcus and Zambre, 2019), and the general education expansion occurring in many developed and developing countries.

Therefore, our study contributes to both the literature on the relationship between education and family formation as well as the literature on policies that affect family formation. An additional contribution that this study has to offer is the detailed compilation of dates (and primary sources) for several relevant education reforms in West Germany, where there is some ambiguity in the previous literature. These reforms do not just include the short school years but also regulations regarding school entry ages, the beginning of the school year, and compulsory schooling. The collection of reform dates and law sources allows

for isolating the short school year reform from previous changes of the school year start and assigning the short school years more precisely compared to previous studies. This framework that we propose in the accompanying discussion paper Koebe and Marcus (2020) can also be used by other researchers.

The remainder of the paper is structured as follows: Section 2 describes the institutional setting and Section 3 the data, while Section 4 outlines the empirical strategy. This is followed by the main results in Section 5, sensitivity analyses in Section 6 and additional results on human capital, gender differences, longer time horizons, and subsequent births in Section 7. Section 8 concludes.

2. Institutional background

We study a policy change in West Germany in 1966/67 that reduced the length of the education phase by introducing short school years (SSY). These short school years compressed two school years into 16 calendar months. The SSY were introduced in an effort to harmonize the start of the school year across states.

In October 1964, the Ministers of Education of the West German federal states decided, in what is known as the Hamburg Accord (*Hamburger Abkommen*), that the school year would begin in the fall in all eleven states (Froese, 1969, pp.327-323). Before this decision, Easter marked the begin of the school year in most states, while in Bavaria the school year began in fall.²

Seven states (Baden-Württemberg, Bremen, Hesse, North Rhine-Westphalia, Rhineland-Palatinate, Saarland, Schleswig-Holstein) achieved the shift of the school year start by introducing so-called short school years (*Kurzschuljahre*), in which two school years were put in about 16 calendar months (see Helbig and Nikolai, 2015, p.70-73): The first SSY started on April 1, 1966, and ended on November 30, 1966, while the second SSY year started on December 1, 1966, and ended on July 31, 1967. Due to these short school years, affected

² This was actually not the first change of the start of the school year: During the Nazi regime, in 1941, the start of the school year was shifted to a common start in fall (see *Reichsgesetzgebung* in 1941 in Table B.12 in Koebe and Marcus (2020)), after the Second World War most federal states successively switched back to a starting date at Easter (KMK, 1962). In February 1955, the states' Ministers of Education proclaimed Easter as a uniform start of the school year across all federal states in the so-called Düsseldorf Accord (*Düsseldorfer Abkommen*) (Froese, 1969, pp.307-311). However, the Bavarian parliament voted against the implementation of this resolution. As a result, children in Bavaria have started their school year in the fall ever since 1941. For a comprehensive collection of schooling laws related to the shift of the start of the school year in the German federal states, see Appendix B in Koebe and Marcus (2020).

individuals graduated about two-thirds of a school year earlier (or about eight months of calendar time), but with the same degree and curriculum taught.³

Three states did not introduce SSY: Bavaria (where schools already started in fall before the Hamburg Accord) as well as Hamburg and West-Berlin. The two latter states opted for a long school year to transition to the uniform start in fall. In both states, students who were in their graduating year and had once begun their school career with a school start at Easter also graduated in March. Hence, students from Berlin and Hamburg attended the regular amount of time required without any school year reductions despite the school year transition to a start in the fall.

In West Germany, students in all states opt for one of three secondary school tracks after four years of joint primary schooling at around age ten (see Dustmann et al. (2017) for a detailed synopsis on the German tracking system): basic (*Hauptschule*, grades 5-8/9), middle (*Realschule*, grades 5-10), or academic track (*Gymnasium*, grades 5-13). Generally, the shifting of the school year start in 1966/67 affected students in primary school as well as in all secondary school tracks. Several states changed compulsory schooling regulations during our observation period from eight to nine years (Backhaus, 1963; Leschinsky and Roeder, 1980; Petzold, 1981). These changes mainly affect students in basic track and we discuss compulsory schooling regulations in more detail in Koebe and Marcus (2020) and potential consequences for our estimates in the robustness section (Section 6).

Lower Saxony was the only state that differentiated between tracks in terms of SSY exposure. Basic track students were unaffected by SSY, as for graduating classes their short school year losses were added in their final year. Depending on their school starting cohort, middle track students were ambiguously affected by SSY due to changing regulations with respect to the graduating classes in the years after the short school years,⁴ while academic track students were fully affected by the policy reform without any school year extensions for graduating classes. Pischke (2007) assigns the seven states that introduced short school years to the treatment group, while he assigns the three states that did not introduce short school years to the control group. Lower Saxony is partly assigned to the treatment group and partly due to the control group, depending on the institutional details described above.

³Pischke (2007) provides evidence that the curriculum was not affected by SSY, meaning that human capital acquisition is likely to be unaffected.

⁴As described in more detail by Pischke (2007), students entering their final year with the first SSY were exposed to one SSY, the next three cohorts starting their final year in December 1966 through August 1968 were exposed to two SSY. The next six cohorts, however, who were in their last school year from August 1969 to August 1974 were subject to school year extensions such that they graduated from March 1971 until March 1976 after the regular amount of schooling of 10 years. See Table B.7 in Koebe and Marcus (2020) for an overview.

3. Data

Our analysis uses mainly data from the German Micro Census (RDC, 2019), a one percent sample of all German households. Once drawn for the survey, participation is mandatory and, hence, selective non-response and attrition is not a concern. We use the scientific use file, a 70 percent random sample of the data, and the 18 waves from 1976 to 2003.⁵ The data set is well-suited for our analysis as it contains rich information on family structure, marriage, and education. Importantly, each of the 18 waves includes about 300,000 to 400,000 individual observations in the West German states, providing a large number of observations in target cohorts and allowing for a precise estimation of reform effects.

For auxiliary analyses, we make use of two additional data sets that allow us to overcome shortcomings of the Micro Census with respect to potential measurement error due to remarriage, regional mobility, family relationships, and residence of children. First, we make use of the Socio-Economic Panel (SOEP), a representative, multi-cohort survey asking all individuals in selected households since 1984 about a variety of topics (Goebel et al., 2019). Because the same people are surveyed every year, it is possible to track individual marriage biographies as well as changes in an individual’s state of residence.⁶ Second, we draw on the DJI Family Survey of 1988 (Bertram, 1991), which is likewise a representative survey asking 18-55-year old individuals from West Germany about their family relationships.⁷

3.1. Outcome measures

Our main outcome variables are based on the Micro Census and relate to the timing of marriage and parenthood. More specifically, we look at the probabilities to be married and to have a child in or before period $p \in \{5, 8, 10\}$, i.e., five, eight, and ten years after the regular graduation age. We focus on specific years after graduation as patterns of family formation are more similar across tracks when looking at years after graduation and not at specific ages (see Section 3.5).⁸

⁵The Micro Census was conducted in 1976, 1978, 1980, 1982, 1985, 1987, 1989, 1991, 1993, and in all years from 1995 to 2003. 2003 is the last Micro Census wave in our main sample as we focus on respondents up to age 39 and on birth cohorts up to 1964.

⁶We apply the sample restrictions as in the Micro Census (see below). This results, however, in a much smaller sample size compared to the Micro Census of 3,620 individuals for whom we have information on remarriage before age 40 and 3,964 individuals for whom we observe residential behaviour.

⁷Again, we apply the sample restrictions as in the Micro Census (see below), which leads to a sample size of 1,904 individuals (with 3,211 children) for whom we know the nature of their family relationship and 1,737 individuals (with 2,950 children) for whom we identify their current residence.

⁸The results are very similar, when we look at specific ages (see Table A.1 in the Appendix). While focusing on specific years after graduation means that individuals in different tracks are evaluated at different ages, it allows for a more compact layout of the main regression tables.

More formally, the outcome variables $Y^{e,p}$ are defined as

$$Y^{e,p} = \begin{cases} 1 & \text{if } e \text{ in } t \leq p \\ 0 & \text{otherwise,} \end{cases} \quad (1)$$

where $t \in [1, 10]$ indicates the time (measured in years after the regular graduation age) the event $e \in \{\text{marriage, parenthood}\}$ took place. The regular graduation age refers to the age at which individuals usually graduate from a specific track, i.e. in the absence of short school years: age 15 when in basic track, at age 16 when in middle track, and at age 19 when in academic track. In further analyses, we also look at other time intervals.

We construct the marriage outcomes based on the wedding year of the current marriage as provided in the Micro Census. For the construction of the fertility outcomes we make use of the birth information of all children in the household. Based on this information, we calculate the parents' age at the birth of their first child as the difference between the year of birth of the oldest child in the household and that of the parents.

3.2. Treatment assignment

We basically follow Pischke (2007) in assigning the treatment variable and restricting the sample, but make some small changes to allow for a more precise assignment of the length of schooling.⁹ SSY exposure depends on three characteristics: the federal state of students' school location, students' school starting cohort, and the secondary school track (see Section 2). In principle, SSY affected all students in treatment states who were enrolled in primary or secondary school in 1966/67. However, at a given point in time, additional cohorts are enrolled in academic and middle track compared to the basic track, as the basic track caters grades 5-8 or 5-9,¹⁰ the middle track grades 5-10, and the academic track grades 5-13.

Figure 1 is a stylized graph that visualizes SSY exposure in treatment states and the resulting years spent in school until graduation, depending on individuals' secondary school track and school starting cohort. A student entering primary school in a treatment state in 1952 had already left secondary school by the start of the first SSY in 1966, irrespective of the attended track. The same holds for the school starting cohorts 1953 and 1967-1970. For the school starting cohorts 1954-1958, however, the choice of secondary school track matters for SSY exposure. For instance, students from the 1957 school starting cohort were exposed to

⁹Pischke (2007) finds that SSY exposure had no adverse long-term effect on human capital acquisition and we can replicate this finding using his sample restrictions (see Table A.2 in the Appendix).

¹⁰Several states increased the number of years of compulsory schooling from eight to nine (see the discussion in Section 6 and Koebe and Marcus (2020)).

two SSY in academic track, while they were exposed to only one SSY in middle track and no SSY in basic track. For school starting cohorts 1959-1966, students in all school forms were exposed to two SSY and, hence, graduated two-thirds of a school year earlier, while the four school starting cohorts 1964, 1965, 1966 (first SSY), and 1966 (second SSY) were all exposed to the reform during primary school. In sum, SSY exposure depends on federal state, school starting cohort, and secondary school track; consequently, we assign the treatment variable based on these three characteristics.

Our treatment variable takes on the value 1 if an individual was exposed to two SSY and the value 0 if an individual was not exposed to SSY. Individuals who were in the last year of secondary school when SSY were introduced as well as individuals who started primary school with the second SSY were only exposed to one SSY. For these individuals, the treatment variable is set to 0.5.

3.3. Measurement error in key variables

This section discusses the extent of measurement error in key variables and potential consequences for the estimation results. We begin by discussing measurement error in the outcome variables and continue with potential measurement error in the assignment of the treatment indicator.

Measurement error in outcome variables

We construct the marriage outcomes based on the wedding year of the current marriage. While divorced and widowed individuals are also asked about the year of their last marriage, there is no information on whether the current marriage is also respondents' first marriage. Hence, remarriages can induce measurement error in our marriage outcomes, as we do not assign the correct year of first marriage. We limit the extent of measurement error by considering only the information of respondents up to age 40. To provide some information on the extent of remarriages in our sample, we resort to the SOEP data. Figure 2a shows that about 7 percent of individuals remarried below age 40. This share is slightly lower in the academic track (4.4 percent) and slightly higher in the basic track (8.3 percent). Hence, for about 7 percent of individuals, we do not assign the correct year of first marriage.¹¹

There is also measurement error in the fertility outcomes. In our data, individuals are not directly asked about their biological children. However, birth information is available for

¹¹Note that not all of these 7 percent actually constitute measurement error in our marriage outcomes. For instance, if an individual's first marriage is three years after graduation and their second marriage is seven years after graduation, we still correctly code the outcome relating to being married eight and ten years after graduation.

all children in the household. Hence, there is measurement error in households that adopted the oldest child or households in which the oldest child died or has left the household. This measurement error is of particular concern as older children are more likely to move out of the household. Therefore, we restrict the analyses to individuals who are up to 39 years at the time of the interview, as it is less likely they have children who have already left the household.¹² In order to provide information on family structure for cohorts of our main sample, we make use of the DJI Family Survey 1988. This survey asks each respondent for every child whether or not child is biological, child of partner, adopted, or foster child as well as the current residence of each child. Figure 2b shows that nearly all children (i.e., about 95 percent for male respondents and 98 percent for female respondents) from a parent belonging to our main sample are biological children. Figure 2c further reveals that when restricting age to below 40 at the time of the interview, nearly all children live in the same household with their parents (about 97 percent). In the robustness section, we show that our results are insensitive to alternative cut-off ages.

What are the consequences of these types of measurement error in the dependent variable? The answer to this question depends on whether the measurement error is related to our key explanatory variable, the introduction of short school years, or not. Assuming that remarriages are unrelated to short school years, the measurement error is random and, hence, does not bias our estimators of SSY exposure (Wooldridge, 2015). However, if SSY exposure not only increases the chances to be married at a given age but also the chances to be remarried at a given age, we will underestimate the effect of SSY exposure on first marriages. Similarly, if SSY exposure not only decreases the age at child birth but also the age at which children move out of the household, the SSY effect will be biased toward zero: Some respondents are assigned to have no child at a given age (because the child already moved out) and this incorrect assignment happens (slightly) more often for treated individuals.

Generally, we face a trade-off between looking at longer time horizons and systematic measurement error in our outcome variables due to children moving out of the household and remarriages. Therefore, our main specification focuses on outcomes up to five, eight, and ten years after graduation. For these outcomes, measurement error will be smaller compared to outcomes like higher order parities or completed fertility, which we examine in Section 7.3.

¹²This cut-off below 40 is recommended by Krapf and Kreyenfeld (2015), who compare the number of children based on this procedure with the number of biological children.

Measurement error in right-hand side variables

When assigning the treatment variable, we must deal with several challenges due to the nature of the Micro Census data. First, we only have information on the current state of residence, but not on the state where an individual went to school. This is a common issue when working with the Micro Census, but residential mobility across states is very low in Germany and the current state of residence is found to be a good proxy for the state in which an individual went to school (see, e.g., Pischke, 2007; Pischke and von Wachter, 2008; Jürges et al., 2011).

To provide information on regional mobility, we again rely on the SOEP and examine whether individuals in our target group move between federal states. We see that a share of 9.7 percent changed their state of residence at least once in the SOEP. Figure 2d plots this share by secondary school track. As expected, residential mobility is higher among individuals with an academic track degree. Residential mobility does not necessarily lead to an incorrect assignment of the SSY treatment indicator. While individuals might move to a different state, they might later move back to the previous state. Further, we also correctly assign the treatment indicator for individuals who move from one treatment state to another treatment state and for individuals who move from one control state to another control state. However, for individuals who move from a treatment state to a control state (or vice versa), we incorrectly assign the treatment indicator. Again, the extent of this measurement error is limited by considering only respondents below the age of 40.

Second, retrospective information on the attended school track in a given year is not provided in the Micro Census. However, switching tracks and degree upgrading as adults is not very common in the analyzed cohorts in Germany (Dustmann et al., 2017). Generally, students in secondary schooling may change school tracks at any grade. However, based on a School Census for two states, Dustmann et al. (2017) find that only few students (about 2 %) make use of this opportunity. Switching tracks is another common issue when working with this data set and we follow the procedure of previous studies (see, e.g., Pischke, 2007; Pischke and von Wachter, 2008; Jürges et al., 2011; Cygan-Rehm and Maeder, 2013), assigning the school track based on information on the highest secondary school degree obtained. Again, the age limit of 40 reduces the extent of measurement error.

Third, information on grade repetition is not available. Therefore, we mistakenly assign grade repeaters from the last cohort before the SSY introduction to the control group, even though they actually experienced SSY due to their grade repetition. This misassignment of

the treatment variable results in a (small) downward bias. Our results are robust to excluding the last pre-treatment cohort (see Section 6).¹³

Fourth, the data lacks information on the exact month of birth, which would allow a more precise assignment of students' school starting cohorts based on the respective legal school-starting age cut-off in each federal state at the time (see Koebe and Marcus (2020) for a comprehensive collection of educational laws on school-starting cut-off rules for all West German states). The Micro Census data provide only information on the season of birth, that is, whether individuals were born between January and April or between May and December. In most federal states, the age cut-offs coincide to a large extent with this season of birth information. While Pischke (2007) and Braakmann (2010) assign the treatment indicator only based on the year of birth and assume a school start in the year the child turns seven, we exploit the season of birth information to reduce misassignment of actual treatment status.¹⁴

Wrongly assigning the treatment indicator, our estimators are biased toward zero: We assign some individuals to the treatment group who are not treated and some individuals to the control group who are actually treated.

Summing up the discussion on measurement error, we see that measurement error in both the outcome variables and the treatment assignment is likely to bias our estimators toward zero. However, we expect that this downward bias is not very large due to the aforementioned reasons and because we limit the sample to include respondents only up to age 40.

3.4. Sample restrictions

We restrict our analysis to German respondents in private households in West Germany and exclude individuals who obtained their school degree in East Germany. We impose several additional sample restrictions and, in Section 6, we show that our conclusions also hold if we apply different constraints. In our main analysis, we consider cohorts starting school between 1952 and 1970. This translates to using birth years ranging from 1945-1964. However, we exclude school starting cohorts 1964, 1965, and both school starting cohorts in 1966 in our main specification. These cohorts were exposed to SSY during primary school and Pischke (2007) shows that these cohorts have a lower probability of enrolling in either the middle or academic tracks. Hence, for these cohorts, the secondary school track is endogenous. The remaining birth cohorts in our sample made their track choice before the SSY introduction.

¹³Further, Pischke (2007) shows that SSY exposure increases grade repetition among cohorts who were exposed in primary school. We exclude these cohorts from our main analysis (see below).

¹⁴Our results are robust to assigning the treatment based only on the year of birth (see Section 6).

While Pischke (2007) and Braakmann (2010) use birth cohorts from 1943-1964, we focus on a slightly smaller window of cohorts (1945-1964). The main reason for this is that, in several states, the birth cohorts 1943 and 1944, thus, school starting cohorts 1950 and 1951, were affected by previous changes of the start of the school year from fall to Easter in line with the Düsseldorf Accord. However, for the federal state of Saarland, narrowing the sample window does not allow for isolating the impact of the SSY, as more than the first two cohorts were affected by altered length of schooling (see also footnote 2, Figure B.1 and Tables B.1-B.12 in Koebe and Marcus (2020)).¹⁵ Therefore, we exclude Saarland from our main analysis.

We also impose a restriction on the respondents' age. We only consider the information of respondents ten to twenty years after regular graduation.¹⁶ We impose the lower age limit of ten years after graduation in order to have the same sample for all outcome variables - irrespective of the considered time window after graduation. We apply the upper age limit of twenty years after graduation to reduce measurement error in our outcome variables due to second marriages and children who have already moved out (see the discussion above). Twenty years after graduation also means that for individuals in the academic track, the maximum age of respondents is 39, which is also below 40, the age cut-off recommended by Krapf and Kreyenfeld (2015). Consequently, basic track students' reporting age is between 25 and 35 years, middle track students' between 26 and 36 years and academic track students between 29 and 39 years of age. Our sample restrictions lead to the final estimation samples of 203,501 observations in basic track, 98,448 in middle, and 109,199 in academic track, a grand total of 411,148 observations.

3.5. Descriptive statistics

Table 1 reports sample summary statistics by track. Several points are worth noting. First, the share of females is substantially lower in the academic track in the considered cohorts. Second, while the samples in all three tracks comprise the same school starting cohorts 1952-1970, the average birth year differs between the tracks. This is due to the general trend of increasing educational attainment over time: Whereas 59 percent of students from the 1952 school starting cohort graduated from basic track, by the end of our sample period in 1970, this share had decreased to 37 percent. Third, the age difference between the

¹⁵Saarland was last to implement the common start of the school year at Easter in 1957 and, hence, had to switch start of the school year again within a shorter time period. Our results are robust to including Saarland (see Section 6).

¹⁶We drop 879 observations with births before age 15 (accounting for 0.2 percent of the final sample). We assume that these young ages rather relate to classification errors as we only observe children currently living in the household – and not necessarily only biological children.

tracks is a result of the sample restriction to only consider the answers of respondents ten to twenty years after graduation. Fourth, at the time of the interview, 22% of individuals in the basic track had not married, while this share is 25% for the middle, and 32% for the academic tracks.¹⁷ Among individuals who are married (or who have been married), the average age at marriage is clearly lower for individuals in the basic track (22.7 years) than in the middle (24 years) or academic (26.7 years) tracks. Similar differences across tracks can be obtained for the age at birth of the first child. Fifth, at the time of the interview (i.e. ten to twenty years after graduation), the share of individuals without children is ten percentage points higher in the academic track than in the basic track.

Next, we examine the patterns of family formation between the tracks in more detail. Figures 3a and 3b show that the higher the school track, the lower the probability to be married or to have a first child at any given age.¹⁸ This observation is in line with both the human capital effect of education and the duration effect of education.¹⁹ It is striking that the differences between individuals in basic and middle tracks are smaller compared to the differences between middle and academic tracks. This reflects that the length of the education phase is more similar between individuals in basic (eight to nine years) and middle tracks (ten years) than individuals in the academic track (thirteen years). Moreover, the graphs are also in line with the lock-in effect of schooling. Individuals in the academic track are clearly less likely to be married or to have a first child before the age of 19, the age when they finish secondary education. When accounting for the different lengths of secondary schooling (as in Figure 3c and Figure 3d), the differences between the tracks are still existing, which is in line with the human capital effect of education. However, the differences get much smaller, providing suggestive evidence for the duration effect of education.

¹⁷Note that about 4-5% of individuals in our sample are divorced. However, we can correctly assign the year of marriage for them as the Micro Census also asks both divorced and widowed individuals about the year of their marriage.

¹⁸These figures are based on our main sample, which includes respondents ten to twenty years after graduation. Therefore, the lines for the basic track ends at an earlier age compared to the other tracks. Figure A.1 in the Appendix displays the patterns of family formation separately for males and females. It is evident that women are more likely than males to marry and to have a first child at younger ages. We examine effect differences between males and females in Section 7.

¹⁹The differences are also in line with a pure selection effect in the sense that individuals with preferences for earlier marriage and earlier parenthood select themselves into lower tracks.

4. Empirical strategy

To identify the effect on family formation of shortening the education phase through the introduction of short school years, we estimate the following difference-in-differences (DiD) equation separately for each track J , with $J \in \{basic, middle, academic\}$:

$$Y_{isc}^J = \beta^J SSY_{sc}^J + \gamma_s^J + \lambda_c^J + X_{isc}' \delta^J + \varepsilon_{isc}^J, \tag{2}$$

where Y_{isc} is the outcome for individual i in state s and school starting cohort c . SSY_{sc} denotes the variable of interest. Based on an individuals' school starting cohort and state, it takes on the value 1 if an individual is exposed to two SSY and the value 0 if an individual is not exposed to SSY (see Section 3.2 for a detailed description of the exposure of different cohorts in the different states to short school years). Hence, β denotes the effect of being exposed to two SSY, which is equivalent to graduating about two-thirds of a school year earlier. γ_s and λ_c are fixed effects for state and school starting cohorts, respectively, thus taking into account general differences in the outcomes between states and across school starting cohorts. X_{isc} is a vector of pre-determined individual characteristics. In the baseline specification, it includes a gender dummy and fixed effects for the wave of the Micro Census and, in our main specification, it includes interactions between the gender dummy and the fixed effects for cohort, state, and wave. Finally, ε_{isc}^J denotes the error term that is allowed to be clustered at the cohort-state level, i.e. the level the treatment variable is assigned.

We estimate Equation (2) separately by track for several reasons. First, the pattern of family formation differs between individuals across the three tracks in the sense that individuals in the basic track get married earlier and also give birth to children earlier (see Figure 3). Second, other reforms (in particular, the compulsory schooling reforms) implemented in a similar time period affected only specific tracks. Estimating the regressions separately for each track, allows for considering these other reforms more easily (see Section 6). Third, different cohorts are affected by SSY in the different tracks due to the differences in the number of years needed for graduation.

Our DiD identification strategy assumes that the track-specific family-formation outcomes would have evolved in parallel in the treated and control states, if SSY were not introduced. This common trend assumption could be violated if SSY exposure provoked parents to move to control states. Since the decisions on the SSY introduction were made at the beginning of 1966 and were communicated shortly before its implementation, it is unlikely that parents

would have had enough time to move to avoid SSY exposure for their children. Further, moving across states is rather costly for families.

5. Results

We begin our discussion of the results by looking at the effect of the short school years on the probability to be married several years after graduation. We focus on the estimates for β from Equation (2), the SSY effects. Taking the first coefficient in column (1) of Table 2 at face value suggests that for individuals in the basic track being exposed to two short school years (i.e. graduating about two-thirds of a school year earlier) increases the probability to be married five years after graduation by 0.4 percentage points (pp). This effect is not statistically significant at conventional levels and it does not change substantially with the inclusion of gender-specific fixed effects for cohort, state, and wave (column 2). The marriage effect for individuals in the basic track remains statistically insignificant eight and ten years after graduation (columns 3-6). However, for individuals in the middle and academic tracks, the probability to be married five years after graduation increases substantially and significantly. While we see that the effects wash out over time, they are still statistically significant eight and ten years after graduation. The effect for the academic track decreases from 4.6 pp (five years after) to 4.1 pp (eight years after) and to 2.5 pp (ten years after), while in the middle track it remains 3.7 pp five years and eight years after, then decreases to 2.6 pp.

Next, we focus on SSY effects on the probability to have a first child five, eight, and ten years after graduation (Panel B). For individuals in the basic track, the earlier graduation due to SSY does not appear to result in increased probabilities to have a first child five, eight, or ten years after graduation. This is similar for individuals in the middle track, although the effect ten years after graduation is borderline significant. For individuals in the academic track affected by SSY, the probability to have a first child increases by about 3 pp eight and ten years after graduation.

While Table 2 focuses on three specific years after graduation, Figure 4 graphically shows the effects separately for one to ten years after graduation. In all three tracks, we observe a hump-shaped pattern for the marriage outcome: SSY exposure increases the probability to be married for the first years after graduation, but the effect fades over time. There is also evidence that SSY exposure significantly affects individuals in the basic track but only up to four years after graduation. For the fertility outcome, the point estimates are generally positive for the basic and middle tracks but statistically insignificant. However, for individuals in the academic track, SSY exposure does not only affect the timing of marriage,

but also the timing of fertility: In Panel (f) of Figure 4 the point estimate is statistically significant for four to ten years after graduation.²⁰

Taken together, Table 2 and Figure 4 provide evidence that earlier graduation affects the timing of marriage for individuals across tracks, but that it shifts forward the birth of the first child mainly for individuals in the academic track. In the following, we discuss some explanations for these patterns. Once an individual enters the labor market, the German tax system induces strong incentives for getting married. For example, Germany’s combination of progressive taxes and joint taxation (*Ehegattensplitting*) provides large financial benefits for married couples – in particular, for spouses with strongly differing income – and the social health insurance in Germany allows for insuring (non-working) spouses without cost. These incentives affect individuals in all three school tracks and, therefore, might explain why we find that individuals prepone marriage across tracks. Regarding the differential fertility response, Figure 3 provides evidence that the age at first birth is much higher for individuals in the academic track compared to individuals in the other tracks. Therefore, the fertility response to an earlier labor market entry might be stronger for individuals in this group, e.g., because they are closer to the biological age limit for childbirth or because they prefer that the age does not differ too much between parents and children.

6. Sensitivity analysis

Table 3 examines the sensitivity of our findings to alternative age, wave, and state restrictions.²¹ In our main specification, we construct the outcome variables based on information provided ten to twenty years after graduation. We set the lower bound to ten years in order to have a constant sample for five, eight, and ten years after graduation. Further, we do not include information from more than twenty years after graduation, as the more years after graduation we include, the more likely it is that marriages have been dissolved and/or children have moved out of the household. At the same time, a larger number of observations would increase the precision of our estimates. Hence, there is a bias-variance trade-off.

²⁰Similar to Humlum et al. (2017), our main specification focuses on years after graduation and not on specific ages. When estimating the effects by age instead of years after graduation (see Table A.1 in the Appendix), we obtain similar results. The marriage effect first kicks in for individuals in the basic track with significant coefficients at ages 16-19, while for the middle track the largest point estimates are obtained at ages 20-25 and for the academic track at ages 23-27. The higher the education, the later the strongest effects. At age 29, the marriage effects remain significant only for the academic track. Panel B of this table shows significant fertility effects in the academic track for ages 21-29.

²¹This Table presents the results for marriage and first birth eight years after graduation, Tables A.3 and A.4 in the Appendix show the results for five and ten years after graduation, respectively.

In the following, we analyze whether our results are robust to alternative ways of dealing with this bias-variance trade-off. Column (1) considers information by individuals ten to fifteen years after graduation, while column (2) considers information eight to twenty years after graduation. Column (3) relies on information provided between age 30 and 40. The next two sensitivity analysis restrict the sample to fewer waves. Column (4) drops the last two waves (2002 and 2003) and column (5) disregards the first two waves (1976 and 1978). Columns (6) and (7) relate to the number of included states. Column (6) drops Lower Saxony from the analysis, where SSY exposure was track-specific (see Section 2), while column (7) additionally includes Saarland, which was excluded in the main analyses due to earlier shifts of the start of the school year that affected many school starting cohorts in this particular state. Column (8) uses individuals' birth years (1945-1963) to identify treatment status by assuming a start of primary school in the year individuals turn seven, similar to the approach of Pischke (2007), but leaving all other sample restrictions unchanged. Column (9) clusters the standard errors at the level of the federal state and not at the state-cohort level. Our conclusions are insensitive to all the alternative specifications in Table 3.

In Appendix Table A.5, we deal with alternative cohort restrictions. First, we also include cohorts that were affected by SSY exposure in primary school. Second, we exclusively consider cohorts with two SSY and disregard cohorts with only one SSY. Third, we exclude the last pre-treatment cohort as the treatment status in this cohort would be wrongly assigned for individuals who repeated a grade. Again, our results are robust to these alternative specifications. Co-treatments in the form of other policies are one threat to our identification strategy. For this reason, we excluded the school starting cohorts 1950 and 1951 (birth cohorts 1943 and 1944) from our main analysis. While these cohorts were included in previous SSY studies (Pischke, 2007; Braakmann, 2010), they were affected by reductions in the length of specific school years in four federal states (Baden-Württemberg, Bremen, Saarland, and West Berlin) due to moving the start of the school year from fall to Easter in line with the Düsseldorf Accord (DA) in 1955 (see also Footnote 2 and Koebe and Marcus (2020)). We aim to only identify variations in the length of the schooling phase induced by the 1966/67 short school years, for which previous studies show that they did not hamper human capital accumulation. That is why we excluded Saarland completely from the main analysis: in this state, later cohorts were also affected by other changes in the length of the schooling phase.

Further, during our observation period, several states increased compulsory schooling from eight to nine years. This change mainly affected the basic track, as the regular school length is ten years in the middle track and thirteen years in the academic track. However, previous studies use two slightly different sets of compulsory schooling reform dates (we discuss this

in more detail in Appendix B in Koebe and Marcus (2020)). Therefore, we searched for the original law texts and propose refined reform dates, which we display together with the primary sources. We use these reform dates to assign to each cohort in each state the compulsory number of school years. Table A.6 shows the results for the basic track when we control for compulsory schooling reforms using our refined dates or reform dates used in Pischke (2007). Controlling for compulsory schooling does not change our results meaningfully – irrespective of the used set of reform dates.

Our difference-in-differences identification strategy builds on the assumption that—in the absence of the short school years—the outcome variables would follow the same trend in treatment and control states. While it is generally not possible to prove this common trend assumption, we conduct falsification exercises with placebo outcomes and placebo treatments to assess the plausibility of this assumption.

For the placebo-outcome analysis, we examine the “effect” of short school years on track choice. As our main specification exclusively includes cohorts that were already in secondary school at the time of SSY implementation, SSY should have no effect on track choice. A significant SSY coefficient would indicate that the selection into tracks evolves differently in treatment and control states. In this analysis, we have to restrict the sample to cohorts and states for which the assignment of the treatment variable does not depend on the track. Hence, we have to drop Lower Saxony and the cohorts 1954 – 1958 (see Figure 1). Otherwise, there would be mechanic effects. Table A.7 shows that there is no “effect” of short school years on the probability to attend the academic track. However, there is indication that the share of students in basic and middle tracks develops slightly, but statistically significantly, differently in treatment and control states. This significant coefficient remains if we consider two additional pre-treatment cohorts (see column 2).²² Table A.8 analyzes whether the slight differential selection into basic and middle track between treatment and control states affects our overall conclusions. In this table, we re-estimate our main specification but pool all observations from basic and middle track. This specification confirms our conclusion that short school years have no effect on fertility outcomes for individuals in basic and middle track. Further, Table A.8 confirms that SSY affects the timing of marriage. Hence, we conclude that while there is some evidence that the selection into basic and middle track develops slightly differently in treatment and control states, this differential development

²²In column (1) of Table A.7, we can only work with two pre-treatment cohorts due to the necessary additional sample restrictions for this specification. The results in column (2) are very similar if we exclude the four states that were treated in 1950 or 1951 in line with the Düsseldorf Accord (see Appendix B in our working paper (Koebe and Marcus, 2020)).

does not change our overall conclusions. Moreover, selection into the academic track, for which we obtain the largest effects for both marriage and fertility outcomes, does not evolve differently in treatment and control states.

For the placebo-treatment analysis, we restrict the sample to the pre-treatment cohorts only. Further, we include two additional cohorts (1950 and 1951) to increase statistical power.²³ Hence, the sample includes the cohorts 1950 - 1957 for the basic track, 1950 - 1956 for the middle track, and 1950 - 1953 for the academic track. We then pretend that SSY were introduced 1, 2, and 3 years before the actual SSY introduction, respectively, and estimate whether there is a significant “effect” of this placebo SSY introduction. Table A.9 in the Appendix shows that the 18 estimated placebo effects are generally small and statistically insignificant.²⁴ While this is not a proof of the common-trend assumption, Table A.9 suggests that our outcomes developed similarly in treatment and control states before the short school years, making it more plausible that trends would be similar in the absence of the short school years as well.

7. Further results

This section presents different sets of additional results on (i) effect differences by gender; (ii) the exact timing of marriage and parenthood; (iii) longer time horizons and subsequent births; and (iv) human capital-related outcomes.

7.1. Gender-specific results

Thus far, we pool the effects for males and females. Table 4 splits the sample according to an individual’s gender. It shows that the obtained marriage and fertility effects are driven by both males and females. Generally, the effects for five years after graduation are slightly larger for females than for males (with only the marriage-effect difference for middle track individuals being statistically significant). One reason for this difference might be that females are, on average, younger than males when they marry and have children (see also Figure A.1). For instance, about 30% of middle track women in our sample are married five years after graduation, while the corresponding share is only 12% for men. The effect differences between women and men become smaller eight and ten years after graduation,

²³The results of the falsification exercise are very similar if we drop the four states (Berlin, Baden-Württemberg, Bremen and Saarland) that were treated in 1950 or 1951 in line with the Düsseldorf Accord (see Appendix B in our working paper (Koebe and Marcus, 2020)).

²⁴There is one statistically significant coefficient (placebo treatment three years earlier for the marriage outcome in the middle track). Given the number of tests (18) that we perform, this is roughly what one would expect.

i.e. when men are also more likely to be married and have a first child. The effect differences eight and ten years after graduation are statistically insignificant throughout.

7.2. *Exact timing of marriage and parenthood*

While the previous analyses focus on the probability of family formation *at or before* a specific point in time, the next analysis looks at the probability of family formation *at* a specific point in time.²⁵ This analysis helps to better understand the effects of SSY exposure on the exact timing of marriage and parenthood. Figure 5 graphically presents the associated results. Generally, the largest positive point estimates for marriage are found in periods when marriage is most likely (compare Figure 3c). Interestingly, significant and positive point estimates for marriage precede negative coefficients in all three tracks. For instance, individuals in the academic track are significantly *more* likely to marry in the second, third, fourth, and fifth years after graduation due to SSY exposure, while they are significantly *less* likely to marry in the eighth and ninth years after graduation. This suggests that the control cohorts are catching-up over time and that the head start of the treated cohorts diminishes over time, suggesting that SSY exposure affects the timing of marriage, but not the overall probability to ever marry. Moreover, this pattern also shows why point estimates in our main specification (with cumulative marriage rates) are higher five years after graduation than ten years after graduation. When looking at the timing effects separately for females and males (Figures A.2 and A.3 in the Appendix), it can be seen that the developments of the effects for males lag slightly behind those for females. This is in line with the observation that we obtain the largest marriage effects in periods when marriage is most likely (compare Appendix Figure A.1). For parenthood, in the academic track all coefficients for $t \in [2, 9]$ years after graduation are clearly positive and some coefficients are significant at the 10% level. Further, the magnitude of the coefficients declines for $t \in [8, 10]$ years after graduation, suggesting a similar pattern as for the marriage outcomes.

7.3. *Longer-run effects and subsequent births*

To analyze whether SSY affect only the timing of family formation or also the probability to ever marry/have children, this subsection focuses on longer time horizons. More

²⁵For this analysis, we redefine our outcome variables from Equation (1) as

$$Y^{e,p} = \begin{cases} 1 & \text{if event } e \text{ in } t = p \\ 0 & \text{otherwise.} \end{cases}$$

specifically, Table 5 looks at family formation 10 to 15 years after graduation.²⁶ The table shows a striking pattern for our two main outcomes (Panels A and B): The longer the considered time horizon, the smaller the previously significant effects. For instance, while individuals in the middle track are 2.6 percentage points more likely to marry in the first ten years after graduation due to SSY exposure, this effect decreases to 1.8 percentage points twelve years after graduation and to 0.1 percentage points fifteen years after graduation. All-in-all, the first two panels of Table 5 suggest that the SSY introduction affects the timing of marriage and parenthood, but not the probability to ever marry or become a parent.

The next two panels of Table 5 examine whether short school years also affect the timing of the birth of the second and third children. For these analyses, we redefine the outcome variable in such a way that it only takes on the value of 1 if *two* children (Panel C) and *three* children (Panel D), respectively, were born at or before a specific point in time (instead of just one child). Panel C provides evidence that SSY exposure increases the probability for individuals in the academic track to have two children ten years after graduation by about 1.5 percentage points. As expected, the point estimates are smaller than the effects for the first child. There is also some evidence for positive effects in the middle track ten to twelve years after graduation. While SSY exposure does not affect third births in the middle track (Panel D), there is some evidence that individuals in the academic track are slightly more likely to have a third child twelve to fifteen years after graduation.

The results in Table 5 have to be taken with a grain of salt as we only observe children living in the household. The older the respondents are, the higher the chance that their children have already moved out of the house. Hence, measurement error is much larger for the outcomes ten to fifteen years after graduation. Measurement error is even larger when looking at completed fertility. This is what we do in Table 6. Here, we use the number of children up to age 45 as an outcome variable and consider the answers of individuals from our cohorts who are at least 45 years old. The table shows that while there is no effect on completed fertility for individuals with basic or middle track degrees, there is a statistically significant effect of SSY exposure on the number of children for individuals in the academic track, suggesting that, for these individuals, SSY exposure does not just affect the

²⁶While our main analyses focuses on the reports of individuals 10 to 20 years after graduation, these analyses consider reports of individuals p to 20 years after graduation, where $p \in [10, 15]$ is the number of years until which the event could have taken place in the respective analysis. Therefore, the number of observations differs across the specifications in Table 5. However, Appendix Table A.10 shows that we obtain a similar pattern when we work with the same sample size in all specifications by considering only reports of individuals 15 to 20 years after graduation (as in the last column of Table 5). Hence, the observed pattern is unlikely to be driven by different sample compositions (or potentially different sample attrition patterns).

timing of family formation but also completed fertility. However, due to the aforementioned measurement issues, we do not want to over-interpret this finding.²⁷

7.4. Human capital-related outcomes

The interpretation of our results as evidence for the duration effect hinges on the assumption that SSY exposure has no effect on human capital acquisition. While previous studies show that this reform had no adverse effects on health outcomes (Braakmann, 2010) or on wages and employment (Pischke, 2007), in this section we provide further evidence that the reform has no impact on human capital-related outcomes. First, we replicate the finding of Pischke (2007) that SSY exposure neither reduces wages nor the probability to be employed - we show that these results hold both for a replication sample based on Pischke (2007) as well as our sample (see Table A.2 and the first two panels of Table 7 in the Appendix). Second, we show that the reform has no impact on tertiary education degrees.²⁸ More specifically, we find that the reform does not affect the probability to obtain a college degree for individuals in the academic track. Similarly, for individuals in basic and middle tracks, we show that short school years do not affect the probability to obtain a vocational education degree (see the last two panels of Table 7). Taken together, these findings are in line with our interpretation of a duration effect as there is no evidence for human capital effects of SSY exposure.

8. Discussion and conclusion

This study examines the effects on the timing of fertility and marriage of a policy that allowed students to finish secondary school about eight months earlier – with the same degree and the same curriculum taught. We find that earlier graduation leads to earlier marriages. There is also evidence that the earlier graduation affects the timing of childbirth. We find that the effects fade over time, indicating that the short school years affect the timing of marriage and parenthood but not the probability to ever marry or to become a parent. We further show that the obtained effects are driven by both males and females. Additionally, we find suggestive evidence that the reform also affects subsequent births and completed fertility.

²⁷If SSY exposure decreases not only the age at child birth but also the age at which children move out of the household, we will underestimate the effect of SSY exposure on completed fertility. The SSY effect will be biased toward zero as we assume that some individuals have fewer children at a given age (because we do not observe an older child who already moved out) and this incorrect measure of the number of children happens (slightly) more often for treated individuals.

²⁸For these outcomes, we can only rely on the Micro Census waves from 1996 onward as in the previous waves individuals are asked about their last vocational education level, but not their highest obtained degree.

Our findings highlight that policies altering the duration of specific life phases can affect the timing of marriage and childbirth. This is relevant for both academic and political discussions. Our study contributes to the literature on policies that affect family formation by highlighting the importance of unintended consequences of policies that reduce or extend specific life phases. Our study also contributes to the literature on the relationship between education and family formation by proposing the “duration effect” as a third mechanism, how education might causally affect family formation. While the previous literature focuses on human capital effects and lock-in effects, the duration effect is so far neglected. We provide evidence that education influences family formation through its effect on the timing of subsequent life phases that individuals typically traverse before forming families. Another contribution of this study is the compilation of relevant education reforms in West Germany and the respective law sources, which can be also used by other researchers.

Our findings carry also important messages for policymakers. On the one hand, policymakers should be aware of the consequences for family formation when discussing policies that affect the duration of specific life phases (e.g., changing the duration of compulsory military service, secondary schooling, or university education). On the other hand, our study highlights that these policies might offer a tool for policymakers who would like to change the timing of family formation. Both aspects are particularly relevant, given that not only is the age at marriage and age of first birth increasing in many countries, but the duration of the education phase is also increasing.

While there are some worries that the lockdown policies enacted in many countries (including school closures) to fight the Covid-19 Pandemic will prolong the education phase, its effects on marriage and fertility are ambiguous as these lockdown policies might not only affect the duration of the education phase but also human capital acquisition.

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Declarations

Availability of data: This study uses data from the German Micro Census for the years 1976 to 2003. The Micro Census data are provided by the German Statistical Offices and can be ordered through their website (<https://www.forschungsdatenzentrum.de/en>). For auxiliary analyses, we further use the SOEP (v.35) and the DJI Family Survey 1988.

Code availability: Available upon request.

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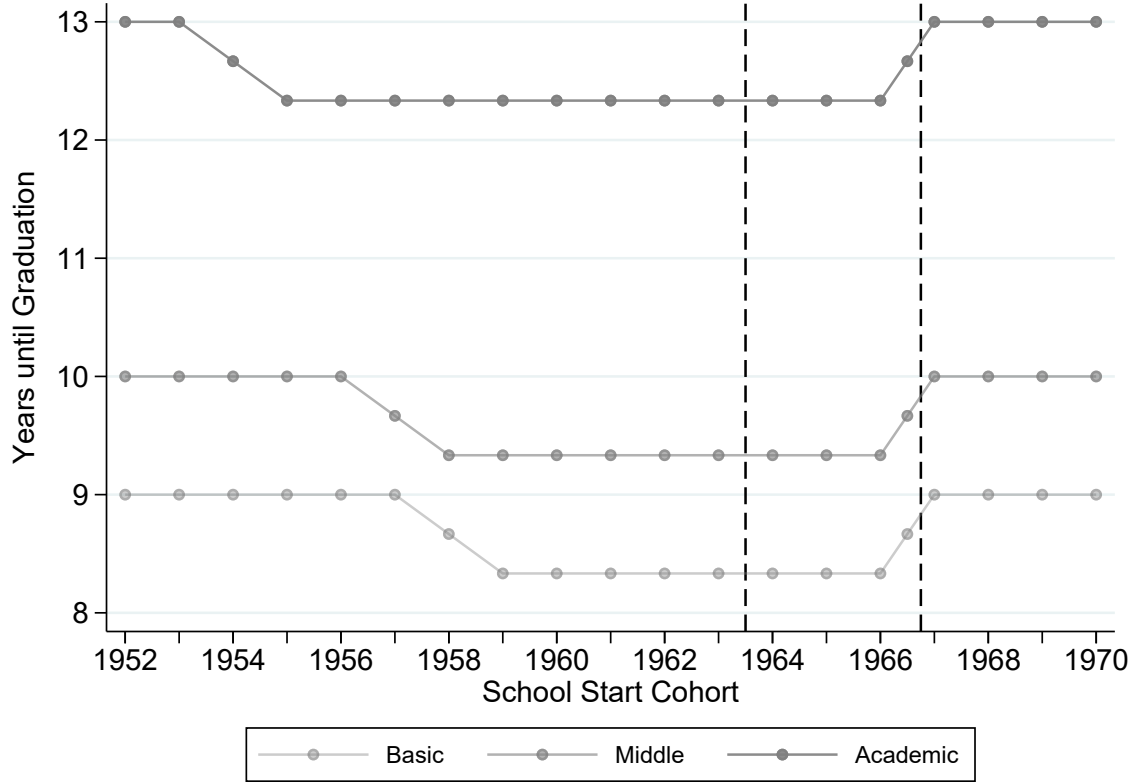
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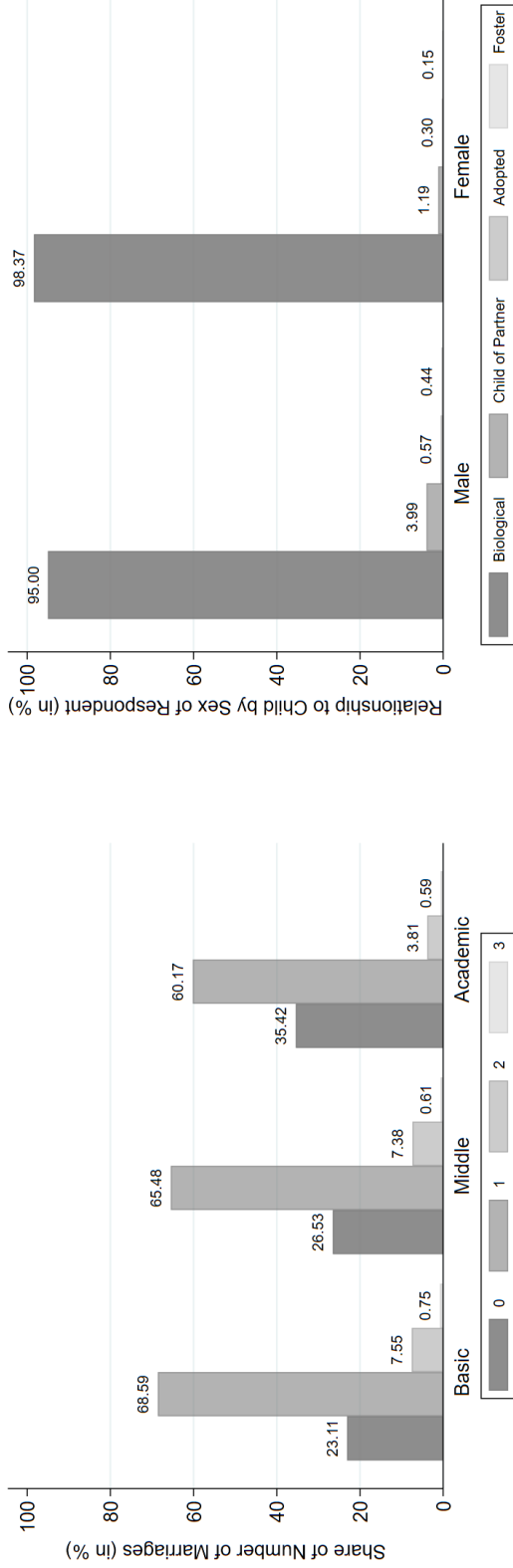
Tables and Figures

Figure 1: Exposure to Short School Years by Secondary School Track and Cohort



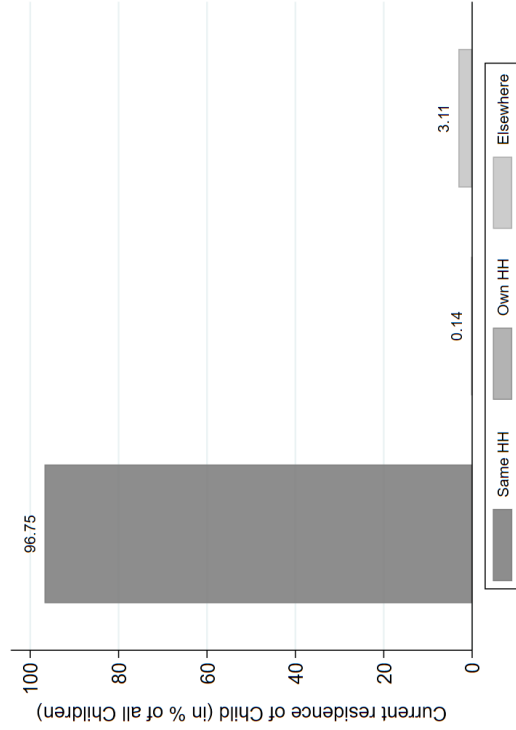
Notes: This stylized figure visualizes treatment identification in a given treatment state, depending on school start cohort and secondary school track. School starting cohorts within the dashed lines are cohorts that were affected by the short school years during primary school and that are excluded in our main analyses. Deviations from the regular school years required in each track (9 in basic, 10 in middle, and 13 in academic) imply exposure to short school years. Own calculations and graphical display.

Figure 2: Assessing Measurement Error

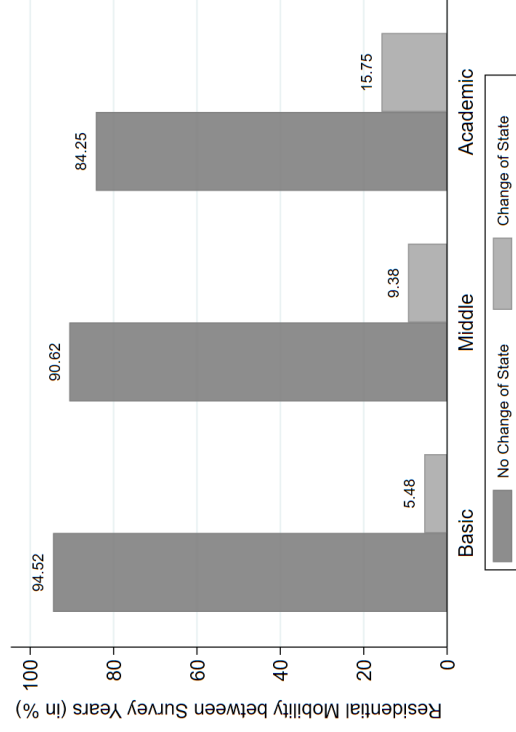


(a) Share of Number of Marriages below Age 40

(b) Relationship to Child by Sex of Respondent



(c) Current Residence of Child

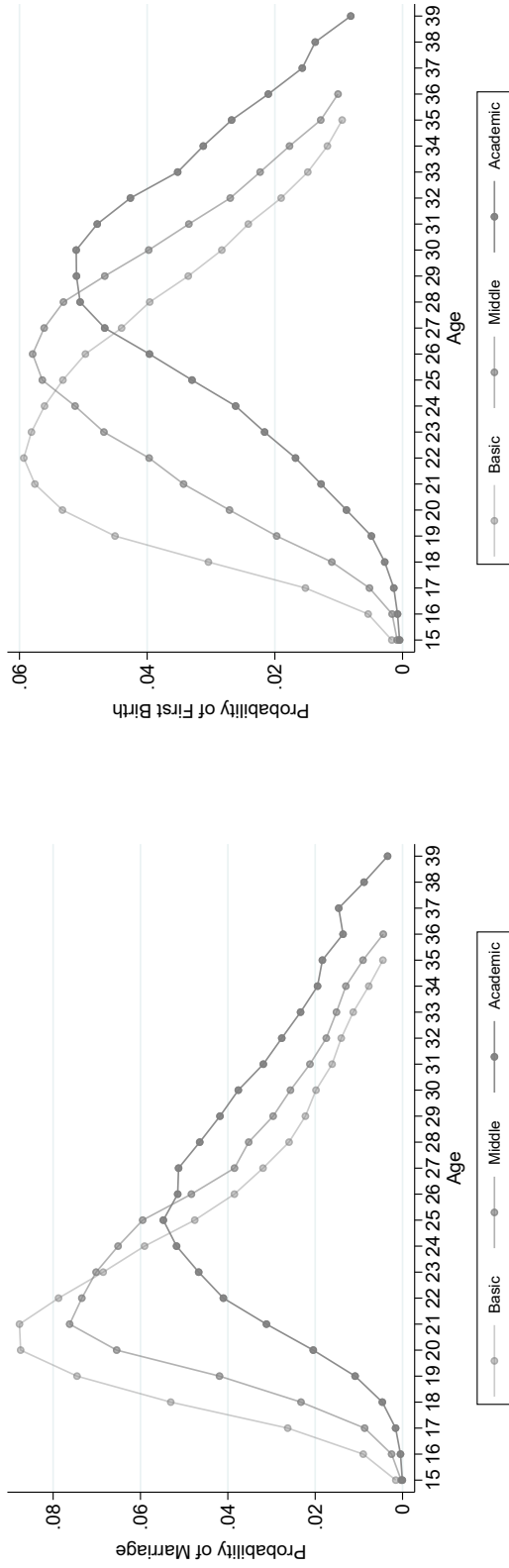


(d) Residential Mobility between Survey Years

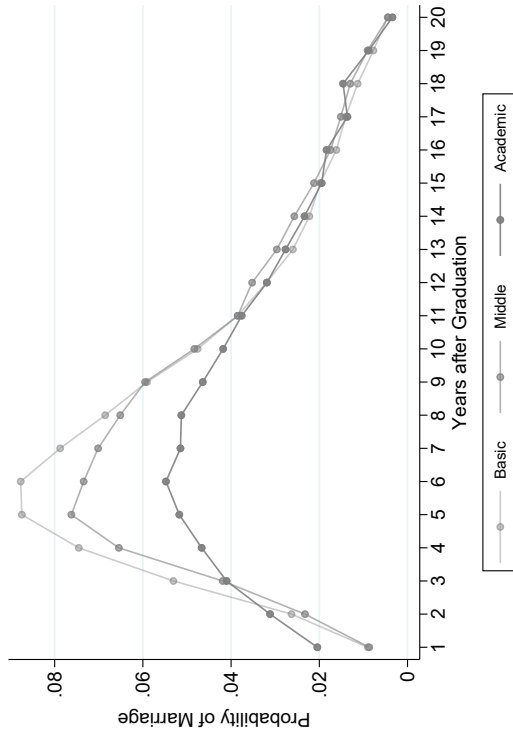
Notes: All four graphs provide descriptive statistics to assess the size of potential measurement errors. Figure 2a displays the share of second marriages before age 40, while Figures 2b and 2c deal with the concern of biased fertility outcomes and display the relationship to the child by sex of respondent and the current residence of the child. Figure 2d provides information on regional mobility.

Sources: Figures 2a and 2d: SOEP v35. Figures 2b and 2c: DJI Family Survey 1988.

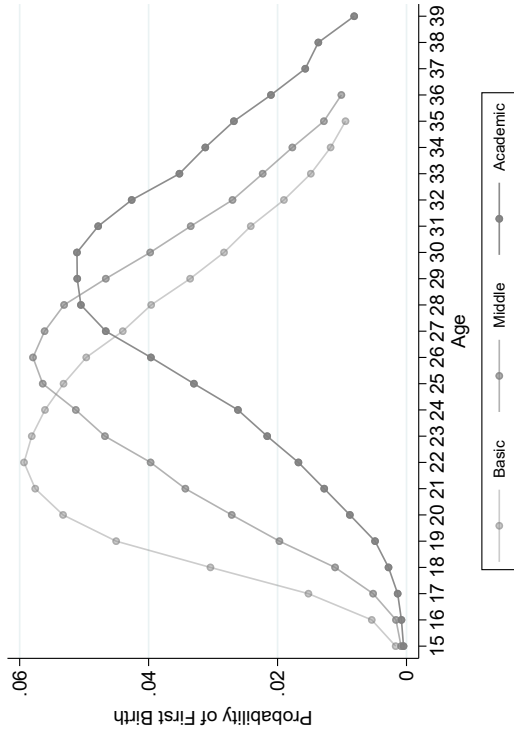
Figure 3: Probability of Marriage and First Birth by Age and Years after Graduation



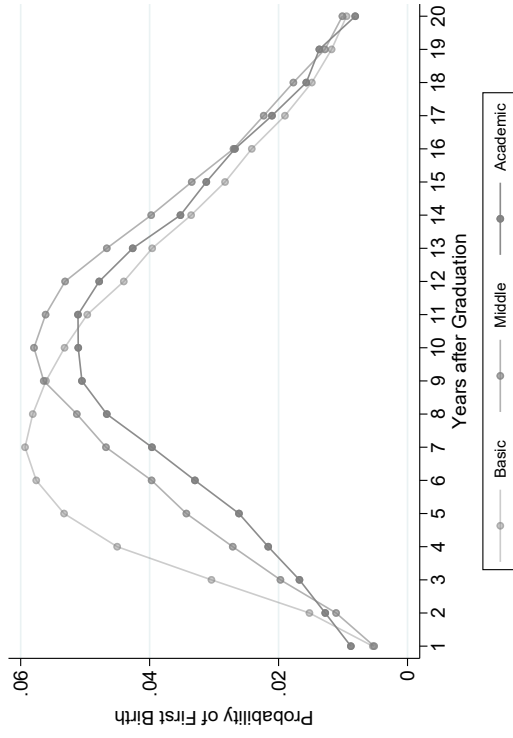
(a) Marriage by Age



(c) Marriage by Years after Graduation



(b) First Birth by Age

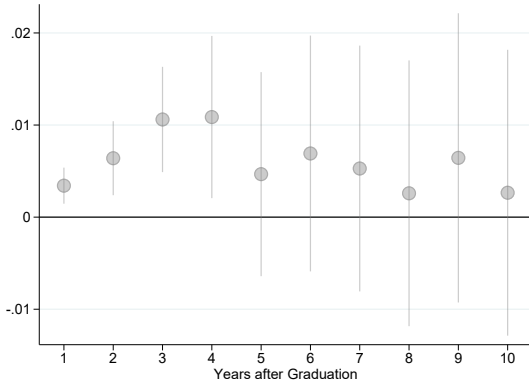


(d) First Birth by Years after Graduation

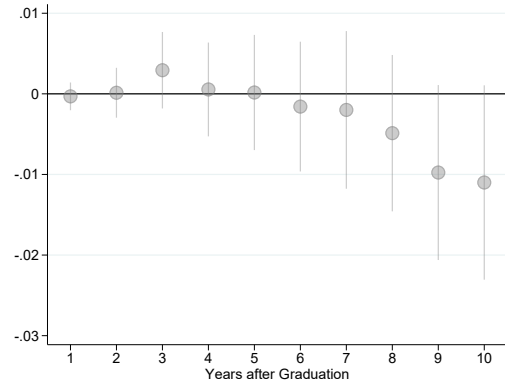
Notes: All four graphs show probabilities of being married and becoming a parent by age (Figures 3a and 3b) and by years after graduation (Figures 3c and 3d), using graduating ages for basic track at 15, for middle track at 16, and academic track at 19 as year 0. Source: Micro Census, main estimation sample. Own calculations and graphical display.

Figure 4: Effect of Short School Years on Marriage and First Birth

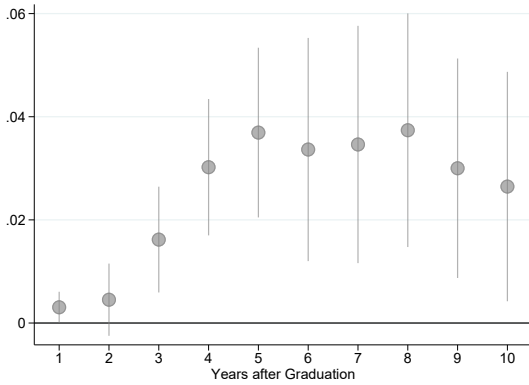
(a) Marriage - Basic



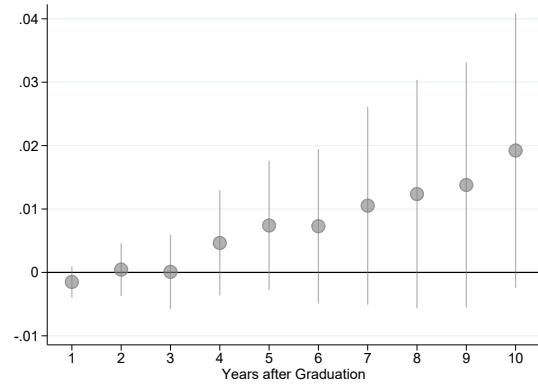
(b) First Birth - Basic



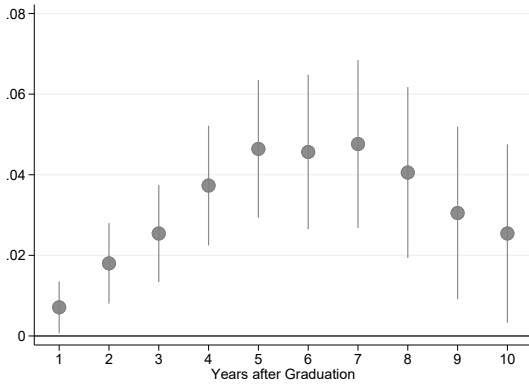
(c) Marriage - Middle



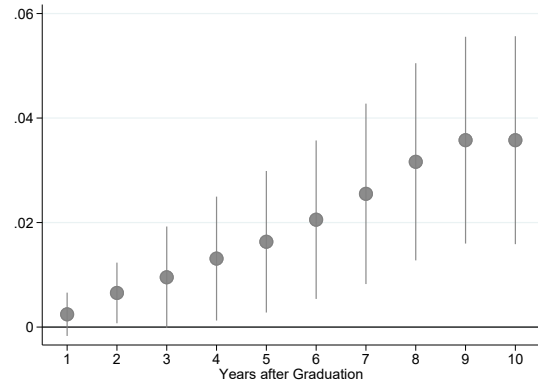
(d) First Birth - Middle



(e) Marriage - Academic



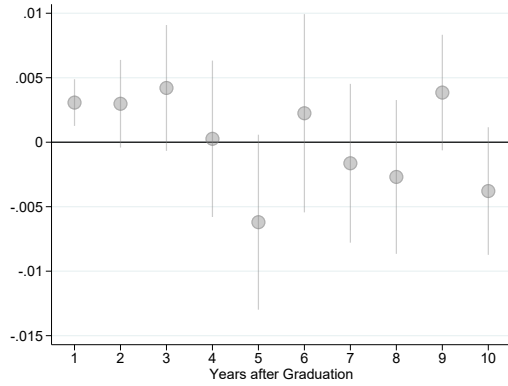
(f) First Birth - Academic



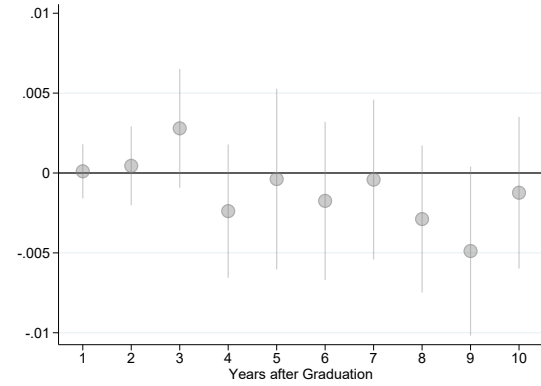
Notes: The graphs show the estimated short school year effects (and their 95% confidence intervals) *up* to different years after graduation. The underlying samples are identical to the samples in Table 2. All coefficients are based on Equation (2).

Figure 5: Effect of Short School Years *at* different Years after Graduation

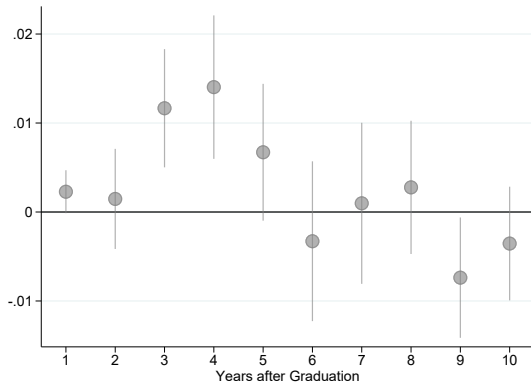
(a) Marriage - Basic



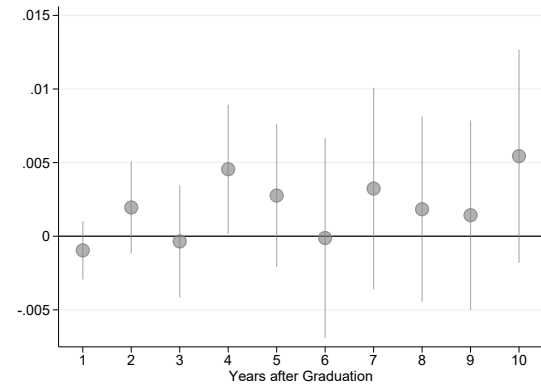
(b) First Birth - Basic



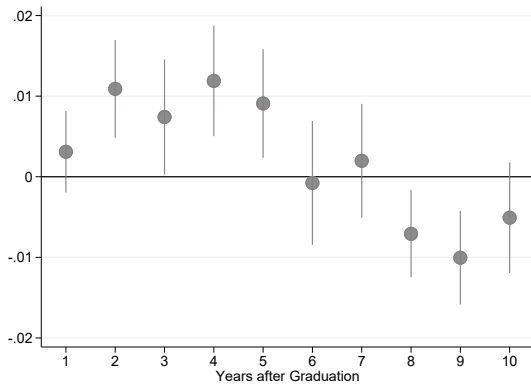
(c) Marriage - Middle



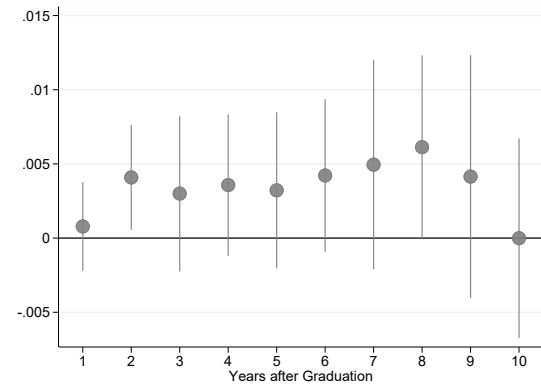
(d) First Birth - Middle



(e) Marriage - Academic



(f) First Birth - Academic



Notes: The graphs show the estimated short school year effects (and their 95% confidence intervals) *at* different years after graduation. The underlying samples are identical to the samples in Table 2 and all coefficients are based on Equation (2). The figure is related to Figure 4, which shows short school year effects *up* to different years after graduation.

Table 1: Summary Statistics by Secondary School Track

	Basic			Middle			Academic		
	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
Female	0.49	0	1	0.58	0	1	0.42	0	1
Birthyear	1954.38	1945	1964	1956.54	1945	1964	1957.15	1945	1964
<i>Birthmonth</i>									
Jan-Apr	0.36	0	1	0.37	0	1	0.37	0	1
May-Dec	0.64	0	1	0.63	0	1	0.63	0	1
Age at Interview	30.75	25	35.5	31.93	26	36.5	34.89	29	39.5
School Start Year	1961.04	1952	1970	1963.19	1952	1970	1963.80	1952	1970
Years in School	8.52	8	9	9.85	9.33	10	12.79	12.3	13
<i>Marital Status</i>									
Single	0.22	0	1	0.25	0	1	0.32	0	1
Married	0.73	0	1	0.70	0	1	0.64	0	1
Widowed	0.00	0	1	0.00	0	1	0.00	0	1
Divorced	0.05	0	1	0.05	0	1	0.04	0	1
Age at Marriage	22.65	15.5	35.5	23.99	15.5	36.5	26.74	15.5	39.5
<i>Share Married</i>									
5 Years after Graduation	0.25	0	1	0.22	0	1	0.21	0	1
8 Years after Graduation	0.49	0	1	0.43	0	1	0.37	0	1
10 Years after Graduation	0.59	0	1	0.53	0	1	0.45	0	1
Age at First Birth	23.73	15	35.5	25.58	15	36.5	28.44	15	39.5
<i>Share First Birth</i>									
5 Years after Graduation	0.15	0	1	0.10	0	1	0.10	0	1
8 Years after Graduation	0.33	0	1	0.24	0	1	0.22	0	1
10 Years after Graduation	0.44	0	1	0.35	0	1	0.32	0	1
Childless at Interview	0.38	0	1	0.43	0	1	0.48	0	1

Notes: The table displays for relevant variables sample mean, minimum and maximum by secondary school track.

Table 2: Effect of Short School Years on Marriage and First Birth

	5 Years		8 Years		10 Years	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Marriage						
Basic	0.004 (0.006)	0.005 (0.006)	0.002 (0.007)	0.003 (0.007)	0.003 (0.008)	0.003 (0.008)
N	203,501	203,501	203,501	203,501	203,501	203,501
Middle	0.038*** (0.008)	0.037*** (0.008)	0.038*** (0.011)	0.037*** (0.011)	0.026** (0.011)	0.026** (0.011)
N	98,448	98,448	98,448	98,448	98,448	98,448
Academic	0.046*** (0.009)	0.046*** (0.009)	0.041*** (0.011)	0.041*** (0.011)	0.025** (0.011)	0.025** (0.011)
N	109,199	109,199	109,199	109,199	109,199	109,199
Panel B: First Birth						
Basic	-0.000 (0.004)	0.000 (0.004)	-0.005 (0.005)	-0.005 (0.005)	-0.011* (0.006)	-0.011* (0.006)
N	203,501	203,501	203,501	203,501	203,501	203,501
Middle	0.008 (0.005)	0.007 (0.005)	0.013 (0.009)	0.012 (0.009)	0.019* (0.011)	0.019* (0.011)
N	98,448	98,448	98,448	98,448	98,448	98,448
Academic	0.016** (0.007)	0.016** (0.007)	0.032*** (0.010)	0.032*** (0.010)	0.036*** (0.010)	0.036*** (0.010)
N	109,199	109,199	109,199	109,199	109,199	109,199
School Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Sex FE	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
School Cohort-Sex FE	No	Yes	No	Yes	No	Yes
State-Sex FE	No	Yes	No	Yes	No	Yes
Wave-Sex FE	No	Yes	No	Yes	No	Yes

Notes: The table shows the effect of the short school years on marriage and first birth five, eight, and ten years after graduation from secondary school for individuals in different secondary school tracks based on Equation (2). Standard errors clustered at the cohort-state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Robustness Checks - Alternative Age, Wave, State, and SE Specifications Eight Years after Graduation

	Other Age Restrictions			Wave Restrictions			State Restrictions			Birthyear	SE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)		
Panel A: Marriage											
Basic	0.004 (0.009)	0.008 (0.007)	-0.003 (0.007)	0.003 (0.007)	0.006 (0.007)	-0.006 (0.008)	0.003 (0.007)	0.005 (0.008)	0.003 (0.013)		
N	99,250	228,149	237,942	203,501	166,094	180,009	207,963	192,430	203,501		
Middle	0.037** (0.014)	0.036*** (0.010)	0.032*** (0.009)	0.037*** (0.011)	0.040*** (0.011)	0.043*** (0.014)	0.038*** (0.011)	0.029*** (0.010)	0.037** (0.014)		
N	44,503	111,614	118,417	98,448	89,000	86,245	99,807	89,226	98,448		
Academic	0.045*** (0.017)	0.044*** (0.012)	0.035*** (0.010)	0.039*** (0.011)	0.039*** (0.010)	0.045*** (0.011)	0.040*** (0.011)	0.036*** (0.011)	0.041* (0.021)		
N	51,566	122,504	113,732	103,698	106,139	98,348	110,756	97,768	109,199		
Panel B: First Birth											
Basic	0.001 (0.007)	-0.003 (0.005)	-0.016*** (0.006)	-0.005 (0.005)	-0.007 (0.005)	-0.008 (0.006)	-0.005 (0.005)	-0.001 (0.005)	-0.005 (0.004)		
N	99,250	228,149	237,942	203,501	166,094	180,009	207,963	192,430	203,501		
Middle	0.007 (0.013)	0.013 (0.008)	-0.001 (0.007)	0.012 (0.009)	0.015* (0.009)	0.012 (0.010)	0.014 (0.009)	0.010 (0.009)	0.012 (0.008)		
N	44,503	111,614	118,417	98,448	89,000	86,245	99,807	89,226	98,448		
Academic	0.052*** (0.012)	0.033*** (0.009)	0.021** (0.009)	0.033*** (0.010)	0.030*** (0.009)	0.030*** (0.009)	0.033*** (0.010)	0.030*** (0.010)	0.032*** (0.008)		
N	51,566	122,504	113,732	103,698	106,139	98,348	110,756	97,768	109,199		
Years after Grad.	10-15	8-20	Age 30-40	10-20	10-20	10-20	10-20	10-20	10-20		
Age Basic	25-30	23-35	30-40	25-35	25-35	25-35	25-35	25-35	25-35		
Age Middle	26-31	24-36	30-40	26-36	26-36	26-36	26-36	26-36	26-36		
Age Academic	29-34	27-39	30-40	29-39	29-39	29-39	29-39	29-39	29-39		
Waves	1976-2003	1976-2003	1976-2003	1976-2001	1980-2003	1976-2003	1976-2003	1976-2003	1976-2003		

Notes: The table shows the effect of the short school years eight years after graduation for various alternative specifications. Columns (1)-(3) rely on information provided in differing numbers of years after graduation, as indicated in the bottom part of the table. Column (4) disregards the last two waves of the Micro Census, and column (5) drops the first two waves. Columns (6)-(7) drop or include certain states from the analysis (Lower Saxony (LS), Saarland (SL), respectively). Column (8) relies on treatment assignment based on individual's birth year and column (9) on standard errors clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Effect Heterogeneity by Gender

	5 Years		8 Years		10 Years	
	Female (1)	Male (2)	Female (3)	Male (4)	Female (5)	Male (6)
Panel A: Marriage						
Basic	0.007 (0.009)	0.002 (0.004)	-0.004 (0.008)	0.009 (0.010)	0.010 (0.011)	-0.005 (0.008)
N	99,685	103,816	99,685	103,816	103,816	99,685
p-value of the difference	0.557		0.243		0.136	
Middle	0.054*** (0.010)	0.013 (0.010)	0.042*** (0.013)	0.031** (0.014)	0.023 (0.016)	0.029*** (0.011)
p-value of the difference	0.001		0.459		0.660	
N	57,381	41,067	57,381	41,067	41,067	57,381
Academic	0.035** (0.014)	0.055*** (0.010)	0.029** (0.014)	0.049*** (0.012)	0.033*** (0.013)	0.015 (0.014)
p-value of the difference	0.215		0.150		0.193	
N	46,289	62,910	46,289	62,910	62,910	46,289
Panel B: First Birth						
Basic	-0.001 (0.006)	0.002 (0.003)	-0.004 (0.008)	-0.005 (0.005)	-0.007 (0.008)	-0.015* (0.008)
N	99,685	103,816	99,685	103,816	103,816	99,685
p-value of the difference	0.586		0.925		0.455	
Middle	0.010 (0.007)	0.004 (0.006)	0.018 (0.011)	0.005 (0.010)	0.014 (0.013)	0.023* (0.013)
N	57,381	41,067	57,381	41,067	41,067	57,381
p-value of the difference	0.477		0.227		0.532	
Academic	0.022* (0.011)	0.012** (0.006)	0.033** (0.014)	0.031*** (0.009)	0.033*** (0.010)	0.040*** (0.014)
N	46,289	62,910	46,289	62,910	62,910	46,289
p-value of the difference	0.335		0.891		0.607	

Notes: The table shows the effect of the short school years separately for males and females and presents p -values of a t -test for the difference in the effects between males and females. Standard errors clustered at the cohort-state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Short School Year Effects: Longer Time Horizons

	Years after Graduation					
	10	11	12	13	14	15
Panel A: Marriage						
Basic	0.003 (0.008)	-0.002 (0.008)	-0.004 (0.008)	-0.008 (0.009)	-0.006 (0.009)	-0.008 (0.009)
N	203,501	188,330	173,232	157,588	140,755	122,803
Middle	0.026** (0.011)	0.022** (0.009)	0.018* (0.010)	0.013 (0.009)	0.011 (0.010)	0.001 (0.009)
N	98,448	91,869	84,934	77,690	69,823	61,960
Academic	0.025** (0.011)	0.014 (0.010)	0.008 (0.009)	0.007 (0.009)	-0.001 (0.009)	-0.008 (0.010)
N	109,199	101,972	94,829	87,503	78,097	68,785
Panel B: First Birth						
Basic	-0.011* (0.006)	-0.015** (0.006)	-0.009 (0.007)	-0.008 (0.007)	-0.005 (0.007)	-0.003 (0.008)
N	203,501	188,330	173,232	157,588	140,755	122,803
Middle	0.019* (0.011)	0.015 (0.011)	0.020* (0.011)	0.024** (0.011)	0.020* (0.011)	0.012 (0.010)
N	98,448	91,869	84,934	77,690	69,823	61,960
Academic	0.036*** (0.010)	0.031*** (0.011)	0.027** (0.011)	0.025** (0.011)	0.020 (0.013)	0.017 (0.013)
N	109,199	101,972	94,829	87,503	78,097	68,785
Panel C: Second Birth						
Basic	-0.004 (0.005)	-0.011** (0.005)	-0.008 (0.006)	-0.010* (0.005)	-0.010* (0.006)	-0.007 (0.007)
N	203,501	188,330	173,232	157,588	140,755	122,803
Middle	0.013*** (0.005)	0.012** (0.006)	0.012* (0.007)	0.012 (0.008)	0.014 (0.009)	0.014 (0.009)
N	98,448	91,869	84,934	77,690	69,823	61,960
Academic	0.016** (0.007)	0.010 (0.007)	0.006 (0.008)	0.002 (0.010)	0.005 (0.012)	0.001 (0.013)
N	109,199	101,972	94,829	87,503	78,097	68,785
Panel D: Third Birth						
Basic	0.004** (0.002)	0.005** (0.002)	0.006** (0.003)	0.005* (0.003)	0.007* (0.004)	0.007* (0.004)
N	203,501	188,330	173,232	157,588	140,755	122,803
Middle	-0.000 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.003)	0.003 (0.003)	0.004 (0.004)
N	98,448	91,869	84,934	77,690	69,823	61,960
Academic	0.002 (0.002)	0.002 (0.002)	0.006** (0.003)	0.007** (0.003)	0.006 (0.004)	0.010** (0.004)
N	109,199	101,972	94,829	87,503	78,097	68,785

Notes: The table shows the effect of the short school years on various outcomes as indicated in the panel header for individuals in different secondary school tracks based on Equation (2). Standard errors clustered at the cohort-state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Completed Fertility

	N of children before age 45	
	(1)	(2)
Basic	0.003 (0.013)	0.003 (0.013)
N	206,241	206,241
Middle	0.007 (0.023)	0.007 (0.023)
N	76,418	76,418
Academic	0.086*** (0.032)	0.089*** (0.034)
N	74,847	74,847
School Cohort FE	Yes	Yes
State FE	Yes	Yes
Sex FE	Yes	Yes
Wave FE	Yes	Yes
School Cohort-Sex FE	No	Yes
State-Sex FE	No	Yes
Wave-Sex FE	No	Yes

Notes: The table shows the effect of the short school years on completed fertility - the number of children up to age 45 - for individuals in different secondary school tracks based on Equation (2). The sample is restricted to individuals who are at least 45 years old. Standard errors clustered at the cohort-state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

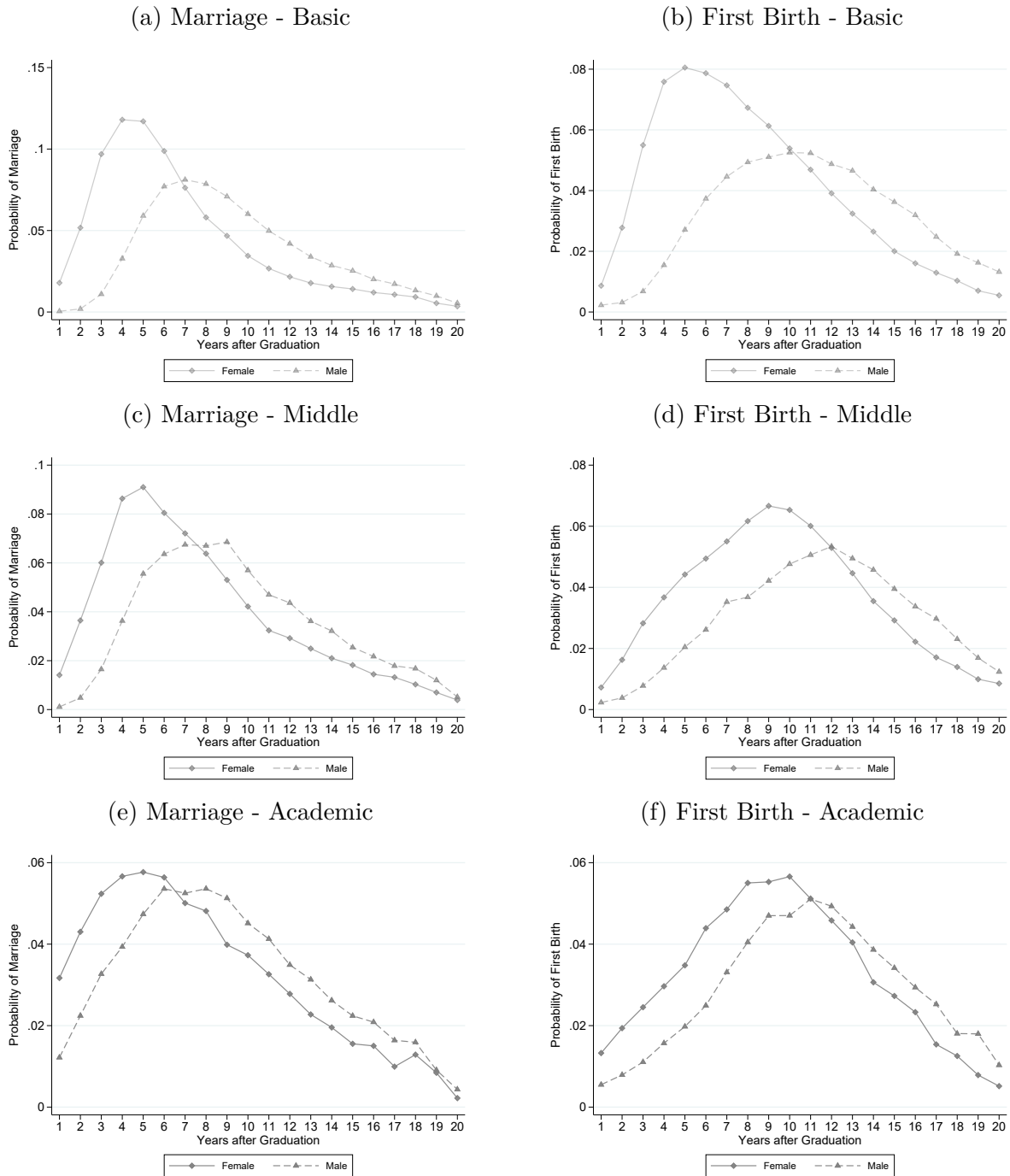
Table 7: Effect of Short School Years on Human Capital Outcomes

	Main	
	(1)	(2)
Panel A: Wage		
SSY (All)	0.005 (0.014)	0.008 (0.014)
N	411,148	411,148
Panel B: Employment		
SSY (All)	0.003 (0.003)	0.003 (0.003)
N	411,148	411,148
Panel C: Apprenticeship		
SSY (Basic and Middle)	0.004 (0.007)	0.003 (0.007)
N	140,588	140,588
Panel D: University		
SSY (Academic)	0.013 (0.015)	0.014 (0.015)
N	58,136	58,136
School Cohort FE	Yes	Yes
State FE	Yes	Yes
Sex FE	Yes	Yes
Wave FE	Yes	Yes
Track FE	Yes	Yes
School Cohort-Track FE	Yes	Yes
State-Track FE	Yes	Yes
Sex-Track FE	Yes	Yes
Wave-Track FE	Yes	Yes
School Cohort-Track-Sex FE	No	Yes
State-Track-Sex FE	No	Yes
Wave-Track-Sex FE	No	Yes

Notes: This table shows SSY effects on wages, employment and tertiary education decisions using our main estimation sample. In Panel A, the dependent variable is the log hourly wage and estimation is performed with a Tobit model accounting for the left censoring of the outcome variable (results are very similar with a Poisson model). In Panel B, the dependent variable is a dummy for being employed in the survey week. The outcome in Panel C is the probability of obtaining an apprenticeship as highest vocational degree, while the outcome in Panel D is the probability of holding a university degree. The latter two outcomes are only available in the Micro Census starting from wave 1995, hence the reduced sample size. Standard errors in parentheses are clustered at the track-cohort-state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A. Appendix: Additional Robustness Checks and Further Results

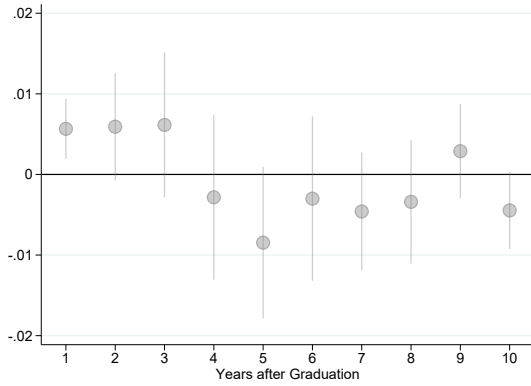
Figure A.1: Probability of Marriage and First Birth by Years after Graduation and Gender



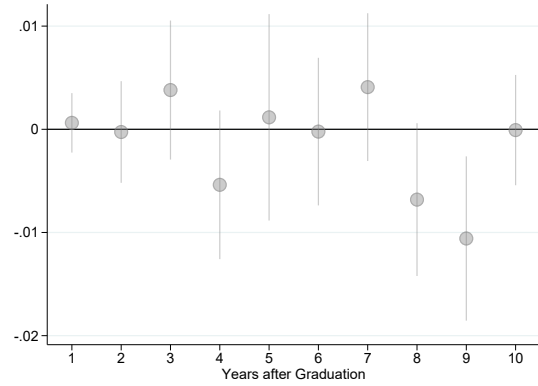
Notes: The table presents graphs from Figure 3 separately for men and women.

Figure A.2: Effect of Short School Years *at* different Years after Graduation (Females)

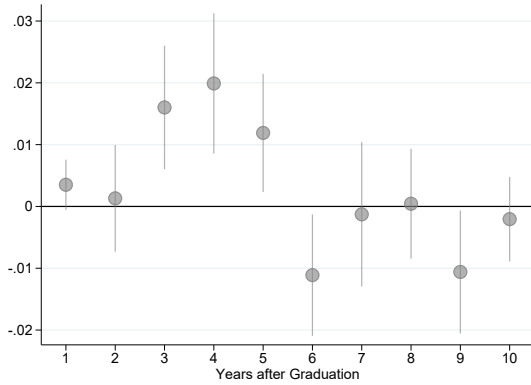
(a) Marriage - Basic



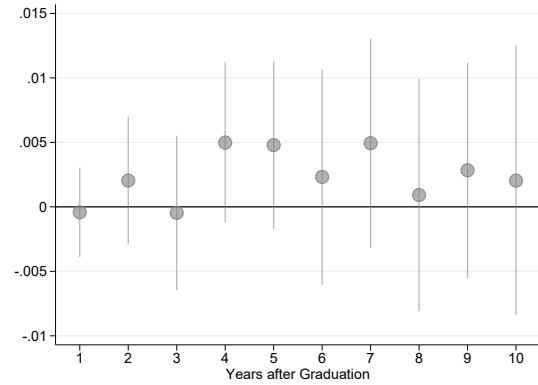
(b) First Birth - Basic



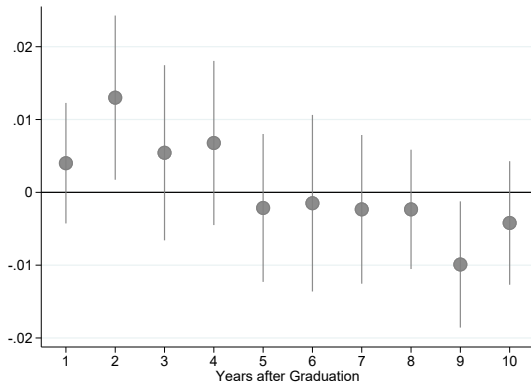
(c) Marriage - Middle



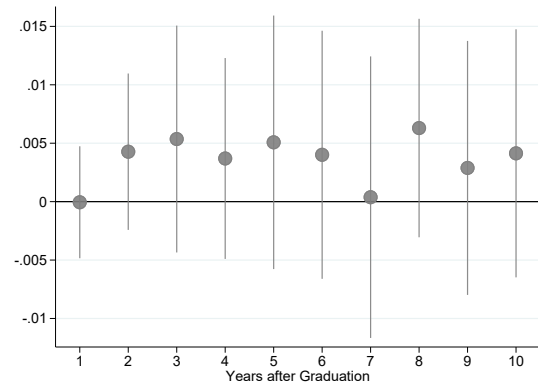
(d) First Birth - Middle



(e) Marriage - Academic



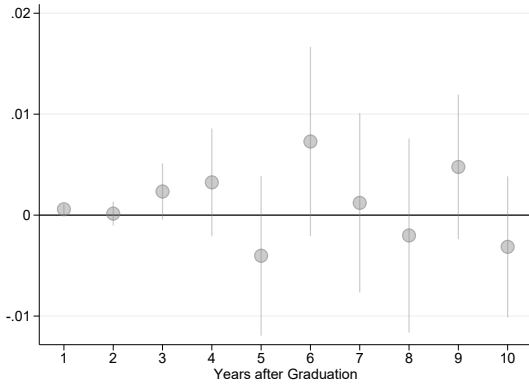
(f) First Birth - Academic



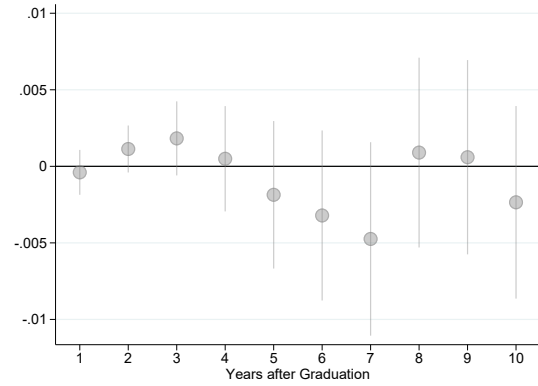
Notes: The graphs show the estimated short school year effects (and their 95% confidence intervals) *at* different years after graduation for females. The underlying samples are identical to the samples in Table 2 and all coefficients are based on Equation (2). The figure is related to Figure 4, which shows short school year effects *up* to different years after graduation.

Figure A.3: Effect of Short School Years *at* different Years after Graduation (Males)

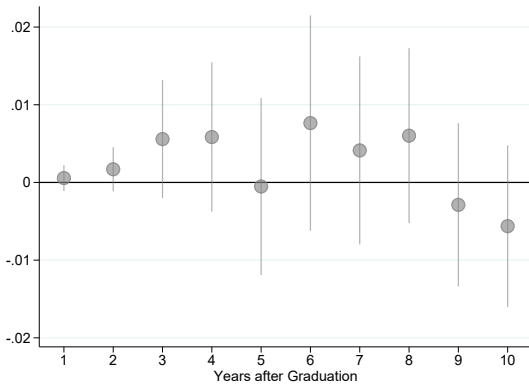
(a) Marriage - Basic



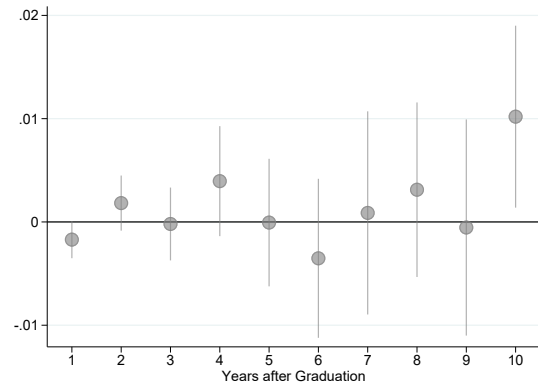
(b) First Birth - Basic



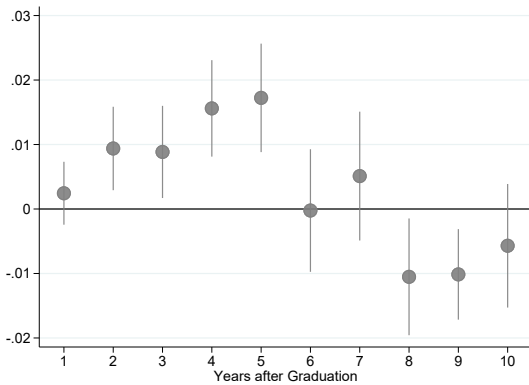
(c) Marriage - Middle



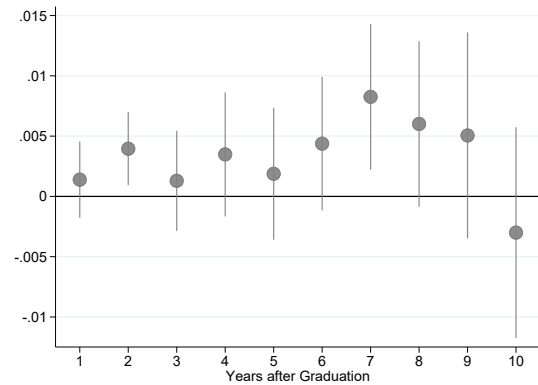
(d) First Birth - Middle



(e) Marriage - Academic



(f) First Birth - Academic



Notes: The graphs show the estimated short school year effects (and their 95% confidence intervals) *at* different years after graduation for males. The underlying samples are identical to the samples in Table 2 and all coefficients are based on Equation (2). The figure is related to Figure 4, which shows short school year effects *up* to different years after graduation.

Table A.1: Effect of Short School Years on Marriage and First Birth after Graduation from Secondary Schooling by Age

		Age													
		16	17	18	19	20	21	22	23	24	25	26	27	28	29
Panel A: Marriage															
Basic	0.003*** (0.001)	0.006*** (0.002)	0.011*** (0.003)	0.011** (0.004)	0.005 (0.006)	0.007 (0.006)	0.005 (0.007)	0.003 (0.007)	0.006 (0.008)	0.003 (0.008)	-0.002 (0.008)	-0.004 (0.008)	-0.008 (0.009)	-0.008 (0.009)	-0.006 (0.009)
N	203,501	203,501	203,501	203,501	203,501	203,501	203,501	203,501	203,501	203,501	188,330	173,232	157,588	140,755	140,755
Middle	0.001	0.003** (0.002)	0.005 (0.004)	0.016*** (0.005)	0.030*** (0.007)	0.037*** (0.008)	0.034*** (0.011)	0.035*** (0.012)	0.037*** (0.011)	0.030*** (0.011)	0.026** (0.011)	0.022** (0.009)	0.018* (0.010)	0.013 (0.009)	0.013 (0.009)
N	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	91,869	84,934	77,690	77,690
Academic	0.000	0.001* (0.001)	0.002 (0.001)	0.004** (0.002)	0.007** (0.003)	0.018*** (0.005)	0.025*** (0.006)	0.037*** (0.008)	0.046*** (0.009)	0.046*** (0.010)	0.046*** (0.010)	0.048*** (0.011)	0.041*** (0.011)	0.031*** (0.011)	0.025** (0.011)
N	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199
Panel B: First Birth															
Basic	-0.000 (0.001)	0.000 (0.002)	0.003 (0.002)	0.001 (0.003)	0.000 (0.004)	-0.002 (0.004)	-0.002 (0.005)	-0.005 (0.005)	-0.010* (0.005)	-0.011* (0.006)	-0.015** (0.006)	-0.009 (0.007)	-0.008 (0.007)	-0.008 (0.007)	-0.005 (0.007)
N	203,501	203,501	203,501	203,501	203,501	203,501	203,501	203,501	203,501	203,501	188,330	173,232	157,588	140,755	140,755
Middle	-0.001	-0.002 (0.001)	0.000 (0.002)	0.000 (0.003)	0.005 (0.004)	0.007 (0.005)	0.007 (0.006)	0.011 (0.008)	0.012 (0.009)	0.014 (0.010)	0.019* (0.011)	0.015 (0.011)	0.020* (0.011)	0.024** (0.011)	0.024** (0.011)
N	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	98,448	91,869	84,934	77,690	77,690
Academic	-0.000 (0.000)	-0.000 (0.001)	0.000 (0.001)	0.002 (0.002)	0.002 (0.002)	0.007** (0.003)	0.010* (0.005)	0.013** (0.006)	0.016** (0.007)	0.021*** (0.008)	0.021*** (0.008)	0.025*** (0.009)	0.032*** (0.010)	0.036*** (0.010)	0.036*** (0.010)
N	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199	109,199

Notes: The table shows the effect of the short school years by age instead of by years after graduation. All regressions include the same set of fixed effects as the main regression specification. Standard errors clustered at the cohort-state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Effect of Short School Years on Wage and Employment

	Main	Male
	(1)	(2)
Panel A: Wage		
Pischke (2007) Results	0.017 (0.011)	0.001 (0.011)
N	723,470	430,859
Pischke (2007) Replication	0.014 (0.014)	-0.007 (0.015)
N	755,093	454,518
Panel B: Employment		
Pischke (2007) Results	0.016*** (0.006)	0.013* (0.007)
N	1,032,744	509,770
Pischke (2007) Replication	0.020** (0.008)	0.015* (0.009)
N	1,071,256	536,235

Notes: This table replicates wage and employment regressions using the same sample and wave restrictions as specified in Pischke (2007). Exposure to short school years is assigned according to secondary school track, birth cohort (1943-1964), and state of residence. The first row of each Panel displays estimation results taken from Table 5 and Table 8 in Pischke (2007), respectively. In Panel A, the dependent variable is the log hourly wage, in Panel B a dummy for being employed in the survey week. Standard errors in parentheses are clustered at the track-cohort-state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Robustness Checks - Alternative Age, Wave, State and SE Specifications *Five* Years after Graduation

	Other Age Restrictions			Wave Restrictions			State Restrictions			Birthyear	SE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)		
Panel A: Marriage											
Basic	0.005 (0.008)	0.010* (0.005)	-0.005 (0.006)	0.005 (0.006)	0.006 (0.006)	0.006 (0.006)	0.005 (0.006)	0.006 (0.006)	0.006 (0.005)	0.005 (0.005)	
N	99,250	250,131	237,942	203,501	166,094	180,009	207,963	207,963	192,430	203,501	
Middle	0.039*** (0.011)	0.033*** (0.007)	0.041*** (0.007)	0.037*** (0.008)	0.038*** (0.008)	0.041*** (0.009)	0.037*** (0.008)	0.037*** (0.008)	0.030*** (0.008)	0.037*** (0.007)	
N	44,503	127,077	118,417	98,448	89,000	86,245	99,807	99,807	89,226	98,448	
Academic	0.055*** (0.013)	0.042*** (0.009)	0.039*** (0.008)	0.046*** (0.009)	0.044*** (0.008)	0.048*** (0.009)	0.047*** (0.009)	0.047*** (0.009)	0.046*** (0.009)	0.046*** (0.014)	
N	51,566	140,849	113,732	103,698	106,139	98,348	110,756	110,756	97,768	109,199	
Panel B: First Birth											
Basic	0.005 (0.006)	0.001 (0.003)	-0.007* (0.004)	0.000 (0.004)	-0.002 (0.004)	0.000 (0.004)	0.000 (0.004)	0.000 (0.004)	0.003 (0.004)	0.000 (0.003)	
N	99,250	250,131	237,942	203,501	166,094	180,009	207,963	207,963	192,430	203,501	
Middle	0.008 (0.007)	0.008* (0.004)	0.004 (0.005)	0.007 (0.005)	0.009* (0.005)	0.007 (0.006)	0.008 (0.005)	0.008 (0.005)	0.008 (0.006)	0.007 (0.005)	
N	44,503	127,077	118,417	98,448	89,000	86,245	99,807	99,807	89,226	98,448	
Academic	0.030*** (0.009)	0.010 (0.006)	0.011* (0.006)	0.017** (0.007)	0.015** (0.007)	0.015** (0.007)	0.017** (0.007)	0.017** (0.007)	0.018** (0.007)	0.016** (0.006)	
N	51,566	140,849	113,732	103,698	106,139	98,348	110,756	110,756	97,768	109,199	
Years after Grad.	10-15	5-20	Age 30-40	10-20	10-20	10-20	10-20	10-20	10-20	10-20	
Age Basic	25-30	20-35	30-40	25-35	25-35	25-35	25-35	25-35	25-35	25-35	
Age Middle	26-31	21-36	30-40	26-36	26-36	26-36	26-36	26-36	26-36	26-36	
Age Academic	29-34	24-39	30-40	29-39	29-39	29-39	29-39	29-39	29-39	29-39	
Waves	1976-2003	1976-2003	1976-2003	1976-2001	1980-2003	1976-2003	1976-2003	1976-2003	1976-2003	1976-2003	

Notes: The table shows the effect of the short school years five years after graduation for various alternative specifications. Columns (1)-(3) rely on information provided in different periods after graduation, as indicated in bottom part of the table. Column (4) disregards the last two waves of the Micro Census and column (5) drops the first two waves. Columns (6)-(7) drop or include certain states from the analysis (Lower Saxony (LS), Saarland (SL), respectively). Column (8) relies on treatment identification based on individual's birthyear and column (9) on standard errors clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Robustness Checks - Alternative Age, Wave, State and SE Specifications *Ten* Years after Graduation

	Other Age Restrictions		Wave Restrictions		State Restrictions		Birthyear		SE
	(1)	(2)	(3)	(4)	-LS (5)	+SL (6)	(7)	(8)	(8)
Panel A: Marriage									
Basic	0.004 (0.009)	-0.001 (0.008)	0.003 (0.008)	0.005 (0.007)	-0.006 (0.009)	0.004 (0.008)	0.007 (0.008)	0.003 (0.014)	0.003 (0.014)
N	99,250	237,942	203,501	166,094	180,009	207,963	192,430	203,501	203,501
Middle	0.028* (0.014)	0.023*** (0.009)	0.026** (0.011)	0.028** (0.011)	0.031** (0.013)	0.027** (0.011)	0.019** (0.009)	0.026* (0.013)	0.026* (0.013)
N	44,503	118,417	98,448	89,000	86,245	99,807	89,226	98,448	98,448
Academic	0.039** (0.018)	0.022** (0.010)	0.027** (0.012)	0.024** (0.011)	0.032*** (0.011)	0.025** (0.011)	0.022* (0.012)	0.025 (0.025)	0.025 (0.025)
N	51,566	113,732	103,698	106,139	98,348	110,756	97,768	109,199	109,199
Panel B: First Birth									
Basic	-0.005 (0.008)	-0.024*** (0.007)	-0.011* (0.006)	-0.013** (0.007)	-0.018*** (0.006)	-0.011* (0.006)	-0.006 (0.006)	-0.011 (0.007)	-0.011 (0.007)
N	99,250	237,942	203,501	166,094	180,009	207,963	192,430	203,501	203,501
Middle	0.009 (0.015)	0.012 (0.008)	0.019* (0.011)	0.023** (0.011)	0.019 (0.012)	0.020* (0.011)	0.016 (0.011)	0.019 (0.012)	0.019 (0.012)
N	44,503	118,417	98,448	89,000	86,245	99,807	89,226	98,448	98,448
Academic	0.059*** (0.012)	0.026*** (0.010)	0.038*** (0.010)	0.035*** (0.010)	0.037*** (0.010)	0.037*** (0.010)	0.036*** (0.011)	0.036*** (0.010)	0.036*** (0.010)
N	51,566	113,732	103,698	106,139	98,348	110,756	97,768	109,199	109,199
Years after Grad.	10-15	Age 30-40	10-20	10-20	10-20	10-20	10-20	10-20	10-20
Age Basic	25-30	30-40	25-35	25-35	25-35	25-35	25-35	25-35	25-35
Age Middle	26-31	30-40	26-36	26-36	26-36	26-36	26-36	26-36	26-36
Age Academic	29-34	30-40	29-39	29-39	29-39	29-39	29-39	29-39	29-39
Waves	1976-2003	1976-2003	1976-2001	1980-2003	1976-2003	1976-2003	1976-2003	1976-2003	1976-2003

Notes: The table shows the effect of the short school years ten years after graduation for various alternative specifications. Columns (1)-(2) rely on information provided in differing numbers of years after graduation, as indicated in the bottom part of the table. Column (3) disregards the last two waves of the Micro Census, and column (4) drops the first two waves. Columns (5)-(6) drop or include certain states from the analysis (Lower Saxony (LS), Saarland (SL), respectively). Column (7) relies on treatment identification based on individual's birth year and column (8) on standard errors clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Robustness Checks: Alternative Cohort Restrictions

		5 Years			8 Years			10 Years				
	Main	+Prim	-0.5 SSY	-last pre-treat	Main	+Prim	-0.5 SSY	-last pre-treat	Main	+Prim	-0.5 SSY	-last pre-treat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Marriage												
Basic	0.005 (0.006)	0.001 (0.005)	0.006 (0.006)	0.006 (0.006)	0.003 (0.007)	-0.000 (0.007)	0.006 (0.007)	0.006 (0.007)	0.003 (0.008)	-0.000 (0.007)	0.006 (0.008)	0.005 (0.008)
N	203,501	247,074	190,370	189,221	203,501	247,074	190,370	189,221	203,501	247,074	190,370	189,221
Middle	0.037*** (0.008)	0.027*** (0.006)	0.036*** (0.008)	0.037*** (0.008)	0.037*** (0.011)	0.026*** (0.009)	0.035*** (0.011)	0.038*** (0.012)	0.026*** (0.011)	0.014 (0.009)	0.025*** (0.011)	0.026*** (0.011)
N	98,448	125,936	93,999	94,046	98,448	125,936	93,999	94,046	98,448	125,936	93,999	94,046
Academic	0.046*** (0.009)	0.031*** (0.008)	0.046*** (0.009)	0.049*** (0.008)	0.041*** (0.011)	0.023*** (0.010)	0.040*** (0.011)	0.045*** (0.010)	0.025*** (0.011)	0.014 (0.010)	0.025*** (0.011)	0.029*** (0.011)
N	109,199	143,134	106,052	106,285	109,199	143,134	106,052	106,285	109,199	143,134	106,052	106,285
Panel B: First Birth												
Basic	0.000 (0.004)	0.000 (0.003)	0.001 (0.004)	-0.001 (0.004)	-0.005 (0.005)	-0.003 (0.005)	-0.003 (0.005)	-0.005 (0.005)	-0.011* (0.006)	-0.006 (0.006)	-0.008 (0.006)	-0.010 (0.006)
N	203,501	247,074	190,370	189,221	203,501	247,074	190,370	189,221	203,501	247,074	190,370	189,221
Middle	0.007 (0.005)	0.005 (0.004)	0.006 (0.005)	0.007 (0.005)	0.012 (0.009)	0.008 (0.006)	0.009 (0.009)	0.012 (0.009)	0.019* (0.011)	0.012 (0.008)	0.016 (0.011)	0.019* (0.011)
N	98,448	125,936	93,999	94,046	98,448	125,936	93,999	94,046	98,448	125,936	93,999	94,046
Academic	0.016*** (0.007)	0.008 (0.006)	0.016*** (0.007)	0.018*** (0.007)	0.032*** (0.010)	0.019*** (0.008)	0.031*** (0.009)	0.035*** (0.009)	0.036*** (0.010)	0.026*** (0.009)	0.035*** (0.010)	0.040*** (0.010)
N	109,199	143,134	106,052	106,285	109,199	143,134	106,052	106,285	109,199	143,134	106,052	106,285

Notes: The table shows the effect of the short school years based on Equation (2) using different cohort restrictions. Columns (1), (5), and (9) repeat for comparison the main results for five, eight, and ten years after graduation, respectively. Columns (2), (6), and (10) include also cohorts that were affected by short school years in primary school, while columns (3), (7), and (11) disregard cohorts with only one short school year. Finally, columns (4), (8), and (12) exclude the last pre-treatment cohort. Standard errors clustered at the cohort-state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Controlling for Compulsory Schooling (CS) Reforms

	Main	CS Control	
	(1)	(2)	(3)
Panel A: Marriage - Basic Track			
5 Years after Graduation	0.005 (0.006)	-0.001 (0.006)	0.005 (0.007)
8 Years after Graduation	0.003 (0.007)	-0.006 (0.008)	-0.006 (0.007)
10 Years after Graduation	0.003 (0.008)	-0.007 (0.009)	-0.008 (0.008)
N	203,501	203,501	203,501
Panel B: First Birth - Basic Track			
5 Years after Graduation	0.000 (0.004)	-0.000 (0.004)	-0.001 (0.004)
8 Years after Graduation	-0.005 (0.005)	-0.004 (0.006)	-0.007 (0.005)
10 Years after Graduation	-0.011* (0.006)	-0.009 (0.007)	-0.014** (0.007)
N	203,501	203,501	203,501
No CS Control	Yes	No	No
CS Control Refined	No	Yes	No
CS Control Pischke (2007)	No	No	Yes

Notes: The table shows the effect of the short school years on marriage and first birth five, eight, and ten years after graduation for individuals in the basic track, without controlling for compulsory schooling reforms (column 1), controlling for compulsory schooling reforms according to our refined dates (column 2), and the reform dates used by Pischke (2007) in column (3). The latter reform dates coincide with reform dates used in Pischke and von Wachter (2005, 2008). Standard errors clustered at the cohort-state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Placebo Outcome: Track Choice

	Sample 1952	Sample 1950
	(1)	(2)
Basic	0.030*** (0.008)	0.027*** (0.009)
N	271,634	290,336
Middle	-0.024*** (0.004)	-0.020*** (0.005)
N	271,634	290,336
Academic	-0.006 (0.008)	-0.006 (0.007)
N	271,634	290,336

Notes: The table shows the effect of the short school years on secondary school track choice to help identify potential differential trends in treatment and control states. We therefore exclude cohorts 1954-1958 and Lower Saxony in both models to assign the treatment indicator independent from track. Sample 1952 comprises the other cohorts from our main sample, while Sample 1950 additionally includes cohorts 1950 and 1951. In both samples, we control for cohort, state, sex, wave FE and all interactions with sex. Standard errors clustered at the cohort-state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Results for Basic and Middle Track Pooled

	5 Years	8 Years	10 Years
	(1)	(2)	(3)
Panel A: Marriage			
Basic and Middle	0.013*** (0.004)	0.013* (0.007)	0.011 (0.007)
N	301,949	301,949	301,949
Panel B: First Birth			
Basic and Middle	0.001 (0.003)	-0.000 (0.005)	-0.003 (0.006)
N	301,949	301,949	301,949

Notes: The table shows the effect of the short school years on marriage and first birth five, eight, and ten years after graduation from secondary school for individuals pooled together in basic and middle secondary school track based on Equation (2). Standard errors clustered at the cohort-state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Placebo Treatments

Placebo Treatment in			
	-3 years (1)	-2 years (2)	-1 years (3)
Panel A: Marriage			
Basic	0.009 (0.008)	0.011 (0.008)	0.009 (0.007)
N	66,333	66,333	66,333
Middle	0.033** (0.016)	0.012 (0.015)	-0.014 (0.011)
N	17,565	17,565	17,565
Academic	0.021 (0.016)	0.008 (0.020)	-0.023 (0.018)
N	7,043	7,043	7,043
Panel B: First Birth			
Basic	-0.004 (0.006)	-0.003 (0.007)	-0.009 (0.006)
N	66,333	66,333	66,333
Middle	0.007 (0.010)	-0.014 (0.009)	-0.017 (0.011)
N	17,565	17,565	17,565
Academic	0.017 (0.010)	0.006 (0.011)	0.008 (0.011)
N	7,043	7,043	7,043

Notes: The table presents the effects of placebo treatments for our outcomes measured five years after graduation. Placebo treatments assume that the treatment took place 3, 2, and 1 years earlier, respectively. The sample includes just pre-treatment cohorts, i.e. cohorts 1950-1957 for basic, 1950-1956 for middle, and 1950-1953 for academic track. Results look very similar when the four states that have been treated in 1950 or 1951 in line with the Düsseldorf Accord (see Appendix B and Figure B.1 in Koebe and Marcus (2020)) are excluded from this analysis. Standard errors clustered at the cohort-state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Longer Time Horizons (constant sample)

	Years after Graduation					
	10	11	12	13	14	15
Panel A: Marriage						
Basic	0.001 (0.010)	-0.002 (0.010)	-0.006 (0.010)	-0.007 (0.010)	-0.006 (0.010)	-0.008 (0.009)
N	122,803	122,803	122,803	122,803	122,803	122,803
Middle	0.026** (0.011)	0.026** (0.010)	0.018* (0.010)	0.012 (0.010)	0.007 (0.009)	0.001 (0.009)
N	61,960	61,960	61,960	61,960	61,960	61,960
Academic	0.015 (0.010)	0.008 (0.010)	0.006 (0.009)	0.004 (0.009)	-0.003 (0.009)	-0.008 (0.010)
N	68,785	68,785	68,785	68,785	68,785	68,785
Panel B: First Birth						
Basic	-0.014* (0.007)	-0.013* (0.007)	-0.009 (0.008)	-0.006 (0.008)	-0.005 (0.008)	-0.003 (0.008)
N	122,803	122,803	122,803	122,803	122,803	122,803
Middle	0.027** (0.011)	0.020* (0.011)	0.027*** (0.010)	0.023** (0.010)	0.018* (0.010)	0.012 (0.010)
N	61,960	61,960	61,960	61,960	61,960	61,960
Academic	0.019 (0.013)	0.019 (0.014)	0.018 (0.014)	0.019 (0.013)	0.016 (0.013)	0.017 (0.013)
N	68,785	68,785	68,785	68,785	68,785	68,785
Panel C: Second Birth						
Basic	-0.003 (0.006)	-0.009* (0.006)	-0.006 (0.006)	-0.007 (0.006)	-0.010 (0.006)	-0.007 (0.007)
N	122,803	122,803	122,803	122,803	122,803	122,803
Middle	0.013** (0.005)	0.017*** (0.006)	0.014* (0.007)	0.012 (0.009)	0.013 (0.009)	0.014 (0.009)
N	61,960	61,960	61,960	61,960	61,960	61,960
Academic	0.008 (0.009)	0.002 (0.010)	-0.001 (0.010)	-0.001 (0.012)	0.005 (0.013)	0.001 (0.013)
N	68,785	68,785	68,785	68,785	68,785	68,785
Panel D: Third Birth						
Basic	0.007*** (0.002)	0.008*** (0.003)	0.008** (0.003)	0.007* (0.004)	0.007* (0.004)	0.007* (0.004)
N	122,803	122,803	122,803	122,803	122,803	122,803
Middle	-0.002 (0.002)	0.000 (0.002)	0.001 (0.003)	0.000 (0.003)	0.001 (0.003)	0.004 (0.004)
N	61,960	61,960	61,960	61,960	61,960	61,960
Academic	0.002 (0.002)	0.002 (0.003)	0.005* (0.003)	0.005* (0.003)	0.005 (0.004)	0.010** (0.004)
N	68,785	68,785	68,785	68,785	68,785	68,785

Notes: The table shows the effect of the short school years on various outcomes as indicated in the panel header for individuals in different secondary school tracks based on Equation (2). In contrast to Table 5, the sample in each track is constant across specifications. Standard errors clustered at the cohort-state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.