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ABSTRACT

New Evidence on the Effects of the Shortened School Duration in the German States: An Evaluation of Post-Secondary Education Decisions

Most German states have reformed university preparatory schooling during the last decade by reducing its duration from 13 to 12 years without changing the graduation requirements. In this paper, we use nationwide data on high school graduates and apply a difference-in-differences approach to evaluate the reform effects on post-secondary education decisions. The results show that enrollment in university education in the first year after high school graduation is reduced in all analyzed states, while participation in voluntary service or staying abroad is increased. In some subgroups, depending on state, gender and family background, university enrollment is decreased additionally beyond the first year.

JEL Classification: I21, J18, C21

Keywords: school duration, learning intensity, post-secondary education decisions, Germany

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1 Introduction

The choice and definition of the optimal duration of schooling is a fundamental issue for education policy. On the one hand, the rising demands of the (academic) labor markets require high quality education that provides enough time for students to develop skills and to discover their own tastes and talents. On the other hand, earlier entry into the labor market is considered to be a more efficient use of young peoples' human capital, and the resulting longer working life helps to ensure economic prosperity and the sustainability of the tax and social security systems. To solve the trade-off between the length of schooling and the length of labor market participation, the duration of university preparatory schooling in Germany has been reduced from 13 to 12 years over the last decade. However, graduation requirements and the total number of lessons were not changed, so the curriculum has been compressed into the shorter school duration. The main argument for this compression was the aim to achieve the same level of education in a shorter period of time. Graduates should be enabled to start their university education and, subsequently, their occupational career one year earlier.

However, the reform could have several conceivable effects. The notable increase in learning intensity (i.e., students must learn more curriculum per school year and per school week) and the shorter duration of schooling could be either beneficial or detrimental to the quality of education. A number of studies have shown that instructional time is causally related to school achievement. Longer school days improve achievement (e.g., Bellei, 2009), whereas a reduction of instructional time decreases performance (e.g., Marcotte, 2007; Krashinsky, 2014). Consequently, the reform in Germany could decrease the quality of university preparatory schooling. This effect has been shown by Büttner and Thomsen (2015) in an analysis based on data from the state of Saxony-Anhalt. However, there is little empirical evidence for the role of school duration and instructional time for education decisions after high school graduation. Some studies suggest that the content of the high school curriculum affects post-secondary education decisions. For example, Aughinbaugh (2012) and Falch et al. (2014) have found that taking more math classes in high school increases enrollment in higher education. In addition, time spent in school not only provides skills but also helps students to discover their talents and preferences (Schultz, 1968). Because high school graduates affected by the reform enter post-secondary education at a younger age and with less experience of life, they could be less oriented with respect to their decision on further education (cf. Malamud, 2011). Using data from the 2007 double cohort of high school graduates in the state of Saxony-Anhalt, Meyer and Thomsen (2016) have analyzed the effects of the shortened school duration on post-secondary education decisions. The results show that females have delayed university enrollment and increased participation in vocational education.

The present paper complements the available evidence by evaluating the effects of the shortened school duration on a nationwide level. The analysis is based on nationally representative data, namely the graduation cohorts 2006, 2008, 2010 and 2012 of the panel survey of high school graduates in Germany, conducted by the German Centre for Research on Higher Ed-

ucation and Science Studies (DZHW, formerly HIS). Because the reform was not introduced nationwide but introduced separately in most states between 2001 and 2008 (with the first affected cohorts graduating between 2007 and 2016), we can use this timing difference in the reform's implementation across states to identify causal effects. The effects are estimated by applying a difference-in-differences (DiD) approach, using several alternative definitions of the treatment and control groups. Therefore, our analysis provides insights for different states and years on whether and how education decisions made after graduation from high school are influenced by the reform (i.e., by having experienced a shorter school duration and a higher learning intensity at school) and whether the effects are similar or heterogeneous by state.

Our findings show that students affected by the reform are significantly less likely to be enrolled in university education in the first year after school graduation. When we also consider planned enrollment in university education beyond the first year, the effect disappears in most (but not all) specifications in the female sample but tends to remain significant at a lower size for male students. The reason for reduced or delayed university enrollment differs between West and East Germany, at least in the case of female students. While students affected by the reform in West Germany are more likely to spend a year abroad or to engage in a year of voluntary service after high school graduation, affected students in East Germany have a higher probability of starting vocational education and therefore delay university attendance by more than one year. However, participation in vocational education is also slightly increased for male students in West Germany. Altogether, the results are consistent with the literature on the role of school duration. The robustness of the findings is also supported by the results provided in Meyer and Thomsen (2016) for the reform in Saxony-Anhalt. Using other data and a different estimation strategy, their main result – reduced or delayed university enrollment and increased participation in other activities after high school graduation – can be found for all analyzed states.

The rest of the paper is organized as follows. The education system in Germany and the reform are described in section 2. The empirical approach is contained in section 3. Section 4 presents the results and robustness checks. Section 5 concludes the paper.

2 The Reform

2.1 The Education System in Germany

Although education policy in Germany is the responsibility of the federal states, it is nevertheless broadly similar across states. After primary schooling of four years, students are tracked into one of three secondary school types. The basic and intermediate tracks include schooling up to grade 9 and 10, usually followed by vocational education in the German apprenticeship system. The higher secondary school track (high school, *Gymnasium*) leads to the university admittance qualification (*Abitur*), which is obtained – depending on the state law – after 12 or 13 years of schooling in total. In addition to high school, it is also possible to obtain the university admittance qualification (or at least the qualification for admittance to universities of applied

sciences) at vocational high schools or comprehensive schools. However, these schools represent only a small part of the German school system.

After graduation from high school, students can choose between two tracks of post-secondary education. According to numbers provided by Quast (2012), approximately 85% of high school graduates choose *university education*, which can be distinguished into studying at a university, a university of applied sciences or a professional college. After three years, university education leads to a bachelor's degree, often followed by two years to obtain a master's degree. Both degrees correspond to an ISCED qualification level of 5 (UNESCO, 1997). The other track, chosen by almost 25% of high school graduates, is *vocational education* (approximately 10% of graduates do both university and vocational education in succession). Vocational education consists in the majority of cases of an apprenticeship. The German apprenticeship system is internationally quite unique and combines practical on-the-job training in a firm or in public service where the trainee is employed with part-time attendance at a vocational school. Usually, it takes about three years until completion. The German apprenticeship system is acknowledged to provide a high-quality education (OECD, 2010) and is classified as post-secondary non-tertiary education. A completed vocational education corresponds to an ISCED qualification level of 4 if the student has also obtained the university admission qualification; otherwise, it corresponds to a level of 3.

The process of gathering information about post-secondary education possibilities and thinking about personal preferences and aims starts in the last years of high school. Approximately 55% of students begin two or three years before graduation, whereas approximately 45% do not start before the last year of high school (Schneider and Franke, 2014, p. 25). To start a vocational education, students must apply for their desired job several months before the intended starting date, which is usually August or September. To start a university education, students must apply to their desired university for their desired subject. Some subjects in Germany have restricted admission; here, applications must be submitted to the desired university by July for enrollment in October. In addition, a very few subjects (medical sciences and pharmacy) require an application to the Foundation for University Admission (*Stiftung für Hochschulzulassung*, formerly *Zentralstelle für die Vergabe von Studienplätzen, ZVS*). Other subjects, especially the so-called STEM subjects, which include natural sciences, technology, engineering and mathematics, can mostly be studied without restriction.

Some students take a year off between high school graduation and starting university or vocational education for other activities, for example, performing an internship or engaging in voluntary service or spending a year abroad (approximately 15 to 25% of graduates, see Schneider and Franke, 2014, p. 121-122). Until 2011, males were principally obliged to engage in military or civilian service for nine months, which often started shortly after school graduation.

2.2 The Reform of School Duration

The debate about the duration of university preparatory schooling has a long history in Germany. West Germany had a decades-long tradition of 13 years, which became increasingly questioned in the middle and late 1990s. This debate was supported by the fact that the East German states used a 12-year policy until the German reunification in 1990 (after reunification, they mostly adopted the West German system). As a consequence, between 2001 and 2008, most federal states eliminated the last year of high school (see Table 1). The only exceptions were Saxony and Thuringia, which maintained the duration of 12 years after the reunification, and Rhineland-Palatinate, which left its system of 12.5 years unchanged.

The implementation of the reform, by and large, was similar across states. In all states, graduation requirements were maintained, which means that the curriculum was compressed into the shorter school duration. The reform implementation was completed in each state with the so-called double cohort of graduates, which includes the first cohort graduating after the shorter school duration of 12 years and the last cohort graduating after 13 years. The first double cohort graduated in Saxony-Anhalt in 2007, followed by one or more other states in each of the subsequent years (see Table 1). Despite these similarities, some differences should be noted. Whereas in many states, the first affected cohort was students entering grade 5 (which is the first grade of the *Gymnasium*), in some states, the change was introduced in higher grades. Furthermore, it depends on the state whether the reform applies exclusively to high schools or to comprehensive schools as well.

Insert Table 1 about here

As a consequence of the reform, students had to learn the same curriculum within a shorter time period. Therefore, the learning intensity experienced at school (i.e., the amount of learning content per school year or school week) notably increased. This could have positive as well as negative effects. On the one hand, the efficiency of learning and the ability to cope with academic requirements could be improved. In this case, post-secondary education decisions should not be affected or even more students could choose university education or challenging university subjects. On the other hand, it could be detrimental for learning outcomes, for example, by overtaxing students or by leaving fewer possibilities for teaching and revising the learning content with the necessary depth. Consequently, students could be or could feel less prepared for university and could choose a less demanding track or subject in their post-secondary education. In addition, post-secondary education decisions could be influenced by the reform through another channel. Due to the shorter school duration and the younger age at graduation, students have one year less to get to know their own abilities and to develop occupational preferences. Therefore, the insecurity about what to do after school graduation could be increased. This could prolong the time until entry into post-secondary education or could lead more students to, as a precaution, first start a less demanding course of post-secondary education.

The empirical evidence available to date confirms several implications of the reform (see

Thomsen, 2015, for an overview). Büttner and Thomsen (2015) have found a negative effect on achievements in mathematics at the end of high school. Furthermore, an increase in grade repetition is reported by Huebener and Marcus (2015). Analyses with respect to personality traits show only small (Thiel et al., 2014) or mixed effects (Dahmann and Anger, 2014). Furthermore, an effect on education decisions after high school graduation, namely a later (but not lower) enrollment of female students in university education, has been identified by Meyer and Thomsen (2016) using data from Saxony-Anhalt. Finally, some studies indicate that the reform has not affected students' success at university (Kühn, 2014; Dörsam and Lauber, 2015). Although Meyer and Thomsen (2013) observe a few shifts at the intensive margin (measured by subjective perceptions of motivation and abilities), these changes are not large enough to have an impact on the extensive margin, i.e., on the probability of dropping-out of university education. However, the question remains whether the identified effects are generally valid, i.e., whether they can be applied to all states in Germany, which had slightly different contexts for reform introduction.

3 Empirical Approach

3.1 The Data

For the empirical analysis of reform effects on post-secondary education decisions, we use data of high school graduates from all German states that are provided by the German Centre for Research on Higher Education and Science Studies (DZHW). Every two or three years since 1976, the DZHW has surveyed students from randomly selected schools in all federal states (approximately 12% of all high schools). Since 2006, the surveys have been conducted as short panels with a first wave half a year before high school graduation. Students are asked by means of a written questionnaire about their experiences at high school, their plans after school graduation, the process of information collection and related problems. In a second wave half a year after graduation, the same students are asked about their realized or firmly planned post-secondary education. The final and third wave for each cohort is conducted three and a half or four and a half years after graduation, by which the observations on post-secondary education are updated.[1] The data used in this paper include the 2006, 2008, 2010 and 2012 cohorts of graduates. Data from the third wave are currently only available for the 2006 and 2008 cohorts. Thus, post-secondary education decisions can only be investigated with respect to the time period of half a year after school graduation. However, education plans are sufficiently concrete at that point in time. Data from the 2006 and 2008 cohorts show that 93% of students who state in the second wave that they firmly plan to enroll in university education have realized this plan three or four and a half years after school graduation. Another 3% are still firmly planning to attend university. The correspondence between plans and realization is further supported by Ajzen (1991), who has shown that an intention is a basic precondition and usually a reliable predictor of eventual action. Therefore, information provided half a year after school

[1] A description of data collection can be found, for example, in Schneider and Franke (2014).

graduation can be considered to represent the educational pathways of students in at least the first two or three years after school graduation.

Some modifications of the data are necessary for the analysis. First, all students who did not graduate from high school (but graduated, e.g., from comprehensive schools or vocational high schools) or who did not obtain the general university admittance qualification (but obtained, e.g., only the qualification for admittance to universities of applied sciences) are excluded because the reform does not apply to vocational high schools, integrative comprehensive schools and, in some states, cooperative comprehensive schools. We also exclude students, who do not belong to the respective birth cohorts.[2] Thus, students who repeated or skipped a grade are not considered. The estimation sample therefore includes 5,383 observations in 2006, 10,380 in 2008, 9,353 in 2010 and 13,374 in 2012 of students who participated in the first survey half a year before graduation. Unfortunately, the data are affected by panel attrition, and not all students participated in the second wave of the survey. Thus, information about post-secondary education decisions is only available for 2,855 observations in 2006, 3,005 observations in 2008, 3,582 observations in 2010 and 5,690 observations in 2012. It should be mentioned that the analyses on the basis of these observations below take into account panel attrition by means of a weighting factor.

Enrollment in university and vocational education is observed half a year after school graduation and is analyzed with respect to two outcome dimensions. A first binary variable includes actual enrollment, whereas the second dummy indicates whether a student is actually enrolled or firmly plans to enroll in university or vocational education as first post-secondary education (e.g., after having completed military, civilian or voluntary service or some other activity in the year after school graduation). To minimize a potential bias from insecure plans and decisions, only students who have already decided on their post-secondary education are included in the second dummy variable. However, more than 97% of students have made this decision at the time of the survey, and in almost all cases, they intend to enroll one year after school graduation.[3]

Moreover, three other activities in the year after high school graduation are captured covering (1) military or civilian service, (2) internship or temporary work, and (3) voluntary service or a year abroad. Finally, we distinguish the choice of university subjects, which is measured with regard to actual and firmly planned university enrollment. University subjects are categorized into six groups: (1) humanities, (2) education and social sciences, (3) law and economics, (4) engineering, (5) natural sciences and mathematics, and (6) medical sciences. Due to the particular importance of engineering, natural sciences and mathematics (the so-called STEM

[2]The cut-off birth date for a school year in Germany is 30 June. Hence, students in a given cohort are usually born between 1 July of the respective year and 30 June of the following year. Only these students are included.

[3]The question in the survey from which the variable *firmly planned enrollment* is obtained, contains three response categories: (1) "I have decided to enroll in university/vocational education (or to do something else)", (2) "I have not finally decided, but I will probably enroll in university/vocational education (or do something else)", and (3) "I have until now absolutely no idea about my further education". Only category (1) is considered in the variable on planned enrollment, but it contains almost all students. Fewer than 3% of students belong to categories (2) and (3).

subjects), these subjects are additionally considered as a group. Medical sciences are content-related to STEM subjects, but it is not clear whether they belong to this group. Therefore, STEM subjects are considered in the analysis first with a narrow definition (without medical sciences) and second with a broader definition (including medical sciences).

3.2 Identification Strategy

To evaluate the reform effects on post-secondary education decisions, we use a difference-in-differences approach. The different timing of the introduction of the reform in the federal states provides regional variation, which enables a comparison of students who graduated under the old system with 13 years and under the new system with 12 years. Ten states completed the reform between 2007 and 2012, which means that the first students graduated from high school after 12 years of schooling (see Table 1).

Therefore, we can use 2012 as the post-reform year and the cohorts 2006, 2008 and 2010 as the pre-reform periods. However, we concentrate on 2008 as the pre-reform year because the education decisions of the 2010 graduation cohort could be influenced by the upcoming double cohorts in 2011 in two large federal states (Bavaria and Lower Saxony). Students from the 2010 cohort had an incentive to accelerate their enrollment in post-secondary education to avoid competition with the double cohort. This did in fact take place in the case of female students (see Figure 2 in the next section). Hence, estimates based on 2010 as the pre-reform year could be biased. However, the 2008 cohort is not affected by these or other influences and can therefore be used as the pre-reform period. Further including the year 2006 is therefore not necessary; moreover, the greater distance in time between 2006 and 2012 may potentially allow other unintended effects for our analysis. Nevertheless, we will carry out alternative estimations with the 2006 cohort.

In our main specification, the years 2008 and 2012 represent the pre- and post-reform periods. The treatment group consists of the three West-German states that completed the reform introduction in 2010 and 2011 – Bavaria, Hamburg and Lower Saxony (*treatment group 1*). All other reform states are not included for the following reasons: Saxony-Anhalt and Mecklenburg-Western Pomerania completed the reform in 2007 and 2008 and are therefore not considered. For the small state of Saarland (completed the reform in 2009), no observations are available in 2012. Finally, the four states with a double graduation cohort in 2012 (Baden-Wuerttemberg, Berlin, Brandenburg and Bremen) are not included because the first affected cohort could be viewed as a special situation, which is not representative. Especially in a large state such as Baden-Wuerttemberg an increased competition for study places could confound the analysis.[4] The four states that had no graduates with the reduced school duration before 2013 (North

[4] A double cohort in one small state in one year should not be a problem, but the occurrence of several double cohorts in one year, and additionally in larger states, could limit the availability of study places.

Rhine-Westphalia, Hesse[5], Schleswig-Holstein and Rhineland-Palatinate[6]), can be used as the comparison group (see panel (a) of Figure 1).[7]

Insert Figure 1 about here

In addition, we estimate the reform effects with alternative definitions of the treatment group.[8] First, we additionally include three states with a double cohort in 2012 (Berlin, Brandenburg and Bremen) in the treatment group (*treatment group 2*). Of course, in 2012, the entire double cohort is not included but only the students with 12 years of schooling. Baden-Wuerttemberg is still left aside due to the potential effects from competition for study places within this state. As a second alternative, we supplement the original treatment group with two additional states, namely Saxony-Anhalt and Mecklenburg-Western Pomerania, which completed the reform introduction in 2007 and 2008 (*treatment group 3*). This treatment group can be used for the estimations with 2006 as the pre-reform year. Finally, we use the years 2006 and 2010 as pre- and post-reform periods and define a different treatment group, namely the states that completed the reform between 2007 and 2010. These are two larger states from East Germany, Saxony-Anhalt and Mecklenburg-Western Pomerania, as well as two small states from West Germany, Saarland and Hamburg (*treatment group 4*). Because Saxony-Anhalt and Mecklenburg-Western Pomerania represent more than 70% of the students in this treatment group, this specification can be seen as an analysis of the reform effects in East Germany, in comparison to the original specification, which is focused on the effects of the reform in West Germany. The corresponding control group consists of all states with reform completion after 2010 (Baden-Wuerttemberg, Bavaria, Berlin, Brandenburg, Bremen, Hesse, Lower Saxony, North Rhine-Westphalia, Rhineland-Palatinate and Schleswig-Holstein).

The causal effect of the reform is then estimated by the following difference-in-differences probit model:

$$Prob(E_{p,s,i} = 1) = \Phi(\beta_0 + \beta_1 TREATGR_i + \beta_2 POST_i + \beta_3 DID_i + \beta_4 \mathbf{X}_i + \gamma_s). \qquad (1)$$

The binary outcome variable is denoted by $E_{p,i}$. $Prob(E_{p,i} = 1)$ is then the probability of individual i from state s being enrolled in a specific type of post-secondary education p (i.e., university education, vocational education, several other activities in the year after school graduation, and several university subjects). Each outcome is estimated separately. On the right

[5]Hesse introduced the reform in not one cohort but in three subsequent cohorts, depending on the school. In 10% of the schools, the first affected students graduated in 2012, whereas in 90% of the schools, the first cohort with only 12 years of schooling graduated in 2013 or 2014. The few students graduating after 12 years in 2012 are not included in the sample.

[6]As mentioned before, Rhineland-Palatinate has not introduced the reform but kept constant its system with 12.5 years of schooling. However, in practice, it is a system with 12.7 years and therefore more similar to graduation after 13 than after 12 years.

[7]Table A.1 in the appendix presents the composition of the treatment and control groups by federal state.

[8]It would be conceivable to also include additional states in the control group, namely Saxony and Thuringia, which had no reform but maintained the system of 12 school years. However, although the duration of high school did not change, the number of lessons was increased in both states. Thus, the learning intensity increased as it did in the treatment states, which makes it problematic to use Saxony and Thuringia as control states.

hand side of equation (1), β_0 is the constant. $TREATGR_i$ is a dummy variable that takes the value 1 if an individual belongs to the treatment group and 0 if an individual belongs to the control group. Thus, the coefficient β_1 captures the non-reform difference between students from the treatment and control groups. $POST_i$ indicates the time period and equals 0 for the pre-reform (2008) and 1 for the post-reform period (2012), with the coefficient β_2. The interaction term between $TREATGR_i$ and $POST_i$ is denoted by DID_i, which is equal to 1 if an individual belongs to the treatment group in the post-reform year. The marginal effect, derived from the corresponding coefficient β_3, indicates the impact of the reform, namely the average treatment effect (ATE).[9]

To consider differences between years and groups and to increase the efficiency of the estimates, further variables influencing post-secondary education decisions are included in the regression in $\mathbf{X}_i$. These are dummy variables that indicate whether at least one parent has an academic degree, whether the student has a migration background, and whether the student belongs to the older group of students in the respective cohort (i.e., born between 1 July and 31 December).[10] In addition, the number of books owned by parents as well as the current or most recent occupational position of parents (measured by the International Socio-Economic Index of Occupational Status, ISEI; see Ganzeboom et al., 1992) are considered by two dummy variables for the middle and upper category of each of the two categorical variables. Finally, state dummies γ_s capture the influence of characteristics of the federal state in which the students have graduated from high school.[11] The analysis is conducted separately for males and females because post-secondary education decisions differ by gender (see, e.g., Buchmann et al., 2008; for Germany see, e.g., Lörz et al., 2012), and the reform in Saxony-Anhalt showed different effects for males and females (Büttner and Thomsen, 2015; Meyer and Thomsen, 2016).

We compare the outcomes of high school graduates in 2008 (pre-reform) and 2012 (post-reform) in the treatment group (first difference). Then, we compare this difference with the respective difference in the outcomes of the control group, which is not affected by the reform. From this comparison (second difference), the causal effect of the reform is obtained. With this procedure, any common time trend between 2008 and 2012 as well as differences in the students' characteristics between the treatment and control groups are eliminated from the analysis.

3.3 Validity of the Identification Strategy

Identification requires that no selection bias between groups is present. This requirement should be fulfilled because from the students' perspective, the reform was randomly introduced. In the respective states, reform implementation took place within a short period of time (especially in

[9] Puhani (2012) has shown that in nonlinear difference-in-differences models, the incremental effect of the coefficient of the interaction term represents the treatment effect.

[10] Several studies have shown that older students within a cohort show better education outcomes than younger students. This relative age effect can persist even beyond secondary schooling (Fredriksson and Öckert, 2014; Bedard and Dhuey, 2006; Crawford et al., 2010).

[11] Dummies are included for each state except for one state from the treatment group and one state from the control group (reference states).

Bavaria and Hamburg).[12] Students had only very few possibilities to evade the reform. Evasion would only be possible by moving or commuting to another state, by switching to another type of school (in two of the three treatment states, it is still possible to graduate after 13 years at comprehensive schools), or by skipping or repeating a grade. However, moving or commuting to another state would include very high monetary and non-monetary costs. Moving to another school type would be easier, but official statistics by the Federal Statistical Office (n.y.a) do not provide indications that more students in the treatment states moved to comprehensive schools relative to the *Gymnasium* after the introduction of the reform. Furthermore, the German education system normally does not provide the possibility of fast-tracking school by skipping a grade. Finally, official statistics do not indicate a large increase in grade retention in the treatment states. The shares of students dropping out of their cohort in the last two years of high school are not very different between the cohorts graduating from 2006 to 2013 in the treatment states (see Table A.3 in the appendix).

There are further assumptions that must be fulfilled for identification (see, for example, Meyer, 1995). There should be no interaction between time and groups except for the treatment, i.e., any time trends must be equally existent in both groups, and any group impacts must be constant over time. This should be the case here because the analyzed period contains only four years, over which social and macroeconomic conditions have not changed differently across states. Furthermore, student characteristics should not be too different between treatment and control groups, and any changes in these characteristics should be similar between groups. This will be checked in the next section.

However, an interaction between time and groups could occur if other education reforms have been introduced in the states at different points in time. An overview of other reforms is provided in Table A.4 in the appendix. The first candidate would be other changes in the high school system that have been implemented a few years ago in several states (e.g., central final examinations, earlier tracking, changed curriculum). However, central final examinations have been in force in many states for a long period of time and in the remaining states since no later than 2008. Tracking has not been changed in the treatment and control states except for Lower Saxony, where tracking was moved forward from grade 7 to 5 since the 2011 graduation cohort. Nevertheless, earlier tracking should not have a large effect on upper secondary schooling.

The changes in the high school curriculum (e.g., restricted subject choice, additional examination subjects) vary across states with respect to content and timing. In some states, the changes were already in force in 2008, in other states, they were introduced for graduation cohorts between 2008 and 2012, and in still other states, no changes have been made. Although these reforms were not as substantial as the shortened school duration, the different timing of introduction could potentially confound the analysis. Therefore, this issue will be checked in section 4.6.

A second reform to consider is the change of the study programs to the bachelor's and

[12]Table A.2 in the appendix provides more information on the reform introduction in the treatment states.

master's degrees (the so-called Bologna process). However, this reform was introduced almost simultaneously in all German states and had been largely completed by 2008. The share of university entrants being enrolled in a bachelor's program was approximately 68% in 2008 and 77% in 2012 (the difference from 100% is because some study programs have not been affected by the Bologna process). These numbers are also valid for the subgroups of students from the treatment and control groups (cf. Schneider and Franke, 2014, p. 155-159). Similarly, the abolition of military service in 2011 affects students from treatment and control states equally (cf. Table 3 in the next section).

A final, possibly relevant reform is the introduction of university tuition fees in some states. However, in most states under investigation, the introduction had already taken place in 2008 and was still valid in 2012 or fees had not been introduced in either of the two years. Moreover, empirical evidence suggests that the introduction of university tuition fees in Germany had no influence on university enrollment (e.g., Helbig et al., 2012, Bruckmeier and Wigger, 2014). Thus, conditions for post-secondary education decisions could be assumed to be the same in both years even in states with changes in tuition fees.

A key assumption in any difference-in-differences analysis is that the outcomes for students in the treatment and control groups would follow the same time trend in the absence of the treatment. Although the counterfactual outcome is unobservable, similar pre-treatment trends could be seen as a verification of this common trend assumption. Figure 2 shows the shares of students who started (or firmly plan to start) university education after high school graduation between 2002 and 2012. The outcomes have similar values and show similar development in the treatment and control groups until reform introduction. This is the case for female and male students as well as for actual and firmly planned enrollment. Only with respect to females' actual enrollment, the year 2010 represents a special situation. In this year, students in Bavaria and Lower Saxony had a large incentive to start university as soon as possible due to the large expected inflow of high school graduates from the double cohort in 2011. Therefore, the 2010 graduation cohort is not included in the analysis (as mentioned in section 3.2). However, enrollment trends are parallel until 2008, and the common trend assumption is therefore fulfilled.

Insert Figure 2 about here

The presented identification strategy has the advantage that the analyzed students do not represent the first affected cohorts. This means that any implementation effects or temporary effects, possibly caused by the double cohorts in 2010 and 2011, are unlikely to be still present in 2012. It is also not likely that access to university education is restricted above average in the analyzed states in 2012. According to official statistics from the Federal Statistical Office (n.y.b), the number of study places was increased in 2011 in Bavaria and Lower Saxony as well as in the neighboring states for the additional university entrants from the double cohorts. In 2012, the situation in these states had largely normalized. Confounding influences from the states with a double cohort 2012 are also unlikely to exist. The number of additional students from the small states of Berlin, Brandenburg and Bremen is too low to have an influence on the states in the

treatment and control groups (approximately 9,000 additional high school graduates compared to a total of approximately 305,000 high school graduates in Germany in 2012). Furthermore, the larger double cohort from Baden-Wuerttemberg had almost no influence on the neighboring state of Bavaria because the number of students from Baden-Wuerttemberg starting university in Bavaria 2012 is not significantly higher than the number in 2010 (an increase of 650 students from Baden-Wuerttemberg compared to approximately 64,000 university entrants in Bavaria). Finally, a comparison of admission grades at some universities in Bavaria and Lower Saxony shows that grades remained relatively constant in most subjects between 2009 and 2012 (see Table A.5 in the appendix), which indicates that competition for study places had not increased due to the double cohorts.[13]

3.4 Descriptive Statistics

Table 2 contains a description of several characteristics of students from the treatment and control groups in the pre- and post-reform period. The age of students appears to be equally distributed within cohorts. Only the 2012 graduates from the control group are a bit younger on average. Approximately 15% of students have a migration background, which varies only slightly across years and groups. The educational background of students is also similar for both groups and years. The share of students whose father or mother had also graduated from high school is approximately 50% in the female sample and approximately 55% in the male sample. With respect to the question of whether at least one parent possesses an academic degree, a slight time trend can be observed in the treatment group. Compared to 2008, slightly more students come from a non-academic family in 2012. This could be due to the increasing social openness of high schools in Germany for many years (see, for example, Trautwein and Neumann, 2008). The third indicator of students' educational background, the number of books at the parental home, has a similar distribution and development between groups. Finally, the occupational position of the parents, as measured by the International Socio-Economic Index of Occupational Status, is mostly similar across time and groups. Altogether, it can be concluded that the values of most variables are similar between groups and have similarly developed over time. Thus, a selection bias is unlikely to exist. The few differences do not reveal a systematic pattern. For example, the two significant differences in the female sample could indicate that students in the treatment group come from families with a slightly higher education and occupational status. However, the share of students whose parents have an academic degree has decreased more in the treatment group than in the control group. Therefore, the differences should not be a problem. Nevertheless, they underscore that consideration of sociodemographic and family background characteristics in the analysis is reasonable.

Insert Tables 2 and 3 about here

[13] In a few subjects, an increase in admission grades can be observed. However, this should not be a problem, because in some cases, admission grades also increased in other years, which are not related to the double cohorts (e.g., from 2009 to 2010). Furthermore, not only admission grades but also the average grades of high school graduation can vary across years.

A description of outcome variables is shown in Table 3, which also provides first indications of reform effects. Half a year after high school graduation, approximately 75% of females are enrolled in post-secondary education. This share declined to nearly 60% in the treatment group in 2012, while it slightly increased to 80% in the control group. The decline is mainly driven by reduced university enrollment and a notably increased share of students engaging in voluntary service or spending a year abroad. In the case of male students, the enrollment shares in post-secondary education have increased from approximately 50% in 2008 to more than 70% in 2012. The increase is larger in the control group than in the treatment group. The reason for the increase in both groups is that the obligation to perform military or civilian service was eliminated in 2011. Therefore, the share of male students performing an activity other than post-secondary education has decreased from approximately 50% to 26% and 16% in the treatment and control states, respectively.

The enrollment shares in post-secondary education increase for both genders to between approximately 97% and 99% when firmly planned enrollment (which in almost all cases takes place one year after graduation, e.g., after having completed voluntary service) is included. Altogether, approximately 80% of female students start or firmly plan to start university education, and approximately 20% vocational education. Male students are slightly more likely than females to enroll in university but have a lower probability of choosing a vocational education. In addition, one can observe a decrease in started and planned university enrollment as well as an increase in vocational education in the male treatment group, whereas both shares remain nearly constant in the control group. With respect to the subject of the started or firmly planned university education, almost no differences exist between students from the treatment and control groups.

4 Estimation Results

4.1 Reform Effects on Post-Secondary Education Decisions

The estimation results of our main specification are presented in the first column of Table 4 (pre-/post-reform years 2008/2012, treatment group 1). Only the reform effects are reported, but all other explaining variables are considered in the estimations as well. Effects on university and vocational education are shown with respect to actual enrollment (i.e., already started six months after graduation) as well as actual and firmly planned enrollment (i.e., already started or firmly planned within the next year). The reform has significantly reduced enrollment in university education in the first year after high school graduation by approximately 15 percentage points for male and female students. If enrollment plans are included, the effect on male students decreases to approximately 8 percentage points but remains statistically significant. However, the effect on female students disappears, which is also true with respect to vocational education. In contrast, the actual and planned enrollment in vocational education of male students is slightly increased by approximately 7 percentage points.

It should be noted that the results do not imply that for female students, no effects exist

beyond the first year. Enrollment plans only include initial post-secondary education and not later revisions or participation in additional courses of education (e.g., attending university after vocational education). Therefore, it is still possible that the probability of starting university or vocational education is affected by the reform in the long-term.

Insert Table 4 about here

In addition to starting university or vocational education, students often use the year after high school graduation for other activities. Performing military or civilian service (obligatory for many males), spending a year on voluntary service in social, ecological or cultural institutions, spending a year abroad (e.g., work and travel, au pair), engaging in an internship or working temporarily are the most common activities. Military and civilian service is not affected by the reform because it is under the control of the official authorities. It should be noted that the obligation for males to do military or civilian service was abolished in 2011. The probability of taking an internship or temporary work is slightly increased for female high school graduates by 5 percentage points, but no effect can be observed for male students. Compared with this, the reform has increased the probability of spending a year abroad or performing voluntary service by approximately 12 percentage points for females and 8 percentage points for males.

Finally, the choice of the field of study for actual or firmly planned university education is not changed by the reform. The effects on almost all subjects are small and insignificant. Only the probability of male students studying education sciences, in particular teaching professions, is slightly reduced (result not shown). More importantly, the STEM subjects (engineering, natural sciences, mathematics, medical sciences), which have particular importance for labor supply and macroeconomic prosperity, are not affected – neither the individual subjects nor the STEM subjects as a whole. These findings are valid for both genders and regardless of whether all high school graduates are considered or only those who started or firmly plan to start university education.

4.2 Estimations with Alternative Definitions of the Treatment Group

In addition to the results presented above, we conduct estimations with several alternative specifications (see section 3.2). The results are presented in the second to fifth and seventh to tenth column of Table 4. At first, we additionally include Berlin, Brandenburg and Bremen, the states with a double cohort of graduates in 2012, in the treatment group. This does not change the results, as shown by the second and seventh column of Table 4. Only the effects on voluntary service and staying abroad as well as on male university enrollment beyond the first year after school graduation decrease slightly in size, but they are still statistically significant.

Next, we use 2006 as the pre-reform year instead of 2008. This is done once with the original treatment and control groups (results in columns 3 and 8) and once with two additional treatment states, namely Saxony-Anhalt and Mecklenburg-Western Pomerania (TG 3, results in columns 4 and 9). These specifications confirm the findings for both genders with respect

to actual university enrollment as well as voluntary service and staying abroad. However, in the male sample, the effects on university and vocational education including firmly planned enrollment become a bit smaller and statistically insignificant. However, some significant effects occur that cannot be found in the original specification. The probability of females choosing a STEM subject (narrowly defined) is slightly reduced by 5 percentage points if Saxony-Anhalt and Mecklenburg-Western Pomerania are included. Male students are more likely by 6 to 9 percentage points to engage in an internship or temporary work and less likely by 10 to 15 percentage points to study a STEM subject. However, the latter result must be interpreted with caution because the male students in the treatment group in 2006 show an atypical high share of enrollment in STEM subjects, while the numbers are similar in all other years and in the control group. Thus, the results with respect to subject choice based on 2008 as the pre-reform year are more credible.

The final alternative specification uses a different treatment group, namely the states of Saxony-Anhalt, Mecklenburg-Western Pomerania, Saarland and Hamburg, which completed the reform between 2007 and 2010 (TG 4). In contrast to the previous treatment groups, which are concentrated on the West German states, this treatment group consists largely of students from East Germany.[14] The corresponding control group includes all states with reform completion after 2010, and the years 2006 and 2010 represent the pre- and post-reform periods. The results in columns 5 and 10 show that the reform has a slightly different impact in these states and years. The participation of female students in university education is not only reduced in the first year after high school graduation but also with respect to firmly planned enrollment by 9 percentage points. In addition, the probability of choosing a vocational education is slightly increased by approximately 7 percentage points. However, the effect on performing voluntary service or staying abroad is lower than in the original specification. In contrast, the effects on the university and vocational enrollment of male students are smaller and insignificant (except for enrollment in vocational education in the first year after graduation, which is increased in this specification). Moreover, similar to the other specifications using 2006 as the pre-reform year, the probability of male students studying a STEM subject is reduced by 17 to 19 percentage points.

A possible explanation for the slightly different findings between the first and last specifications is that the reform delayed enrollment in university education in both cases, but students affected by the reform in West and East Germany chose different alternatives. In West Germany, the increased delay of female students is caused by a notably higher participation in voluntary service or staying abroad for one year, whereas this mechanism is of smaller importance in East Germany. Here, the reform effect works via a higher probability of choosing vocational education (which takes longer than one year). The other difference, i.e., finding no significant effects on males' enrollment in university and vocational education in 2010, could be explained by the fact that many male students were obliged to perform military or civilian service until

[14]Further restricting this specification to only students from East Germany, i.e., excluding Hamburg and Saarland, leads to very similar results.

2011. This gave them a further year to decide on their post-secondary education, which may have reduced the influence of the reform on male students until 2011.

Apart from the different findings in the last specification, the other alternative specifications lead to consistent estimation results, which are not different from those obtained by the main specification. Only the effects on males' enrollment beyond the first year after school graduation cannot be found in all cases.

4.3 Estimations with fewer Treatment States

Although the reform implementation was in principle similar across the federal states, some differences exist. To investigate potential effect heterogeneity across states and to rule out that the reform effects are driven by a specific state, regressions are conducted, in each of which one state from the treatment group is excluded. This means that only two treatment states (instead of three) are considered at a time. The results in Table 5 show that the effects of the reform on actual enrollment in university education, on engaging in voluntary service or staying abroad and on females' participation in an internship or temporary job remain valid. Nevertheless, some effect heterogeneity across states can be observed. If Hamburg is omitted, most effects become larger, especially in the male sample, whereas the effects decrease in size if Lower Saxony is excluded from the treatment group. Furthermore, the effects on males' enrollment in university and vocational education beyond the first year remain constant in size but lose their statistical significance if Bavaria or Lower Saxony are excluded. However, the p-values are mostly near to the 10%-level.

Insert Table 5 about here

These slightly heterogeneous results suggest that students in the small state of Hamburg are less influenced by the reform, whereas effects are a bit larger in Bavaria and Lower Saxony. This may indicate that the effects of the reform can vary slightly depending on the states that are analyzed. However, it remains unclear whether this is due to characteristics of the student body or due to the way of reform implementation. Nevertheless, it should be noted that the main effects of the reform are similar across states.

4.4 Effect Heterogeneity according to Students' Family Background

The findings obtained so far represent average effects across all high school graduates. However, it could be the case that students with different characteristics are differently affected by the reform. It is therefore important to examine the heterogeneity of the reform effects. Because one of the main determinants of post-secondary education decisions is the educational family background of the students, we split the sample into students coming from a family in which at least one parent has an academic degree and students from a non-academic background.

Insert Table 6 about here

The results from the separate estimations are presented in Table 6. Generally speaking, almost all reform effects are driven by students coming from non-academic families. In the male sample, the effects are large in the non-academic subgroup, while only a few effects can be found for students from an academic family. In the case of female students, university enrollment in the first year after high school graduation is negatively affected in both groups. However, students from a less educated family background are affected to a larger extent, and their enrollment is additionally reduced beyond the first year. An interesting picture emerges with respect to the other activities in the year after high school graduation. Similar to the other outcomes, the probability of engaging in an internship or working temporarily is only increased for females coming from a non-academic family. In contrast, the effect on voluntary service or staying abroad is larger for female students with an academic background.

4.5 Robustness Check I: Placebo Difference-in-Differences Estimation

A common sensitivity check in difference-in-differences analyses is to perform placebo tests. We use observations not affected by the reform as the treatment group. First, the two northern states from the control group (Schleswig-Holstein, North Rhine-Westphalia) are used as the treatment group and are compared to the two southern states in the control group (Hesse, Rhineland-Palatinate). Second, we compare the states from East Germany, which only had the system of graduation after 12 years in 2008 and 2012 (Saxony, Saxony-Anhalt, Thuringia) to the original control group. Thirdly, students from the three states that had a double cohort of graduates in 2012 (Berlin, Brandenburg, Bremen) are used as the treatment group (but only students graduating after 13 years) and compared to the original control group. If the abovementioned findings represent causal effects of the reform, they should disappear in the three placebo tests. This is mostly the case, as almost all effects are small and insignificant (Table A.6 in the appendix). Only four out of 48 coefficients (one in the female sample and three in the male sample) are slightly statistically significant. Therefore, we conclude that the placebo tests support the identified reform effects.

4.6 Robustness Check II: Possible Confounding Effects of Other Reforms

Finally, as mentioned in section 3.3, a few other education reforms have been introduced in the analyzed period. Because they could possibly confound the effects of the shortened school duration, we have conducted several robustness checks. At first, the states that changed their high school curriculum between 2008 and 2012 are excluded from the analysis (Bavaria, Hamburg and Schleswig-Holstein; see Table A.4 in the appendix). As column (1) of Table A.7 in the appendix shows, the reform effects remain stable. Only the effects on voluntary service and staying abroad become insignificant in the male sample.

Secondly, only the state of Bavaria is used as the treatment group. In contrast to Lower Saxony and Hamburg, it is not possible in Bavaria to obtain the university admittance qualification at comprehensive schools (and almost no comprehensive schools exist). In addition, tracking

was not changed and the reform was implemented very quickly. Although official statistics do not indicate a movement of students from *Gymnasium* to comprehensive schools after reform introduction (as mentioned in section 3.3), this check can be seen as an additional test for a potential selection bias. However, the results in column (2) of Table A.7 confirm the original findings. In addition, an effect on females' university enrollment even beyond the first year after school graduation occurs.

A further robustness check addresses the introduction of university tuition fees in some federal states, although it is unlikely that the introduction has confounded the results. No evidence is found for an influence of tuition fees on enrollment at university by Helbig et al. (2012) and Bruckmeier and Wigger (2014). Furthermore, several states did not introduce tuition fees, and in other states, the introduction had already taken place in 2008 and was still valid in 2012. Only in Hamburg and North Rhine-Westphalia students had to pay tuition fees in 2008 that were abolished or going to be abolished in 2012.[15] However, students had sufficient possibilities to study in a state without tuition fees. In addition, even if tuition fees reduced university attendance, the negative effect of the reform on university enrollment would represent a lower bound estimation. Nevertheless, as a robustness check, we exclude the two potentially critical states from the analysis. As the results in column (3) of Table A.7 show, the effects remain significant in the female sample. Compared with this result, the effects on male students lose their statistical significance. However, the effects on university and vocational enrollment beyond the first year after school graduation remain constant in size and are still very close to the 10%-significance level. Therefore, the insignificance could be a result of the lower number of observations, and does not necessarily contradict the previous findings.

Finally, the original estimations are conducted with two additional control variables, which indicate whether a student in a given state and year graduated from high school according to the changed curriculum and whether a student was expecting university tuition fees in his home state. The results are very similar to the initial ones. The effect on vocational education in the male sample becomes stronger, whereas the effect on voluntary service or staying abroad decreases in size and significance (with a p-value of 0.10).

Altogether, the robustness checks confirm the original findings. All effects in the female sample and the majority of effects in the male sample remain constant. The few effects on males, which are no longer statistically significant, still have a similar size and are mostly close to the 10% level of statistical significance.

5 Conclusion

The importance of the duration of schooling results from the fact that time spent in school contributes to the development of skills as well as to the discovery of tastes and talents. A major education reform in Germany reduced the duration of university preparatory schooling

[15] In Hamburg, tuition fees were introduced in 2007, but the elimination was resolved in 2011 and implemented in 2013. In North Rhine-Westphalia, fees were introduced in 2006 but abolished in 2011.

by one year (from 13 to 12 years). However, the requirements for final graduation were not changed, which means that the curriculum had to be taught and learned in a shorter time. In this paper, we have evaluated the impact of this reform on post-secondary education decisions in several German states. The evaluation is based on nationally representative data of high school graduates. The effects are identified using the different timing of the reform introduction in the German states (difference-in-differences estimation). One strength of the analysis is that we have used several definitions of the treatment group, which enables us to investigate the effects of the reform in a number of federal states.

The results show that the reform has reduced enrollment in university education in the first year after high school graduation by approximately 15 percentage points for students of both genders. At the same time, the probability of engaging in a year of voluntary service or spending a year abroad is increased by up to 12 percentage points for females and by approximately 8 percentage points for males. In addition, male students affected by the reform are slightly less likely to study at university beyond the first year (i.e., when firmly planned enrollment is included), which is accompanied by a slight increase in the probability of starting vocational education. In contrast, almost no effect from the reform on university subject choice can be observed. The effects on female students remain stable in all specifications and robustness checks. In the male sample, effects beyond the first year lose their statistical significance in some cases but are confirmed by most robustness checks. Most of the findings are driven by students coming from a non-academic family, although effects can also be observed for (female) students with an academic family background. Students with a non-academic family background are even more affected by the reform than the main results suggest. Females show reduced university attendance also beyond the first year after high school graduation. In the case of male students, effects on enrollment in university and vocational education beyond the first year are large and statistically significant.

Altogether, the analysis reveals that post-secondary education decisions are affected by the reduced school duration. Because the impacts are mostly similar across federal states, they can be considered to be generally valid. This also largely confirms the findings from the previous study based on detailed data for Saxony-Anhalt (Meyer and Thomsen, 2016). However, a few differences exist, which should be discussed. The first difference is that no effect on university enrollment beyond the first year and no effect on vocational education can be observed for female students. We conclude that the reform has delayed enrollment in university education in both cases, but the reason for delay differs between West and East Germany. Affected students in West Germany more frequently decided to engage in voluntary service or to stay abroad by one year, whereas affected students in East Germany choose vocational education instead, which normally takes about three years. This interpretation is underlined by the robustness checks, which have revealed some heterogeneity in the reform effects by federal state. In particular, the analysis with a different treatment group mainly consisting of East German students from Saxony-Anhalt and Mecklenburg-Western Pomerania (see columns 5 and 10 of Table 4) has

identified significant effects on female enrollment in university education beyond the first year and in vocational education. The second difference, finding significant effects also for male students, can be explained by the elimination of compulsory military or civilian service in 2011. Before its elimination, compulsory service gave many male students an additional year after high school graduation to think about their post-secondary education. However, after elimination, male students are in the same position in terms of their decision as female students. Once again, this conclusion is supported by the results in columns 5 and 10 of Table 4 and emphasizes the importance of the orientation function of schooling. The final difference is related to university subject choice, where no effects have been identified in this paper (if the main specification is used).[16] Therefore, it could be the case that the reform has an impact on subject choice only in some states, e.g., Saxony-Anhalt. This conclusion is supported by the observation that the effect on STEM subjects in the specification comparing 2006 and 2010 (columns 5 and 10 of Table 4) is driven mainly by students from Saxony-Anhalt. Altogether, the partly different findings underline that some effects of the reduced school duration could vary by state (depending on how reform is introduced or student characteristics). However, the main result of a reduced university enrollment in the short-term and a corresponding increase in alternative activities or courses of education can be considered to be generally valid.

A reasonable explanation for the reform effects is that students graduating after a shortened and more compressed school duration in Germany feel less prepared and/or less oriented with respect to university education. This leads to lower university enrollment in the first years after high school graduation. Additional information in the data shows that students affected by the reform are more likely to say that they have delayed enrollment in post-secondary education because they were uncertain about their post-secondary education decision or because they wanted to take a break or do something else before continuing their educational career. This underscores that feeling less oriented is a relevant problem as well as a reasonable explanation for the reform effects.

The objective of the reform was to achieve the same quality of education within a shorter time and therefore to allow an earlier start of university education and occupational career. However, because the analysis in this paper covers only short-run effects of the reform, it is difficult to answer the question as to whether this objective will be achieved. If 15% of students delay university enrollment by one year, 85% of students will enter university, and subsequently the labor market, one year earlier. From this perspective, the reform could be seen as an efficiency gain in education production. However, it is unclear whether the increased share of male students not attending university education will start university at a later time. Furthermore, it cannot be ruled out yet that the reform affects the duration of university education. In addition to orientation problems, the lower enrollment in university education in the first years after school graduation could indicate that students are (or feel) not as well

[16] As mentioned above, the lower probability of male students studying natural sciences and mathematics, which has been found using the specifications with 2006 as the pre-reform year, can be explained by the fact that the share of students in the treatment group choosing these subjects is atypically high in 2006 compared to other years.

prepared for higher education as the 13-year cohorts.

The results in this paper provide evidence that the duration of university preparatory schooling is relevant for education decisions made after school graduation. The reform impacts are similar across a number of federal states and similar to those obtained by Meyer and Thomsen (2016), making it unlikely that they represent temporary, implementation or state-specific effects. They are also in line with previous findings in the literature, which have shown negative effects from reducing instructional time on school achievement (e.g., Marcotte, 2007; Krashinsky, 2014). Thus, when reducing school duration it is important to consider at least two aspects. The curriculum and its teaching should be organized so that students are as well prepared and motivated for university education as those who experienced the longer duration of schooling. Moreover, it should be ensured that sufficient academic and occupational guidance is provided.

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Tables and Figures

Figure 1: The German States: Treatment Group (dark grey) and Control Group (light grey)

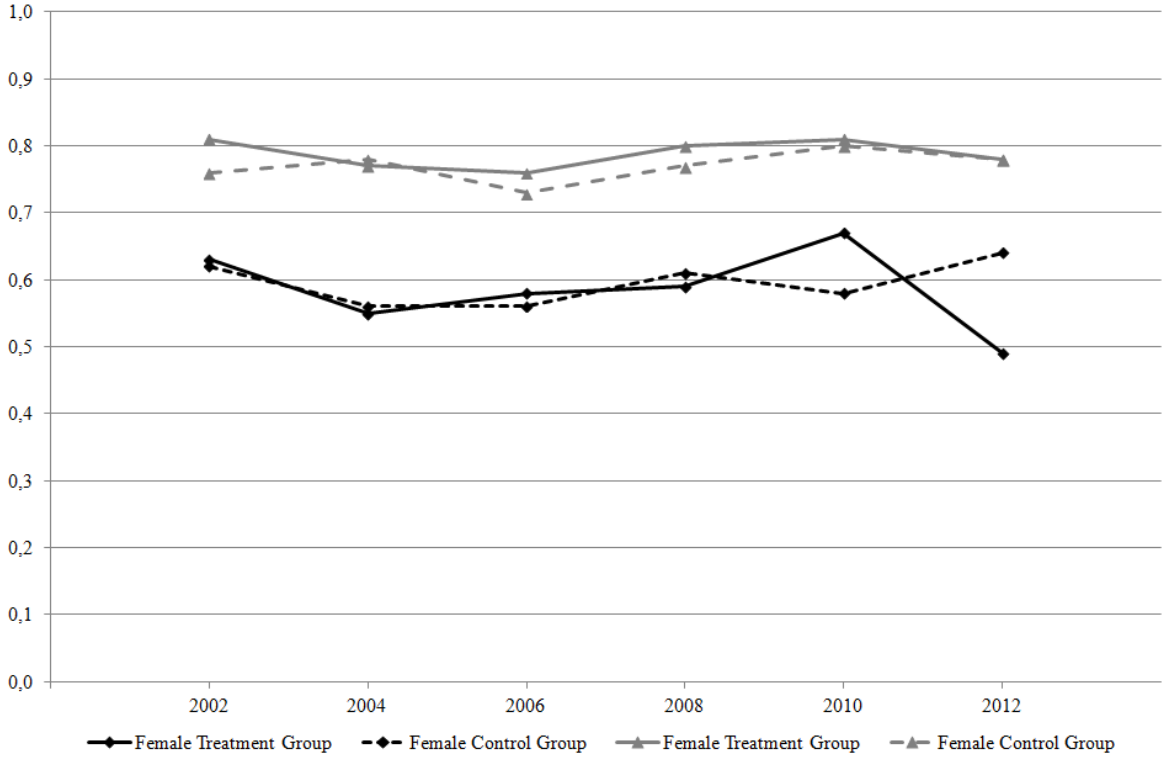
(a) Treatment Group 1

(b) Treatment Group 2

(c) Treatment Group 3

(d) Treatment Group 4

Figure 2: Share of Students enrolled in University Education (Actual Enrollment: Black; Actual and Firmly Planned Enrollment: Grey)



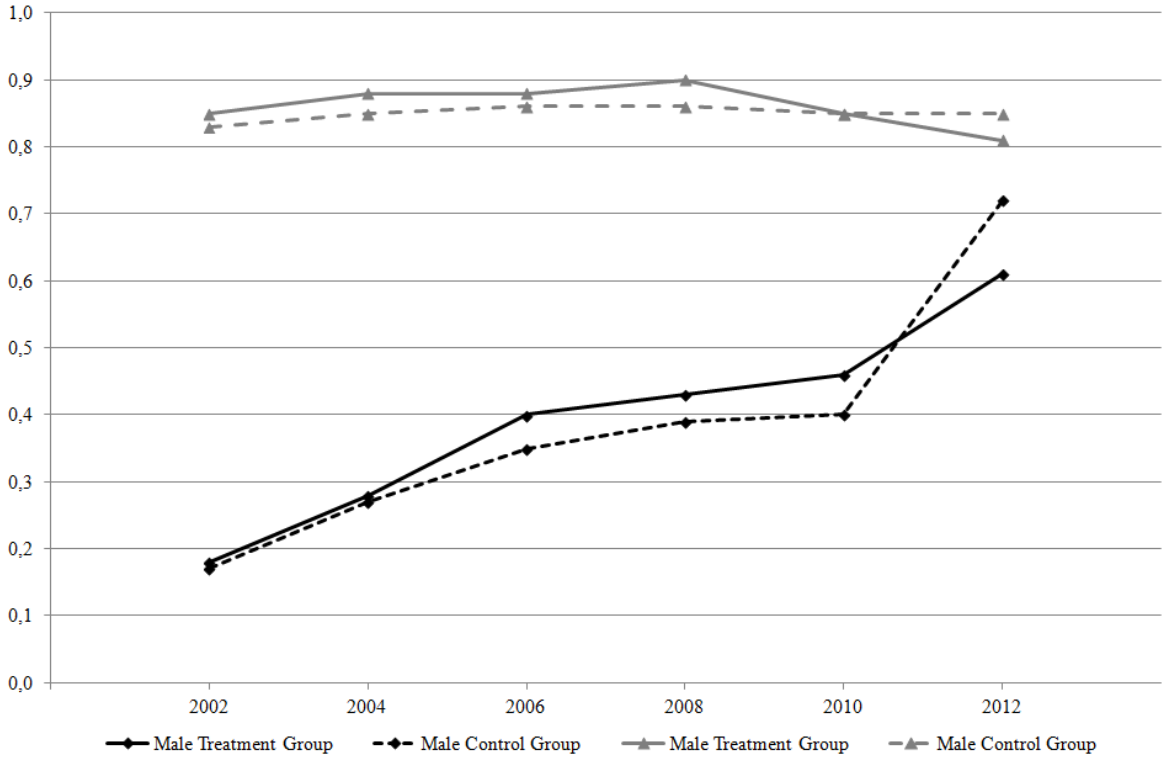


Table 1: Introduction of the Shortened School Duration of 12 Years according to Federal State

	Reform Introduction	Double Cohort of Graduates[a]
Saxony (SX)	always 12 years	–
Thuringia (TH)	always 12 years	–
Saxony-Anhalt (ST)	2003	2007
Mecklenburg-Western Pomerania (MW)	2001	2008
Saarland (SL)	2001	2009
Hamburg (HH)	2003	2010
Bavaria (BA)	2004	2011
Lower Saxony (LS)	2004	2011
Baden-Wuerttemberg (BW)	2004	2012
Bremen (BR)	2004	2012
Berlin (BE)	2006	2012
Brandenburg (BB)	2007	2012
North Rhine-Westphalia (NW)	2005	2013
Hesse (HE)	2005-2006	2013-2014
Schleswig-Holstein (SH)	2008	2016
Rhineland-Palatinate (RP)	never 12 years	–

[a] The double cohort includes the first cohort graduating after 12 years and the last cohort graduating after 13 years of schooling.
· The difference between the year of reform introduction and the year of the double cohort varies across states, since the reform was introduced for the first affected cohorts in the states in different grades.

Table 2: Means of Background Characteristics of Students

	Female Sample					Male Sample				
	Treatm.Gr.		Contr.Gr.		Diff-in-	Treatm.Gr.		Contr.Gr.		Diff-in-
	2008	2012	2008	2012	Diff [a]	2008	2012	2008	2012	Diff [a]
Born in First Half Year of Cohort	0.52	0.52	0.47	0.43	0.05	0.48	0.51	0.51	0.45	0.10*
Migration Background[b]	0.13	0.13	0.16	0.14	0.02	0.19	0.13	0.17	0.15	-0.05
High School Graduation of Parents[c]	0.52	0.49	0.49	0.48	-0.02	0.54	0.50	0.54	0.55	-0.04
Academic Degree of Parents[d]	0.59	0.53	0.57	0.56	-0.04	0.63	0.57	0.60	0.61	-0.06
Occupational Status of Par.: low[e]	0.13	0.12	0.14	0.14	-0.01	0.07	0.11	0.12	0.14	0.02
Occupational Status of Par.: middle[e]	0.42	0.39	0.39	0.43	-0.06	0.40	0.40	0.38	0.40	-0.01
Occupational Status of Par.: high[e]	0.45	0.49	0.46	0.44	0.06*	0.53	0.49	0.50	0.46	0.00
Books of Parents: 0 to 100	0.21	0.21	0.19	0.26	-0.07**	0.16	0.22	0.20	0.26	-0.01
Books of Parents: 101 to 500	0.48	0.53	0.50	0.52	0.04	0.48	0.50	0.50	0.50	0.03
Books of Parents: more than 500	0.32	0.26	0.31	0.22	0.03	0.36	0.28	0.30	0.24	-0.02
N	416	839	795	1121		201	408	340	548	

[a] Difference-in-Differences = $(TG_{2012} - TG_{2008}) - (CG_{2012} - CG_{2008})$. Stars denote the significance of the Diff-in-Diff as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
[b] Migration background is defined as follows: Student is born abroad, or has foreign citizenship, or at least one parent is born abroad, or language at parental home is not only German.
[c] At least one parent has graduated from high school, i.e. has a university entrance qualification. (The share is lower than having an academic degree, because university education can be attended not only with high school graduation, but also with the entrance qualification to universities of applied sciences.)
[d] At least one parent has an academic degree.
[e] Occupational Status is measured by the International Socio-Economic Index of Occupational Status (ISEI). Low status is from 0 to 49, middle from 50 to 67, and high from 68 to 85.

Table 3: Means of Post-Secondary Education Decisions of Students

	Female Sample				Male Sample			
	Treatment Gr.		Control Gr.		Treatment Gr.		Control Gr.	
	2008	2012	2008	2012	2008	2012	2008	2012
First Post-Secondary Education[a]								
University Education started	0.59	0.49	0.61	0.64	0.43	0.61	0.39	0.72
University Education started/planned	0.80	0.78	0.77	0.78	0.90	0.81	0.86	0.85
Vocational Education started	0.15	0.13	0.16	0.16	0.07	0.12	0.07	0.12
Vocational Education started/planned	0.19	0.18	0.20	0.20	0.08	0.16	0.11	0.13
Post-Secondary Education started	0.75	0.62	0.77	0.80	0.50	0.73	0.45	0.84
Post-Secondary Education started/planned	0.99	0.97	0.97	0.98	0.98	0.97	0.97	0.98
Other Activities in the Year after High School Graduation[b]								
Military or Civilian Service	0.00	0.00	0.00	0.00	0.41	0.01	0.44	0.01
Internship or Temporary Work	0.12	0.14	0.09	0.07	0.04	0.11	0.04	0.07
Voluntary Service or Stay Abroad	0.10	0.22	0.12	0.11	0.04	0.14	0.06	0.08
Sum of Other Activities after High School	0.22	0.36	0.21	0.18	0.49	0.26	0.54	0.16
Subject of started/planned University Education[c]								
Humanities	0.13	0.12	0.12	0.11	0.06	0.05	0.08	0.06
Education and Social Sciences	0.24	0.24	0.26	0.27	0.14	0.11	0.11	0.16
Law and Economics	0.16	0.17	0.19	0.19	0.18	0.22	0.16	0.23
Engineering	0.07	0.09	0.07	0.07	0.30	0.24	0.29	0.22
Natural Sciences and Mathematics	0.12	0.10	0.08	0.10	0.16	0.19	0.18	0.18
Medical Sciences	0.10	0.08	0.09	0.07	0.06	0.02	0.06	0.04
N	416	839	795	1121	201	408	340	548

[a] Share of high school graduates being enrolled (or having decided to enroll in near future) in university or vocational education half a year after school graduation.
[b] Share of high school graduates participating in different activities half a year after school graduation.
[c] Share of high school graduates being enrolled or planning to enroll in a specific university subject.

Table 4: Difference-in-Differences Estimates of Reform Effects (Marginal Effects)

	Female Sample					Male Sample				
Pre-/Post-Reform Year	2008/2012		2006/2012		2006/2010	2008/2012		2006/2012		2006/2010
Treatment Group	TG 1	TG 2	TG 1	TG 3	TG 4	TG 1	TG 2	TG 1	TG 3	TG 4
University Education (started)[a]	-0.145***	-0.154***	-0.177***	-0.172***	-0.168***	-0.147***	-0.157***	-0.171***	-0.175***	-0.110
	(0.037)	(0.034)	(0.037)	(0.034)	(0.053)	(0.051)	(0.048)	(0.051)	(0.048)	(0.078)
University Educ. (started/planned)[b]	-0.030	-0.042	-0.019	-0.026	-0.091**	-0.083**	-0.071*	-0.059	-0.051	-0.032
	(0.031)	(0.029)	(0.032)	(0.030)	(0.042)	(0.040)	(0.037)	(0.041)	(0.038)	(0.053)
Vocational Education (started)[a]	-0.016	-0.002	0.021	0.014	0.067*	-0.001	-0.000	0.030	0.047	0.090*
	(0.027)	(0.025)	(0.028)	(0.026)	(0.035)	(0.034)	(0.032)	(0.037)	(0.035)	(0.051)
Vocational Educ. (started/planned)[b]	0.003	0.015	0.035	0.032	0.076*	0.065*	0.059*	0.058	0.049	0.006
	(0.030)	(0.028)	(0.031)	(0.028)	(0.039)	(0.037)	(0.035)	(0.039)	(0.036)	(0.042)
Internship or Temporary Work[c]	0.052**	0.066***	0.053**	0.047**	0.045	0.023	0.038	0.088**	0.063**	0.031
	(0.023)	(0.022)	(0.023)	(0.021)	(0.037)	(0.029)	(0.028)	(0.038)	(0.031)	(0.030)
Voluntary Service or Stay Abroad[c]	0.118***	0.096***	0.102***	0.113***	0.061	0.083**	0.058**	0.083**	0.095***	0.088*
	(0.026)	(0.024)	(0.027)	(0.025)	(0.039)	(0.033)	(0.029)	(0.034)	(0.033)	(0.046)
STEM Subjects (narrow definition)[d]	-0.006	-0.013	-0.038	-0.048*	-0.027	0.032	0.001	-0.104*	-0.145***	-0.190**
	(0.029)	(0.027)	(0.029)	(0.026)	(0.040)	(0.054)	(0.050)	(0.055)	(0.050)	(0.080)
STEM Subjects (broad definition)[d]	-0.011	-0.020	-0.023	-0.038	-0.041	0.012	-0.011	-0.120**	-0.153***	-0.167**
	(0.033)	(0.030)	(0.033)	(0.030)	(0.047)	(0.054)	(0.050)	(0.055)	(0.051)	(0.081)
N	3,099	3,456	3,045	3,527	3,883	1,458	1,635	1,465	1,672	1,790

· Treatment group 1: Bavaria, Hamburg, Lower Saxony.
· Treatment group 2: Bavaria, Hamburg, Lower Saxony, Berlin, Brandenburg, Bremen.
· Treatment group 3: Bavaria, Hamburg, Lower Saxony, Saxony-Anhalt, Mecklenburg-Western Pomerania.
· Treatment group 4: Hamburg, Mecklenburg-Western Pomerania, Saarland, Saxony-Anhalt; different control group: Baden-Wuerttemberg, Bavaria, Berlin, Brandenburg, Bremen, Hesse, Lower Saxony, North Rhine-Westphalia, Rhineland-Palatinate, Schleswig-Holstein.
[a] Dependent variable: Dummy indicating actual enrollment in university/vocational education.
[b] Dependent variable: Dummy indicating actual or firmly planned enrollment in university/vocational education.
[c] Dependent variable: Dummy indicating participation in the year after high school graduation in an internship or temporary work / in a voluntary service or stay abroad.
[d] Dependent variable: Dummy indicating actual enrollment or firmly planned enrollment in a STEM university subject (STEM subjects narrowly defined include engineering, natural sciences and mathematics, STEM subjects broadly defined additionally include medical sciences).
· Regressions are separately run for each outcome.
· Regressions include further explaining variables: post-reform-dummy, treatment-group-dummy, academic degree of parents, occupational status of parents, (number of books of parents,) migration background, half year of birth, and state dummies.
· Marginal effects are average marginal effects. Standard errors are shown in parenthesis below marginal effects. Stars denote significance of the estimates as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Difference-in-Differences Estimates of Reform Effects (Estimations Without One Treatment State; Marginal Effects)

	Female Sample			Male Sample		
	w/o BA	w/o HH	w/o LS	w/o BA	w/o HH	w/o LS
University Education (started)[a]	-0.140***	-0.162***	-0.127***	-0.154**	-0.162***	-0.126**
	(0.047)	(0.038)	(0.044)	(0.069)	(0.052)	(0.057)
University Education (started/planned)[b]	0.016	-0.047	-0.049	-0.078	-0.095**	-0.075
	(0.040)	(0.033)	(0.038)	(0.054)	(0.041)	(0.046)
Vocational Education (started)[a]	-0.055	-0.004	0.005	-0.009	0.019	-0.019
	(0.035)	(0.028)	(0.033)	(0.045)	(0.035)	(0.038)
Vocational Education (started/planned)[b]	-0.039	0.012	0.032	0.079	0.080**	0.037
	(0.038)	(0.031)	(0.036)	(0.051)	(0.039)	(0.042)
Internship or Temporary Work[c]	0.046*	0.054**	0.050*	0.019	0.030	0.016
	(0.027)	(0.023)	(0.025)	(0.038)	(0.029)	(0.032)
Voluntary Service or Stay Abroad[c]	0.145***	0.117***	0.090***	0.074*	0.078**	0.097**
	(0.033)	(0.027)	(0.030)	(0.043)	(0.033)	(0.039)
STEM Subjects (narrow definition)[d]	-0.012	-0.011	0.005	0.002	0.030	0.050
	(0.037)	(0.030)	(0.034)	(0.075)	(0.056)	(0.062)
STEM Subjects (broad definition)[d]	-0.019	-0.023	0.009	-0.027	0.018	0.026
	(0.042)	(0.034)	(0.039)	(0.076)	(0.056)	(0.062)
N	2,482	2,976	2,612	1,114	1,410	1,262

[a] Dependent variable: Dummy indicating actual enrollment in university/vocational education.
[b] Dependent variable: Dummy indicating actual or firmly planned enrollment in university/vocational education.
[c] Dependent variable: Dummy indicating participation in the year after high school graduation in an internship or temporary work / in a voluntary service or stay abroad.
[d] Dependent variable: Dummy indicating actual enrollment or firmly planned enrollment in a STEM university subject (STEM subjects narrowly defined include engineering, natural sciences and mathematics, STEM subjects broadly defined additionally include medical sciences).
· Regressions are separately run for each outcome.
· Regressions include further explaining variables: post-reform-dummy, treatment-group-dummy, academic degree of parents, occupational status of parents, number of books of parents, migration background, half year of birth, and state dummies.
· Marginal effects are average marginal effects. Standard errors are shown in parenthesis below marginal effects. Stars denote significance of the estimates as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Difference-in-Differences Estimates of Reform Effects (Separate Estimations for Students with Academic and Non-Academic Family Background; Marginal Effects)

	Female Sample		Male Sample	
	academic family[e]	non-academic family[e]	academic family[e]	non-academic family[e]
University Education (started)[a]	-0.119**	-0.171***	-0.077	-0.294***
	(0.048)	(0.057)	(0.067)	(0.079)
University Education (started/planned)[b]	0.016	-0.091*	0.004	-0.222***
	(0.037)	(0.053)	(0.049)	(0.070)
Vocational Education (started)[a]	-0.030	0.004	-0.055	0.074
	(0.031)	(0.048)	(0.042)	(0.062)
Vocational Education (started/planned)[b]	-0.044	0.068	-0.029	0.205***
	(0.034)	(0.052)	(0.045)	(0.068)
Internship or Temporary Work[c]	0.019	0.090***	0.005	0.062
	(0.030)	(0.035)	(0.040)	(0.042)
Voluntary Service or Stay Abroad[c]	0.140***	0.080**	0.078*	0.128**
	(0.036)	(0.040)	(0.043)	(0.062)
STEM Subjects (narrow definition)[d]	0.034	-0.042	0.120*	-0.101
	(0.039)	(0.043)	(0.070)	(0.087)
STEM Subjects (broad definition)[d]	0.043	-0.067	0.106	-0.127
	(0.045)	(0.048)	(0.070)	(0.087)
N	1,746	1,353	877	581

[a] Dependent variable: Dummy indicating actual enrollment in university/vocational education.
[b] Dependent variable: Dummy indicating actual or firmly planned enrollment in university/vocational education.
[c] Dependent variable: Dummy indicating participation in the year after high school graduation in an internship or temporary work / in a voluntary service or stay abroad.
[d] Dependent variable: Dummy indicating actual enrollment or firmly planned enrollment in a STEM university subject (STEM subjects narrowly defined include engineering, natural sciences and mathematics, STEM subjects broadly defined additionally include medical sciences).
[e] A student is defined to come from an academic family if at least one parent has an academic degree.
· Regressions are separately run for each outcome.
· Regressions include further explaining variables: post-reform-dummy, treatment-group-dummy, occupational status of parents, number of books of parents, migration background, half year of birth, and state dummies.
· Marginal effects are average marginal effects. Standard errors are shown in parenthesis below marginal effects. Stars denote significance of the estimates as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A Appendix

Table A.1: Composition of Treatment and Control Groups (Number of Observations)

	Female Sample				Male Sample			
	2006	2008	2010	2012	2006	2008	2010	2012
Bavaria	197	196	429	440	125	117	242	234
Hamburg	44	29	15	96	15	11	6	39
Lower Saxony	160	191	246	303	72	73	117	135
Treatment Group	401	416	690	839	212	201	365	408
Hesse	122	132	193	180	63	58	60	82
North Rhine-Westphalia	535	469	451	703	224	198	229	334
Rhineland-Palatinate	46	154	137	125	20	67	33	72
Schleswig-Holstein	54	40	66	113	25	17	28	60
Control Group	757	795	847	1,121	332	340	350	548

Table A.2: Introduction of the Reform in the States of the Treatment Group

	Decision of Introduction	Imple-mentation	First Affected Cohort	Affected School Types[a]	Double Cohort of Graduates
Bavaria	July 2004	August 2004	Grade 6	HS	2011
Hamburg	June 2003	August 2003	Grade 6	HS and CS	2010
Lower Saxony	June 2003	August 2004	Grade 6	HS and CS	2011

[a] School Types: HS = high school, CS = comprehensive school (only cooperative comprehensive school)
· Source: Own investigation on the basis of law decisions, school laws and information from the state ministries of education.

Table A.3: Share of Students Dropping Out of Cohort in the Last Two Years of High School

	Graduation Cohort							
	2006	2007	2008	2009	2010	2011	2012	2013
Bavaria	0.07	0.08	0.08	0.08	0.07	0.06	0.09	0.08
Hamburg	0.14	0.15	0.13	0.12	0.14	0.20	0.13	0.12
Lower Saxony	0.14	0.14	0.17	0.18	0.16	0.19	0.20	0.19

· Share of students who entered the second last year of high school but did not graduate from high school on time (i.e. two years after entry).
· The exceptional high number in Hamburg 2011 could be due to the implementation of a new type of secondary school in 2010/2011 in this state, which could have led to some statistical reporting errors in this year.
· Source: Federal Statistical Office (n.y.a).

Table A.4: Introduction of Other Education Reforms in Germany

	Reduced School Duration	Central Final Examinations	Tracking after Grade 4	Changed High School Curriculum	University Tuition Fees[a]
Treatment Group					
Bavaria	since 2011	since 1946	always	since 2011	2006 - 2012
Hamburg	since 2010	since 2005	always	since 2011	2006 - 2011
Lower Saxony	since 2011	since 2006	since 2012[b]	since 2008	2005 - 2013
Control Group					
Hesse	since 2013-14	since 2007	always	since 2005	2006 - 2008
North Rhine-Westphalia	since 2013	since 2007	always	never	2006 - 2010
Rhineland-Palatinate	never	never	always	never	never
Schleswig-Holstein	since 2016	since 2008	always	since 2011	never

[a] The years correspond to the time when the introduction or elimination of tuition fees was resolved.
[b] Until 2010, students in Lower Saxony were tracked after grade 6. The 2011 cohort was tracked after grade 5.
· The year indicates the high school graduation cohorts which are affected by the respective reform.
· Source: Own investigations on the basis of school laws, high school regulations, information from the state ministries of education, and information from the standing conference of the ministers of education of the German states.

Table A.5: Admission Grades in Selected Subjects at Universitities and Universities of Applied Sciences in Bavaria and Lower Saxony

	WS 2009/10	WS 2010/11	WS 2011/12	WS 2012/13
University of Bamberg				
Business Administration and Economics	all admitted	all admitted	all admitted	all admitted
Psychology	1.8	1.6	1.7	1.6
Teaching in Primary School	1.9	1.8	1.6	1.9
Teaching in High School	1.4	1.2	1.5	1.6
Ludwig-Maximilians University Munich				
Business Administration and Economics	not available	2.2	1.7	1.8
Geography	3.1	2.8	3.1	3.1
Law	2.2	2.3	2.2	2.1
Psychology	1.4	1.4	1.3	1.3
Teaching in Primary School	2.5	2.3	2.4	2.4
University of Passau				
Business Administration and Economics	all admitted	all admitted	2.6	2.4
Business Informatics	all admitted	all admitted	all admitted	all admitted
Political Sciences	2.5	all admitted	all admitted	all admitted
Teaching in Primary School	2.5	2.5	2.5	2.7
East Bavarian Technical University Regensburg				
Business Administration	3.0	2.6	2.7	2.4
Business Informatics	all admitted	3.0	3.1	2.9
Engineering	3.0	2.9	2.7	2.7
Social Work	2.4	2.1	2.2	2.1
Leibniz University Hannover				
Biology	2.3	2.3	2.1	2.4
Business Administration and Economics	3.1	3.0	3.3	3.4
Business Engineering	2.6	2.4	2.6	2.9
Engineering	all admitted	all admitted	all admitted	all admitted
Geography	2.7	2.7	2.6	2.4
Law	3.1	2.7	3.1	3.0
Political Science	2.5	2.7	2.7	all admitted
University of Osnabrück				
Biology	2.1	2.1	2.1	2.2
Business Administration and Economics	all admitted	all admitted	3.2	2.9
Geography	all admitted	2.8	2.8	all admitted
Law	all admitted	2.8	3.3	all admitted
Psychology	1.6	1.5	1.4	1.4

· Admission to university education is based on at least two factors: (1) average grade at high school graduation (ranging between 1 [very good] and 4 [sufficient], i.e., lower grades indicate higher achievement), and (2) number of semesters waiting for university enrollment. Both factors are considered with different weighting, depending on the university. The grades shown above represent admission grades without waiting semesters, i.e., for students attending university in the same year as they graduated from high school.
· WS denotes the winter semester, starting in October of the respective year.
· Source: Information provided by the respective universities.

Table A.6: Robustness Check I: Placebo Tests (DiD Estimates, Marginal Effects)

	Female Sample			Male Sample		
	(1)	(2)	(3)	(1)	(2)	(3)
University Education (started)[a]	0.069	-0.032	0.036	0.124*	-0.104*	-0.056
	(0.047)	(0.041)	(0.054)	(0.066)	(0.058)	(0.077)
University Education (started/planned)[b]	0.049	0.009	0.035	0.018	-0.027	0.026
	(0.042)	(0.035)	(0.045)	(0.053)	(0.045)	(0.058)
Vocational Education (started)[a]	-0.029	-0.030	-0.049	-0.014	0.017	-0.044
	(0.037)	(0.031)	(0.040)	(0.049)	(0.039)	(0.049)
Vocational Education (started/planned)[b]	-0.041	-0.021	-0.059	-0.029	0.045	-0.013
	(0.040)	(0.034)	(0.043)	(0.053)	(0.042)	(0.054)
Internship or Temporary Work[c]	0.001	0.001	0.013	-0.021	-0.039	0.060
	(0.027)	(0.023)	(0.030)	(0.035)	(0.029)	(0.042)
Voluntary Service or Stay Abroad[c]	-0.037	0.048*	0.008	-0.041	0.063*	-0.052
	(0.030)	(0.028)	(0.034)	(0.037)	(0.036)	(0.046)
STEM Subjects (narrow definition)[d]	0.039	-0.031	-0.032	0.035	-0.026	0.027
	(0.037)	(0.032)	(0.041)	(0.073)	(0.062)	(0.084)
STEM Subjects (broad definition)[d]	0.011	-0.030	-0.016	0.009	-0.030	-0.005
	(0.042)	(0.036)	(0.048)	(0.074)	(0.063)	(0.085)
N	1,872	2,744	2,237	870	1,272	1,043

(1) Placebo-Test: Using two control states as treatment group (Schleswig-Holstein, North Rhine-Westphalia), compared to the other two states from the control group (Hesse, Rhineland-Palatinate).
(2) Placebo-Test: Using states from East Germany, which only had the system of graduation after 12 years in 2008 and 2012 (Saxony, Saxony-Anhalt, Thuringia) as treatment group, compared to the original control group.
(3) Placebo-Test: Using three states, which had a double cohort in 2012 (Berlin, Brandenburg, Bremen; but using only the G13 students), as treatment group, compared to the original control group.
[a] Dependent variable: Dummy indicating actual enrollment in university/vocational education.
[b] Dependent variable: Dummy indicating actual or firmly planned enrollment in university/vocational education.
[c] Dependent variable: Dummy indicating participation in the year after high school graduation in an internship or temporary work / in a voluntary service or stay abroad.
[d] Dependent variable: Dummy indicating actual enrollment or firmly planned enrollment in a STEM university subject (STEM subjects narrowly defined include engineering, natural sciences and mathematics, STEM subjects broadly defined additionally include medical sciences).
· Regressions are separately run for each outcome.
· Regressions include further explaining variables: post-reform-dummy, treatment-group-dummy, academic degree of parents, occupational status of parents, number of books of parents, migration background, half year of birth, and state dummies.
· Marginal effects are average marginal effects. Standard errors are shown in parenthesis below marginal effects. Stars denote significance of the estimates as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Robustness Check II: Potential Influence of Other Education Reforms (DiD Estimates, Marginal Effects)

	Female Sample				Male Sample			
	(1) w/o BA, HH, SH	(2) Treat.-Gr. only BA	(3) w/o HH, NW	(4) Additional Contr. Var.	(1) w/o BA, HH, SH	(2) Treat.-Gr. only BA	(3) w/o HH, NW	(4) Additional Contr. Var.
University Education (started)[a]	-0.179***	-0.153***	-0.108**	-0.145***	-0.198***	-0.145**	-0.088	-0.170**
	(0.051)	(0.046)	(0.047)	(0.046)	(0.075)	(0.059)	(0.068)	(0.069)
University Education (started/planned)[b]	-0.008	-0.090**	-0.014	-0.011	-0.102*	-0.094*	-0.082	-0.088*
	(0.044)	(0.042)	(0.039)	(0.037)	(0.057)	(0.049)	(0.051)	(0.048)
Vocational Education (started)[c]	-0.040	0.036	-0.023	-0.036	0.033	0.010	0.008	0.010
	(0.038)	(0.037)	(0.033)	(0.032)	(0.048)	(0.041)	(0.044)	(0.040)
Vocational Education (started/planned)[d]	-0.031	0.061	-0.013	-0.017	0.110**	0.056	0.066	0.093**
	(0.042)	(0.040)	(0.036)	(0.035)	(0.054)	(0.045)	(0.046)	(0.045)
Internship or Temporary Work[e]	0.047	0.053**	0.062**	0.054*	0.041	0.026	0.014	0.034
	(0.029)	(0.026)	(0.031)	(0.028)	(0.043)	(0.033)	(0.038)	(0.038)
Voluntary Service or Stay Abroad[f]	0.149***	0.084***	0.101***	0.126***	0.062	0.089**	0.060	0.065
	(0.034)	(0.031)	(0.037)	(0.031)	(0.043)	(0.039)	(0.046)	(0.039)
STEM Subjects (narrow definition)[g]	-0.028	-0.003	0.015	-0.008	-0.008	0.049	0.030	-0.032
	(0.041)	(0.036)	(0.038)	(0.037)	(0.083)	(0.065)	(0.069)	(0.073)
STEM Subjects (broad definition)[g]	-0.049	-0.007	-0.017	-0.027	-0.019	0.037	0.013	-0.036
	(0.046)	(0.041)	(0.043)	(0.042)	(0.084)	(0.065)	(0.069)	(0.073)
N	2,210	2,489	1,831	3,099	989	1,214	892	1,458

(1) Estimation without Bavaria, Hamburg and Schleswig-Holstein, which changed high school curriculum between 2008 and 2012.
(2) Estimation with only Bavaria as treatment state, since Bavaria had almost no comprehensive schools, no change in tracking, and a very fast reform introduction.
(3) Estimation without Hamburg and North Rhine-Westphalia, which had university tuition fees in 2008, but not in 2012.
(4) Estimation with additional control variables considering university tuition fees and a changed high school curriculum.
[a] Dependent variable: Dummy indicating actual enrollment in university/vocational education.
[b] Dependent variable: Dummy indicating actual or firmly planned enrollment in university/vocational education.
[c] Dependent variable: Dummy indicating participation in the year after high school graduation in an internship or temporary work / in a voluntary service or stay abroad.
[d] Dependent variable: Dummy indicating actual enrollment or firmly planned enrollment in a STEM university subject (STEM subjects narrowly defined include engineering, natural sciences and mathematics, STEM subjects broadly defined additionally include medical sciences).
· Regressions are separately run for each outcome.
· Regressions include further explaining variables: post-reform-dummy, treatment-group-dummy, academic degree of parents, occupational status of parents, number of books of parents, migration background, half year of birth, and state dummies.
· Marginal effects are average marginal effects. Standard errors are shown in parenthesis below marginal effects. Stars denote significance of the estimates as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.