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The Effect of Increasing Education Efficiency on University Enrollment

Evidence from Administrative Data and an Unusual Schooling Reform in Germany

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The effect of increasing education efficiency on university enrollment: Evidence from administrative data and an unusual schooling reform in Germany

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Abstract

We examine the consequences of compressing secondary schooling on students' university enrollment. An unusual education reform in Germany reduced the length of academic high school while simultaneously increasing the instruction hours in the remaining years. Accordingly, students receive the same amount of schooling but over a shorter period of time, constituting an efficiency gain from an individual's perspective. Based on a difference-in-differences approach using administrative data on *all* students in Germany, we find that this reform decreased enrollment rates. Moreover, students are more likely to delay their enrollment, to drop out of university, and to change their major. Our results show that it is not easy to get around the trade-off between an earlier labor market entry and more years of schooling.

Keywords: University enrollment, G8, workload, difference-in-differences, education efficiency
JEL: I28, J18, D04

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1 Introduction

It is well-established that more schooling is beneficial in an array of different dimensions (see e.g. Card, 1999; Lochner, 2011). At the same time, the more years individuals spend in education, the later they enter the labor market. Hence, there is a trade-off between an earlier labor market entry and more years of schooling. In light of aging populations, this trade-off is particularly relevant for countries trying to increase the pool of active labor market participants by allowing for earlier labor market entries.

Several proposals have been made to reduce the age at labor market entry. Yet, the existing literature suggests that these have negative consequences: Lowering the general school starting age (Bedard and Dhuey, 2012), shortening the school year (Pischke, 2007), reducing the number of years required for specific degrees (Webbink, 2007; Morin, 2013; Krashinsky, 2014), and reducing the years of compulsory schooling (see e.g., Card, 1999) are found to have adverse effects on students' educational and labor market outcomes. An unusual education reform in Germany bears the potential to decrease the length of schooling without compromising other education outcomes. This so-called G8 reform reduced the number of years of schooling necessary to earn the university entrance qualification at academic high schools but simultaneously increased instruction hours in the remaining years in order to avoid detrimental effects on students' human capital. In this setting, students receive the same amount of schooling but over a shorter period of time. From an individual's perspective this clearly marks an efficiency gain.

The G8 reform did not just spark a lively discussion regarding the potential negative effects for affected students due to the higher workload and the younger age at graduation, but it also stimulated a growing literature on this topic (see Huebener and Marcus (2015a) for an overview of existing studies). Most of these studies are confined to the short-term consequences of the reform, either examining students during or at the end of high school. We study medium-term outcomes of the reform relating to the goal of earlier labor market entries and to human capital acquisition: (i) enrollment rates in university, (ii) the timing of enrollment, and (iii) students' study progress at university.

Several arguments suggest that the compression of secondary schooling will affect higher education decisions – despite the intentions of policy makers. First, the one year reduction in the length of academic high school implies a reduction in students' age at high school graduation. Younger students might be more likely to prefer present gains over higher future gains (Lavecchia et al., 2016), thus making university education less attractive. Additionally, students at high school graduation have now one year less time for orientation, less time to discover their talents and less time to develop their preferences, which might increase uncertainty about post-secondary educational choices. If students are aware of their relative age advantage, this may further entice them to take things more slowly and to delay their enrollment decisions. Second, the compensating increase of instruction hours in the remaining years implies a higher weekly workload as measured by weekly instruction hours. Students able to meet these higher requirements may be even better prepared for the learning requirements at university. However, for students who are unable to cope with the higher workload this may result in worse performance. Indeed, existing evidence suggests that students' performance in school is negatively affected by the reform (Büttner and Thomsen, 2013; Huebener and Marcus, 2015b; Trautwein et al., 2015). Additionally, affected students report increasing levels of stress and strain in school due to the reform (Meyer and Thomsen, 2015; Quis, 2015; Trautwein et al., 2015), which might also reduce the desire and motivation for further learning (Jürges and Schneider, 2010). Given that students' performance in school is one of the most important determinants for the enrollment decision as well as for success in university (Bowen and Bok, 1998), we expect adverse effects on higher education decisions.

We exploit the differential timing of the reform implementation across states in a difference-in-differences setting. Relying on administrative data on the universe of students in Germany, we find that, due to the G8 reform, the share of students who enroll in university within one year after high school graduation decreases by about 6 percentage points (pp). The impact on enrollment rates within two or three years after graduation is of similar magnitude, thus suggesting that enrollment rates do not catch-up. Further, we find evidence that the achievement of the reform's main goal in bringing university graduates earlier to the labor market is mitigated: As a consequence of the reform, students are 6.8 pp more likely to delay their enrollment and 2.6 pp less likely to make expected progress during their first year at university. The latter is explained by a higher probability to drop out of university and a higher probability to change majors. The main mechanism driving our results is not the age difference of students; instead our analysis suggests that the higher workload experienced during high school explains our findings. The negative reform effects seem to be general consequences of the reform as we find little evidence for effect heterogeneity between states, cohorts, or gender. We perform a battery of robustness checks and falsification exercises to support the identifying assumption of common trends in the outcome variables in treatment and control states.

The results of our study are not only informative for the German context but also for policy makers in other countries who are trying to increase the number of active labor market participants in order to address the challenges of an aging society. However, our study shows that it not easy to get around the trade-off between more years of schooling and an earlier labor market entry.[1]

The remainder of the paper is structured as follows. In Section 2 we provide details about the reform implementation before summarizing existing evidence on the reform effects. Section 3 introduces the data and describes the construction of our outcome variables, while Section 4 outlines the empirical approach. Section 5 presents the empirical evidence of the reform effects on higher education decisions. In Section 6 we show the robustness of these results to various model specifications. Section 7 examines effect heterogeneities, including gender and state specific treatment effects as well as the development of the treatment effect over time. A discussion on potential channels is addressed in Section 8, while Section 9 concludes.

2 The G8 reform

In most German states students complete four years of primary school before being assigned to different tracks of secondary schooling based upon their ability. The G8 reform analyzed in this study affects only one of these tracks, the academic high school (*Gymnasium*), which is the high-ability track that prepares students for university. It is attended by about one-third of a cohort.

The idea of the G8 reform is to shorten the length of academic high school without affecting students' human capital. The intermediate aim of the reform is to allow for an earlier labor market entry of young people, thereby helping to achieve three further goals. First, to increase the number of contributors to the public pay-as-you-go pension system, which is under pressure due to an aging population. Second, to compensate for the skilled-worker shortage. Third, to make German university graduates more competitive on the international labor market by reducing their comparatively high age at graduation from university.

The G8 reform can be depicted as consisting of two parts. The first part is a reduction of the time until leaving the academic high school with the general university entrance qualifica-

[1]Note that as the reform was only implemented recently, it is not yet possible to directly examine outcomes at labor market entry. Only a small and highly selective group of affected students are already on the labor market.

tion, the *Abitur*, from 13 to 12 years, making students one year younger at school graduation.[2] The second part is an increase in the weekly load of instruction hours in the remaining years as the number of instruction hours required for graduation was left unchanged.[3] On average the required number of weekly instruction hours at academic high schools increased from 29.4 to 33.1 hours per week (or 12.5%) and resulted in an increase in weekly workload. This second part was meant to compensate for the loss in instruction hours due to the omitted 13^{th} grade. Therefore, the G8 reform can be seen as a redistribution of instruction hours from the last grade to the previous grades. Due to the additional weekly instruction hours after the reform's implementation each grade covered also some material that was previously taught in higher grades. Note that by construction of the reform, the first G8 cohort and the very last cohort under the old G9 regime graduated in the same year. This cohort is referred to as the double graduation cohort. Figure 1 provides an overview of the timing of the G8 reform and shows that the first exclusive G8 cohorts graduated in different points in time in different states. The figure also shows that two states always had G8, while two other states did not switch to G8 during our observation period. Our empirical strategy exploits this regional and temporal variation.[4]

The introduction of the G8 reform sparked a lively discussion about potential negative effects for affected students due to the higher workload and the younger age at graduation. It has stimulated a growing number of research on this topic (see Huebener and Marcus (2015a) for an overview of existing studies). Most of these studies examine short-term effects and analyze outcomes at the end of academic high school. There is evidence for slightly weaker performance at the end of school (Büttner and Thomsen, 2013; Trautwein et al., 2015), increased grade repetition rates (Huebener and Marcus, 2015b), higher experienced levels of stress (Quis, 2015), and less time for working in a side job (Meyer and Thomsen, 2015). Further, these studies find no effects on graduation rates (Huebener and Marcus, 2015b), but show that affected students feel more strained by learning (Meyer and Thomsen, 2015). The evidence with respect to the impact on personality traits is mixed. While Dahmann and

[2]The reform derives its name G8 from the fact that – after usually four years of joint primary schooling – graduation requires now eight years of schooling at an academic high school instead of nine. Note that three states offer six years of joint primary schooling. Although the term G8 is not accurate for these states, the term G8 is widely used within Germany. Therefore, we stick to this term and use the term G9 to refer to the previous regime.

[3]Unless explicitly stated *graduation* refers to graduation from academic high school.

[4]Some states have already decided to switch back to the G9 regime or leave the decision on track length to individual schools. However, these changes are outside of our observation period (see Huebener and Marcus (2015a) for more details).

Anger (2014) find that affected students are more extroverted and less emotionally stable, Thiel et al. (2014) do not find an effect on personality traits of students.

Only two studies analyze medium-term consequences of the overall G8 reform. The first is Meyer and Thomsen (2014, 2016), who find that females delay their university enrollment, while there is no comparable effect for males. In contrast, they report no significant differences between G8 and G9 students with respect to dropping out, motivation and self-reported abilities. These findings do, however, rely exclusively on the double graduation cohort in two cities in a single federal state (Saxony-Anhalt) to identify the reform effects. Additionally, the double graduation cohort is a very particular cohort, as G8 students graduated together with the last cohort of the previous regime, which could affect their post-secondary education choices, due to the larger cohort size (Bound and Turner, 2007) and the increased competition for university resources. Hence, it is unclear to what extent the results based on one double cohort in a single state are also valid for later cohorts and other German states.[5]

The working paper by Meyer et al. (2015) is the only other study looking at post-secondary education choices based on data covering all German states. Their findings suggest that students affected by the reform are less likely to enroll in university in the year of school graduation. If not only actual enrollment but also intended enrollment is considered, the effect disappears for females and decreases but persists for males. The study further finds an increase in the probability of spending a year abroad or performing voluntary services, which may partly explain the delayed enrollment effects. There is also some evidence that students are more likely to start vocational education. We complement and extend this working paper in several ways: First, by analyzing a time period up to three years after high school graduation, we can disentangle the effect on the timing of enrollment from the actual enrollment choice.[6] Second, we investigate further outcomes, revealing the effect on students' study progress, their dropout behavior as well as the likelihood of students to change their major. These outcomes strongly relate to the reform's major goal of reducing the age at labor market entry. Third, given the longer time horizon, we can investigate whether the effects are only of transitory nature or whether they persist across subsequent cohorts. Fourth, we make several methodological improvements, e.g. by accounting for the special incentives of the last pre-treatment cohort, by clustering standard errors at the level of the policy change,

[5]Furthermore, the first G8 cohort in Saxony-Anhalt was already in grade 9 when they were informed about the shortening of the school duration, making this cohort even more peculiar.

[6]Based on their dataset, Meyer et al. (2015) can only look at actual choices six months after high school graduation.

and by using the variation in the timing of the reform implementation more efficiently. And fifth, our analysis relies on a full population survey, such that attrition, item non-response, and non-representativeness are of little concern.[7]

3 Data

3.1 The German Student Register

Our empirical analysis is based on administrative data from the German Student Register *("Studentenstatistik")* that covers all students enrolled in any German university between 2002 and 2014 (RDC).[8] Each university in Germany is obligated to provide the Federal Statistical Office with information on each individual student. The dataset contains individual level information but, due to tight data protection regulations, information on individual students cannot be linked over time. In addition to information on the year of first-time enrollment, choice of study program and institution, the data also contains information on when and in which state the student graduated from high school. This is the crucial information for determining treatment status. It is further registered which type of university entrance qualification the student has earned and whether it was earned at an academic high school. Given this information, we can identify students who were affected by the reform and those who were not. As is common for administrative data in Germany, background information is limited to gender, nationality, and date of birth.

This data set comes with at least three main benefits. First, as it is a full population survey, the sample size is large, which allows precise estimates of the reform effects. Second, as it is administrative data panel attrition, non-representativeness, and item non-response are of little concern. Third, data quality can be regarded as high, as each institution is obligated to record the information by law. Despite these advantages, the data set is not used much at the individual level, with Görlitz and Gravert (2015) and Horstschräer and Sprietsma (2015) being the exceptions.[9]

In our analysis we exclude all students who earned their university entrance qualification

[7]The survey data used by Meyer et al. (2015) suffers from high attrition rates (over 50% during the course of a year).

[8]There are several types of higher education institutions in Germany: public universities, private universities, universities of applied science, as well as colleges specializing in theology, music, art, or education. We refer to all these institutions as "universities", unless explicitly stated otherwise.

[9]Hübner (2012) and Bruckmeier and Wigger (2014) use an aggregated version of the German Student Register that is publicly available and provided by the German Federal Statistical Office (2016).

in Hesse because this state gradually implemented the G8 reform over a period of three years. Thus, we are unable to distinguish treated from untreated students. Furthermore, we keep only students who earned their general university entrance qualification from an academic high school as the reform only affected this track.[10]

3.2 Outcomes

In the following, we describe how we construct our three main outcome variables: *enrollment rate*, *timing of enrollment*, and *study progress*. For robustness purposes, we also work with alternative measures of our outcome variables. Generally, we use the individual level information and aggregate it at the state-cohort level as our treatment also varies at this level. Note that performing the analysis at the aggregate level yields the same results as performing the analysis at the individual level, if no individual control variables are included and the aggregate level analysis is appropriately weighted (Angrist and Pischke, 2009, 235). Furthermore, note that the construction of our outcome variables requires individual level data, as the aggregated data provided by the German Federal Statistical Office (2016) does not include the relevant information to determine treatment status and construct our outcomes.

Enrollment rate. A frequently stated policy goal in Germany, as well as in many other countries, is to increase the number of university students (OECD, 2014). The share of university educated individuals is often seen as a driver of economic growth (see e.g. Moretti, 2004) and associated with a range of non-monetary returns, like improved health (see e.g. Lochner, 2011) and participation in democratic activities (see e.g. Glaeser et al., 2007). Not surprisingly, a large number of studies investigate enrollment behavior. Each analysis of cohort enrollment rates must cope with right-censored data as not all students make their enrollment decisions immediately after high school graduation. In particular, until July 2011 males in Germany were obligated to complete military or civilian service, which most completed prior to entering post-secondary education. Additionally, some high school graduates take some time off before enrolling in university in order to stay abroad, do an internship or voluntary service, or just enjoy some free time. Further, some high school students complete a vocational degree before enrolling in university. Many studies focus on immediate enrollment after high school graduation (Hübner, 2012; Bruckmeier and Wigger,

[10] Other types of university entrance qualifications can be earned from other schools that were not affected by the G8 reform. We discuss potential selection issues in the robustness section.

2014; Meyer and Thomsen, 2016), neglecting that a substantial share of students enrolls a year later. We extend this time window and focus on individuals who enroll in the year of high school graduation or the year after, thereby capturing the majority of students who eventually enroll in university (see also Table 1). Additionally, we will further alter this time window and analyze enrollment rates up to three years after high school graduation.

In order to analyze general enrollment rates, we combine the individual level dataset on all students enrolled in university with annual information on the number of graduates from academic high schools in each state (German Federal Statistical Office, 2015). From these two sources, we calculate aggregate enrollment rates for each state and graduation cohort. More specifically, the enrollment rate is given by the share of freshmen students who enrolled in university within one year after graduating from an academic high school.

$$Enrollment\ rate_{sc} = \frac{ENR^{t}_{sc} + ENR^{t+1}_{sc}}{GRAD_{sc}}\ , \tag{1}$$

where ENR_{sc} refers to the number of freshman students, who graduated in state s and graduation cohort c and enrolled in university in the year of high school graduation (t) or the year after ($t+1$). Note that this measure is not affected by students' decisions to move to a different state in order to pursue university education, as the crucial information for our measure is the state of high school graduation and not the state in which students enroll in university. $GRAD_{sc}$ denotes the respective number of graduates from academic high schools.

Timing of enrollment. A main goal of the G8 reform is to allow for an earlier labor market entry. The effectiveness of the reform in achieving this goal will be mitigated, if the reform induces students to delay their enrollment. Hence, we analyze the *timing of enrollment* as our second main outcome.

We construct a measure for the timing of enrollment ("speed of enrollment") by dividing the number of students who enroll in the year of high school graduation by the number of students enrolling within one year after high school graduation, i.e. in the same year or the year after high school graduation.

$$Speed\ of\ enrollment_{sc} = \frac{ENR^{t}_{sc}}{ENR^{t}_{sc} + ENR^{t+1}_{sc}} \tag{2}$$

This measure indicates how many students delay their enrollment decision and allows us to disentangle changes in the timing of enrollment from general enrollment decisions.[11] Students typically graduate from high school in June, such that enrollment in the same year means starting university in October, i.e. in the following winter term.

Study progress. Similar to *timing of enrollment*, the outcome *study progress* relates to the reform's main goal in achieving an earlier labor market entry. Students not making regular study progress are unlikely to finish their university studies in the regular time. Unfortunately, our data does not include an individual panel identifier that would allow for following individuals over time. However, we can obtain a measure of study progress at the cohort level by exploiting the following particularity of the German higher education system: For administrative purposes, at the beginning of each winter term, the German higher education system not only counts the number of semesters students are enrolled in university (*Hochschulsemester*; semester at university), but also the number of semesters students are enrolled in the same major (*Fachsemester*; semester in same major). For students with a regular study progress these two numbers do not differ. We focus on students' study progress within the first year and calculate the share of students with a regular study progress out of all students who enrolled within one year after graduating from an academic high school.[12]

$$Regular\ study\ progress_{sc} = \frac{REG_{sc}}{ENR^{t}_{sc} + ENR^{t+1}_{sc}}\ , \tag{3}$$

where REG refers to the number of students with a regular study progress one year after enrollment, i.e. students for whom the number of university semesters equals the number of semesters enrolled in the same major at the beginning of the third semester. Similar to the *timing of enrollment* the outcome *study progress* is only defined for students who enroll in university. Hence, both have a conditional-on-positives interpretation.

There are three main reasons for a non-regular study progress. First, students can drop out of university. Second, students may change their major.[13] Third, students may formally

[11]Note that this measure only looks at the timing of enrollment for students that enroll in the year of high school graduation or the year after. In the robustness section, we use alternative measures for *timing of enrollment*.

[12]Note that the relevant information on study progress during the first year originates from the beginning of the third semester. Our dataset covers the full student population only in winter terms. Hence, unlike the other two outcomes, regular study progress is based on students who started university in the winter term; students who started in summer term are not included in this measure.

[13]Unlike in the US, students in Germany have to decide on their major at the time they enroll in university. Changing one's major usually results in an increased duration of study.

request a temporary interruption of their university studies (*Urlaubssemester*). In this case the number of interruption semesters is only added to the number of university semesters, while it does not increase the number of semesters in the same major. Among others, reasons for such temporary interruptions are maternity leaves, long-term illnesses, care responsibilities, and studying abroad - although the last is not very common within the first year of studies. We will also decompose *regular study progress* and differentiate between dropout, changing major, and temporary interruption. These three further outcomes are generated analogously to Equation 3, in which we successively substitute the numerator with the number of students who drop out, change their major or interrupt their studies, respectively.[14]

3.3 Descriptive statistics

Table 1 displays summary statistics related to our outcome variables. In our sample, 47% of high school graduates enroll in university in the same year they graduate from high school. One year later, three-quarters of the graduation cohort is enrolled, while this share increases only marginally to 82% two years after graduation, and to 86% three years after. These numbers indicate that the majority of a cohort enrolls in university in the year of graduation or the year after, i.e. within the first year after high school graduation. After this, only a small share of graduates enroll. Thus, our main analysis focuses on students who enroll in university within one year after high school graduation. Table 1 further shows that 61% of students who enrolled within one year did so in the year of graduation; this is our main measure for the timing of enrollment. Among students who enrolled within one year, 7% completely drop out of university within the first year of studies, while 11% change their major and 1% take a formal interruption; the remaining 81% of students show a regular study progress.

Note that due to the different timing of the reform implementation, the number of states already affected by the reform varies depending on the outcome under consideration. In the sample of our main analysis, we try to include as many observations as possible in order to fully exploit all available information. Therefore, sample sizes differ between the outcomes. Our conclusions, however, do not change when we apply more restrictive sample selection criteria (see Section 6).

[14]Note that students switching university are not counted as dropouts in our measure. However, for students who drop out, it might be possible that they enroll again after a break. These students are still counted as dropouts in our measure. Further note that changing major comprises changing major at the same university as well as changing major combined with switching to another university in Germany.

4 Estimation strategy

In order to estimate the effect of the G8 reform on (i) the *enrollment rate*, (ii) the *timing of enrollment*, and (iii) *study progress*, we apply a difference-in-differences strategy of the following form:

$$y_{sc} = \beta_1 G8_{sc} + \beta_2 DC_{sc} + \beta_3 lastG9_{sc} + \kappa_s + \mu_c + \varepsilon_{sc}, \tag{4}$$

where y_{sc} refers to one of the outcomes for graduation cohort c in state s; s denotes the individual's state of high school graduation not the state of the university enrollment. β_1 depicts the effect of the G8 reform and is the coefficient of interest.

κ_s is a set of state fixed effects and captures general differences between states (like time constant differences in states' education systems). A set of time fixed effects (μ_c) takes into account general time trends in the outcomes. This is an essential element of our identification strategy, as, for instance, the share of a birth cohort entering higher education is steadily increasing in Germany. Further, the time fixed effects also capture shocks that are common to all states, like the suspension of military service in 2011 (which is particularly relevant for *timing of enrollment*). The equation further includes an indicator variable, DC_{sc}, for the double graduation cohort; we thereby assign the double cohort neither to the treatment nor to the control group for two main reasons. First, the data only contains information on the year and state of high school graduation, not the individual G8 status. Thus, we cannot exactly determine treatment status for individuals in the double cohort. Second, students from the double graduation cohort may be affected rather differently by the reform, as students might have perceived the competition for available slots in university as well as vocational education as higher. We further augment this baseline model by adding a binary variable for the last cohort before the double cohort ($lastG9_{sc}$), which is the last exclusive G9 cohort. This is important because students in this cohort had a particularly strong incentive for speedy enrollment in order to avoid beginning to study with the double cohort. Hence, the G8 reform has spill-over effects on the graduation cohort directly preceding the double cohort. Finally, ε_{sc} is the error term. As the error term is likely to be correlated within states, we follow the recommendation of Bertrand et al. (2004) and cluster the standard errors at the level of the policy change.[15] Note that all our aggregate-level regressions are weighted

[15] Additionally, we apply wild cluster bootstrapping in the robustness section, which Cameron et al. (2008) recommend for situations with few clusters.

so that our results exactly equal individual level regressions (Angrist and Pischke, 2009, p. 235).[16]

5 Results

5.1 Main results

Table 2 presents our main results. While column (1) shows the results for the baseline difference-in-differences specification, which controls for state and time fixed effects as well as for the double graduation cohort, column (2) - our preferred specification - further controls for the last G9 cohort. The results in Panel A column (1) indicate that the enrollment rate declined by 5.1 percentage points due to the G8 reform. Controlling for the last cohort before the double graduation cohort (column 2) slightly increases this effect in absolute terms to 6 percentage points. The estimated reform effect amounts to a 8% decline in enrollment.[17] The decline in the enrollment rate of 6 percentage points is quite large compared to other findings in the literature. For example, for the much debated introduction of tuition fees in Germany of 500 EUR per term, Hübner (2012) identifies a decrease in enrollment by 2.7 percentage points. Other studies even find smaller or insignificant reductions in enrollment rates (Helbig et al., 2012; Bruckmeier and Wigger, 2014). Comparing our result to findings for financial aid in Germany, Steiner and Wrohlich (2012) estimate that an annual increase in financial aid by 1000 Euro increases enrollment rates by 2 percentage points. Estimated effect sizes of financial aid are similar for Denmark (Nielsen et al., 2010) and slightly larger for the U.S. (see e.g. Dynarski, 2002). Compared to these effect sizes, our estimate suggests that the negative G8 reform effect on enrollment is substantial.

We further find that the timing of enrollment changes as a consequence of the reform (Panel B). Among those who enroll in the year of graduation or the year after, the probability to immediately enroll decreases by 6.8 percentage points (column 2), indicating that a non-trivial fraction of students delay their enrollment. The estimation results presented in Panel C show that the probability of a regular study progress also decreases significantly. The

[16]For each outcome the weights are given by the outcome's denominator, i.e. enrollment rates are weighted with the number of graduates and the other two outcomes with the number of freshmen students who enrolled in the year of graduation or the year after. Our results are very similar if we perform the analysis without weights (see Section 6).

[17]For the calculation of the percentage change we use the average enrollment rate, as reported in Table 1, as a baseline.

share of students with a regular study progress during the first year of studies decreases by 2.6 percentage points.

Table 2 also reports the coefficients for the double graduation cohort and the last exclusive G9 cohort, i.e. the cohort before the double cohort. Both cohorts are assigned neither to the treatment nor the control group in our main specification. It is worthwhile noting that the reduction in university enrollment in the double graduation cohort is even larger than the G8 effect. The probability to enroll within one year after graduation is reduced by more than 8 percentage points. The effect for this cohort is significantly different from the G8 effect and underlines that the double cohort is peculiar and findings for this cohort do not necessarily translate to later G8 cohorts. Further, this finding is in line with the argument that wages are lower in larger cohorts (Welch, 1979) and that rational students will take this into account in their enrollment decision (Bound and Turner, 2007). As we are unable to distinguish between the cohort's G8 and G9 students, the coefficient for the double cohort displays the joint effect for both G8 and G9 students.[18]

It is also evident that the last cohort before the double cohort is a particular cohort as this cohort is already affected by the upcoming double graduation cohort. For this cohort, the probability to enroll in university within one year after graduation decreases by about 1.6 percentage points. Graduates of the last G9 cohort also strongly responded to the incentive to enroll in the year of their graduation, in order to avoid starting with the double graduation cohort (which is eligible to enter university one year after the last G9 cohort): The probability to enroll immediately after graduation increases by 6.9 percentage points for these cohorts. This further strengthens the argument that graduates take into account the cohort size in their decision to enroll in university. Thus, it seems advisable to control for these cohorts in our main specification and to assign neither to the treatment nor the control group.

Taken together, the results of this section suggest that fewer graduates enroll in university as a consequence of the G8 reform. On top of that, the reform's success in reducing the age of labor market entry may be mitigated: More students delay their enrollment and fewer students show a regular study progress.

[18] In Table A.1 in the Appendix, we approximate the treatment status for individuals in the double cohorts based on information on students' birthday and school entry regulations. Due to grade retention, this is only an imperfect approximation and, therefore, not our preferred specification. Nevertheless, within the double cohort, G8 students are also less likely to enroll in university and more likely to delay their enrollment than G9 students. However, we find enrollment rates to decrease also for G9 students in this cohort. The negative effect for the double cohort with respect to study progress even seems to be driven by G9 students.

5.2 Outcome-specific supplementary results

In this section we provide further evidence on the reform effects. These results complement our main analysis as presented in the previous section.

Enrollment rates: The previous section focused on enrollment within one year after high school graduation as the majority of students who enroll in university do so within this time frame (see Table 1). However, from a human capital accumulation perspective it is important to analyze whether students refrain from enrolling entirely or just delay their enrollment beyond the first year. Thus, we redefine the numerator of Equation (1) and study the G8 effect on enrollment rates within 2 and 3 years after graduation. For completeness, and in order to compare our estimates with existing evidence on enrollment rates, we also report the effect on enrollment rates in the year of graduation. We compare these effects to the estimated reform effect on enrollment rates within one year after graduation - our main measure of enrollment.

Table 3 shows that the reform effect is most pronounced for enrollment in the same year, which decreases by 8 percentage points (column 1). This is no surprise, given the evidence that students delay their enrollment. This is the only effect that we can directly compare to estimates in Meyer et al. (2015), as they only observe students in the year of their graduation. Relying on survey data, the authors find an even higher decrease in enrollment in the same year (of about 15 percentage points). The effect on enrollment within two years after graduation (column 2) is as large as the effect on enrollment within one year after graduation. Similarly, the effect on enrollment within three years after graduation (column 3) is only marginally smaller in absolute terms. Note that enrollment within three years after high school graduation provides students starting a vocational education directly after high school graduation enough time to complete this degree (earning a vocational degree usually takes 2-3 years) and enroll in university afterwards. However, even considering these later enrollment decisions, three years after graduation still fewer students enroll in university in response to the G8 reform, providing no evidence for a quick catch-up of enrollment.

Due to the recency of the reform and the related right-censoring, we do not observe a cohort's lifetime enrollment rate (individuals could theoretically also enroll at the age of 25 or 80). However, it seems questionable whether lifetime enrollment for G8 students will catch up with those of G9 students for two reasons. First, three years after graduation the effect size is, with about 4.3 percentage points, still substantial. And second, in the past only few graduates enrolled in university later than three years after graduation.

Timing of enrollment: Our measure for the timing of enrollment as defined in Equation (2) involves some degree of arbitrariness with respect to the student population that we look at (denominator) as well as the timing of enrollment (numerator). Hence, Table 4 shows the effect of the reform if we use alternative definitions of *timing of enrollment*. First, we only change the denominator and look at students who enroll within 2 and 3 years after graduation (instead of within 1 year, as in our main definition). Extending the time period between high school graduation and university enrollment, does not change our conclusion: The G8 reform significantly decreases the probability to enroll in the year of graduation (Panel A). Second, we additionally alter the numerator of our outcome measure and look at enrollment within one year (instead of immediate enrollment, as in our main definition). Panel B in Table 4 shows that the timing of enrollment changes also at other margins. Among those who enroll within two years after graduation, also the probability to enroll within one year after graduation is significantly reduced by the reform. Thus, using alternative definitions we can substantiate our finding that the G8 reform induces students to delay their enrollment.

Regular study progress: Three reasons explain the decrease in regular study progress found in the previous section. Firstly, more students might quit their studies altogether and drop out of university. Secondly, students might change their major more frequently. And thirdly, more students may formally request to temporarily interrupt their studies. In order to separate these three reasons, we generate three new outcome variables. Similar to the main outcome *regular study progress*, these three outcomes refer to all students who enrolled within one year after graduation. Table 5 shows that the probability to drop out of university increases by about one percentage point (column 1). While this effect may appear rather small, it corresponds to an increase of 14%. We also find evidence that the reform increases the likelihood of students to change their major by 1.6 percentage points (or 15%). The effect on study interruptions is negligible and insignificant. These results suggest that affected students are less certain about their choices and consequently more likely to adjust their decisions than students before the reform. Table 5 also shows that the decrease in regular study progress is mainly driven by an increased probability of students to change their major; this effect accounts for about 62% of the overall decrease in regular progress, while 37% can be attributed to an increase in dropout rates.

6 Robustness

This section deals with various robustness and specification issues. We start by discussing several threats to a causal interpretation of our results. We proceed by showing that our results are robust to various alternative model specifications.

The key identifying assumption for a causal interpretation of our results is the existence of parallel outcome trends in G8 and G9 states in the absence of the G8 reform. As the common trend assumption cannot be tested directly, we examine whether the trends in outcomes differed before the treatment by means of placebo regressions. In the estimations in Table 6, we pretend that the reform took place two, three or four years before the actual reform and include one additional regressor per column that picks up the effect of the respective placebo policy. The results of our placebo regressions strongly support a causal interpretation of the G8 reform effects; all placebo reform indicators are insignificant and close to zero.

The common trend assumption would also be violated, if the timing of the reform implementation was correlated with other factors that are related to the outcomes we investigate. States that implemented the reform early, thus contributing relatively more to our findings, may have been on different trajectories regarding our outcomes than those states implementing the reform later or those states that did not experience any changes. When researching states' decisions on when to implement the reform, we found no evidence that these decisions are related to the outcomes we investigate: Saxony-Anhalt and Mecklenburg-Vorpommern, two eastern German states, were the first to implement the reform. They were already familiar with G8 as they had a G8 system in the 1990s and before reunification. Saarland, the third to implement the reform, is a rather small state on the French border with close links to France. Here, policy makers were eager to quickly implement the G8 reform as they saw their graduates at a disadvantage compared to the French graduates, who graduated one year earlier. While 2012 is the year with the most double graduation cohorts (4 states), it was reasonable for the most populous state, North Rhine-Westphalia, to have its double graduation cohort one year later. In order to refute any related concerns, in Table 7 we relax the common trend assumption and allow for state-specific linear time trends (column 2). All effects remain statistically significant and are of similar magnitude to those of our main specification.

Co-treatments, in the form of other policy reforms, are a related threat to the parallel trends assumption. Note that policy changes implemented at the federal level and common to all states (like the suspension of military service in 2011) are already taken into account by the time fixed effects. During our observation period, German states, however, implemented a set of other secondary schooling and university policies. These reforms were implemented at different points in time in different states and none of these policies are perfectly collinear to the G8 reform (for an overview of affected states and cohorts, see Table A.2 in the appendix). At the secondary school level, these policy reforms include the introduction of centralized school exit examinations, changes in the timing of secondary school tracking, as well as the reduction in subject choice during the last two years of academic high school. At the university level, these policies include the introduction (and subsequent abolition) of tuition fees as well as the introduction of the two-tier degree system (introduction of Bachelor's and Master's degrees) as part of the Bologna reforms.[19] In column (3) we explicitly account for these other policy reforms. This does not change our conclusions.

Another threat to our identification strategy relates to compositional changes in treatment and control states. As the G8 reform only affected academic high schools, the composition of treated and untreated students might change as students try to evade the reform. This could happen in several ways. First, students could move to a different state that has not yet implemented the reform. Second, academic high school students might switch to a lower secondary school track that is unaffected by the G8 reform. Third, students might switch to alternative school types that offer university entrance qualifications. In all three cases, fewer individuals would graduate from academic high schools. However, in an analysis using full population data, Huebener and Marcus (2015b) find no effect of the reform on the number of graduates from academic high schools and we can confirm this finding for an extended time window (see Table A.3 in the appendix). Further, Dahmann and Anger (2014) do not find evidence for increased mobility of academic high school students between states, and Huebener et al. (2016) show that - based on observable student characteristics - the composition of students did not change due to the reform. Moving to a different state and/or switching to a different school type in order to avoid the reform might be easiest in the city states of Berlin, Bremen, and Hamburg due to the regional proximity of other states and the availability of further schools types. Column (4) in Table 7 shows that our results do not change when we exclude these three states.

[19]Universities, and even departments within universities, were free to decide when to switch to the two-tier degree system. Thus, we use the share of students enrolled in a Bachelor program in the year before students' high school graduation as a proxy for the likelihood of enrolling in a Bachelor program. As the majority of students start university in their home state (about 65%), we use the corresponding share in students' state of high school graduation.

As pointed out by several other studies, economic conditions cannot only influence enrollment decisions but also students' decision to stay in education and continue their studies (e.g. Bedard and Herman, 2008; Sievertsen, 2016). Thus, in column (5) we control for GDP growth, unemployment rate, and youth unemployment in the state and year of students' high school graduation. Changes in the state's economic condition could also be seen as a potential co-treatment. All our estimates are robust to controlling for these potential co-treatments. Similarly, as argued by Bound and Turner (2007) and Bruckmeier and Wigger (2014), cohort size, specifically, the number of students earning a university entrance qualification, may affect students' enrollment decisions. It may further affect study progress if students are unwilling to continue their studies in crowded lectures and study classes. Therefore, in column (6) of Table 7 we control for the log of the number of high school graduates from all school types in each state and year; again, our estimates remain unchanged.

There are several specifics of the reform implementation that could potentially affect our estimates. First, a double graduation cohort in one state might influence students' enrollment decisions in neighboring states. In column (7) we consider these potential spill-over effects by additionally controlling for the existence of double graduation cohorts in neighboring states. Second, in general, students in the first G8 cohort knew that they would graduate after 8 instead of 9 years when they entered academic high school. However, in Saxony-Anhalt and Mecklenburg-Vorpommern students in the first G8 cohort were only informed about the shortening of the school duration, when they were in grade 9. Thus, this and the following two cohorts were surprised by the G8 reform and exposed to an even higher increase in weekly workload, which makes these cohorts quite distinct. In column (8) of Table 7 we control for the two cohorts after the double graduation cohort in Saxony-Anhalt and Mecklenburg-Vorpommern in order to rule out that our effects are driven by these cohorts. Third, there are four states that did not experience any change in the length of schooling during our observation period (see Figure 1). We exclude these four states from our estimation sample to examine if these results depend on specific trends in states that did not change treatment status (see column 9). None of these alternative model specifications change our estimates significantly.

The last three columns of Table 7 deal with various specification issues. Column (10) shows that our results for the first two outcomes are insensitive to using the same cohorts as for the last outcome. As there is a discussion about the appropriateness of weighting in difference-in-differences settings, we also estimate a specification without weighting (see column 11). Furthermore, column (12) shows that our conclusions do not change when applying wild-cluster bootstrapping (1000 replications, Mammen weights, testing under H_0) as an alternative method of inference.

Overall, the results of our robustness analysis as presented in Tables 6 and 7 support a causal interpretation of our effects.

7 Heterogeneity of the treatment effect

All results described in the previous sections represent average treatment effects. To investigate whether these average effects mask relevant differences, this section examines treatment effect heterogeneities across time, federal state, and gender.

7.1 Heterogeneities over time

It is important for researchers and policy makers alike to analyze whether the estimated reform effects are only temporary or lasting. As the reform is relatively new, we cannot look at a long post-treatment horizon. Nevertheless, we can examine the size of the treatment effect for several cohorts after the reform implementation.[20]

Table 8 displays the results of our main specification, in which we substitute the single G8 indicator by a set of binary variables capturing the reform effect for cohorts 1, 2, 3, and 4 or more years after the reform implementation (i.e. after the double graduation cohort). With respect to *enrollment rate*, there is no clear pattern of the treatment effect over time (see column 1). The effect for the first cohort after the implementation is of similar magnitude to the overall effect. The effect for the second cohort after the implementation is larger, while the effect for the third cohort is smaller. However, the effect for the cohorts four or more years after the double graduation cohort is similar to the effect after one year. Thus, there is little evidence that the reform's effect on *enrollment rate* is fading over time. Further, we demonstrate the validity of our approach by comparing these point estimates to effects in the cohorts before the double cohort. Column (2) shows that the effects for the cohorts 2-4 years before the reform are statistically insignificant, which is in line with the placebo regressions in Table 6. Further, the magnitude of these estimates is close to zero and clearly smaller than the effects for the G8 cohorts. The coefficients for the last cohort before the double cohort and the double cohort are significant, as before, indicating that both cohorts are affected by the G8 reform.

[20]We choose to look at the effect up to four years after the double graduation cohort, so that there are always at least two states in the treatment group.

The development of the treatment effect appears to be different for *timing of enrollment* (see column 3). Here, each coefficient is smaller in absolute terms than the coefficient for the previous cohort indicating that the size of the reform effect is declining over time. Again, there is no evidence that the outcome was trending before the last G9 cohort (column 4). With respect to *regular study progress* there is some evidence for an increase in the reform effect over time (column 5) as the coefficients increase almost monotonously across cohorts (in absolute terms). Point estimates for the double cohort and the last G9 cohort are small but significant, while for earlier G9 cohorts coefficients are close to zero and insignificant (column 6).

Overall, Table 8 suggests that while the effect on the timing of enrollment may fade over a longer time period, the effects on enrollment rate and study progress seem to persist.[21]

7.2 Heterogeneities across states

In the following we differentiate the treatment effect by federal state in order to see whether specific states managed to implement the reform without negative consequences. For this purpose, we substitute the binary treatment indicator in Equation (4) with interactions between the treatment indicator and each treatment state. Table 9 shows that the overall G8 effects are not driven by individual states. For *enrollment rate*, the treatment effect is negative and significant in the overwhelming majority of treatment states. These significant coefficients are close to the estimated overall reform effect of about 6 percentage points and vary between -3 and -8 percentage points. There seems to be no general pattern among the coefficients as these are similar for early and late adopters, for states in east and west Germany as well as for city states and other states. A similar picture emerges for *timing of enrollment*. All coefficients are negative and nine are significantly different from zero. As for our first outcome, there is no general pattern across state characteristics. With respect to *study progress*, all coefficients but one are again negative and significant.[22] Also for *study progress* we find little evidence for substantial state differences.

All in all, the results in Table 9 demonstrate that the effects are rather homogeneous across states. Lower enrollment rates, delayed enrollment, and decreased regular study progress

[21]Note that due to the differential timing of the reform implementation in states, a varying subset of treatment states identify the point estimates for the different post-treatment cohorts. Table A.4 in the Appendix presents estimates based on a constant set of treatment states and confirms the patterns regarding the dynamics of the treatment effect. The results for *enrollment rate* are even more stable across cohorts.

[22]Due to the nature of this outcome variable, we have to rely on fewer graduation cohorts (see also Table 1). Therefore, we cannot display coefficients for states that implemented the G8 reform in 2012 or later.

appear to be general consequences of the G8 reform.

7.3 Heterogeneities by gender

Previous research on the G8 reform finds evidence of gender specific differences in the reform effects (see e.g. Dahmann and Anger, 2014; Büttner and Thomsen, 2013; Huebener and Marcus, 2015b; Meyer and Thomsen, 2016). In light of this evidence we examine whether for our outcomes similar heterogeneities by gender exist. Table 10 presents our estimates separately for females and males. We can neither establish differential reform effects for *enrollment rates* nor for the *timing of enrollment*. Our estimates do not confirm the finding for the double cohort in Saxony-Anhalt suggesting that only females delay their enrollment (Meyer and Thomsen, 2016). For *regular study progress* the point estimate for males is higher (in absolute terms) than for females, although these two estimates do not differ significantly. Generally, the results do not suggest that males and females are differently affected by the G8 reform.

Taken together, this section finds little evidence for differential treatment effects across time, state or gender. This underlines the general nature of our results. Regarding the external validity of our findings, it is likely that the G8 reform will have similar consequences in the states that have implemented the reform outside of our sample period.

8 Channels

This section deals with various mechanisms that may explain our results. We first discuss arguments concerning the supply of university slots, before we turn to demand-side arguments.

Supply-side restrictions are one mechanism that could explain the decrease in enrollment rates, i.e. a shortage of university slots. However, this would mainly apply to students in the double graduation cohort, which is roughly double in size and which we excluded from the treatment group. Nevertheless, if universities are unable to provide sufficient places for the double graduation cohort this might have spillover effects on the enrollment decision of subsequent G8 cohorts. If resources were not adequately increased, subsequent G8 students may face more difficulties in being admitted to university since students from the double cohort still queue to gain access to universities. A decrease in enrollment rates may correspondingly only mirror higher competition for study places instead of students' actual choices. However, several arguments suggest that supply-side restrictions are not the key mechanism explaining our results: First, to cover the demand shock induced by the double graduation cohort, the governments of the treated states as well as the federal government continuously increased university funding under the Higher Education Pact *(Hochschulpakt)*. In part, this funding was explicitly directed toward increasing university slots to accommodate the double graduation cohort. Second, if there was a shortage of university slots, universities would have to tighten their admission policies. Consequently, the share of (locally) restricted study programs should increase, i.e. programs that use a cut-off based on the final high school grade points average to select students for admission (*numerus clausus*).[23] However, Table 11 shows that the share of restricted study programs does not significantly increase due to the reform, irrespective of whether we only look at Bachelor's programs (column 1) or also at other first degree programs like state examination (column 2).[24] Hence, there is no evidence that students affected by the reform faced higher competition with respect to being admitted to university. Third, if supply-side restrictions drive the results, we should see students circumventing these restrictions by studying in a different state, one that does not have a double cohort in the same year. Yet, our estimates in column (3) of Table 11 do not suggest a decline in the share of students who study in their home state; if at all, we even find G8 students to be slightly more likely to enroll in their home state. For these reasons, we conclude that supply-side restrictions are unlikely to be the main explanation for the decrease in enrollment rates.

We now turn to demand-side arguments. As outlined in Section 2, the G8 reform can be thought of as consisting of two parts: First, the reduction of the length of academic high school, which makes students one year younger at graduation. And second, the compensating increase in instruction hours in the remaining years, which resulted in a higher weekly workload. The first part, the *age channel*, might be responsible for lower enrollment rates if younger students overemphasize the present (Lavecchia et al., 2016) and therefore prefer immediate returns over higher future gains, making university education less attractive. The *age channel* could also explain delayed enrollment and lower rates of regular study progress if younger students need more time for orientation and/or if they aware of their relative age advantage and, therefore, take things more slowly. The second part, the *workload channel*

[23]Unlike in other countries, admission is only *centrally* restricted for few programs. Generally, universities only set *local* admission restrictions if the number of applications exceeds available slots. This implies that cut-offs are determined retrospectively.

[24]For this specification, we estimate a model in the style of Equation (4), in which we use the share of all restricted Bachelor's programs as well as the share of all restricted first degree programs as an outcome.

might work through worse performance in school if students are unable to cope with the higher workload.[25] Performance at school is one of the most important determinants for enrollment decisions as well as for success in university (Bowen and Bok, 1998; Bound and Turner, 2011). The results by Büttner and Thomsen (2013), Huebener and Marcus (2015b) and Trautwein et al. (2015) suggest that at the end of academic high school, students' performance is indeed adversely affected by the G8 reform. The *workload channel* may also operate through increased levels of stress and strain in school, which are also reported in previous studies on the G8 reform (Meyer and Thomsen, 2015; Quis, 2015; Trautwein et al., 2015) and could reduce the desire and motivation for further learning (Jürges and Schneider, 2010).

It is difficult to determine whether our findings are rather driven by the *age channel* or by the *workload channel* as there is little independent variation between the two channels. Nevertheless, in the following we provide some suggestive evidence. We proceed by first estimating the reform's effect on students' age at university enrollment and then examine whether the reform effects persist when we try to keep students' age constant. In this specification, if the G8 effect is close to zero and insignificant, our findings can mostly be attributed to the age channel. If, on the other hand, we also find a significant effect of the G8 reform on similar aged students, this provides some evidence that the age channel seems to play a minor role.

Focusing on students who enroll within one year after graduation, as in our main specification, Table 12 shows that the reform successfully decreased students' age at enrollment (see column 1), although only by eight and a half months (0.73 years), compared to a potential reduction of a full year.[26] Having established the age effect of the reform, we try to hold students' age constant by looking at G8 and G9 students who graduated from academic high schools at the age of 19.[27] These students are of similar age, but experienced different

[25]A negative effect on performance could also be reinforced by the age channel, given the evidence that (relatively) older students perform better in exams than (relatively) younger students (see e.g. Mühlenweg and Puhani, 2010).

[26]Our results can be compared to findings in Huebener and Marcus (2015b), who show that the G8 reform reduced the age at graduation by 0.86 years. This highlights that the difference between our point estimates and -1 results from two factors: Firstly, already at the time of graduation the reform did not achieve its full potential in terms of age reduction; secondly, graduates delayed their enrollment, as shown in the previous sections.

[27]According to the school entry regulations, posting the cut-off date for school entry at June 30th, we compare G9 students who are born between January and June with G8 students who are born between July and December. Note that for the denominator of enrollment rates (as defined in Equation 1) the information on the exact birth date is not available; thus, we have to assume that all 19-year-old high school graduates entered school according to school entry regulations.

amounts of weekly workload due to the reform. For all three outcome variables, holding students' age constant, the G8 reform indicator is still significant (columns 2-4). For *timing of enrollment* and *regular study progress* the effect is also similar in magnitude as in our main specification, while for *enrollment rate* the reform effect is even larger. The results in Table 12 suggest that the reform's main mechanism does not run through the reduced age of students and that, hence, our findings can rather be attributed to the higher workload the reform entailed.

The estimations in Table 12 may, however, be flawed by potential relative age effects: By analyzing similar aged G8 and G9 students, we compare G9 students who are relatively younger with respect to their graduation cohort with G8 students who are relatively older within their cohort. However, the literature on relative age effects in school suggests advantages for relatively older students (see e.g. Bedard and Dhuey, 2006; Mühlenweg and Puhani, 2010). If relatively older students perform better, we might also expect higher enrollment rates, faster enrollment and a higher probability of a regular study progress for the relatively older G8 students (as compared to the relatively younger G9 students). As we compare older G8 students with younger G9 students, the aforementioned arguments would rather bias our estimates toward zero. Yet, the G8 effects in Table 12 are not smaller than the estimates in our main specification, suggesting that relative age effects do not present a major concern for these estimations.

To sum up, we find little evidence that supply-side restrictions are the main channel that drives our results. We also find little support for the claim that the G8 reform primarily works through the reduced age of students. Hence, the obtained reform impact mainly operates through the higher workload during school.

9 Conclusion

We examine whether it is possible to reduce the length of secondary schooling – thereby allowing for an earlier labor market entry – without negatively affecting university enrollment. If it was possible to learn the same amount of material in a shorter period of time, this would mark an efficiency gain for the individual student. This efficiency gain would not only benefit the individual in terms of increased lifetime earnings but it would also come with benefits for the general public in terms of higher tax revenues and – most importantly – a longer working life, which could help in coping with challenges aging societies face. Against the backdrop of existing evidence on the negative effects of simple reductions in the years of schooling, a novel policy in Germany bears the potential to decrease the length of schooling without affecting students' human capital. This policy reduced the length of the academic high school by one year, but increased weekly instruction hours in the remaining school years in order to fully compensate for the omitted year.

We examine the medium-term consequences of this recent policy change for higher education decisions. We apply a difference-in-differences approach exploiting the variation in reform implementation over time and across states. Using administrative data from the German student register, which covers all students in Germany, we provide evidence for adverse consequences of this policy change: Students are less likely to enroll in university, more likely to delay their enrollment, and less likely to have a regular study progress. For an illustration of the magnitude of the obtained effect sizes consider the following calculations: 213,000 students graduated from academic high schools in 2014 in the twelve treatment states. Taking our point estimates at face value and assuming effect homogeneity across states and cohorts our results suggest that due to the reform more than 12,000 students of the 2014 graduation cohort did not enroll in university; additionally, almost 11,000 students delayed their enrollment by one year, and about 4,000 students did not have a regular study progress during their first year at university.

Further, we show that these reform impacts are quite general in nature. The effects are similar across states and gender, and they do not seem to be only short-lived implementation effects. An investigation of potential channels of the reform suggests that our findings are driven neither by supply-side restrictions nor by the reduction in students' age. Consequently, we argue that the main channel works through the higher workload at school, which resulted in lower school performance and higher reported stress levels.

Increasing education efficiency by reducing the years of schooling while increasing weekly instruction hours sounds like a tempting policy option. However, our empirical evidence shows that even this kind of policy might not come without unintended consequences regarding students' higher education decisions. Lower enrollment rates at universities and higher dropout rates may lower a country's human capital stock and, ultimately, economic prosperity. Additionally, by delaying the enrollment decision and by changing majors more often, students lose some of their initial age advantage, thereby counteracting the reform's main goal of earlier labor market entry. Overall, our study suggests that this reform cannot fully eliminate the trade-off between an earlier labor market entry and constant levels of human capital.

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Figures and tables

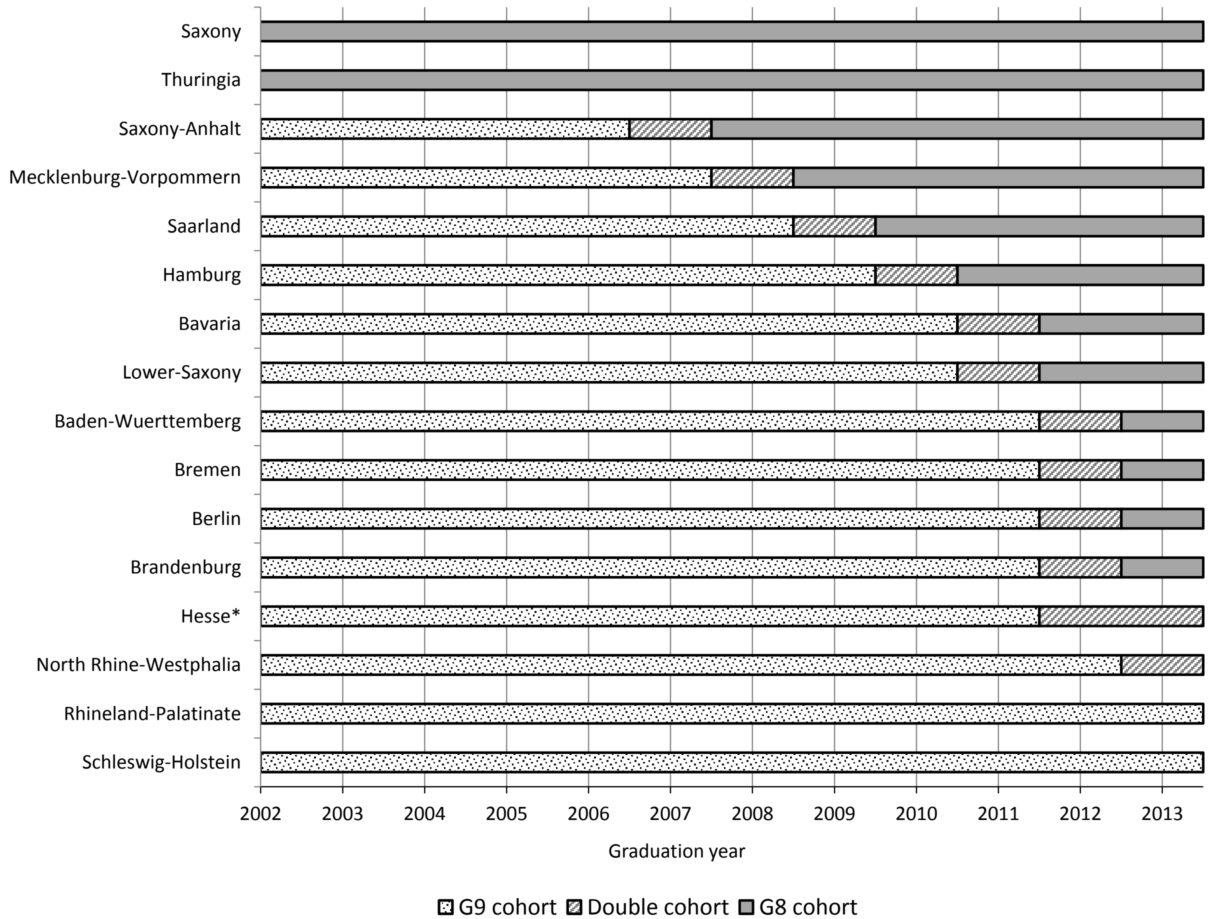

Figure 1: Timing of the G8 reform in the German states

Notes: The figure illustrates the treatment status of different graduation cohorts in the German states.
* Hesse implemented the reform over various years and is not included in our main analysis sample.

Table 1: Descriptive statistics

	Share	N	Included grad. cohorts
Enrollment in the same year	0.47	2,823,274	2002-2014
Enrollment within 1 years	0.76	2,601,880	2002-2013
Enrollment within 2 years	0.82	2,343,454	2002-2012
Enrollment within 3 years	0.86	2,091,000	2002-2011
Timing: Immediate enrollment	0.61	1,987,444	2002-2013
Regular study progress	0.81	1,656,629	2002-2012
Drop out	0.07	1,656,629	2002-2012
Changing major	0.11	1,656,629	2002-2012
Interruption	0.01	1,656,629	2002-2012

Notes: This table presents summary statistics related to our outcome variables. Our three main outcome variables are shown in bold. For all enrollment outcomes (see the first four lines) N refers to the number of graduates from academic high schools, while for the other variables N refers to university students, i.e. graduates from academic high schools who enrolled in university within one year. Further, for each graduation cohort, the time span after graduation that we can observe differs.

Table 2: Effects of the G8 reform: Main results

	Baseline (1)	+ last G9 cohort (2)	% change (3)
Panel A: Enrollment within one year			
G8 reform	-0.051***	-0.060***	7.9%
	(0.015)	(0.017)	
Double cohort	-0.078***	-0.085***	
	(0.008)	(0.011)	
Last G9		-0.016**	
		(0.007)	
$N_{state*cohort}$	180	180	
$N_{individuals}$	2,601,880	2,601,880	
Panel B: Speed of enrollment			
G8 reform	-0.105***	-0.068***	11.1%
	(0.024)	(0.014)	
Double cohort	-0.047***	-0.015	
	(0.014)	(0.009)	
Last G9		0.069***	
		(0.007)	
$N_{state*cohort}$	180	180	
$N_{individuals}$	1,987,444	1,987,444	
Panel C: Regular study progress			
G8 reform	-0.022**	-0.026***	3.2%
	(0.008)	(0.009)	
Double cohort	-0.009	-0.013*	
	(0.006)	(0.007)	
Last G9		-0.007*	
		(0.003)	
$N_{state*cohort}$	165	165	
$N_{individuals}$	1,656,629	1,656,629	

Notes: This table reports the G8 reform effects on different outcomes as indicated by Panel A-C. In all specifications we include fixed effects for federal states and graduation cohorts. Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Table 3: Further results on enrollment rates

	Enrollment ...			
	in the same year (1)	within 1 year (2)	within 2 years (3)	within 3 years (4)
G8 reform	-0.084***	-0.060***	-0.058***	-0.043**
	(0.015)	(0.017)	(0.019)	(0.016)
$N_{state*cohort}$	195	180	165	150
$N_{individuals}$	2,823,274	2,601,880	2,343,454	2,091,000

Notes: This table presents the effect of the G8 reform for additional enrollment outcomes. All estimates are based on our main specification as outlined in Eq. (4). In line with controlling for the last G9 cohort in column 1 and 2, in column 3 (4) we additionally control for the cohorts two (and three) years before the double cohort in order to consider the disincentive for these cohorts to enroll together with the double cohort. Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Table 4: Alternative definitions for the timing of enrollment

	Conditional on enrollment...		
	within 1 year (1)	within 2 years (2)	within 3 years (3)
Panel A: Share of students who enroll in the same year			
G8 reform	-0.068***	-0.065***	-0.032*
	(0.014)	(0.017)	(0.015)
$N_{state*cohort}$	180	165	150
$N_{individuals}$	1,987,444	1,921,285	1,797,470
Panel B: Share of students who enroll within one year			
G8 reform		-0.015***	-0.012
		(0.004)	(0.008)
$N_{state*cohort}$		165	150
$N_{individuals}$		1,921,285	1,797,470

Notes: This table reports estimates of the G8 reform for alternative definitions of the timing of enrollment. The column headers indicate the sample and, hence, refer to the denominator of Eq. (2), while the panels refer to the numerator of Eq. (2). The upper left coefficient, for instance, refers to the effect of the G8 reform on the timing of enrollment, measured as the share of students who enroll in the year of graduation among all students who enroll within one year after graduation. Similarly, the lower right coefficient refers to the share of students who enroll within one year among all those who enroll within three years after graduation. All estimates are based on our main specification as outlined in Eq. (4). In line with controlling for the last G9 cohort in column 1, in column 2 (3) we additionally control for the cohorts two (and three) years before the double cohort in order to consider the disincentive for these cohorts to enroll together with the double cohort. Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Table 5: Decomposing the effect on regular study progress

	Regular study progress (1)	Drop out (2)	Changing major (3)	Interruption (4)
G8 reform	-0.026***	0.010**	0.016**	0.000
	(0.009)	(0.004)	(0.007)	(0.001)
$N_{state*cohort}$	165	165	165	165
$N_{individuals}$	1,656,629	1,656,629	1,656,629	1,656,629

Notes: This table reports the G8 reform effects on different outcomes as indicated by the column headers. All estimates are based on our main specification as outlined in Eq. (4). Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Table 6: Placebo tests

	Placebo reform in...		
	t-2 (1)	t-3 (2)	t-4 (3)
Panel A: Enrollment within one year			
Placebo effect	-0.008	-0.003	0.004
	(0.007)	(0.007)	(0.010)
$N_{state*cohort}$	180	180	180
$N_{individuals}$	2,601,880	2,601,880	2,601,880
Panel B: Speed of enrollment			
Placebo effect	0.007	0.006	0.003
	(0.008)	(0.008)	(0.009)
$N_{state*cohort}$	180	180	180
$N_{individuals}$	1,987,444	1,987,444	1,987,444
Panel C: Regular study progress			
Placebo effect	-0.003	-0.006	-0.008
	(0.005)	(0.004)	(0.005)
$N_{state*cohort}$	165	165	165
$N_{individuals}$	1,656,629	1,656,629	1,656,629

Notes: This table reports various placebo tests for the G8 reform effect on different outcomes as indicated by Panel A-C. All estimates are based on our main specification as outlined in Eq. (4) and additionally include one further regressor per column that picks up the effect of the respective placebo policy. Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Table 7: Robustness tests

		Identification issues					Reform issues			Specification issues		
	Main (1)	+ state trends (2)	+ other reforms (3)	w/o city states (4)	+ econ. controls (5)	+ cohort size (6)	+ DC in neighb. states (7)	+ surpr. cohorts (8)	w/o never chang. (9)	Same cohorts (10)	w/o weights (11)	Wild boot-strap (12)
Panel A: Enrollment within one year												
G8 reform	-0.060***	-0.067***	-0.068***	-0.064***	-0.067***	-0.055***	-0.060***	-0.063***	-0.048**	-0.084***	-0.059***	-0.060***
	(0.017)	(0.019)	(0.016)	(0.015)	(0.011)	(0.015)	(0.016)	(0.018)	(0.021)	(0.022)	(0.013)	[0.000]
$N_{state*cohort}$	180	180	180	144	180	180	180	180	132	165	180	180
$N_{individuals}$	2,601,880	2,601,880	2,601,880	2,395,741	2,601,880	2,601,880	2,601,880	2,601,880	2,171,543	2,343,454		2,601,880
Panel B: Speed of enrollment												
G8 reform	-0.068***	-0.076***	-0.067***	-0.065***	-0.072***	-0.062***	-0.067***	-0.073***	-0.073***	-0.056***	-0.050***	-0.068***
	(0.014)	(0.018)	(0.011)	(0.015)	(0.011)	(0.012)	(0.014)	(0.014)	(0.019)	(0.018)	(0.014)	[0.000]
$N_{state*cohort}$	180	180	180	144	180	180	180	180	132	165	180	180
$N_{individuals}$	1,987,444	1,987,444	1,987,444	1,834,256	1,987,444	1,987,444	1,987,444	1,987,444	1,668,024	1,656,629		1,987,444
Panel C: Regular study progress												
G8 reform	-0.026***	-0.018*	-0.032***	-0.029***	-0.023**	-0.027***	-0.026***	-0.025**	-0.025**	-0.026***	-0.033***	-0.026***
	(0.009)	(0.010)	(0.006)	(0.009)	(0.008)	(0.008)	(0.008)	(0.010)	(0.010)	(0.009)	(0.008)	[0.012]
$N_{state*cohort}$	165	165	165	132	165	165	165	165	121	165	165	165
$N_{individuals}$	1,656,629	1,656,629	1,656,629	1,528,816	1,656,629	1,656,629	1,656,629	1,656,629	1,393,480	1,656,629		1,656,629

Notes: This table reports various robustness tests of the G8 reform effect on different outcomes as indicated by Panel A-C. All estimates are based on our main specification as outlined in Eq. (4). Standard errors are clustered at the state level for columns (1)-(11) and presented in parentheses, the brackets in column (12) present p-values based on wild cluster bootstrapping (1000 replications, Mammen weights, testing under H_0). * p<0.1, ** p<0.05, *** p<0.01.

Table 8: Dynamics of the treatment effect

	Enrollment		Speed		Study progress	
	(1)	(2)	(3)	(4)	(5)	(6)
4 years prior		0.009		0.001		-0.007
		(0.009)		(0.008)		(0.006)
3 years prior		-0.004		0.005		-0.002
		(0.003)		(0.005)		(0.004)
2 years prior		-0.008		0.005		-0.000
		(0.006)		(0.007)		(0.005)
Last G9	-0.015*	-0.011**	0.070***	0.066***	-0.007*	-0.005*
	(0.007)	(0.005)	(0.007)	(0.006)	(0.003)	(0.003)
Double cohort	-0.085***	-0.081***	-0.013	-0.017*	-0.013*	-0.012
	(0.011)	(0.009)	(0.009)	(0.010)	(0.007)	(0.007)
1 year after	-0.057***	-0.053***	-0.071***	-0.075***	-0.024**	-0.024**
	(0.016)	(0.013)	(0.013)	(0.013)	(0.010)	(0.010)
2 years after	-0.071***	-0.066***	-0.067***	-0.070***	-0.029***	-0.031***
	(0.018)	(0.016)	(0.014)	(0.014)	(0.008)	(0.009)
3 years after	-0.037	-0.033	-0.056**	-0.058**	-0.037***	-0.039***
	(0.022)	(0.021)	(0.025)	(0.024)	(0.006)	(0.007)
4 or more years after	-0.051**	-0.047**	-0.022	-0.023	-0.039***	-0.043***
	(0.022)	(0.020)	(0.019)	(0.018)	(0.009)	(0.009)
$N_{state*cohort}$	180	180	180	180	165	165
$N_{individuals}$	2,601,880	2,601,880	1,987,444	1,987,444	1,656,629	1,656,629

Notes: This table reports the G8 reform effects on different outcomes as indicated by the column header. All estimates are based on our main specification as outlined in Eq. (4), where we substitute the single G8 indicator by a set of binary variables capturing the reform effect for cohorts before and after the reform implementation. Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Table 9: Heterogeneities by federal state

	Enrollment (1)	Speed (2)	Study progress (3)
Saxony-Anhalt	-0.055***	-0.010	-0.030***
	(0.016)	(0.006)	(0.003)
Mecklenburg	-0.057***	-0.045***	-0.040***
	(0.014)	(0.009)	(0.004)
Saarland	-0.032*	-0.028**	-0.049***
	(0.015)	(0.010)	(0.004)
Hamburg	-0.017	-0.088***	-0.015**
	(0.015)	(0.012)	(0.006)
Bavaria	-0.078***	-0.095***	-0.029***
	(0.015)	(0.012)	(0.007)
Lower-Saxony	-0.082***	-0.070***	-0.005
	(0.016)	(0.012)	(0.007)
Baden-Wuerttemberg	-0.054***	-0.088***	
	(0.015)	(0.011)	
Bremen	-0.082***	-0.088***	
	(0.015)	(0.011)	
Berlin	0.030*	-0.060***	
	(0.015)	(0.011)	
Brandenburg	-0.047***	-0.031**	
	(0.016)	(0.011)	
$N_{state*cohort}$	180	180	165
$N_{individuals}$	2,601,880	1,987,444	1,656,629

Notes: This table reports the G8 reform effects by federal state on the outcomes indicated by the column header. All estimates are based on our main specification as outlined in Eq. (4) where we substitute the G8 indicator by interaction terms between this indicator and each treatment state. Standard errors are clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 10: Heterogeneities by gender

	Female	Male
	(1)	(2)
Panel A: Enrollment within one year		
	shstud1yfem	shstud1ymal
G8 reform	-0.061***	-0.058**
	(0.015)	(0.020)
$N_{state*cohort}$	180	180
$N_{individuals}$	1,452,630	1,149,250
Panel B: Speed of enrollment		
G8 reform	-0.063***	-0.069**
	(0.013)	(0.026)
$N_{state*cohort}$	180	180
$N_{individuals}$	1,069,225	918,219
Panel C: Regular study progress		
G8 reform	-0.020**	-0.034***
	(0.009)	(0.011)
$N_{state*cohort}$	165	165
$N_{individuals}$	894,127	762,502

Notes: This table reports the G8 reform effects on different outcomes as indicated by Panel A-C separately by gender. All estimates are based on our main specification as outlined in Eq. (4). Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Table 11: Supply-side restrictions

	All locally restricted Bachelor programs	All locally restricted first degree programs	Enrollment in home state
	(1)	(2)	(3)
G8 reform	0.027	0.044	0.019
	(0.056)	(0.037)	(0.015)
$N_{state*cohort}$	144	144	180
$N_{individuals}$			1,987,444

Notes: This table reports G8 reform effect on different outcomes as indicated by the column header. All estimates are based on our main specification as outlined in Eq. (4). Standard errors are clustered at the state level. Information on the share of locally restricted Bachelor programs as well as on the share of all first degree programs is only available from 2006 onwards and provided by the German Rectors' Conference (2014) *(Hochschulrektorenkonferenz)*. * p<0.1, ** p<0.05, *** p<0.01.

Table 12: Examining the age channel

	All	Only 19-year-olds		
	Age at enrollment (1)	Enrollment (2)	Speed (3)	Study progress (4)
G8 reform	-0.725***	-0.131***	-0.063***	-0.031*
	(0.070)	(0.019)	(0.017)	(0.010)
$N_{state*cohort}$	180	168	180	165
$N_{individuals}$	1,987,444	1,027,614	617,703	519,762

Notes: This table displays the effect of the G8 reform on different outcomes as indicated by the column header. In column 1 we look at the age at enrollment for students who enrolled within one year after graduation. In columns 2-4 we only consider students who graduated from high school at the age of 19. All estimates are based on our main specification as outlined in Eq. (4). Note that for three states the age-specific number of academic high school graduates is not available for the cohorts between 2002-2005; thus, the number of observations differs in the 2nd column. Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Appendix

Table A.1: Estimating the G8 effect within the double cohort

	Enrollment		Speed		Study progress	
	(1)	(2)	(3)	(4)	(5)	(6)
G8 reform	-0.060***	-0.060***	-0.068***	-0.068***	-0.026***	-0.026***
	(0.017)	(0.017)	(0.014)	(0.014)	(0.009)	(0.009)
Last G9	-0.016**	-0.016**	0.069***	0.069***	-0.007*	-0.007*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.003)	(0.003)
DC	-0.085***		-0.015		-0.013*	
	(0.011)		(0.009)		(0.007)	
G8 in DC		-0.103***		-0.051***		-0.001
		(0.017)		(0.008)		(0.006)
G9 in DC		-0.071***		0.015		-0.022**
		(0.010)		(0.011)		(0.008)
$N_{state*cohort}$	180	191	180	191	165	175
$N_{individuals}$	2,601,880	2,601,880	1,987,444	1,987,444	1,656,629	1,656,629

Notes: In this estimation we aim to disentangle the overall double cohort effect into an effect for the cohort's G8 and G9 students. As the exact treatment status is unknown for students in the double cohort, we assign it based on birth information and school entry regulations. Children who turn six before (after) June 30th of a given year usually start school in that (the following) year. Thus, double cohort graduates, who are older than 19, or 19 and born before June 30th, are assumed to be G9 students; likewise, graduates, who are younger than 19, or 19 and born after June 30th, are assumed to be G8 students. Note that the computation of separate enrollment rates within the double cohort requires separate graduation numbers for a cohorts' G8 and G9 students. Two states lack this information. For these two states we assume that the ratio of G8 and G9 students within the double cohort is the same as in the other eleven treatment states, which provide the relevant information. All estimates are based on our preferred specification and include state and time fixed effects. Standard errors are clustered at the state level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.2: Implementation of G8 and other education reforms in the federal states

	School policies				University policies
	G8	Central exit examination	Tracking in grade 7	Restricted upper-secondary subject choice	Tuition fees
			Change from G9 to G8		
Saxony-Anhalt	from 2007	all	2006-2009	from 2005	none
Mecklenburg-Vorpommern	from 2008	all	none	from 2008	none
Saarland	from 2009	all	none	from 2010	2007-2009
Hamburg	from 2010	all	none	from 2011	2007-2011
Bavaria	from 2011	all	none	from 2011	2007-2012
Lower-Saxony	from 2011	from 2006	until 2011	from 2008	2006-2013
Baden-Württemberg	from 2012	all	none	from 2004	2007-2011
Bremen	from 2012	from 2007	until 2011	all	none
Berlin	from 2012	from 2007	all	all	none
Brandenburg	from 2012	from 2005	all	none	none
North Rhine-Westphalia	from 2013	from 2007	none	all	2007-2010
			Always G8		
Saxony	all	all	none	from 2010	none
Thuringia	all	all	none	from 2011	none
			Always G9 (in observation period)		
Rhineland-Palatinate	none	all	none	from 2011	none
Schleswig-Holstein	from 2016	from 2008	none	from 2011	none
			Excluded from estimation sample		
Hesse	from 2012	from 2007	none	from 2005	2007

Notes: This table informs about changes in education policies during our sample period. For school policies, numbers refer to the affected graduation cohort while for university policies numbers refer to years. *G8* indicates the year of the double graduation cohort. *Centralized school exit examinations* shift the design of exit exams from high schools to federal state institutions such that all students in the specific state sit the same exit exam. *Tracking in grade 7* indicates whether tracking takes place in grade 7 (or earlier). *Restricted upper secondary subject choice* indicates graduation cohorts for whom the set of subject choices for the final two years at academic high schools has been restricted. *Tuition fees* indicates the years in which tuition fees (about 500 Euro per semester) were charged. Sources for the reform dates are available from the authors upon request.

Table A.3: Effect of the G8 reform on the number of graduates

	18-20 year olds (1)	18-19 year olds (2)	19 year olds (3)	log (4)
G8 reform	-0.008	-0.000	-0.006	-0.072
	(0.014)	(0.013)	(0.014)	(0.231)
$N_{state*cohort}$	195	195	195	195

Notes: The table reports the effect of the G8 reform on graduation rates with different normalisations. Columns (1)-(3) normalise the number of graduates from academic high schools by the average size of the populations of 18-20, 18-19 and 19 year olds, respectively. Column (4) takes the log of the number of graduates instead. All estimates are based on our main specification as outlined in Eq. (4) and rely on on the 2002-2014 graduation cohorts. Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.

Table A.4: Dynamics of the treatment effect with different sample restrictions

Only states that we observe for X years after	X=1 (1)	X=2 (2)	X=3 (3)	X=4 (4)
Panel A: Enrollment within one year				
G8 · 1 year after	-0.029***	-0.053***	-0.029*	-0.033*
	(0.008)	(0.014)	(0.013)	(0.017)
G8 · 2 years after		-0.060***	-0.033**	-0.048***
		(0.014)	(0.013)	(0.014)
G8 · 3 years after			-0.022	-0.042**
			(0.015)	(0.018)
G8 · 4 years after				-0.043**
				(0.019)
$N_{state*cohort}$	179	171	165	161
$N_{individuals}$	2,499,260	2,366,357	2,137,760	2,111,021
Panel B: Speed of enrollment				
G8 · 1 year after	-0.046***	-0.073***	-0.035	-0.012
	(0.007)	(0.017)	(0.023)	(0.011)
G8 · 2 years after		-0.050***	-0.040***	-0.030**
		(0.011)	(0.013)	(0.011)
G8 · 3 years after			-0.047**	-0.026*
			(0.021)	(0.012)
G8 · 4 years after				-0.020
				(0.015)
$N_{state*cohort}$	179	171	165	161
$N_{individuals}$	1,911,969	1,804,061	1,628,383	1,609,000
Panel C: Regular study progress				
G8 · 1 year after	-0.014*	-0.025***	-0.034***	-0.031***
	(0.007)	(0.006)	(0.007)	(0.005)
G8 · 2 years after		-0.021**	-0.034***	-0.030***
		(0.008)	(0.007)	(0.004)
G8 · 3 years after			-0.032***	-0.032***
			(0.005)	(0.006)
G8 · 4 years after				-0.032***
				(0.010)
$N_{state*cohort}$	165	161	157	154
$N_{individuals}$	1,594,043	1,474,603	1,460,538	1,449,206

Notes: This table reports the G8 reform effects on different outcomes as indicated by Panel A-C. All estimates are based on our main specification as outlined in Eq. (4), where we substitute the single G8 indicator by a set of binary variables capturing the reform effect for cohorts 1, 2, 3, and 4 or more years after the reform implementation. All treatment observations beyond the time frame indicated by the column header are excluded from the estimation. Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01.