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Better with Bologna? Tertiary education reform and student outcomes

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ABSTRACT

Given the scale of the university reform induced by the Bologna Process, little is known about how the reform impacted those most immediately affected: the students. This paper uses unique micro data from Humboldt-Universität zu Berlin, Germany, to estimate treatment effects on student outcomes. Variation in treatment introduction over time and across subjects generates exogenous assignment of students into treatment (Bachelor) and control groups (Diploma). Results indicate that the Bologna reform led to a significant, sizeable increase in the probability of graduating within planned instructional time; it also decreased standardized study duration and worsened final grades in the treatment group.

1. Introduction

In 1999 the EU member states met in Bologna, Italy, to agree upon the creation of a common higher education area. This meeting initiated a harmonization process, with the objctives to improve international competitiveness of the European higher education system, to increase mobility among university staff and students, and to enhance students' employability. In order to implement the requirements of the Bologna Process over the next years, all signatory countries had to adapt their education systems in accordance with a two-tier system consisting of an undergraduate level (Bachelor) and a graduate level (Master). In 1999, 30 countries thus created the European Higher Education Area (EHEA), today comprising 48 countries. The Bologna Reform is therefore far-reaching in ambition, scale, and in practical implications for the design of tertiary education in each country.

In Germany, the transformation from the traditional and renowned Diploma system to the new degree structure was highly controversial (De Rudder 2010) and went on for almost a decade: specifically, Figure 1 displays the number of first-year students in Germany by degree type and shows that the main transition period lasted from the year 2000 until 2010.[1] One argument for switching from comprehensive single-tier degrees (Diploma and Magister) to shorter Bachelor degrees was to increase efficiency of the higher education system, by providing the labor market with adequately qualified university graduates within a shorter period of time.

Any attempt at answering this overarching question puts those into the spotlight who are most immediately affected by the practical implementation of the Bologna process: the students. To date relatively little is known about how this fundamental and far-reaching reform of tertiary education affected student outcomes. In order to help fill this important knowledge gap, our analysis
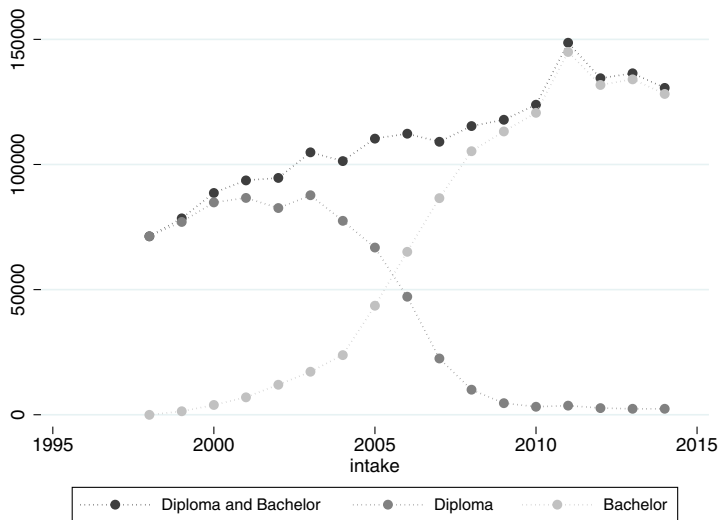
Figure 1. Number of first-year students in Germany by degree, 1998–2014.

investigates the reform success at the individual student level, using micro data from Humboldt-Universität zu Berlin (HU). Specifically, we can investigate reform effects on students' graduation probability, study duration, and final grades. Whereas, evidently, this cannot provide a full assessment of the efficiency of the higher education system, the analysis is informative concerning the course of studies and early predictors of subsequent labor market success (Heckman, Stixrud, and Urzua 2006; Borghans et al. 2016).

The data that we were able to compile provide a key basis for analyzing effects of the Bologna Process for a number of reasons. First, HU is one of the largest universities in Germany, and we observe more than 24,000 students from the 1990s onwards. Secondly, the data cover the universe of 15 annual student cohorts, which, in turn, are homogenous over time, as we will show. Thirdly, the data also comprise many important dimensions of student heterogeneity, such as a broad set of subject choices and geographical origin of the pre-university education. Fourth, and most importantly, the sequential implementation of the reform at the department level generates exogenous variation to identify the Bologna Reform treatment effect. Given these main pillars of the analysis, we believe - and discuss in detail – that the precise estimation of reform effects at the university level is informative for a broad set of universities in Germany, and across Europe.

In the next section we introduce the economic implications and theoretical underpinnings of the Bologna Reform. Section three provides an overview of the related literature on the evaluation of the Bologna Process, and the relevant institutional details at HU. The fourth section presents the data and a descriptive analysis. Section 5 discusses identification - i.e. the Bologna reform as a natural experiment, and IV estimation – and presents empirical estimates of the effect of the Bologna Reform on students' educational outcomes. We also discuss mechanisms. Section 6 concludes.

2. Economic dimension and labor market relevance

During the 20th century higher education systems throughout Europe faced a vast expansion. While structural change was the driving force behind the increased demand for skilled labor in the whole industrialized world, the way in which this demand was satisfied differed across countries. In Germany, the well-established apprenticeship system ensured that the majority of the workforce was trained at a competitive level, while in other countries almost all post-secondary education

was provided by higher education institutions. The heterogeneity in educational systems led to differing shares of university graduates across countries, impeding international comparability of the population's skill levels. This is illustrated in Figure 2. It also hindered the international recognition of qualification levels and limited the free movement of labor as one of the cornerstones of European integration. In order to fully exploit the academic and economic potential of the European higher education institutions, in 1998 Germany, France, Italy and the UK signed the Sorbonne declaration, in which they officially committed to striving towards structural compatibility and cooperation among European universities in order to promote mobility and international competitiveness. Only one year later, 30 countries signed the Bologna declaration which formally stated the goal of creating a common European Higher Education Area (EHEA) by 2010. Currently (2019), the EHEA comprises 48 countries.

In addition to the goals stated at supranational level, for some of the signatories the Bologna Process was associated with several accompanying reforms. Germany and Italy, in particular, wanted to use the new two-tier higher education system to increase the efficiency of their higher education systems. In Germany, besides high dropout rates the problem of relatively long study durations was heavily discussed throughout the 1990s (Destatis 1995). In 1999, the median German university graduate studied 12 semesters and was 28 years old when attaining the first university degree (Destatis 2003). Due to demographic change and baby boomer retirement these comparatively long educational periods were expected to cause shortages of skilled labor as well as problems for social security systems. Hence, replacing the comprehensive traditional degrees by two separate and relatively short study periods was supposed to significantly reduce the age at which university graduates enter the labor market.

From the individual's perspective the introduction of Bachelor degrees effectively offered school graduates a choice of an alternative educational level. Before the Bologna reform secondary school graduates could basically choose between a practically oriented three-year apprenticeship and a five-year program of scientific university education. The rather labor market focused Bachelor degree nowadays offers students a third way by getting some university education, but allowing them to enter the labor market after this short period of tertiary education, if they do not want to pursue one of the more research-oriented Master's programs.

According to human capital theory, the reduction of the regular study duration to six semesters reduces students' direct and indirect cost of obtaining a first university degree (Bachelor). Thus,
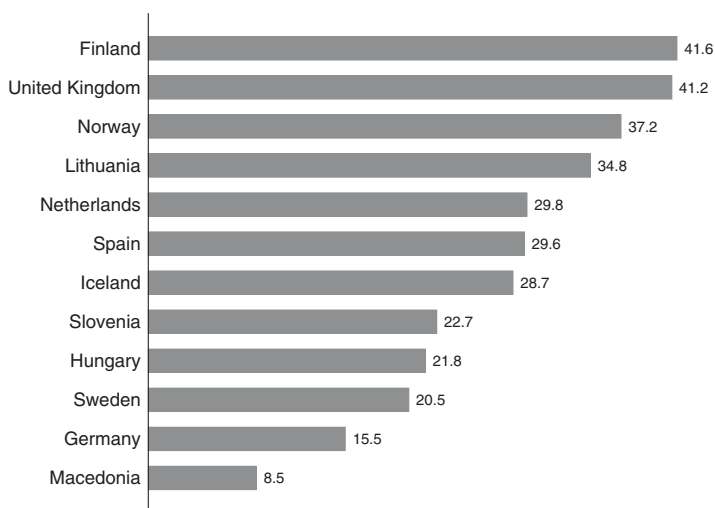

Figure 2. Tertiary education graduates (ISCED 5-6) aged 20–29 per 1000 of the corresponding age population, selected European countries in 1998.
Source: Eurostat.

ceteris paribus lower costs might for example reduce the pressure to work while studying and allow more students to focus on their academic obligations, which might have a positive effect on their educational outcomes. At the same time, the reform introduced a second type of university degree (Master), and the relative value of investing in either of the two relative to the old system (Diploma) is not clear a priori. Considering the potential channels and mechanisms of costs and return on investment therefore does not yield straightforward predictions of the impact of the Bologna Reform on students' educational outcomes (in particular study duration). We intend to answer this question empirically in the paper.

In the context of asymmetric information about employees quality, the new educational level also yields a more differentiated signal for graduates' quality (Bratti, Staffolani, and Broccolini 2006). Therefore Bachelor students might have a stronger incentive to graduate compared to Diploma students, because taking advantage of the Sheepskin effect is less costly for fewer years of education (Horstschräer and Sprietsma 2015).

Since some of the Bachelor graduates do not want to attain a Master's degree, from a macroeconomic perspective the distinction between two cycles in higher education can reduce the cost of education per university graduate. This could allow more students to get access to higher education and eventually lead to a larger share of university graduates in the population. Consequently, the Bologna Reform could improve Germany's position in international OECD comparisons on the skill level of the workforce. However, whether this desired effect can actually be realized by the reform crucially depends on students' individual reaction to the new institutional framework.

3. Related work and institutional setting

3.1. Previous evidence on the Bologna reform effects and contribution of the study

Given the scale and ambition of the Bologna reform, so far there are relatively few empirical economic studies on its effects on the higher education system, and especially student performance. In part this may be due to the relative recentness of the full implementation of the reform. To date, many studies have been produced that look at the broader political and administrative changes and implications of the Bologna process, both from an international point of view (e.g Heinze and Knill 2008; OECD 2011; Voegtle, Knill, and Dobbins 2011; Crosier and Parveva 2013) and from national perspectives (e.g. Suchanek et al. 2012 for Germany). Several authors focus on demand-side effects by analyzing enrollment rates, e.g. Cardoso et al. (2008) who find an increased demand for higher education in Portugal. A theoretical contribution by Mechtenberg and Strausz (2008) analyzes how student mobility induced by Bologna affects multi-cultural skills and quality of universities; Agasisti and Bolli (2013) test some implications of the model and – using data from Switzerland – find that the Bologna reform appears to have enhanced university productivity.

As student drop out played a particularly important role in Italy before the reform, most of the literature about the reform effects on the efficiency of tertiary education is based on Italian data (e.g. Boero, Laurenti, and Naylor 2005). This thematic focus is congruent with a long-standing line of educational research analyzing drop-out from higher education and its determinants (e.g. Bean 1980; Arulampalam, Naylor, and Smith 2004; Araque 2009). Specifically analyzing the Bologna reform, Cappellari and Lucifora (2009) use individual survey data to conduct a before-after-comparison for school graduates of 1998 and 2001 in Italy and find a significantly higher enrollment probability and a small negative impact on university drop out induced by the new degree structure. D'Hombres (2007) extends the concept of dropout by including inactive students in the definition. Her results indicate a decrease in drop-out / inactivity probability between 2.5 and 5.7 percent due to the reform. Di Pietro and Cutillo (2008) use Oaxaca-Blinder decomposition techniques to disentangle the effect due to students' behavioral change from the student composition effect of the reform. Their results suggest that even when controlling for changing characteristics of the student body there remains a negative effect of the reform on student drop-out.

Data on quantitative student performance indicators in Germany are mainly provided by the Federal Statistical Office (Destatis) and the German Centre for Higher Education Research and Science Studies (DZHW). While Destatis calculates aggregate graduation rates for student cohorts ten years after enrollment based on administrative cross-sectional data, DZHW administers its own student survey panel and combines it with Destatis data to compute dropout rates. A first analysis of student dropout in the context of the Bologna Reform for Germany was done by Horstschräer and Sprietsma (2015). Using Destatis' administrative student data they analyze enrollment rates and conduct short-term (one year after enrollment) drop out analyses using cohort size comparisons, and do not find any significant effects of the reform. However, the identification of causal effects at the aggregate level is complicated by the fact that students frequently switch universities, but that after switching their treatment status may not remain constant.[2]

Our paper contributes to the identification of student's actual behavioral response to the Bologna Reform efforts in several ways. First of all, considering a longer time horizon in our study, we are able to analyze a wider range of outcome variables (e.g. graduation and final grades) which allows us to take a look at a broader set of reform effects. Focussing on graduation yields relevant implications for the labor market not only for universities, but also for firms and policy makers, as their interest lies in who graduates, how fast (or slow) and what final grade is attained. These outcomes of tertiary education are important early predictors of later labor market outcomes (Heckman, Stixrud, and Urzua 2006; Borghans et al. 2016). Moreover, the fact that we exploit treatment variation at the individual level allows us not only to analyze different outcome variables, but also to control for individual level characteristics that might be relevant for academic success.

As we explain in detail in the next section, our analysis focuses on one specific university. While this might seem to limit external validity at first glance, it also has, in fact, several important advantages. First, given an overall rather loose and unspecified regulatory framework and timeline of the reform process, a homogenous institutional framework within the university allows to pin down the precise content and timing of treatment. Second, the possibility to observe longitudinally whether a student who started a program is still part of the cohort in subsequent years, allows for an actual identification of effects at the micro level. Moreover, we do not only observe a sample but the universe of students at HU.

Third, Humboldt-Universität is representative – in the context of this analysis – for a broader set of universities in Germany, namely those located in urban centers, offering a wide range of subjects, and embedded into many international and, in particular, intra-European cooperations. Fourth and finally, from a decidedly overall European perspective, the 'Bologna treatment' can be seen as a representative intervention affecting all universities, and from this perspective also students across Europe constitute a 'homogeneous' treated population. Whereas we cannot claim that our effect sizes will be exactly the same in other contexts, a precise estimation of reform effects at the university level will be informative for a broad set of universities in Germany, and across the continent.

3.2. Higher education reform at Humboldt-Universität

HU is a public university with a long-lasting tradition. It was founded in 1810, became popular as Friedrich-Wilhelm-Universität and was renamed 'Humboldt-Universität' in 1949. After German reunification in 1990, the university – which is located in the eastern part of Berlin - underwent an extensive transformation process to adapt to the federal republic's higher education system. This implied that most of the academic staff were replaced, and all degree programs had to be redesigned. While this process took up the first part of the 1990s, right after the transformation was accomplished a federal higher education reform was initiated. It intended to give students more guidance in order to increase their graduation probability and to reduce their study duration. Important aspects of the reform efforts were: Establishment of a Dean of Studies und student advisory centers at each department, introduction of the ECTS (European Credit Transfer System), modularization of study contents, and pre-structured study plans.

In the context of international cooperation some departments at HU even worked on the introduction of Bachelor and Master's degrees before the Bologna Declaration was signed. Agricultural and horticultural science participated in the SOKRATES program and were the first subjects to implement the new degree structure at HU in 2000. Interestingly, during this experimental phase, it was proclaimed that the new and the old degree structure would be going to coexist. Hence, the Bachelor was perceived as an exit option for students who were not interested in a scientific career and did not want to pass a comprehensive Magister or Diploma degree. Most professors did not want to turn back from their traditional and internationally acknowledged degrees. In order to guarantee the permeability between the old and new degrees, ECTS and modules were introduced in the old degree programs as well. In some cases, a smooth transition between Bachelor and Diploma was achieved by using a '4+2' model for the Bachelor curriculum, where the first four semesters consisted of the same content as the former *Grundstudium* (introductory study period) and the two last semesters allowed for a specialization equivalent to approximately one half of the former *Hauptstudium* (advanced study period).

The reform process at HU was significantly accelerated by the decision to award BA/MA-degrees in teachers' education, since almost all other subjects (mathematics, physics, geography etc.) needed to – at least partly – adopt the new system in order to contribute to the teacher-training curriculum. However, some departments were still reluctant to implement the Bachelor as the main degree. Especially from the perspective of some humanities it was perceived as quite challenging to modularize the whole study content. Several external factors also hindered the implementation of a new degree structure: in some fields of study there did not yet exist a labor market for Bachelor graduates (e.g. chemistry) or federal legislation did not allow Bachelor graduates to carry on their profession (e.g. psychology). The natural sciences, in particular, did not want to sacrifice their comprehensive and well accepted Diploma degrees. Some departments probably deferred the transition because they anticipated that eventually it would be impossible to maintain a parallel structure of new and old degree programs in the face of financial constraints and ongoing budget cutbacks (e.g. in 2004 and 2009). In fact, in 2003 it was decided to no longer maintain the old degrees in subjects where new degrees were already implemented. Also, even departments that still held on to their old degrees had to modularize their study content. Thus, mathematics, computer science, psychology and geography introduced modules and modular examinations in their Diploma degrees in the winter term 2003/ 04. At that time biology, chemistry and physics were still working on the modularization of their degree programs. Biology and physics eventually switched to the new degree system in 2005.

In addition to the key changes at the departmental level, in the winter term 2005/06 a new guideline for application and selection procedures at HU was passed. It included quotas for international applicants, for applicants without a university entrance certificate and for cases of hardship. These new guidelines were valid for all programs awarding a first university degree (Bachelor and Diploma programs). Master programs had separate rules for program access, e.g. with respect to the first degree that was required for a consecutive program. By 2009 the main part of the Bologna Reform was accomplished and all HU departments (except for the faculty of law) had introduced Bachelor and Master's degrees.

4. Data and descriptive analysis

The data contain anonymized information about HU students from the beginning of the 1990s to March 2015. Given the transition process of East German universities in the first years after German reunification (see previous section), we use data from winter term 1997 onward. We exclude the very recent student intakes from 2012 onward, since their regular instructional time would not fit in the observation period. We observe a total of 15 student cohorts (intakes 1997 to 2011) and include all degree programs that exist in both a pre Bologna variant (Diploma) and a post Bologna variant (Bachelor).[3] The final data contain observations for 24,675 students in 15 different subjects awarding single honors Diploma and Bachelor degrees.
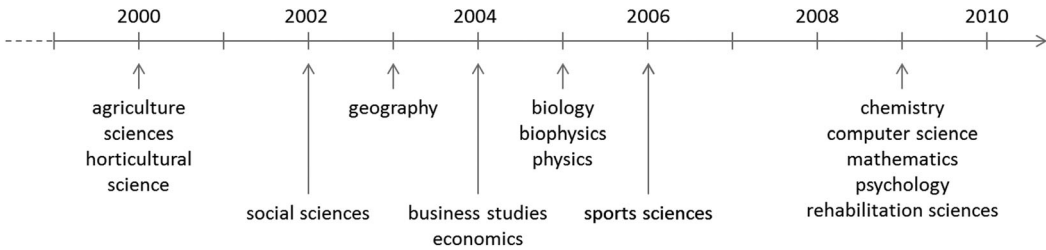
Figure 3. Coming-into-effect of the Bologna reform treatment at HU departments.

Source: Authors' illustration based on HU student statistics.

Figure 3 depicts the time line of the implementation of the Bologna Reform at the subject level. Analogous to the process in the whole of Germany (recall Figure 1), putting into practice the Bologna reform at HU took almost a decade. Figure 4 presents the distribution of the resulting absolute numbers of first-year Diploma and Bachelor students over time. The figure shows a – transitory – reduction in the overall number of first-year students during the second half of the 2000s.[4]

4.1. Descriptive statistics

The data comprise students' individual characteristics, information on programs studied at HU, study duration, graduation status, and final grades. Given legal regulations of this type of administrative data in Germany, no additional information on family characteristics such as parents' educational or financial background can be contained in the data. Whereas this limits our set of potential control variables, the data do contain key background information at the individual student level. Note also that our data encompass the population of HU students. Table 1 provides the list of variables and their means for Diploma ($N = 15,408$) and Bachelor ($N = 9,267$) students. As most variables are indicator variables, averages correspond to the share of Diploma or Bachelor students belonging to the respective group.

The first panel describes student characteristics at the time of enrollment. Notably, the mean enrollment age and the time span between secondary school graduation and university entrance are almost identical for both groups. As mentioned above, cohort sizes in Bachelor programs were
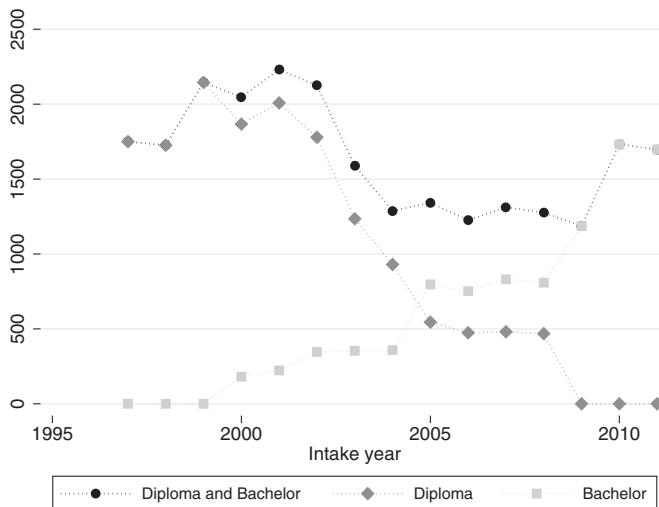

Figure 4. Number of first-year students at HU by degree, 1997–2011.

Source: Authors' illustration based on HU student statistics.

Table 1. Summary statistics.

Variable	Diploma	Bachelor
Student characteristics		
Intake age	23.03	22.94
Months since high school graduation	35.83	33.02
Size of intake cohort	175.9	126.4
Female (percent)	47.3	50
First-time enroller (percent)	70.2	72.4
Geographical origin of pre-university education (percent)		
Bavaria	2.3	3.1
Brandenburg	12.5	13.4
Berlin	48.9	43.4
Bremen	0.6	0.6
Baden Wurttemberg	3.8	4.3
Hamburg	0.9	1.2
Hessen	2.3	2.6
Mecklenburg-West Pomerania	3.1	3.2
Lower Saxony	3.4	4.0
North Rhine Westphalia	4.8	5.7
Rhineland Palatinate	0.9	1.1
Saarland	0.2	0.2
Schleswig-Holstein	1.1	1.6
Saxony	2.3	2.1
Saxony Anhalt	2.3	2.2
Thuringia	1.4	1.5
Foreign university entrance certificate	5.9	9.3
Unknown	3.5	0.8
Student outcomes		
Duration of studies (semesters)	12.78	7.24
Duration of studies (index)	1.49	1.27
Program graduate (percent)	32.7	33.9
Graduation within planned instructional time (percent)	2.17	15.8
Still enrolled (percent)	3.28	14.6
Final grade	1.91	2.19
Number of students in sample	15,408	9,267
Number of graduates in sample	5,045	3,141

Table entries are sample averages.

smaller compared to Diploma programs, so the mean values for cohort sizes differ significantly. Slight differences in the share of female students in the sample arise from the fact that we observe more Diploma than Bachelor students – particularly for those subjects that are more popular among male students (e.g. math, computer science and chemistry). The share of students starting their academic career at HU ('first-time enrollers') remained relatively unaltered by the reform, so there is no indication that there might be significantly more experienced first-year students in one or the other group.[5]

The variables in the second panel of Table 1 describe the geographical origin of students' higher education entrance certificates. For almost all of the federal states except for Berlin, we find essentially identical shares in the Bachelor and Diploma subsample. At the same time, the share of students with foreign university entrance certificate among Diploma students was about 5.9 percent, while among Bachelor students 9.3 percent received secondary schooling abroad. This may point to an increased international mobility in higher education which was intended by the Bologna Reform. The further decrease of the (small) share of unknown geographical origin results from improved HU student statistics.

The last panel in Table 1 presents mean values for various student outcome variables. First, Bachelor graduates have been substantially faster than their Diploma counterparts: looking at the average duration of studies reveals that Diploma graduates took 12.8 semesters, while Bachelor graduates required 7.2 semesters to finish their program.[6] This illustrates that the reform was successfully implemented in line with its objective to reduce study duration; the significant reduction also reflects the generally shorter curricula of the Bachelor versus the Diploma programs, since post-

Bologna some contents of the curricula of Diploma programs would be shifted to the Master programs.

The data allow us to calculate an even more precise measure of duration based on exact enrollment and final examination dates. By computing the difference between the two dates (in days) and dividing by the regular instructional time of the particular program, we obtain a standardized study duration of Diploma and Bachelor students in continuous time. If the index takes on the value 1 for a given person, this indicates that the actual time spent studying equals the regular instructional time determined by the program. The index thus measures the ratio between the actual educational lifetime dedicated to graduating from a given program relative to the planned time this is supposed to take according to program regulations.[7] Table 1 shows that, according to this index, both the pre-Bologna and post-Bologna groups on average take longer than the planned instructional time to graduate, with index values of 1.49 and 1.27, respectively. That is, while Bachelor students extend the planned time-to-graduation by an average of about 25 per cent, Diploma students do so by almost 50 per cent, with a difference in average prolongation between the two groups of 22 percentage points.

In addition to looking at mean differences in study duration, Figure 5 presents the frequency distribution of time-to-graduation in semesters for Bachelor and Diploma students. The figure shows a strong clustering of Bachelor graduates at a duration of six semesters, while for Diploma students the distribution peaks at eleven semesters. A relatively large fraction of Diploma students graduates within ten semesters, but a very small fraction finishes within nine semesters, which is the planned instructional time for most of the Diploma students.[8] Planned instructional time generally amounts to six semesters for Bachelor students. Figure 6 plots the frequency distribution for the standardized duration index and shows that the fraction of students finishing in or even before instructional time is larger for Bachelor students, while the fraction of students taking longer is always higher for Diploma students. Taken together, the findings from these figures indicate that the time needed for graduation has decreased significantly for post-Bologna students – both in absolute and relative terms.

Besides the study duration, student outcomes in Table 1 (bottom panel) show that the share of graduates comprises about a third of students in the data for both Diploma and Bachelor programs (32.7 and 33.9 per cent, respectively).[9] When combining this information with planned study duration, almost half of the Bachelor graduates (16 percent of all post-Bologna students in the data) studied for a maximum of six semesters, but among Diploma graduates only about every fifteenth
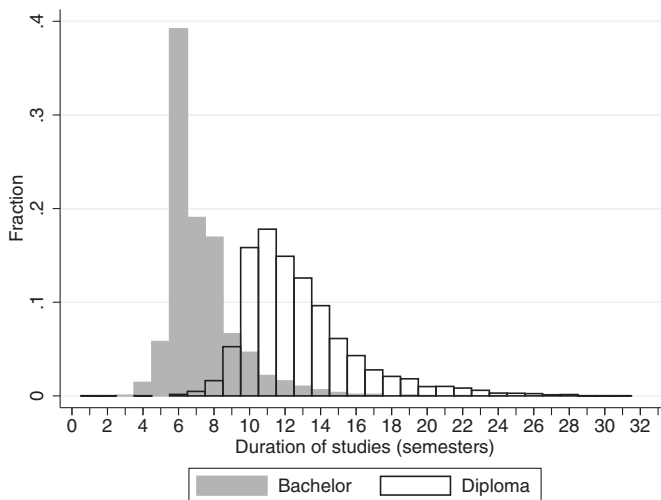

Figure 5. Absolute duration of studies for program graduates in semesters (discrete values).
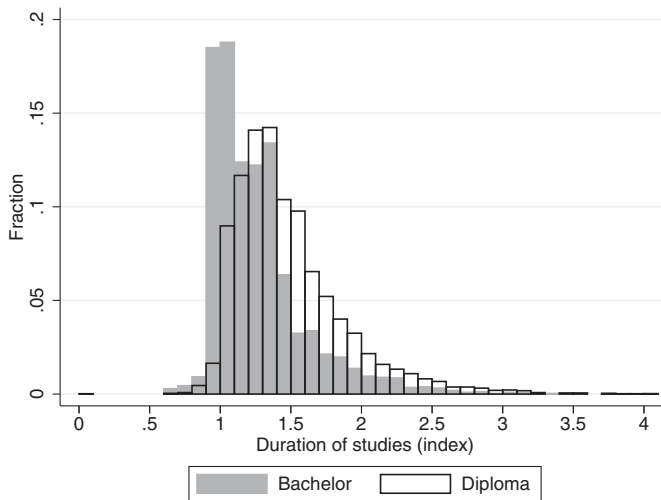
Figure 6. Standardized index: duration of studies for program graduates relative to regular instructional time (continuous values).

student managed to graduate within regular instructional time (2.2 percent of all pre-Bologna students). Whereas almost 15 percent of the Bachelor students in our data are still enrolled at the end of our observation period (i.e. their student status is still active, they did not drop out or graduate), this is the case for only 3.3 percent of Diploma students. Bachelor students' final grades – given by a scale ranging from 1.0 (best) to 4.0 (minimum non-failing grade) – turn out to be worse on average compared to Diploma students' grades by 0.3 grade points (1.9 vs. 2.2).

4.2. Choice of outcome variables

The interest of our study lies in analyzing whether the reform affected key student outcomes, which can also serve as early predictors of later labor market outcomes. Evidently, we are aware that Diploma and Bachelor degrees differ in several respects (highlighted throughout the previous sections). From a policy perspective, however, it is of key relevance to compare the two pathways to the first labor market qualifying tertiary degree in the old and new system. Moreover, we make the two degrees comparable in the choice and transformation of our outcome variables, as outlined in this section, and we control for selection into the programs (see the methodological discussion below). Finally, the alternative comparison between Diploma and Master students would neglect selectivity issues caused by the additional application process between Bachelor and Master level education.

The recentness of the reform imposes some challenges for the definition of adequate outcome variables to address our research question. Ideally, we would like to compare graduation rates and overall time required for graduation, but the sequential nature of the setup imposes different censoring for Diploma and Bachelor students. As Diploma students by design remain much longer in the sample than Bachelor students, we observe a larger share of Bachelor students that is still enrolled (recall Table 1). This might spur the comparison of overall graduation rates as well as graduates' study durations. Moreover, the difference in the planned duration of both types of programs renders a simple comparison of absolute study duration useless. In order to overcome this issue, we focus on the probability to graduate within regular instructional time as our key outcome variable. As we observe all students at least for the duration of their regular instructional time, this is the appropriately comparable measure tackling both of the aforementioned problems. Finally, by also analyzing the standardized duration required for graduation as a homogenized measure across Bachelor and Diploma degrees, we gain further insights about the effects for the whole student distribution, since the measure 'graduation within time' may cover mainly the faster students.

Given our interest in reform effects on the supply of skilled labor, we do not explicitly analyze the effects of Bologna on student enrollment. The limited explanatory power of student enrollment with respect to demand for higher education is due to the fact that the determination of study capacities at public universities is the result of a political bargaining process between universities and local governments. At leading German universities like HU, demand for higher education (which is basically free of charge for students) usually exceeds the supply of study capacities, so access is typically restricted by a numerus clausus. Relaxing the capacity constraint in case of excess demand thus automatically leads to a higher number of students – independent of the program's attractiveness. Consequently, enrollment as a demand indicator merely captures the market clearing result of supply and demand for higher education. Therefore, potential differences in student enrollment cannot exclusively be attributed to the Bachelor or Diploma systems, but rather reflect the political will to increase capacities in the higher education sector.[10] Lastly, enrollment measures the decision to start university education, but not the eventual outcomes of this decision which are in fact more relevant for the supply of skilled labor, and hence for the key interest of our study.

4.3. Analysis of outcome dynamics

Before assessing the quantitative effects of the Bologna Reform on outcome variables, it is interesting to investigate how the outcome variables for the treated population evolve over time, and whether there is some movement in these variables around the discontinuity, i.e. the point(s) in time of introduction of the reform.

Let Y denote the outcome variable in general. The introduction of the new degree structure in the different subjects occurred at one of seven points in time, i.e. in the years 2000, 2002, 2003, 2004, 2005, 2006 or 2009 (recall Figure 3). Hence, for every subject we observe at least three cohorts before treatment and three cohorts succeeding treatment, depending on the precise year when the reform was introduced. Consequently, we observe a larger number of cohorts with Diploma students for a subject that introduced the Bachelor later, and vice versa.

This information lets us investigate the dynamics in outcome variables at the discontinuity in the following way: Let t denote the number of cohorts since treatment – i.e. the Bologna Reform being put into practice at the departmental level – occurred. If t is negative, the treatment occurs $-t$ cohorts later. Treatment is standardized to occur at $t = 0$ for all subjects – independent of the specific year of Bachelor introduction in real time. If there are k data cohorts, then there are observations for $t \in [-(k-3), (k-4)]$. In order to indicate whether an individual is observed in t we define a set of $2(k-1)$ dummy variables D_{id}^t, where id is a subscript denoting that student i is enrolled in degree program d with $d \in \{\text{Diploma}, \text{Bachelor}\}$. The average value of the outcome variable at point in time t is

$$\hat{Y}_d^t = \frac{1}{N_d^t} \sum_i (D_{id}^t Y_i^t), \tag{1}$$

where N_d^t is the number of students for which $D_{id}^t = 1$. Using $d \in \{\text{Diploma}, \text{Bachelor}\}$ in this equation is necessary, since for three subjects (economics, business, geography) at time $t = 0$ both a Diploma and a Bachelor cohort exist.

Note that the closer we move towards $t = 0$ from both sides, the more precise the computations become, because the number of students as well as the subject set contributing to the average outcomes increases. Specifically, for $t \in [-3, 2]$ we observe the population of students from all subjects of study. In order to not give too much weight to single fields of study, we calculate Equation (1) only for cohorts comprising students from at least three subjects.

Figures 7 through 9 present the corresponding results, depicting the outcome dynamics around treatment introduction for three outcomes: (a) the probability to graduate within planned instructional time (Figure 7), (b) the standardized duration index that measures the ratio between actual
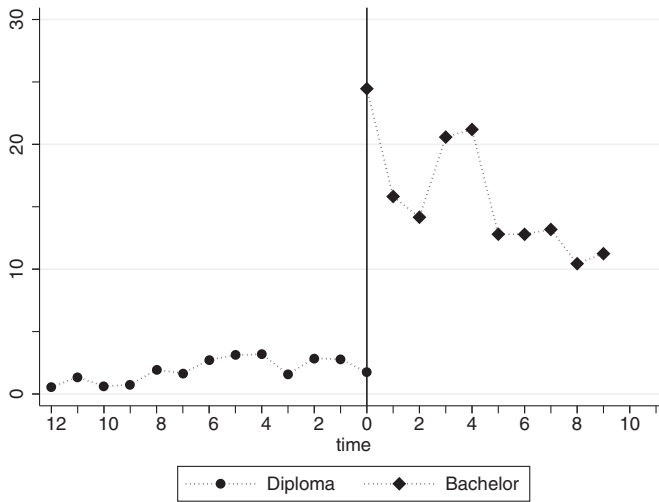
Figure 7. Outcome dynamics around treatment introduction: Probability to graduate within regular instructional time (in per cent).
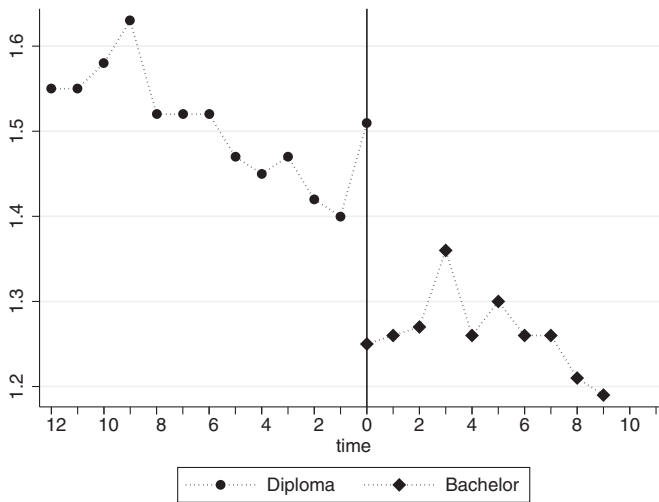

Figure 8. Outcome dynamics around treatment introduction: Duration index.

educational lifetime dedicated to graduating relative to the planned instructional time according to regulations (Figure 8), and (c) the final overall grade at graduation (Figure 9).

All three figures indicate a visible discontinuity in the outcome variables occurring with treatment introduction, along with persistent patterns before and after time $t = 0$. In fact, both the large increase in the probability to graduate within planned time (Figure 7) and the drop in the standardized duration index (Figure 8) are very pronounced: the average probability to graduate within planned time rises from less than 5 per cent to approximately 15 per cent, and the average duration index decreases from about 1.5 to about 1.25, indicating that post-reform planned instructional time is on average exceeded by only about a quarter (as opposed to about one half pre-reform). Moreover, the two figures show that the differences induced at time $t = 0$ are persistent during the observed time periods of up to 12 years before and up to 9 years after treatment introduction.
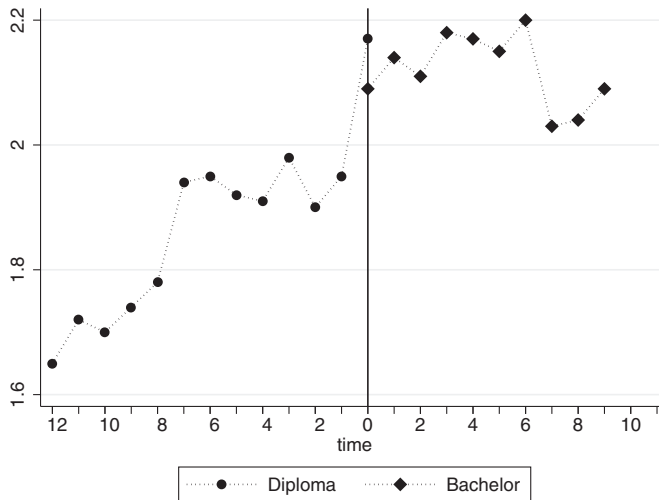
Figure 9. Outcome dynamics around treatment introduction: Final grade.
Note: In the German grading system grades range from 1.0 (best) to 4.0 (minimum non-failing grade), a 5.0 is 'fail'.

Regarding the third outcome, final overall grade at graduation, Figure 9 shows that the discontinuity at time $t = 0$ is less pronounced than for the other two outcomes; in particular, no immediate strong increase or decrease can be seen. Looking at the full time windows before and after treatment introduction, however, average final grades appear visibly lower – i.e. better – during the pre-Bologna period than during the post-Bologna period. The dynamic patterns in Figure 9 indicate some increase from an average of approximately 1.7 to approximately 1.9 moving towards the Bologna reform introduction, and an overall average final grade of about 2.1 during the time period afterwards. This would imply a noticeable deterioration in overall final grades after treatment introduction (recall that grades range from 1.0 (best) to 4.0 (minimum non-failing grade)).

Statistical tests on the pronounced pre-post mean differences in Figures 7 to 9 show that for each of the three outcomes the difference is statistically significant. At the same time, when we conduct these graphical analyses for the covariates (e.g. gender, age, time since graduation from secondary school; omitted here for brevity but available upon request) the profiles are essentially flat.

5. Identification and empirical analysis

5.1. The Bologna reform as a natural experiment: OLS impact estimates

We are interested in estimating the causal effect of the Bologna reform on students' educational outcomes. In the first step of our empirical strategy we estimate the following equation

$$Y_i = \alpha + \beta Bologna_i + \gamma X_i + \delta_g + \theta_c + \tau_s + \epsilon_i. \tag{2}$$

Y_i denotes the outcome of interest for student i. Specifically, we consider the probability of graduating within planned instructional time, the final overall grade, and the study duration index as outcomes (recall previous section). α is a constant, X_i a set of covariates, and β is the average treatment effect of implementing the Bologna reform, i.e. the parameter of interest. The parameter δ_g denotes a state effect (geographic origin), θ_c is an intake effect (intake cohort), τ_s a subject effect (field of study), and ϵ_i the error term.[11]

The identification strategy underlying this estimating equation is based on the idea that German universities (and also HU) effectively faced a continuum of university entrants during the relevant time period from 1997 to 2011.[12] This continuum is constant in composition at different points in time, and was thus affected by the Bologna reform as an exogenous shock assigning students

quasi-randomly to a treatment group (post-Bologna, i.e. Bachelor students) and a control group (pre-Bologna, i.e. Diploma students). The key identifying assumptions to make β an unbiased estimate of the Bologna reform impact are that (i) knowledge of the reform does not selectively affect young adults' choice to go to university or not (*permanence of student intake over time*), (ii) knowledge of the reform does not selectively affect the choice of university (*permanence of geographical distribution of student intake over time*), and (iii) knowledge of the reform and its time-varying implementation across university departments does not selectively affect the choice of field of study (*permanence of subject choice*).

There is evidence that makes these assumptions plausible. Regarding assumption (i), Figure A1 in the appendix displays the distributions of university entrance certification grades (i.e. high-school grades) for several subjects of study and several years before and after the reform. Similarly, Figure A2 shows the respective grade distributions for geography for the years 2005 and 2006, years in which the department admitted students to both types of degree programs. Both figures show that there are no changes in the grade distributions that would indicate any (self-)selection of more or less skilled students either before or after Bologna. Moreover, representative survey data collected by the federal student union (Studentenwerk 1997–2012) indicate that there is no change in the educational composition of students' family background comparing the years 2000, 2003, 2006, 2009, and 2012. Regarding assumption (ii), the summary statistics in Table 1 show that the distribution of geographic origin varies very little between the treatment (Bachelor) and control group (Diploma) samples, with the exception of the intake of international students and students from Berlin. Regarding assumption (iii), Figures A1 and A2 also provide some tentative support, and in general it seems unlikely that students would have been able to make an informed strategic choice of subject, given the large variation in implementation time points across time, universities, and even across departments within universities. Moreover, empirical studies for Germany have shown that the proximity to their home place is one of the most important determinants of students' university choice (see e.g. Krawietz and Heine 2007; Spieß and Wrohlich 2010). Taking this evidence together, it therefore seems plausible that the implementation time points - and thus the assignment to treatment and control group – can indeed be considered exogenous to students' decisions.

In addition to plausibilizing the key identifying assumptions, a few potential threats to identification are important to be discussed in the given context. First, are there any simultaneous reforms that might affect students' outcomes and/or bias our impact estimates? Such reforms could be e.g. the secondary school reform reducing the mandatory number of years to qualify for university education from 9 to 8 years. Implementation of this so-called G8-Reform varied by federal state; within our sampling frame, only secondary school graduates from six of the 16 federal states were affected by the reform, and only during the most recent years (2008 onward). Since we control for intake age and cohort effects our estimates are unlikely to be affected by the small share of G8-graduates in our sample. We empirically test this in the robustness section. A second reform potentially affecting student intake cohorts is the discontinuation of compulsory military service in 2011. This, however, likely causes a time-shift in student intake only, but does not affect composition. Again we empirically test this by estimating impacts for women and men separately, since only men are affected by compulsory military service in Germany.

Finally, in order to make the treatment-control comparison of Bachelor vs. Diploma valid, consistency of the contents of the subjects of study is required; this requirement is satisfied, however, since despite a general and inevitable shortening of the contents, the core subjects remained unchanged (see Section 3.2). While some departments at HU took the opportunity to restructure their programs using new course titles while keeping contents constant (e.g. computer science), other departments retained identical courses (e.g. economics). Moreover, evidently, the basic skills required to become a mathematician, social scientist, etc., that are conveyed during the first years at university, did not change over time.

Table 2 reports OLS estimates of the Bologna effect for the three outcome variables. Standard errors are clustered multi-way at the subject and intake cohort level (Cameron, Gelbach, and Miller

Table 2. Estimated effect of the Bologna Reform on student outcomes (OLS).

	(1) In-time graduation	(2) In-time graduation	(3) Duration index	(4) Duration index	(5) Final grade	(6) Final grade
Bologna	0.136***	0.180***	−0.215***	−0.0901***	0.234***	0.230***
	(0.0277)	(0.0276)	(0.0545)	(0.0262)	(0.0784)	(0.0868)
Female		0.000118		−0.0263**		0.0107
		(0.00466)		(0.0116)		(0.00952)
Intake age		−0.00608***		0.0107***		0.0336***
		(0.00160)		(0.00184)		(0.00243)
Foreign university		−0.0233*		0.0676*		0.122***
entrance certificate		(0.0127)		(0.0346)		(0.0327)
Months since high		0.0000686		−0.000415**		−0.00136***
school graduation		(0.0000663)		(0.000166)		(0.000260)
Standardised intake		0.000205		−0.00689		0.00466
cohort size		(0.00176)		(0.00421)		(0.0120)
Constant	0.0217***	0.158***	1.485***	1.342***	1.902***	1.657***
	(0.00417)	(0.0347)	(0.0451)	(0.0532)	(0.0943)	(0.0822)
State effects	No	Yes	No	Yes	No	Yes
Intake effects	No	Yes	No	Yes	No	Yes
Subject effects	No	Yes	No	Yes	No	Yes
Observations	24675	24675	8186	8186	8163	8163
Adjusted R^2	0.064	0.114	0.071	0.242	0.044	0.262

Standard errors in parentheses are clustered at the subject and intake cohort level.

*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

The columns display OLS treatment effect estimates for the following three dependent variables: graduation within planned instructional time; study duration index; final grade at graduation. 'State effects' are indicator variables for a high school certificate from the 16 federal states. 'Intake effects' are indicator variables for the intake cohort. 'Subject effects' are indicator variables for the field of study.

2011). For each outcome, the table reports the raw effect first (columns 1, 3, and 5), and subsequently the full specification with covariates, state effects for geographic origin, intake cohort effects, and subject of study effects (columns 2, 4, and 6, respectively). The estimation results in Table 2 indicate statistically significant effects of the Bologna reform on the outcomes considered: specifically, first, we estimate an average treatment effect on the treated on the probability to graduate within planned instructional time of 18 percentage points for the full specification (column 2), somewhat larger in size than the raw estimate (14 percentage points, column 1).

Secondly, regarding the treatment effect on study duration, the full specification (column 4) indicates a decrease in the standardized duration index of −0.09, statistically significant and smaller in size than the raw effect of −0.22 (column 3). That is, the typical average exceedance of the planned instructional time by students is reduced by nine percentage points. Whereas the results for these first two outcomes indicate qualitatively positive effects of the Bologna reform, the effects on overall final grades are qualitatively negative, as columns 5 and 6 in Table 2 show: the point estimate of the average increase (i.e. worsening) in grades is 0.23 in both specifications. This implies a noticeable upward shift in the average final grade of HU graduates.

5.2. Instrumental variables estimates of the Bologna effect

The identification strategy in the previous section essentially considers treatment status as exogenous, because the status of being in a Diploma or Bachelor degree program is implicitly (and randomly) determined by the preceding individual choice about which subject and at which university to study. We argued that this set-up generates a natural experiment that allows to estimate unbiased estimates using simple linear regression. However, it might be argued that there are strong utility losses at the individual student level associated with the switch from a Diploma to a Bachelor's degree – e.g. due to long academic traditions at specific universities – and that these losses outweigh any city or university benefits, lower travel costs, etc. That is, consider a case in which some students do make a deliberate effort to 'avoid treatment', which might render our treatment indicator in Equation (2)

endogenous. The potential success of this effort then depends on the number of available alternatives. Specifically, for instance, if all other universities except HU still offer Diploma degrees, avoidance of treatment is easy, and vice versa.

Following this logic, in a second step of the empirical analysis we instrument students' treatment status with the share of first-year Bachelor students among all first-year Bachelor and Diploma students for each subject of study and year. This instrumental variable is a direct measure of treatment probability, varying at the subject and intake group (cohort) level. It is arguably exogenous, since we would not expect this share to have any direct impact on students' educational outcomes other than through the (potentially) endogenous regressor, i.e. being in a Bachelor or Diploma degree program. Figure 10 illustrates the instrument by displaying the timeline of the coming-into-effect of the Bologna reform in Germany by subject – as the share of first-year Bachelor students among all first-year students – and the specific time point at HU.[13] Correspondingly, Table 3 displays the first-stage results and shows that the instrumental variable is highly and significantly correlated with treatment status.

Following the first step of the analysis, we now estimate the equation

$$Y_i = \alpha + \beta \, Bologna(Share)_i + \gamma X_i + \delta_g + \theta_c + \tau_s + \epsilon_i \tag{3}$$

by 2SLS, where the (potentially) endogenous treatment variable Bologna is instrumented using the exogenous variable Share (of first-year Bachelor students among all first-year students). Using the same three outcome variables of interest as above, Table 4 reports the corresponding IV estimates of the Bologna treatment effect.

The results are very similar to the OLS estimates reported in the previous section for the two outcomes 'probability to graduate within planned instructional time' (columns 1 and 2) and final grade (columns 5 and 6). The IV treatment effect coefficient for the in-time graduation is smaller in size than its OLS counterpart, and still indicates a statistically significant increase of 9.7 percentage points. The coefficient for the IV treatment effect on final grades is slightly larger than the OLS estimate (0.3 grade points vs. 0.23 grade points), and is also statistically significant. Again, these results indicate two qualitative directions of Bologna reform effects on student outcomes: the probability to graduate in-time improved, while overall final grades got worse.

Looking at the index of study duration as an outcome (columns 3 and 4 in Table 4), the significant OLS treatment effect coefficient is no longer statistically different from zero in the IV specification. That is, once the potential endogeneity of the Bologna regressor is accounted for, the preferred full specification with state, intake, and subject effects no longer suggests that the reform strongly reduced standardized study duration.

5.3. Subsample results and robustness

In this section, we stratify our sample in several dimensions, and investigate whether specific subgroup results might differ from the findings identified in the previous sections. Specifically, first, we investigate whether treatment effects are different for younger vs. older students (sample cut at the median intake age). Second, we stratify by gender. Third, we include 'switchers', i.e. students who started in the specific subject of study at a different university, then switch to and continue at HU (these were not included in our main estimation sample for reasons explicated in Section 4). Fourth, we condition on the subsample of students with complete information on their university entrance certificate grade. Fifth, we condition on 'local' students (i.e. students originally from Berlin and the surrounding federal state, Brandenburg). These students constitute the largest group in our sample and, given a universal pattern of the time-persistent and inherent inertia of choosing to study at a university where one lives and has grown up, are more likely subject to the exogeneity of the treatment introduction. Sixth and finally, we exclude students from the sample who were
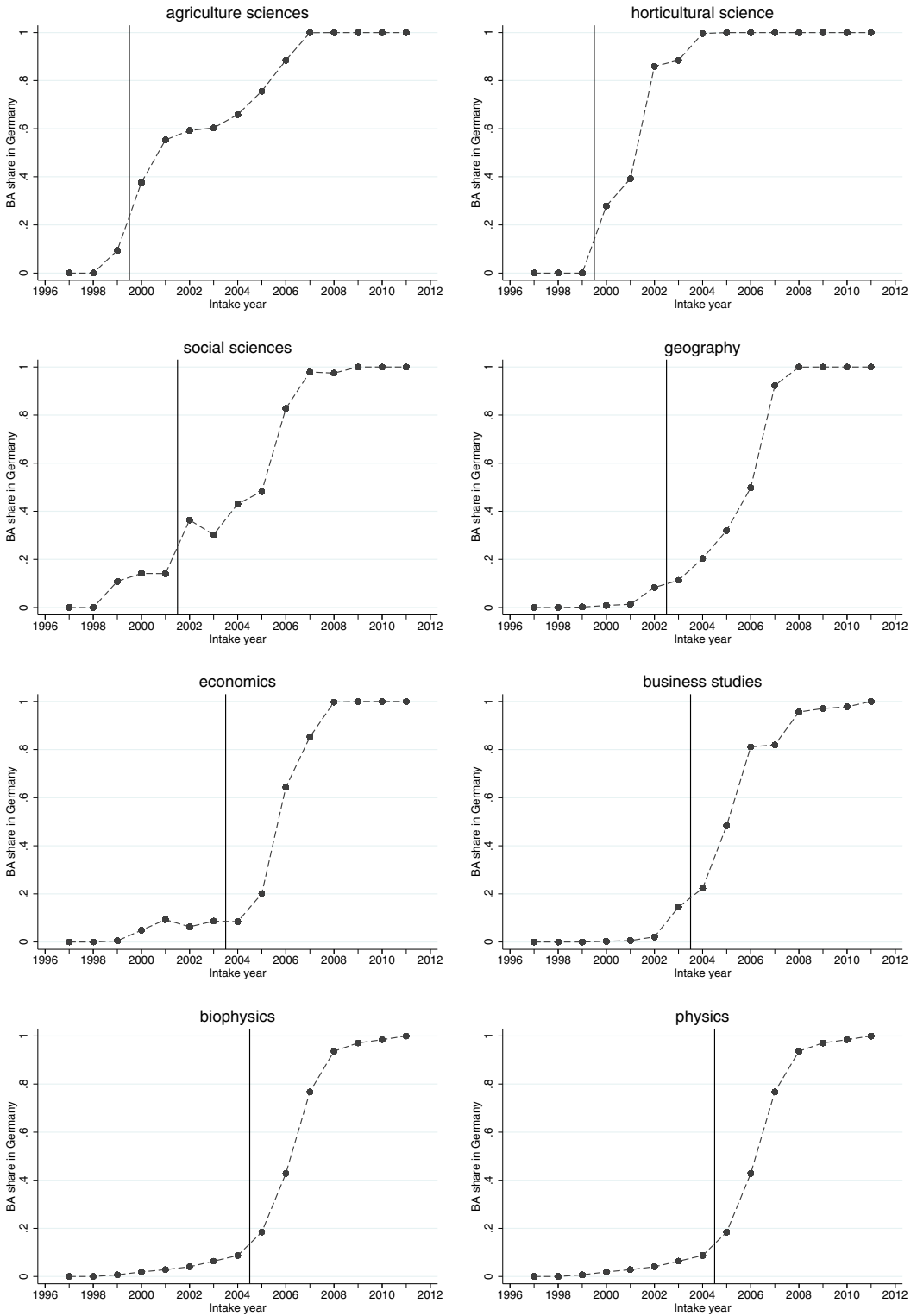
Figure 10. Timeline of treatment introduction in Germany and at HU, by subject.

The graphs display the respective annual share of first-year Bachelor students among all first-year students (Diploma and Bachelor) in Germany. The vertical lines indicate the respective coming-into-effect of Bologna at HU.
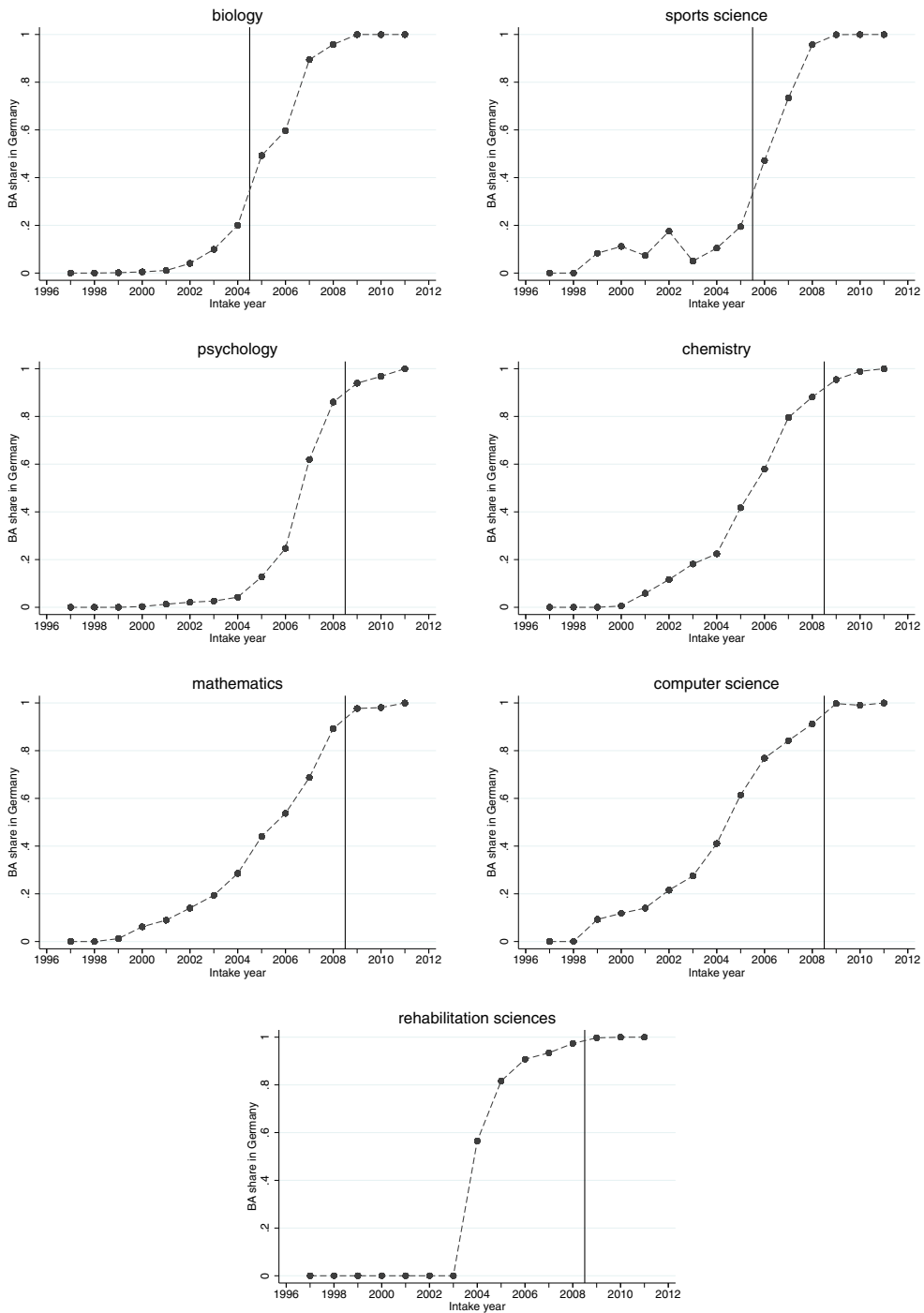
Figure 10. Continued

affected by the reduction in mandatory years of secondary education qualifying for university from 9 to 8.

In line with the previous analysis, we report 2SLS estimates using the full specification for the following outcomes: probability to graduate within planned instructional time (Table 5, first row), study

Table 3. First-stage regression.

	(1) Bologna	(2) Bologna
Share of Bachelor first-year students	0.973***	1.268***
	(0.0568)	(0.276)
Female		0.00542
		(0.00507)
Intake age		0.00101
		(0.000706)
Foreign university entrance certificate		0.0239**
		(0.0118)
Months since high school graduation		0.000107
		(0.0000967)
Standardised intake cohort size		0.00103
		(0.00886)
Constant	−0.0133	−0.0524
	(0.0239)	(0.0813)
State effects	No	Yes
Intake effects	No	Yes
Subject effects	No	Yes
Observations	24675	24675
Adjusted R^2	0.688	0.800

Standard errors in parentheses are clustered at the subject and intake cohort level.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Table 4. Instrumental variables estimates of effect of the Bologna Reform on student outcomes.

	(1) In-time graduation	(2) In-time graduation	(3) Duration index	(4) Duration index	(5) Final grade	(6) Final grade
Bologna	0.113***	0.0972**	−0.296***	0.0450	0.169**	0.301***
	(0.0280)	(0.0433)	(0.0603)	(0.0736)	(0.0766)	(0.102)
State effects	No	Yes	No	Yes	No	Yes
Intake effects	No	Yes	No	Yes	No	Yes
Subject effects	No	Yes	No	Yes	No	Yes
Observations	24675	24675	8186	8186	8163	8163

Standard errors in parentheses are clustered at the subject and intake cohort level.
*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$
The table reports coefficients from a 2SLS estimation, in which the endogenous regressor 'Bologna' is instrumented using the 'Share of Bachelor first-year students' (see text). See footnote for Table 2

duration index (Table 5, third row), and overall final grade (Table 5, fifth row). The results do not indicate pronounced patterns by age group or gender. Younger students seem to benefit slightly more in terms of the treatment effect on graduation probability within planned time. This is also the case for female students relative to males (for whom the point estimate on in-time graduation is not statistically significant), and also with respect to better final grades. The differences, however, are not very large. Including the 'switchers' in the sample (column 5), and focusing on the groups with local origin (column 7) or without the shortened secondary education regime (column 8) overall produces very similar results to the full sample, thus underscoring the robustness of the empirical findings. The subsample with entrance grade data available (i.e. recent entrants, column 6), is quite small and the point estimates correspondingly imprecise. Estimation results for the duration index outcome are not statistically significant throughout, reflecting the full sample results in Table 4.

5.4. Mechanisms

There are several channels through which we expect the Bologna reform to work, and through which it may have caused the effects estimated in this paper. First, the standard conjecture regarding Bologna is that the increased structurization implied by the reform incentivizes students to follow

Table 5. Robustness: IV impact estimates for stratified samples.

	(1) Age below	(2) Age above	(3) Female	(4) Male	(5) Switcher	(6) Entrance grade	(7) Local	(8) G8 excluded
Graduation in time	0.120*	0.0963***	0.131***	0.0541	0.0899**	0.615	0.0978**	0.0892**
	(0.0705)	(0.0165)	(0.0385)	(0.0638)	(0.0407)	(0.788)	(0.0463)	(0.0447)
Observations	12562	12113	11914	12761	29649	4446	14710	21515
Duration index	0.0165	0.0573	−0.0623	0.166	0.109	0.0688	0.0602	0.0443
	(0.0715)	(0.0757)	(0.0630)	(0.109)	(0.0869)	(0.236)	(0.0784)	(0.0793)
Observations	4194	3992	4227	3959	10852	1196	5128	7146
Final grade	0.323**	0.299***	0.280**	0.335***	0.338***	0.147	0.287***	0.320***
	(0.138)	(0.0924)	(0.115)	(0.0697)	(0.0852)	(0.753)	(0.106)	(0.0992)
Observations	4179	3984	4212	3984	10806	1196	5114	7127
State effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Intake effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subject effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses are clustered at the subject and intake cohort level.

$*p < 0.1$, $**p < 0.05$, $***p < 0.01$

The table reports coefficients from a 2SLS estimation, in which the endogenous regressor 'Bologna' is instrumented using the 'Share of Bachelor first-year students' (see text). The columns display treatment effect estimates for the following stratified samples: (1) Students being younger than the median student when enrolling in the program; (2) Students being older than the median student when enrolling in the program; (3) Female students; (4) Male students; (5) Those students are included who enrolled at a more advanced semester switching from another university; (6) Sample of students for which university entrance grades are available (only from 2005 onwards); (7) Students coming from the federal states of Berlin or Brandenburg; (8) Excluding students affected by the reduction of mandatory secondary education qualifying for university from 9 to 8 years.

their studies along a predetermined class schedule – sometimes by giving them a much smaller amount of liberty in choosing topics and classes than before (Suchanek et al. 2012). This could then potentially explain both the increase in the probability to graduate within planned instructional time and the decrease in standardized duration, as some kind of compliance effect (the latter one found in the OLS specification only). Although such a mechanism seems plausible, and may in fact play some role also in our case, there are no data to prove the relevance of this channel conclusively.

A second mechanism related to the idea of a compliance effect is that the new degree structure might generate a stronger connection between student and subject of study. A potential reason for such an increased cohesiveness could be the shorter duration of the program, which reduces the psychological costs associated with graduation. When being able to obtain a degree certificate after three years of higher education (instead of four to five years) students might have a stronger incentive to graduate. Moreover, Bachelor students are able to reconsider their academic interests by switching university for their Master's education, but do not have to drop out of a comprehensive Diploma program to do so.

We can test the hypothesis of increased cohesiveness by estimating the probability of non-retention for treatment and control groups for each semester since enrollment separately. 'Non-retention' here means to choose to not remain in the specific field of study in a given semester; it is not exactly a drop-out rate, since we cannot observe whether students actually stop studying or move, for instance, to a different university. Figures A3 and A4 in the appendix estimate the probability of non-retention during the first and the sixth semester, following the approach used in Section 4. Whereas the same graphs for semesters 2 through 5 (omitted from the appendix, but available on request) show a completely flat profile, the graphs for semesters 1 and 6 do indicate that non-retention rates decrease for the treatment group (corresponding OLS regressions yield a significant coefficient). This would indicate a twofold mechanism: On the one hand, the Bachelor degree program might make it more likely that students hang on to their subject choice during the first semester (*starting effect*); and on the other hand it might make it more likely that they hang on to their subject choice when they are close to finishing and attaining the Bachelor certification (*graduation effect*). This would be an indicator of a potential 'sheepskin effect' induced by the reform. Whereas we want to emphasize the conjectural nature of this conclusion, Figures A3 and A4 do provide tentative support.

Third, Bachelor students might face an increased performance pressure induced by a higher number of examinations and by the fact that all course grades contribute to the final grade at graduation, respectively. On the one hand, this might have a positive impact on students' learning efforts eventually decreasing their non-retention risk (see above). On the other hand it can explain the quantitatively worse final grades for Bachelor students. Whereas the final overall grade in the Bachelor degree is composed of all exam grades during the entire program, the final overall grade in the Diploma degree leaves out the grades of the introductory study phase (pre-Diploma) and has a somewhat stronger emphasis on courses of choice in the second phase. That is, grades from the obligatory classes in the early phase are not counted in the final grade, while grades from chosen classes during the more advanced stages are.

Figure A5 illustrates the potential quantitative impact of this mechanism on differences in final overall grades by comparing distributions of pre-Diploma and final Diploma grades for a subsample of 1,761 graduates[14]: the large observed difference between pre-Diploma grades and the final grade at graduation of 0.5 grade points on average exemplifies the hypothesis that introductory courses impose an upward pressure on students' final grades. Thus, the comparatively worse performance of Bachelor graduates found in our estimations has to be put in perspective, and instead of implying a negative treatment effect on competencies may represent a spurious impact reflecting changes in the way final grades are composed. Whereas this is spurious in terms of graduates' skills, it is of course real in terms of the final grade written on the Bachelor certificate.

Fourth, the new two-tier system introduces additional uncertainty in a student's academic career because having been successfully admitted to a Bachelor program has no direct implications for

potential admittance to a Master's program. That is, the reform generates performance incentives for students during the first stage, as a competitive preparatory step for their subsequent educational or labor market career. This would explain the estimated reform impacts on the higher probability to graduate within planned time and the lower study duration.

6. Conclusions

The Bologna reform did affect university students' educational outcomes: comparing treatment (Bachelor) and control groups (Diploma), we find that the probability to graduate within planned instructional time increases significantly and sizably. We also find some indication (from the OLS specification) that the study duration, measured using a standardized index, decreased. At the same time, the average final overall grade is significantly higher (i.e. worse) for the treatment group. Results for the former two outcomes imply a qualitatively positive effect of the reform on students' outcomes: students are more likely to dedicate an amount of educational lifetime to their studies that is closer to planned instructional time; and they are more likely to graduate in time. These results are in line with the reform objectives.

The impacts we find are robust across identification strategies, and subgroup analyses. In particular, there is little difference between female and male students, and between younger and older students, although impact estimates are slightly more positive (qualitatively) for women and for the younger first-year students. Also results for the 'local' subsample – which is arguably more strongly affected by the exogeneity of the treatment introduction due to students' home bias – reinforce the overall findings.

Regarding the precise channels through which the reform works, one conjecture discussed in the policy debate is the stronger regimentation of the degree programs post-Bologna. This may be likely to play a role also in our case, but there are no data available to prove this conclusively. We do observe, however, significant decreases in the probability of non-retention (i.e. of not remaining in the program) for the treatment group during the first and sixth semester, respectively. This may point to a mechanism of the Bachelor program being more likely to retain students at the very beginning (*starting effect*) and close to finishing their studies (*graduation effect*), and could explain the positive reform effects. Consequently, there is some indication that the possibility to obtain a labor market signal certifying a specific amount of human capital by obtaining a less costly university degree might actually incentivize a larger share of students to graduate. Moreover, the competitive forces induced by the fact that students have to reapply for a Master's program might incentivize them to not take too much time for graduating from the Bachelor program.

Although we find Bachelor graduates' final overall grades to be significantly worse than those of Diploma graduates, this does not necessarily imply a qualitative decrease of student performance: it is in part explained by the fact that the final grade in the Bachelor degree is composed of all exam grades during the program, while the final grade in the Diploma programs leaves out the grades of the first study phase (pre-Diploma) and has a somewhat stronger emphasis on chosen courses in the second phase.

In sum, we conclude that the Bologna reform has (qualitatively) positive effects on some important individual-level educational outcomes. Whereas the implications of these findings on later labor market outcomes are limited – because final grades as another early predictors of later outcomes are not positively affected – the micro level effects on (slightly) reduced study duration and increased within-time graduation rates directly translate into desirable outcomes from a societal perspective, at the very least for the systems of social security and higher education. The results could thus be considered informative for the reform effects in a broader set of universities in Germany and across Europe, for several reasons. First, given an overall rather loose and unspecified regulatory framework and timeline of the reform process, a homogenous institutional framework within the university allows to pin down the precise content and timing of treatment, and thus the treatment

effects. Given that the treatment implied similar consequences for all German universities – e.g. the reduction in study durations – the estimated impacts are therefore more broadly informative.

Second, Humboldt-Universität is representative – in the context of this analysis – for a wider set of universities in Germany, namely those located in urban centers, offering a wide range of subjects, and embedded into many international and, in particular, intra-European cooperations. Also, the framework conditions under which HU implemented the Bologna Reform were not specific to universities in Berlin, but are representative of many other public universities in the country. Third, from a decidedly overall European perspective the 'Bologna treatment' can be seen as a representative intervention affecting all universities, and from this perspective also students across Europe constitute a 'homogeneous' treated population. The estimation of reform effects at the university level will thus be informative for a broad set of universities in Germany, and across the continent, at the very least in the tentative direction of how the Bologna reform affected the educational outcomes of their students.

Notes

1. This end point of the transition process corresponds to the deadline set by the joint declaration of the European ministers of education (Bologna Declaration 1999).
2. This is because in contrast to some of their international counterparts, German universities could decide individually at the department level about the exact time of introduction of the new two-tier degree structure. This causes the aggregate ten-year timeline of the Bologna process displayed in Figure 1. At the micro level, we can use this variation to identify treatment effects; see the subsequent discussion.
3. This excludes e.g. degree programs that were discontinued with the introduction of Bologna, or newly introduced. It also excludes joint honors degrees, since treatment status would be unclear whenever a student is enrolled in a joint program offered across two departments that did not implement the reform at the same point in time. For the few cases of students with multiple study spells, we consider the first spell only.
4. This pattern likely results from the fact that departments' teaching capacities, which were formerly attributed exclusively to Diploma students, are now allocated among courses for Bachelor and Master students. From the year 2010 onwards, approximately, additional public funding has allowed universities to cope with the persistently higher numbers of students.
5. Note that throughout our analysis we consider those students who start a program in the first semester, in order to make sure that students did not change treatment status by switching universities, and to correctly compute their study duration. This includes all first-time enrollers, and also those first-semester students who already studied in another program at HU or elsewhere before.
6. Note that the averages reflect the 'active' number of semesters studied, excluding times during which students suspended their student status (e.g. in order to do an internship or study abroad).
7. Note that the study duration index outcome uses the longer time horizon up to eventual graduation, whereas the probability to graduate in-time uses the shorter time horizon until the end of regular instructional time; both are clearly related outcomes, but ex ante may be affected differentially by the reform.
8. Regular instructional time varies at the department level. While biology, physics and biophysics had an instructional time of ten semester, Diploma students in economics, business and sports science were expected to graduate within eight semesters.
9. Note that these shares represent subject-specific retention rates; i.e. the probability that a student graduates from the one particular university program that he or she started, and not whether he or she graduated at all (perhaps after switching subjects one or several times).
10. One might argue that increased capacity is part of the reform itself, but in our data the introduction of the new degree structure and the increase in program capacities do not coincide: specifically, figure 4 indicates that the overall number of students in the relevant degree programs did not increase.
11. Note that the treatment indicator in our setup varies not only at the department but also at the individual level, as some departments offered Bachelor and Diploma programs in parallel.
12. Note this is a different setting and approach from, for instance, the difference-in-difference strategy implemented by Meghir and Palme (2005) to evaluate major reforms of secondary education in several European countries in the period between 1950 and the mid-1970s.
13. Note that at the individual level the instrument takes on the particular value of the share determined by subject and year; not only the shares at the intersections of the two curves displayed in Figure 10.
14. Due to limited data availability we observe pre-Diploma grades for a subsample of the Diploma students only.

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Disclosure statement

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