**The Impact of Degree Duration on Higher Education Participation: Evidence from a Large-Scale Natural Experiment**

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# Evidence from a Large-Scale Natural Experiment

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*Abstract* – This paper exploits a national university reform, introduced in Italy with an exogenous timing and an unintended delay of treatment scheme, to identify the impact on higher education participation of shortening the degree duration of the first-tier university studies. Using a degree-specific universal database, we estimate that enrolment increased on average between 10.3 and 10.9 percentage points in the first year of the reform, and between 27.3 to 29.3 p.p. in the subsequent steady state. Such increase did not occur at the expense of deteriorating retention and on-time graduation rates. These results are of high policy relevance in two ways: first, because they indicate that the steady state cost-elasticity of participation to higher education is higher than what previously indicated by short-run estimates; second, because the enrolment gains are not suggested to be achieved at the expense of enlarging participation to insufficiently-skilled students.

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## 1. Introduction

Higher education is increasingly recognized as a strategic growth and development factor in all advanced economies; nonetheless, «even if tertiary attainment rates has increased in recent years, less than 35% of both men and women attain tertiary education» in OECD countries [Oecd 2013, p. 27]. It hence comes at no surprise that helping middle- and low-income households to access college education has surged among the priorities of President Obama's administration[i], while making Europe «the most competitive and dynamic *knowledge-based* economy in the world» within 2010 – a stated objective of the European Council held in Lisbon in 2000 [European Council 2000] – is still a top-agenda issue within the strategy for Europe 2020 [European Commission 2010]. This notwithstanding, the access to tertiary education is harmed by policies of financial austerity [Altbach et al. 2009] that progressively shifted the cost of higher education onto students and their families [Teixeira et al. 2007] even in those European countries with a sound tradition of public universities [Oecd 2013, Indicators B3 and B5]. Combining a less costly access to higher education with a limited burden upon governments' budgets is henceforth an increasingly important policy issue that can greatly benefit from the availability of sound empirical evidence.

Providing reliable empirical evidence to guide such policy choices, however, is not an easy task. Actual changes in educational costs, degree durations and/or financial aid that can be exploited in the analysis are often endogenous (both in their timing and cross-sectional dimensions) to factors leading to changes in higher education participation, retention and/or graduation outcomes. This can lead to omitted variable bias issues in the form of systematic cross-sectional and cross-time differences between units exposed and not exposed to the impact-identifying shifts exploited in the analyses. The existing empirical literature on higher education outcomes has taken on these challenges focusing on a variety of different types of data, impact-identifying shifts and estimation models.

A first large stream of the literature focuses on enrolment outcomes and aims at exploiting variations in tuition fees and/or financial aids to produce policy-relevant impact estimates. As argued by Deming and Dynarski [2009], early examples of this literature [see Leslie and Brinkman 1987 and Heller 1997 for a review] typically exploit only cross-State or cross-time fee variations and may hence poorly identify the response of schooling to price. Neill [2009] reviews the more recent examples in the literature and distinguishes between two types of identification strategies. The first relies on exploiting fee variations at the university level [e.g. Cameron and Heckman 2001, Fortin 2005, Johnson and Rahman 2005 and Kane 1994 among those listed by Neill]. This strategy improves on the early literature but, as Kane [2003] points out, it still rests upon the assumption that the supply of enrolment places in higher education institutions is perfectly elastic to enrolment demand, bearing as a consequence the exogeneity of the fee changes. Building on the results on cohort crowding by Card and Lemieux [2003] and Bound and Turner [2006], Neill [2009] argues that this hypothesis may often be unrealistic and approaches the endogeneity issue of university fees by using the local incumbent party as an instrument for tuition policies in Canada. The second type of identification strategy [e.g. Abraham and Clark 2006, Cornwell et al. 2006, Dynarski 2003, Seftor and Turner 2002] exploits shifts in financial aid policies targeted to specific groups of students. This approach lessens endogeneity concerns but bears the risk of yielding results with weak external validity, as financial aid is typically targeted to marginal students from families with a poor educational and financial background; results from such group of students could hence differ quite decisively from those that would be obtained on the general population.

A second, smaller, stream of the literature does not investigate enrolment outcomes but exploits variations in the availability of student services, financial aids and merit scholarships to produce drop-out/retention or graduation impacts. As reviewed by Deming and Dynarski [2009] a first group of these papers [e.g. Angrist et al. 2009] examines the effect of combinations of financial aid and student services on post-enrolment outcomes (grades, credit accumulation, retention, graduations) by analyzing experimental data from few random assignments of the supply of student

services in a small group of universities. These experimental data avoid any omitted-variable bias concern but do not provide ample external validity of the results because of the randomization that took place in a small group of universities.

A second group of papers [Dynarski 2008, Scott-Clayton 2011 and Webber 2012] focuses on observational data. Dynarski [2008] studies the impact of merit-scholarship programs on college completion by exploiting cross-sectional and cross-time variations in the program availabilities to which different cohorts of students are exposed. Scott-Clayton [2011] follows a similar approach to estimate the impacts on completion of financial aid programs that require beneficiary students to maintain to a minimum number of credits per year. Webber [2012] uses data on the students enrolled in the public universities of Ohio and exploits variations in the student- and educational-service expenditures to estimate graduation impacts. Because of a large sample size used in the analysis these observational studies provide results with larger external validity than the randomized-experiment papers.

The present paper aims to produce reliable and policy-relevant empirical evidence on enrolment, drop-out/retention and on-time graduation outcomes, contributing to the literature in three relevant aspects.

First, it exploits the shortening of the duration of the first-tier university degrees (and the implied shift in educational costs) brought by a national reform of higher education that affected, in the vast majority of fields of studies, the entire population of the universities of a large European country (Italy). The Italian reform exploited in our paper followed a European decision (referred to as the «Bologna Process», as further explained in the next section) to standardize and harmonize the European university systems. Such decision led to the adoption of similar reforms in other European countries. The Italian reform, however, offers ideal impact-identification shifts for the analysis because its introduction shares features of a very desirable natural experiment: the reform was implemented with a random and unintended delay-of-treatment scheme and the timing of its introduction was exogenous to underlying trends that may affect enrolment, retention and

graduation outcomes. These features ensure the presence (within a same academic year) of departments with comparable characteristics but different reform adoption status, and they ensure the absence of systematic differences between the underlying enrolment, retention and graduation trends between the pre- and post-reform periods. Thus, the analysis can identify enrolment, retention-rate and on-time graduation impacts under very plausible assumptions, circumventing the major threats to the validity of the analysis to which the bulk of the existing literature is exposed. This is unlike the case of analogous reforms in other European countries. In Portugal, for instance, a similar shortening of the first-tier university degree took place only after a very long adoption process that lasted seven years after the Bologna declaration [Cardoso et al. 2008]. In the Portugal case, therefore, demand-side and cost-reduction effects appear hardly distinguishable. Moreover, Italy was among the very few countries (together with the Netherlands and Norway, of which we are not aware of any empirical assessment: see Litjens [2005]) in which the introduction of the reform was *mandatory* for all universities and in which (for all the fields of studies affected by the reform) access to higher education was granted upon completion of – in principle: any – high school degree with no first-year enrolment restrictions based on a maximum number of admitted students. This implies the complete absence of attrition bias concerns and no self-selection issues between compliant and non-compliant departments. These limitations, instead, affected the German case, where a Bologna-process reform was also introduced from the academic year 2000-01, but its adoption was not ruled as mandatory. As a result, by 2006-07 only 53% of German departments had at least 75% of students enrolled in the newly established 3-year bachelor degrees [Horstschräer and Sprietsma 2010].

Second, our paper uses unique administrative micro-data that cover longitudinally the universe of all existing Italian universities and departments[ii], with degree-specific information on the number of enrollees and graduated students sorted by the year of their first enrollment. The availability of such longitudinal micro-data ensures very high external validity to the analysis and is key to properly exploit the natural experiment features of the Italian case, avoid attrition bias issues

and grant the best possible internal validity of the results. Exploiting such data sets distinguishes our paper from earlier attempts to study the effects of the Italian reform. Boero et al. [2005] and Bratti et al. [2006] rely on data from few single departments. This limits the external validity of the results, and does not allow the analysis to exploit the cross-sectional variations in the reform-adoption status of the departments caused by the delay-of-treatment scheme of the reform; moreover, it exposes the impact estimates to attrition-bias risks in the form of unobserved displacement effects on out-of-sample departments. Cappellari and Lucifora [2009], Di Pietro and Cutillo [2008] and D'Hombres [2005], instead, make use of repeated cross-sectional survey data concerning the post-graduation choices of cohorts of high-school graduates at the national level. With such data, however, they are not able to observe any department-specific feature, and hence they are also unable to exploit the favorable identification scenario brought by the unintended delay with which the Italian reform was introduced.

Third, the degree-duration change exploited in our paper is of high policy relevance as it shares the features of an intervention that actually eased the access to higher education. This is because the Italian reform, in the aftermaths of its introduction was not perceived to majorly affect the job-market value of the first-tier of the university studies, and because no capacity constraints or enrolment access barriers (other than the budget constraints on the part of high school graduates) were in place in the Italian university system in the fields of study affected by the reform. Thus, our results can inform future policy interventions by offering empirical evidence on the cost-elasticity of the participation to higher education, and on the sensitivity of retention and graduation outcomes to enlargements of higher education participation. The latter aspect is of significant importance to test whether or not policy-induced enrolment gains are obtained at the expense of enlarging higher-education participation to insufficiently skilled students. This would result in a deterioration of retention and graduation outcomes that would imply a lack of social efficiency in the enlargement of the higher-education participation.

The results of our analysis show that in its first year of introduction, the degree-duration shortening brought by the reform generated an increase in the baseline yearly change of first-year enrolment between 10.3 and 10.9 percentage points (p.p.). In its second year of introduction, the reform generated an additional increase of 17.0 to 18.4 p.p.in the baseline yearly-change of first-year enrolment. These enrolment gains were not offset in the subsequent years (third, fourth and fifth year of introduction of the reform), for which further positive, but not statistically significant increases are estimated. For retention outcomes, the first year of introduction of the reform produced a 9.1-9.4 p.p. increase in the yearly change of the one-year retention rate and a 28.9-29.2 p.p. increase for the two-year retention rate. For both the one-year and the two-year retention rates, in its second, third fourth and fifth year since its introduction, the reform maintained the first-year gains with no statistically significant further change. For on-time graduation, finally, our results show that the reform increased by 5.6 to 17.7 p.p. the share of students that complete their degree within its statutory duration.

The paper proceeds as follows. The next section illustrates in details why the Italian university reform represents a desirable natural experiment for the analysis. The third section describes the data and illustrates the major descriptive statistics. In the fourth section, we present our impact identification strategy and the estimation model. The fifth section is devoted to illustrate the results of the analysis. The sixth section computes the cost-elasticity figures that derive from our impact estimates. The final section offers some concluding remarks.

## 2. The «Bologna-Process» University Reform in Italy

The Italian university reform exploited in our analysis was introduced in the academic years 2000/01-2001/02, following the implementation of the so-called «Bologna Process», a joint initiative by 29 European Ministers of Education that met in Bologna in 1999 and designed a new standard for university education in Europe that was considered a turning point in the development of European higher education [Litjens 2005; Keeling 2006]. The adoption of the new standard,

forced Italy (together with other European countries[iii]) to shorten the statutory duration of its first-tier University degrees[iv] from four or five years to three years, in a vast majority of fields of study[v]. The reform also set a standard of two-year duration for the graduate (second-tier) degrees[vi].

In Italy, the Bologna Process reform was introduced in a university system composed by a large network of public universities with comparable tuition fees and financial aid opportunities. Private universities, a small minority, were subject to the same national regulations concerning the features of the degree-curricula, financial aid policies and even faculty recruitment process and personnel wage schemes. In all the fields of study affected by the reform, the access to higher education was typically granted upon completion of any high school degree with no enrolment restrictions based on a maximum number of admitted students. These features ensure the possibility of estimating the reform impacts without facing potentially relevant attrition bias threats and enhance the exploitability of both the exogenous timing and the delay-of-treatment scheme related to the reform introduction. Such natural experiment occurrence (which is a key component of our impact identification strategy) can be described as follows.

The reform was formally established in 1999 by the D.M. (Ministerial Decree) 509/99 of the National Ministry of Education. The D.M. 509/99 immediately followed the date in which the European Ministers of Education met in the Italian city of Bologna to set the new standards to harmonize the European university systems. No other reasons (endogenous to Italian institutional evolutions and/or to changes that can also affect the participation to higher education) motivated the timing in which such decree was issued.

The D.M. 509/99 ruled that the final deadline for all Italian Departments to comply with the reform was set eighteen months after the publication of ministerial regulations for the actual implementation of the newly reformed three-year long bachelor degrees and the following two-year long master degrees. The publication of such ministerial regulations was believed to be imminent. This put a lot of pressure on the departments to prepare early-on their administrative work for adopting the reform. Subsequent administrative difficulties, internal to the Ministry of Education,

however, generated an unexpected delay in the publication of the Regulations, which (for the three-year long bachelor degrees) occurred only in August 2000. As a result of this delay, the departments (referred to as *early adopters*) that happened to have previously set the date for their first faculty meeting of the Fall 2000 in advance with respect to the first day of class of the semester, had a chance, if they wanted, to formally approve the establishment of the new degrees in time for the starting of the same academic year 2000/01. The other departments (*delayed adopters*) had to postpone the introduction of the reform to the next academic year (2001/02). Thus, the existence of *early* and *delayed* adopters (which often coexist even within a same University) traces back mainly to a random difference in the date chosen for the first faculty meeting in Fall 2000. No obvious endogenous underlying factors, indeed, were in place to affect both enrolment, retention and graduation outcomes and the decision to implement the reform at a certain time or in a certain group of departments, as also suggested in Bosio and Leonardi [2011] and further supported by the descriptive evidence discussed in the next section.

## 3. Data and Descriptive Statistics

The data used in the analysis is collected by the statistical office of the Italian Ministry of Education (USTAT). Every year the evaluation board of each Italian university is required to send to USTAT a prearranged set of information concerning the number of enrolled, graduated and transferred students (sorted by each single degree curriculum offered). The resulting database covers the universe of all 83 public and private Italian universities[vii] with degree-specific information on: the location (city, province and region) of the department in which the degree curriculum is offered; the type and name of the degree; the number of enrollees and graduated students sorted by the year of their first enrollment. The USTAT database is then merged with the data on the yearly number of students who graduate from high-school in the same region in which the department is located and with yearly data on regional per-capita GDP maintained by ISTAT (the Italian National Statistical Institute). This is required, as discussed later, in order to control for

opportunity-costs and supply-side effects in evaluating the impact of the reform on enrolment outcomes.

The data at our disposal covers the academic years before and after the reform implementation, spanning from 1998/99 to 2004/05. The main units of observations for our analysis are the sub-divisions of Italian universities referred to as *facoltà*[viii]. During the period 1998-2004, the Italian *facoltà* were uniquely in charge of managing each existing university degree (including bachelor degrees) and they were highly specialized by field of study (with up to 36 different types of *facoltà*). The *facoltà* also had autonomous decision power (within certain guidelines set nationally by the Ministry of Education) pertaining all aspects of their degree curricula and they were the university sub-divisions to which each faculty member was strictly and uniquely assigned. For the sake of simplicity, in the remainder of the paper, the term *facoltà* will be referred to as department.

The departments considered in the analysis are all those for which the D.M. 509/99 reform introduced a reduction in the number of years of the statutory duration of their first tier of university studies (undergraduate degrees) available to high-school graduates. For this reason, the departments of Law, Medicine, Veterinarian Medicine and Pharmaceutical Studies were excluded from the analysis, as the statutory duration of their undergraduate degrees was not majorly affected by the reform[ix]. For the large majority of these excluded departments, national regulations limited the number of first-year enrollees to a fixed level (with little or no variation during the time-frame considered in the analysis and with a competitive admission process based on an admission test resulting in a large number of rejected applicants). As a consequence, including in the analysis such departments as control units of observation does not bear useful information to estimate the counterfactual variation (that would have been registered in the absence of the reform) either for the enrolment trends, or for retention and on-time graduation outcomes (the latter due to the student skimming process produced by the admission test, leading to a population of enrolled students not comparable with that of all other departments). Moreover, because of the constant-over-time limit

to first year enrolments, excluding such departments does not expose the analysis to attrition bias risks.

The resulting final database is composed by 354 departments out of the universe of the 505 departments recorded in the USTAT database in the period 1998/99-2004/05[x]. Of the 354 departments used as units of observation for the analysis, 25 departments adopted the reform starting from the academic year 2000/01 (*early adopters*), while 329 departments introduced the reform from the academic year 2001/02 onwards (*delayed adopters*).

Tables 1-3 illustrate the aggregate pre- and post-reform descriptive statistics for enrolment, retention and on-time graduation rates of the entire group of departments considered in the analysis. In the pre-reform period, the total number of first-year enrollees amounts to an average of about 208,500 students per year. In the post-reform period, the yearly number of first-year enrollees reaches an average of about 244,000 students per year. This 17% increase in the number of first-year enrollees clearly contrasts with the baseline demographic trend that shows a 2.8% contraction (between the pre- and post-reform periods) in the yearly number of new high-school graduates.

[Table 1 about here]

The average one-year retention rate of the first-year enrollees is 76.3% for the pre-reform cohorts, and 78.6% for the post-reform cohorts, with an average pre-post reform increase of 2.32 p.p. At two years after their first enrolment, the average retention rate of the first-year enrollees is 68% in the pre-reform period and 72.6% in the post-reform period, with a pre-post reform increase of 4.62 p.p..

[Tables 2.a and 2.b about here]

Finally, the aggregate average percentage of first-year students that graduate on time (conditional on that they are still enrolled in their last year of the degree curriculum) is about 30% in the pre-reform period and 40.1% for the post-reform period, with a pre-post reform difference of 10.25 p.p..

[Table 3 about here]

To best illustrate how the initial characteristics of the early-adopter and delayed-adopter departments compare, Table 4 displays the descriptive statistics for enrolment, retention and on-time graduation of the two groups, limiting the pre-reform period to 1998/99 - 1999/00, when both groups of departments were in the pre-reform status.

[Table 4 about here]

During those years, all the pre-reform enrolment, retention and on-time graduation levels and trends show no statistically significant differences between early-adopter and delayed-adopter departments. The absence of systematic pre-reform differences is further illustrated by comparing the early-adopters' and delayed-adopters's distributions of their Propensity Score (PS) estimated as the predicted probability of becoming an early-adopter as a function of all the pre-reform variables included in Table 4.

[Figure 1 about here]

The box-plots of the two PS distributions largely overlap, indicating that no systematic differences (relevant to our analysis) were in place between early and delayed adopters before the reform was introduced. Such evidence reinforces the notion that the reform was implemented with an unintended, and largely exogenous, delay-of-treatment scheme that enables us to exploit in the

analysis the presence of similar departments with different reform-adoption status within a same academic year, without any relevant self-selection issue.

## 4. Methods of Analysis

The main challenge for the analysis is to separate the effects of the reduction in the degree duration brought by the university reform from the effects on enrolment, retention and on-time graduation deriving from confounding factors such as: demographic trends that change the number and composition of students graduating from high schools; changes in the attractiveness of the specific fields of study related to the university degrees offered in the different departments; changes in the labor markets conditions for the regional economies in which the departments are located. In the following, we deal with this issue separately for each of our outcome variables.

The impact of the reform on **enrolment** is estimated through a conditional difference-in-difference (CDD, e.g. Bondonio and Greenbaum 2014) multiple regression model implemented on longitudinal data at the department level. The key features of the model are as follows. The impact of the reform is identified separately from common enrolment temporal trends (exogenous to the reform) thanks to: a) the exogenous timing of the introduction of the reform (which followed EU decisions for degree standardization in Europe, as previously mentioned); b) the delay of treatment that occurred in the implementation of the reform. Exploiting the presence of departments with different reform-adoption status within a same academic year, the model features a full set of year dummy variables that separate the impact of the reform from temporal enrolment or retention-rate changes that would occur even without the reform implementation.

To further rule out any possible remaining selection bias from the impact estimates, our CDD specifications controls for all unobservable differences between *early* and *delayed adopters* that can be assumed to be fixed-effect components of the error term. In addition, they also include an ample set of observable control variables that capture the department characteristics (that may

affect enrolment independently from the reform) and control for all of the crucial observable confounding factors without relying on fixed-effect assumptions.

In details, the model is obtained by first-differencing the following general specification:

$$Y_{it}=f\,[\rho_t\,,\,Tc_{it},\;X_{it},\,(t*X_i),\;\alpha_i\,,\,u_{it}\,] \qquad (1)$$

The resulting CDD model is as follows:

$$\Delta Y_{it}=f\,[\Delta\rho_t\,,\;\Delta Tc_{it},\;\Delta X_{it},\;X_i,\;\;\Delta u_{it}\,] \qquad (2)$$

where:

- $\Delta Y_{it}$ = $\ln([ENRL]_{it}/[ENRL]_{(it-1)})$ is the yearly change in the number of newly enrolled students in department *i* between academic year *(t)* and *(t-1)*;
- $\rho_t \in \{0,1\}$ with $t = \{2000, 2001, 2002, 2003, 2004\}$ are a set of yearly dummy variables[xi];
- $Tc_{it} \in \{0,1\}$ with $c = \{1,2,3,4,5\}$ are five treatment status variables defined as follows: $T_{1it}$ { = 1 if department *i* first introduced the reform in academic year *t*; =0 otherwise}; $T_{2it}$ { = 1 if the reform is implemented in department *i* and year *t* for the second consecutive year; = 0 otherwise}; $T_{3it}$ { = 1 if the reform is implemented in department *i* and year *t* for the third consecutive year; = 0 otherwise}; $T_{4it}$ { = 1 if the reform is implemented in department *i* and year *t* for the fourth consecutive year; = 0 otherwise}; $T_{5it}$ { = 1 if the reform is implemented in department *i* and year *t* for the fifth consecutive year; = 0 otherwise}[xii];
- $X_{it}$ is a set of time varying characteristics of department *i*; after first differencing these controls, ($\Delta X_{it}$) are in the form of yearly changes *(t)-(t-1)* recorded in department *i*;
- $t*X_i$ is a set of interaction terms between a time trend *t* (specified as the number of years elapsing from the academic year 1998/99) and the initial time-unvarying characteristics of a department;
- $\alpha_i$ is the fixed-effect error component;
- $u_{it}$ is the i.i.d. error term with $E(u_{it}) = 0$ and $Var(u_{it}) = \sigma^2$.

As shown in the previous section, early-adopter departments and delayed-adopter departments have characteristics leading to a large overlap in their propensity score distributions. As discussed elsewhere [e.g. Bondonio and Greenbaum 2007, 2014; Ho et al. 2007], such a large common support in the control variables limits the "model dependence" of the results. Nevertheless, to test how sensible our impact estimates are to the functional form choice of the control variables we estimated the model (1)-(2) through four different specifications. Each of the four specifications is defined by a different selection and functional form of the set of controls $\Delta X_{it}$ and $X_i$. Specification I is the most statistically efficient one, with a linear functional form for all controls $\Delta X_{it}$, while specification IV is the one with the most flexible functional forms and the most complete set of controls $\Delta X_{it}$ (Table 5).

[Table 5 about here]

In details, the control variables $\Delta X_{it}$ included in the four specifications are defined as follows:

- $\Delta CUR_{it}$ = is the yearly change in the total number of degree curricula offered in department *i* between the time *t* and *t-1*. This variable is used in the model to separate the impact of the reform from the impact due to changes in the spectrum of topics covered by degree curricula offered by the departments;
- $\Delta HSGRD_{it}$ = $\left(\ln\left([HSGRD]_{it}/[HSGRD]_{(it-1)}\right)\right)$ is the yearly change (between the time *t* and *t-1*) in the total number of high-school graduates in the same region in which department *i* is located. This variable is used in the model to separate the impact of the reform from the impact on enrolment due to demographic regional time changes (expressed by the number of high-school graduates);
- $\Delta COMP_{it}$ and $\Delta COMPn_{it} \in \{0,1\}$ with $n = \{1,2,3\}$ capture the yearly changes in the local competition faced by a department in attracting perspective students. $\Delta COMP_{it}$ =

$(N\_DEPT_{it}) - (N\_DEPT_{it-1})$ measures the yearly changes in the total number of active departments located within the same province of each department *i*. $\Delta COMPn_{it} \in \{0,1\}$ with $n = \{1,2,3\}$ are three dummy variables operationalized as follows: $\Delta COMP1_{it}$ { = 1 if department *i* is located in a province in which the number of other existing departments decreases between year *t* and *t-1*; = 0 otherwise}; $\Delta COMP2_{it}$ { = 1 if department *i* is located in a province in which the number of other existing departments remains constant between year *t* and *t-1*; = 0 otherwise}; $\Delta COMP3_{it}$ { = 1 if department *i* is located in a province in which the number of other existing departments increases between year *t* and *t-1*; = 0 otherwise};

- $\Delta REGDP_{it}$ and $\Delta REGDPn_{it} \in \{0,1\}$with $n = \{1,2,3,4\}$ capture the yearly changes in the GDP of the region in which a department is located. $\Delta REGDP_{it}$ = $\left(\ln\left(\left[REGDP\right]_{it} / \left[REGDP\right]_{(it-1)}\right)\right)$ is in a linear form; $\Delta REGDPn_{it} \in \{0,1\}$with $n = \{1,2,3,4\}$ are instead four dummy variables based on the quartiles of the $\left(\ln\left(\left[REGDP\right]_{it} / \left[REGDP\right]_{(it-1)}\right)\right)$ distribution of the departments.

The control variables $X_i$ highlighted in Table 5 are defined as follows:

- $SIZE_i$ and $SIZEs_i \in \{0,1\}$ with $s = \{1,2,3,4\}$ capture the initial size of department *i*, in terms of the total number of enrolled students recorded in 1998 (the first period of observation recorded in the data). $SIZE_i$ is in a linear form (i.e. total number of enrolled students in 1998), while $SIZEs_i \in \{0,1\}$with $s = \{1,2,3,4\}$ is a set of four categorical variables based on the quartiles of the size distribution of the departments;
- $FIELD_m \in \{0,1\}$ with $m = \{1,2,\ldots,24\}$; $MAIN_i \in \{0,1\}$; $CAP_i, \in \{0,1\}$; $REGr_i \in \{0,1\}$ with $r = \{1,2,\ldots,20\}$ are sets of time-unvarying initial characteristics of departments *i*, in terms of:
  - field of study characterizing each department (operationalized as a set of 24 dummy variables)[xiii];

- whether or not department *i* is in a main or secondary campus;
- whether or not department *i* is located in a regional capital city;
- region in which department *i* is located (operationalized as a set of 20 regional dummies).

These observable time-unvarying characteristics $X_i$ are included in the model to control for the following events without relying on rigid fixed-effect assumptions: i) large departments may become over time more/less attractive than smaller ones if changes occur on how perspective students value the added cultural experience offered by large departments versus the added convenience offered by smaller departments; ii) certain fields of study may experience a surge/drop in popularity among perspective university students, due to media exposure of certain carrier paths and/or specific trends in the labor market; iii) units located in peripheral campuses may experience a different enrolment trend than those in the main campus because of changes on how perspective students value the added convenience of studying closer to home versus the added benefit of having a main campus experience; iv) units located in regional capital cities may experience a different enrolment trend due to changes on how perspective students value the amenities of a regional capital city versus the convenience and lower living costs of other cities; v) different regions may experience different trends in the labor market providing different alternative opportunities to university enrolment.

For **retention** and **on-time graduation** outcomes, the impact of the reform can be due to two distinct factors: a) changes in the composition of the students (causing changes in the distribution of skills and motivations between pre- and post-reform students) and b) changes in the studying and class attendance time that students can (or are willing) to devote to the completion of the degree (holding constant student composition). In this perspective, it is worth noting that the aim of our analysis is to identify the combined effect of a) and b). We believe that estimating such combined effect does provide policy-relevant empirical evidence: one of the most relevant issues is indeed whether or not the reduced educational costs brought by the reform occurred at the expense

of a combination of diminished efforts put by students in fulfilling the degree requirements and/or the attraction toward university enrolment of individuals with non-suitable skills and higher education achievement potential, causing a deterioration of retention and on-time graduation outcomes. Attempting to separately estimate the impact of the reform on the students composition and on the efforts toward graduation devoted by students, instead, would require student-specific data merged with curriculum-level data that could be gathered only through a costly and extremely-labor intensive collaboration with the single administrative offices of each separate university department. This would be feasible only for a very limited number of departments. We did not pursue this option to avoid undermining the external validity and the statistical efficiency of the analysis; we will anyway partly go back to this issue in our concluding remarks.

The retention outcomes that we used in the analysis are operationalized from the USTAT data in the following way:

$$RR^{I}_{it} = \frac{\left[\sum_s \left[ENR2_{sit+1} \,|\, ENR1_t\right]\right]}{\sum_s [ENR1_{sit}]} \qquad (3)$$

$$RR^{II}_{it} = \frac{\left[\sum_s \left[ENR3_{sit+2} \,|\, ENR1_t\right]\right]}{\sum_s [ENR1_{sit}]} \qquad (4)$$

where:

- $\left[\sum_s \left[ENR2_{sit+1} \,|\, ENR1_t\right]\right]$ = sum of second-year students *s* enrolled in academic year *t+1* and in department *i* who were first-year new enrollees in academic year *t*;
- $\left[\sum_s \left[ENR3_{sit+1} \,|\, ENR1_t\right]\right]$ = sum of third-year students *s* enrolled in academic year *t+2* and in department *i* who were first-year new enrollees in academic year *t*;
- $\sum_s [ENR1_{sit}]$ = sum of first-year students *s* newly enrolled in academic year *t* and in department *i*.

The two retention rates $RR^I$ and $RR^{II}$ express the rate of the first-year newly enrolled students in department *i* who are still enrolled in the same department in their second and third year of studies, respectively. The reform impact on the $RR^I$ and $RR^{II}$ retention rates is estimated through a CDD model similar to that for the enrolment outcome:

$$\Delta Y^k_{it} = f[\Delta\rho_t,\ \Delta Tc_{it},\ \Delta X_{it},\ X_i,\ \Delta u_{it}] \qquad (5)$$

where: $\Delta Y^k_{it} = RR^K_{it} - RR^K_{it-1}$, with $K = \{I, II\}$, is the yearly change in the retention rate between year *t* and *t-1*.

To ensure little model dependence for our results, as in this case of the enrolment estimates, we implemented Model (5) through the four different specifications highlighted in Table 6. These specifications are defined by a different variable composition and functional form choice of the set of controls $\Delta X_{it}$ and $X_i$ and they range from a most rigid and efficient one (specification I) to a most flexible and least efficient one (specification IV).

[Table 6 about here]

For the sake of improving the overall efficiency of the model, all the four specifications of Table 6 contain fewer time-unvarying controls $X_i$, compared to the enrolment specifications. This choice is based on the fact that all controls related to location of the a department – the dummies for main campus (MAIN) and regional capital city (CAP) locations and the regional dummies (REG) – can be easily assumed to be known in advance by the perspective students and thus they capture factors that affect in the same way the students' decision both to enroll in the first year of a department degree curriculum (the denominator of $RR^k$) and to remain enrolled in the same department in the subsequent one or two years (the numerator of $RR^k$). The departments' size (SIZE) and the field of study of their degree curricula (FIELD), instead, can be assumed to be proxies for internal features

of the department (e.g. the ratio between students and instructors, the quality of teaching and of tutoring services to students). Such features may affect the $RR^k$ ratios because they may not be known in advance by the perspective students that decide to enroll in the first year of a certain department (and thus they may affect the likelihood to abandon a certain degree curriculum, without affecting in an equal way the denominator of the $RR^k$ ratios).

The reform impact on the percentage of students who graduate within the statutory duration (in years *n*) of the degree is instead estimated operationalizing the following on-time graduation outcome (OGR) based on the curriculum *j*-level data maintained by USTAT:

$$OGR_{ji(t+n-1)} = \frac{\left[\sum_s \left[GRD_{sji(t+n)} \middle| \right] ENR1_t\right]}{\sum_s \left[ENRn_{sji(t+n-1)}\right]} \qquad (6)$$

where:

- *s* = students; *j* = degree curriculum; *i* = department; *n* = statutory duration of the degree-curriculum; *t* = first-year of enrolment; *t+n-1* = last year of enrolment; *t+n* = last year of on-time graduation;
- $\left[\sum_s \left[GRD_{sjit+n} \middle| \right] ENR1_t\right]$ = sum of students *s* who graduated on-time within academic year *t+n* in a degree curriculum *j*, offered in department *i*, and who were first-year new enrollees in academic year *t*[xiv];
- $\sum_s \left[ENRn_{sjit+n-1}\right]$ = sum of students *s* enrolled at the beginning of their last academic year *t+n-1* in a degree curriculum *j* offered in department *i*.

$OGR_{ji(t+n-1)}$ expresses the share of the students enrolled in their last year *t+n-1* of curriculum *j* (offered in department *i*) that graduate on-time within the academic year *t+n*, with *n* being the statutory duration of the degree and *t* being the year of the students' first enrolment. Because changes in retention-rate outcomes due to the reform are already investigated separately in our

paper, we preferred to measure on-time graduation in this way, instead of adopting as outcome indicator the gross percentage of first-year newly enrolled students who do graduate on time. First-year students may not graduate on-time also because of drop-outs or transfers in the subsequent academic years. Our OGR indicator enables the analysis to pin-point the impact of the reform on the time required to achieve graduation solely for those first-year students who, by reaching their last year of enrollment, show an adequate commitment toward completing the university studies[xv].

Estimating the reform impact on the OGR variable requires comparing the on-time graduation outcomes recorded for the new degree curriculum *j* introduced after the reform, with an estimate of the counterfactual outcomes that would have been recorded in the same period and in the same departments for the pre-reform degree curricula. In the years across the introduction of the reform such estimation is facilitated by the fact that within each same department *i* the newly reformed curricula and the pre-reform curricula are both in existence at the same time. This is because all Italian university departments had a legal obligation toward the pre-reform students to enable them to graduate within the degree curriculum that they started with. Thus, for the academic years between 2001/02 and 2004/05, all departments offered courses both for the first, second and third years of the new 3-year long BA degree curricula and for the second, third and fourth years of the pre-reform 4-year long degree curricula. As a consequence, the OGR outcomes of both the reform and no-reform status are observable within each same department and academic year. Because, as explained previously, possible changes in the composition of the cohorts of enrolled students are part of the reform impact that we want to estimate, this scenario represents ideal quasi-experimental conditions that can minimize selection and time-varying omitted variable bias in the estimation of the reform impact $\alpha$. This can be achieved through the adoption of a fairly simple control-group design model:

$$\alpha = E(OGR_{ji(t+n-1)} \mid T_{ji}=1) - E(OGR_{ji(t+n-1)} \mid T_{ji}=0) \quad (7)$$

where:

- $T_{ji}$ = 1 if the curriculum *j* in department *i* is a post-reform 3-year long degree;

  = 0 if the curriculum *j* in department *i* is a pre-reform 4- or 5 year-long degree.

However, because for a small portion of first-year students that were enrolled in the pre-reform curricula it was feasible to transfer to the new three-year long curricula, implementing the estimator (7) bears a trade-off between some risk of time-varying omitted variable bias and some risk of attrition bias. This is because, on the one hand, focusing on two adjacent cohorts of students of a same department (with the first cohort immediately before and the second one immediately after the introduction of the reform) is the best possible option to eliminate most of the time-varying omitted variable issues (as we would observe the two cohorts of students graduating in the same academic years and in exactly the same departments). On the other-hand, this option is at the expenses of possibly failing to observe the graduation outcomes of the group of pre-reform students that switched to the post-reform three-year long degrees after their first year of enrolment. Since switching from a pre-reform to a post-reform degree was easier for those students that after their first year of enrolment did not successfully complete many courses, it is likely that the small portion of the last cohort (before the introduction of the reform) of pre-reform students that drop out from our estimation is at the lower end of the students' ability distribution (leading to some potential for attrition bias that can cause some degree of over-estimation of the actual on-time graduation rate of such last cohort of pre-reform students).

Separating by one or two academic years the cohorts of pre-reform students from those of post-reform students, instead, on the one hand eliminates the risk of attrition bias almost entirely, while on the other hand, it increases the risk of facing some possible time-varying omitted variable bias (due to the fact that the graduation outcomes of pre-reform students would be observed one to three years earlier than those of post-reform students)[xvi]. In light of such a trade-off, we choose to implement the control group design model (7) with the four specifications portrayed in Table 7, that cover a sufficiently large spectrum of options.

[Table 7 about here]

Specification I is the one that minimizes the risk of time-varying omitted variable bias at the expense of risking some attrition bias (and some reduction in statistical efficiency). This is because it focuses exclusively on the two adjacent cohorts of students that share exactly the same last-year of enrollment *t+n-1* for both the pre-reform and the post-reform degrees (2002 for the early-adopter and 2003 for the delayed adopter departments). Specifications II and III offer some middle-range options by focusing on two pre-reform and post-reform cohorts of students with zero- to two-year gaps between their year of last enrolment. Specification IV is the one that completely eliminates the risk of attrition bias at the expense of the highest risk of some degree of time-varying omitted variable bias. This is done by focusing on two pre-reform and post-reform cohorts of students with a two to three year gap in their last year of enrolment.

## 5. Results

The estimated enrolment impacts of the reform are highlighted in Table 8. On its first year of introduction, the reform produces an average boost (compared to the counterfactual trend) of the yearly enrolment change of 10.3-10.9 percentage points, with specification II that produces the lowest estimate of 10.3 p.p. and specifications III and IV that produce the highest estimate of 10.9 p.p.

The second year of introduction of the reform brings an additional upward shift of the yearly average enrolment trend in the order of 17.0-18.4 p.p. (17.0 p.p. in specifications I and II and 18.1 p.p. and 18.4 p.p. in specifications III and IV, respectively). The upward shift (totalling 27.3 – 29.3 p.p.) of the yearly enrolment change recorded within the first two years of introduction of the reform is not reversed in the subsequent years. This is because the impact estimates for the third, fourth and fifth year of introduction of the reform show, for all four specifications, statistically

insignificant additional increments in the magnitude of 10 p.p. per each year. Among the coefficient estimates for the control variables, it is worth noting that each one unit increment in the number of degree curricula offered by a department leads to a 3.3-3.4 p.p. increase of the baseline yearly change of the number of new enrollees. This result highlights how, independently from the reform, the strategy of increasing the portfolio of curriculum degrees offered by a department paid off quite well in terms of attracting additional enrollees.

[Table 8 about here]

The impact of the reform on the one-year and two-year retention rates are highlighted in Table 9 and 10, respectively. The first year of introduction of the reform is estimated to produce a 9.1-9.4 p.p. boost in the yearly change of the one-year retention rate and a 28.9-29.2 p.p. boost in the yearly change of the two-year retention rate. For both the one-year and the two-year retention rates, in its second, third, fourth and fifth year since its introduction, the reform is estimated to maintain the first-year gains with no statistically significant further change[xvii].

[Table 9 about here]

[Table 10 about here]

Table 11, finally, summarizes the reform impact estimates on the on-time graduation rate $OGR_{ji(t+n-1)}$ (6). The introduction of the reform is estimated to produce an average increase from 5.6 to 17.7 p.p. of the on-time graduation rate. Specification I yields the lowest impact estimate of 5.6 p.p., while specification IV yields the highest impact estimate of 17.7 p.p.. The size and the direction of such estimate range is not surprising: as explained in the previous section, specification I is the one that is the most exposed to the attrition bias issues that can lead to some possible overestimation of on-time graduation rate among pre-reform students (resulting therefore in an underestimation of the

impact of the reform). At the expense of introducing the potential for some small time-varying omitted variable bias, specifications II, III and IV progressively eliminate such attrition bias issues, resulting in progressively larger impact estimates of the reform (with specification IV being completely free from any attrition bias).

[Table 11 about here]

## 6. Cost-Elasticity Figures

In this section we argue in favor of interpreting the degree duration shortening of the Italian reform in terms of a reduction of the educational costs related to accessing the first-tier of university studies (from now on, referred to as BA degrees). This allows us to translate our impact estimates in terms of cost-elasticity of the participation to higher education, thus: i) enlarging the policy relevance of the results outside Europe, to all those countries where families bear most of the cost of higher education; and ii) allowing our estimates to be compared with the bulk of the existing literature.

Our arguments begin by sketching the pay-off tree that high-school graduates face upon graduation. For the sake of simplicity, we can imagine that high-school graduates decide whether to participate to higher education upon a comparison between their budget *b* and the cost of getting the BA degree, described by a generic cost function *c(BA)*. When the cost exceeds the budget, students can only decide to participate to the labor market *L*. Instead, when *b* is larger than *c(BA)* they have to further ascertain whether participation into higher education is worth its price, i.e. whether the payoff of getting a BA degree -– identified by a function *w(BA)* – is larger than the payoff of directly entering the labor market, or not. In the former case students end up enrolling into a tertiary degree; otherwise, they start looking for a job. We can represent this decision process as follows:

- *if* $E[b] < E[c(BA)] \rightarrow L$
- *if* $E[b] \geq E[c(BA)]$*:*

*either* $E[w(BA)-c(BA)] \geq E[w(L)] \rightarrow BA$

*or* $E[w(BA)-c(BA)] < E[w(L)] \rightarrow L$

Because the reform could not have any direct effect on $E[b]$ and $E[w(L)]$, its impact on enrolments depends therefore on how the reform has affected the labor market value of the BA degree $E[w(BA)]$ and its expected costs $E[c(BA)]$.

For what it concerns $E[w(BA)]$, a number of reasons lead us to deem that it was not majorly affected by the Italian reform. First, by adopting a three-year duration for its BA degrees, Italy aligned to the bulk of European countries, allowing its post-reform students to rely upon a higher education system that is directly comparable and compatible throughout the whole European Higher Education Area, and thus spendable on a much larger labor market [Mas-Colell 2003]. Second, interviews to key informants throughout Europe show that much of the reduction from four-year degrees to three-year ones occurred through the elimination of duplicated or redundant courses, and through the reduction of inefficiencies, and that employers do not have problems in accepting that graduates have spent one year less within the university system [Reichert and Tauch 2005]. Third, as in the aftermath of the reform, and for at least a few years after its enforcement, both pre- and post-reform students coexisted within the same classes, there was no possibility for the latter to face easier exams than the former; in other words there was no reason for post-reform students to expect that one year of tertiary education would be easier than before. Fourth, the ministerial address notes that accompanied the introduction of the Bologna process in Italy ruled that the new three-year BA degrees would be considered equivalent to pre-reform BA degrees for jobs in the public sector that require a university degree for application; this is of main relevance, as in many countries a large share of the demand for graduate workers comes from the Public Administration. Fifth, in spite of the low absolute number of graduates, young Italian graduate workers suffer from a well-documented problem of over-education [Cedefop 2012; Isfol 2011], which reinforces the expectation that perspective employers would not discriminate post-reform graduates. Sixth, as

empirically shown in Bosio and Leonardi [2011], the Italian reform did not reduce the employment probability of BA graduates, with no statistically significant change estimated for the female graduates and a positive change of 7 percentage points estimated for male graduates; the impact on wages was comparable in size (from 4 to 7 percentage points) for both males and females, but opposite in sign.

In the view of the payoff tree sketched above, such arguments make us focus on the expected cost *E[c(BA)]* as the primary driver of the impacts we have detected. The reduction of the statutory duration of the BA curricula from four or five years to three years entails a one/two-year reduction of the direct cost of participating into higher education. The Italian National Statistical Office [Istat 2005] finds that in the years after the introduction of the reform, the direct cost of participating to higher education was on average about €2,300 per year (including fees, housing, transports, board, books and educational materials). However, such yearly cost reduction of €2,300 applies directly solely to the students that did not intend to enroll in second-tier higher education studies. For the high-school graduates with a certain commitment to enroll both into the first-tier and, subsequently, into second-tier of university studies, the Italian reform did not entail a clear-cut shortening of the overall duration of their total higher education path. This is because the reform introduced a statutory duration of two-years for the second-tier degrees, de-facto substituting an array of already-existing master degrees with a typical statutory duration ranging from 12 to 18 months. Data from Almalaurea – the Italian research agency devoted to monitoring post-graduation choices of university students[xviii] – show that during the years right after the introduction of the reform (i.e. 2004-2006, the first years for which we have information on post-reform graduates) the share of BA graduates that did not enroll into secondary-tier studies was about 24%. Indeed, the reduction of the duration of the Italian BA degrees provided some advantages also for the high school graduates that intended to enroll both into the first-tier studies and, subsequently, into the second-tier studies. This is because the shortening of the first-tier degree duration reduces the students' risk of failing to complete the planned university studies without achieving any degree

that can be actually exploited on the job market. Nevertheless, we use such 24% figure as a conservative estimate of the fraction of students for which the Italian reform entailed a clear reduction of the direct educational costs of the BA degree. This leads to an (unweighted) average yearly cost reduction estimate of about €2,300 x 0.24 = €552. Because in the years across the introduction of the reform the percentage of high-school graduates that enrolled in the BA curricula with a pre-reform duration of five years (mainly those in engineering and architectural studies) was about 21%, the (weighted) average reduction in the direct educational costs entailed by the reform can be estimated in the order of €668= [€552*0.79] + [(€552 x 2)*0.21].

In addition to reducing the direct educational costs, the Italian reform also entailed a reduction of the opportunity costs of enrolling into a BA degree. Such opportunity-cost reduction can be estimated as follows. Almalaurea data show that the first-tier post-reform students who graduated in the 2004-2006 period and who decided to enter directly into the job market after graduation recorded an average net monthly wage of about €1,070, with an employment frequency of 83%. Thus, for these students the net return for entering the labor market one year in advance was about €1,070 x 12 x 0.83 = €10,600. Because, as previously mentioned, only about 24% of first-tier graduates actually entered the labor market right after their three-year BA, and because 21% of such graduates were enrolled in fields of study for which the reform entailed a two-year degree duration reduction, the average total opportunity-cost reduction implied by the reform can be estimated in about €3,078 = [€10,600 x 0.24 x 0.79] + [ (€10,600 x 2) x 0.24 x 0.21]. Thus, the overall educational cost reduction entailed by the reform can be conservatively estimated in €3,746 = [€668 + €3,078]. Using OECD Purchasing Power Parities for private consumption averaged over the period 2001-2006 ($1 = €0.9), this figure amounts to $4,162.

Such cost-reduction figures enables our enrolment impact estimates to be translated in the following cost-elasticities of the participation to higher education: a $1,000 reduction in the total educational costs entails a 2.5-2.6 p.p. enrolment increase in the short run (one year after the cost-reduction occurrence), and a 6.6-7.0 p.p. increase in the steady state (subsequent to the second year

of the cost-reduction occurrence). Overall, these cost-elasticity estimates are of the same degree of magnitude of those of other empirical studies: in the US, as reviewed by Deming and Dynarski [2009], the most reliable existing literature point toward an impact of about 4 additional p.p. in the number of high-school graduates that enroll into tertiary education, every $1,000 reduction in educational costs. As surveyd by Neill [2009] and Hübner [2012], in Canada and other European countries, such short-run semi-elasticity ranges from a negligible effect to figures close to the 4 additional p.p. every $1,000 estimated for the US. Moreover, in our estimates the marked difference between the short-run and the steady-state cost-elasticity figures suggests that a learning process on the part of the perspective students is in place to determine a greater cost-sensitivity of participation to higher education after more than one academic year elapsed since the initial cost reduction occurrence.

## 7. Conclusions

In this paper we use a unique department-level administrative database to exploit a shift in the degree duration of the first-tier of university studies provided by a national university reform, implemented in Italy with an exogenous timing, an unintended random delay-of-treatment scheme and the absence of potential attrition bias issues. The reform followed a European decision (referred to as the «Bologna Process») to standardize and harmonize the European university systems, and determined a reduction (from 4/5 years to 3 years) of the duration of the Italian first-tier degrees. Such degree duration reduction is of high policy relevance as it shares the features of an intervention to ease access to higher education. This is because in the aftermaths of the introduction of the reform, the job market value of the first-tier of the university studies was not perceived to be majorly affected, and because no capacity constraints or enrolment access barriers (other than the budget constraints on the part of high school graduates) were in place in the Italian university system in the fields of study affected by the reform.

The analysis estimates the impact of the degree duration reduction of the Italian reform on enrolment, retention and on-time graduation outcomes to produce empirical evidence for informing policies aimed at increasing higher education participation. To the best of our knowledge, this is the first time that such empirical evidence is derived from very desirable natural experiment conditions that apply to a policy intervention occurring on the national scale in a large economically-developed country. This bears unprecedented external validity to the analysis.

Our results show that, in its first year of introduction, the degree-duration shortening brought by the reform generated an increase in the counterfactual baseline yearly change of first-year enrolment between 10.3 and 10.9 percentage points (p.p.). In its second year of introduction, the reform generated an additional increase of 17.0 to 18.4 p.p., leading to a permanent increase of 27.3 to 29.3 p.p. in the subsequent steady state. To compare our results to the bulk of the existing literature on participation to higher education, we translated these estimates into cost-elasticities. In this regard, the overall size of our cost-elasticity figures (from 2.5 to 2.6 p.p. and from 6.6 to 7.0 p.p. of enrolment increase every $1,000 cost reduction, in the short-run and in the steady-state situations, respectively) appears in line with the existing literature. The dynamics of our estimates, however, suggest that the cost-sensitivity of participation to higher education may be affected by a learning process on the part of the students that lasts more than one academic year. This implies that the steady-state elasticity is larger – more than twice as much – than the short-run one. Thus, analyses based on short-run evaluations of shifts in fees and financial aids may somehow result in downward-biased elasticity estimates.

Our retention rate and on-time graduation estimates show that the positive impact on first-year enrolment brought by the reform did not occur at the price of a deterioration of retention and graduation outcomes. Such outcomes, to the contrary, were also positively affected by the reform; this is consistent with lowering educational costs in a scenario of invariant students' skill distributions[xix].

These findings reinforce the rationale that supports policy interventions to ease access to higher education on two grounds. First, because the cost-elasticity of the participation to higher education, in the steady state, is shown to be higher than what previously indicated by short-run estimates. Second, because the policy-induced enrolment gains are not shown to be achieved at the expense of enlarging the higher-education participation to insufficiently-skilled students. An occurrence that would result in a lack of social efficiency gains. Rather, our results suggests that such participation gains increase social-efficiency as well, likely improving the higher education opportunities for high-school graduates with adequate academic skills but binding budget constraints.

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***Table 1. Aggregated Descriptive Statistics: Enrolment (First-Year Enrollees)***

| Pre-Reform[(a)] | Post-Reform[(b)] | | New High School Graduates | | |
|---|---|---|---|---|---|
| Total N. per Year (1=1 student) | Total N. per Year (1=1 student) | Δ Pre-Post Reform | Pre-Reform Yearly Total N. (1=1 student) | Post-Reform Yearly Total N. (1=1 student) | Δ Pre-Post Reform |
| 208,526 | 243,985 | 17,0% | 465,086 | 451,867 | -2,8% |

(a) The pre-reform period for early-adopters is 1998/99-1999/00. For delayed-adopters is 1998/99- 2000/01.

(b) The post-reform period for the early-adopters is 2000/01-2004/05. For delayed-adopters is 2001/02- 2004/05.

***Table 2. a) Aggregated Descriptive Statistics: Retention Rate after One-Year*** [(a)]

| Pre-Reform[(b)] 1yr Retention Rate (1=1%) | Post-Reform[(c)] 1yr Retention Rate (1=1%) | Δ Pre-Post Reform (1= Δ p.p.) |
|---|---|---|
| 76.25 | 78.57 | 2.32 |

***Table 2. b) Aggregated Descriptive Statistics: Retention Rate after Two-Years*** [(a)]

| Pre-Reform[(b)] 2yr Retention Rate (1=1%) | Post-Reform[(c)] 2yr Retention Rate (1=1%) | Δ Pre-Post Reform (1= Δ p.p.) |
|---|---|---|
| 67.95 | 72.57 | 4.62 |

(a) For increasing comparability, the one-year and two-year retention rate figures reported in the Table are computed on the set of 276 departments that were always in existence over the entire 1998/99-2004/05 estimation period.

(b) The pre-reform period for early-adopters includes the cohorts of 1st –year students enrolled in 1998/99-1999/00. For delayed-adopters the cohorts of 1st-year students enrolled between 1998/99 and 2000/01.

(c) The post-reform period for the early-adopters includes 1st-year students enrolled between 2000/01 and 2004/05. For delayed-adopters the cohorts of students enrolled between 2001/02 and 2004/05.

***Table 3. Aggregated Descriptive Statistics: On-Time Graduation[(a)]***

| Pre-Reform[(b)] % of Students that Graduate On-Time (1=1%) | Post-Reform[(c)] % of Students that Graduate On-Time (1=1%) | Δ Pre-Post Reform (1= Δ p.p.) |
|---|---|---|
| 29.88 | 40.13 | 10.25 |

(a) For increasing comparability, the one-year and two-year retention rate figures reported in the Table are computed on the set of 276 departments that were always in existence over the entire 1998/99-2004/05 estimation period.

(b) The pre-reform period for early-adopters includes the cohorts of 1st –year students enrolled in 1998/99-1999/00. For delayed-adopters the cohorts of 1st-year students enrolled between 1998/99 and 2000/01.

(c) The post-reform period for the early-adopters includes 1st-year students enrolled between 2000/01 and 2004/05. For delayed-adopters the cohorts of students enrolled between 2001/02 and 2004/05.

***Table 4. Early-Adopter Vs. Delayed-Adopter in the Pre-Reform period***

| | Delayed-Adopters (1) | Early-Adopters (2) | Δ (1)-(2)[(a)] |
|---|---|---|---|
| Yearly % Δ of 1st-Year Enrollees (1=1%) | -1.10<br>(22.81) | -6.09<br>(11.59) | 4.99<br>[0.29] |
| 1yr Retention Rate (1=1%) | 77.50<br>(10.45) | 77.73<br>(6.72) | -0.23<br>[0.92] |
| Yearly Δ of 1yr Ret. Rate (1=1 p.p.) | -0.47<br>(10.52) | 1.56<br>(9.32) | -2.03<br>[0.39] |
| 2yr Retention Rate (1=1%) | 69.61<br>(12.93) | 68.82<br>(9.50) | 0.79<br>[0.78] |
| Yearly Δ of 2yr Ret. Rate (1=1 p.p.) | -0,40<br>(14.13) | -0,85<br>(13.84) | 0.46<br>[0.88] |
| On-Time Graduations (1=1%) | 29.08<br>(23.22) | 32.72<br>(16.02) | -3.64<br>[0.48] |
| Yearly Δ On-Time Graduation (1=1 p.p.) | 6.17<br>(10.15) | 4.17<br>(10.70) | 2.00<br>[0.39] |

( ) = standard deviations.

[ ] = P-values of the two tails T-Test for statistical significance (independent samples).

(a) ***99% significant, **95% significant, *90% significant

***Table 5: Model Specifications ($\Delta X_{it}$, $X_i$) for the Impact Estimates on Enrolment***

| | Specification | | | |
|---|---|---|---|---|
| | (I) | (II) | (III) | (IV) |
| ***Control variables $\Delta Xit$*** | | | | |
| CUR n. of degree curricula offered by dept. i | x | x | x | x |
| HSGRD n. of high-school graduates in the same region of dept. i | x | x | x | x |
| COMP competition within the same province of dept. i (linear) | x | x | | |
| COMP$n$ $\in$ {0,1} $n$ = *1,2,3* compet. within same prov. (3 dummies) | | | x | x |
| REGDP regional GDP (linear) | | x | | |
| REGDP$n$ $\in$ {0,1} $n$ = *1,2,3,4* regional GDP (4 dummies) | | | | x |
| ***Control variables $X_i$*** | | | | |
| SIZE of dept. i (linear) | x | x | | |
| SIZE$_s$ $\in$ {0,1} $s$ = *1,2,3,4* (4 size-class dummies) | | | x | x |
| FIELD$_m$ $\in$ {0,1} $m$ = *1,2,....,24* field of study of dept. i (24 dummies) | x | x | x | x |
| MAIN $\in$ {0,1} dummy for main campus location of dept. i | x | x | x | x |
| CAP $\in$ {0,1} dummy for location of dept. i in a regional capital city | x | x | x | x |
| REG$r$ $_i$ $\in$ {0,1} $r$ = *1,2,....,20* regional location of dept. i (20 dummies) | x | x | x | x |

***Table 6: Model Specifications ($\Delta X_{it}$, $X_i$) for the Impact Estimates on Retention Rates***

| | Specification | | | |
|---|---|---|---|---|
| | (I) | (II) | (III) | (IV) |
| ***Control variables $\Delta X_{it}$*** | | | | |
| CUR n. of degree curricula offered by dept. i | x | x | x | x |
| HSGRD n. of high-school graduates in the same region of dept. i | x | x | x | x |
| COMP competition within the same province of dept. i (linear) | x | x | | |
| $COMP_n \in \{0,1\}$ $n = 1,2,3$ compet. within same prov. (3 dummies) | | | x | x |
| REGDP regional GDP (linear) | | x | | |
| $REGDP_n \in \{0,1\}$ $n = 1,2,3,4$ regional GDP (4 dummies) | | | | x |
| ***Control variables $X_i$*** | | | | |
| SIZE of dept. i (linear) | x | x | | |
| $SIZE_s \in \{0,1\}$ $s = 1,2,3,4$ (4 size-class dummies) | | | x | x |
| $FIELD_m \in \{0,1\}$ $m = 1,2,....,24$ field of study of dept. i (24 dummies) | x | x | x | x |

*Table 7: Model Specifications for the Impact Estimates on On-time Graduation*

| | **Early-Adopters (reform year=2000)** | | | **Delayed-Adopters (reform year=2001)** | | |
|---|---|---|---|---|---|---|
| | **Post-reform degr. $T_{ji}$=1** Cohorts of last-year enrollees (t+n-1) | **Pre-reform degr. $T_{ji}$=0** Cohorts of last-year enrollees (t+n-1) | | **Post-reform degr. $T_{ji}$=1** Cohorts of last-year enrollees (t+n-1) | **Pre-reform degr. $T_{ji}$=0** Cohorts of last-year enrollees (t+n-1) | |
| | | *4-yrs degr.* | *5-yrs degr.* | | *4-yrs degr.* | *5-yrs degr.* |
| Specification (I) | 2002 | 2002 | 2002 | 2003 | 2003 | 2003 |
| Specification (II) | 2002<br>2003 | 2001<br>2002 | 2002<br>2003 | 2003<br>2004 | 2002<br>2003 | 2003<br>2004 |
| Specification (III) | 2002<br>2003 | 2000<br>2001 | 2001<br>2002 | 2003<br>2004 | 2001<br>2002 | 2002<br>2003 |
| Specification (IV) | 2002<br>2003 | 1999<br>2000 | 2000<br>2001 | 2003<br>2004 | 2000<br>2001 | 2001<br>2002 |

***Table 8: Enrolment, Impact Estimates***

| | **Specification I** | **Specification II** | **Specification III** | **Specification IV** |
|---|---|---|---|---|
| Observations | 2007 | 2007 | 2007 | 2007 |
| Prob. > F | <0.0001 | <0.0001 | <0.0001 | <0.0001 |
| | | | | |
| ***Treatment variables*** | | | | |
| First year of reform | **0.104**** | **0.103**** | **0.109**** | **0.109**** |
| | **(0.0514)** | **(0.0513)** | **(0.0512)** | **(0.0512)** |
| Second year of reform | **0.170**** | **0.170**** | **0.181**** | **0.184**** |
| | **(0.0750)** | **(0.0749)** | **(0.0746)** | **(0.0746)** |
| Third year of reform | 0.106 | 0.098 | 0.120 | 0.117 |
| | (0.0954) | (0.0954) | (0.0950) | (0.0953) |
| Fourth year of reform | 0.084 | 0.082 | 0.102 | 0.102 |
| | (0.1143) | (0.1142) | (0.1138) | (0.1140) |
| Fifth year of reform | 0.130 | 0.128 | 0.151 | 0.151 |
| | (0.1335) | (0.1333) | (0.1328) | (0.1329) |
| ***$X_i$ and $\Delta X_{it}$ control variables*** | | | | |
| Absolute change of the number of degree curriculum | **0.034***** | **0.033***** | **0.034***** | **0.034***** |
| | **(0.0036)** | **(0.0036)** | **(0.0036)** | **(0.0036)** |
| Log-change of the number of high-school graduates | 0.217 | 0.232 | 0.168 | 0.186 |
| | (0.1884) | (0.1883) | (0.1880) | (0.1892) |
| Log-change of regional per-capita GDP | | **-1.260**** | | |
| | | **(0.5123)** | | |
| Log-change of regional per-capita GDP in 1st quartile | | | | 0.031 |
| | | | | (0.0264) |
| Log-change of regional per-capita GDP in 2nd quartile | | | | 0.023 |
| | | | | (0.0239) |
| Log-change of regional per-capita GDP in 3rd quartile | | | | 0.014 |
| | | | | (0.0186) |
| Initial department size | **>-0.001***** | **>-0.001***** | | |
| | **(<0.0001)** | **(<0.0001)** | | |
| Initial dept. size in 1st quartile | | | **0.109***** | **0.109***** |
| | | | **(0.0203)** | **(0.0203)** |
| Initial dept. size in 2nd quartile | | | **0.042**** | **0.042**** |
| | | | **(0.0176)** | **(0.0176)** |
| Initial dept. size in 3rd quartile | | | **0.028*** | **0.028*** |
| | | | **(0.0160)** | **(0.0160)** |
| Absolute change of the number of rival departments | **-0.009**** | **-0.009**** | | |
| | **(0.0043)** | **(0.0043)** | | |
| The number of rival departments is unchanged | | | 0.005 | 0.003 |
| | | | (0.0251) | (0.0252) |
| The number of rival departments has grown | | | -0.002 | -0.005 |
| | | | (0.0256) | (0.0258) |
| Main department base | | | | |
| Region main city | 0.015 | 0.015 | 0.018 | 0.018 |
| | (0.0133) | (0.0133) | (0.0130) | (0.0130) |
| Constant | **0.085*** | **0.106**** | -0.021 | -0.030 |
| | **(0.0444)** | **(0.0452)** | (0.0537) | (0.0549) |

**Source:** own estimations on USTAT data. Notes: regional and school fixed effects are not displayed; standard errors in second lines; ***99% significant, **95% significant, *90% significant

***Table 9: One-Year Retention rate, Impact Estimates***

| | **Specification I** | **Specification II** | **Specification III** | **Specification IV** |
|---|---|---|---|---|
| Observations | 1796 | 1796 | 1796 | 1796 |
| Prob. > F | 0.020 | 0.018 | 0.041 | 0.063 |
| ***Treatment variables*** | | | | |
| First year of reform | **0.093*** (0.0333)** | **0.094*** (0.0333)** | **0.092*** (0.0333)** | **0.091*** (0.0333)** |
| Second year of reform | 0.014 (0.0472) | 0.015 (0.0472) | 0.011 (0.0473) | 0.011 (0.0473) |
| Third year of reform | -0.007 (0.0580) | -0.004 (0.0580) | -0.014 (0.0582) | -0.013 (0.0584) |
| Fourth year of reform | 0.001 (0.0672) | 0.003 (0.0672) | -0.007 (0.0675) | -0.005 (0.0677) |
| Fifth year of reform | 0.046 (0.0759) | 0.048 (0.0759) | 0.035 (0.0761) | 0.035 (0.0762) |
| **$X_i$ and $\Delta X_{it}$ control variables** | | | | |
| Absolute change of the number of courses offered | -0.003 (0.0024) | -0.002 (0.0024) | -0.003 (0.0024) | -0.002 (0.0024) |
| Log-change of regional per-capita GDP | | 0.431 (0.3262) | | |
| Log-change of regional per-capita GDP in 1st quartile | | | | -0.008 (0.0156) |
| Log-change of regional per-capita GDP in 2nd quartile | | | | -0.003 (0.0142) |
| Log-change of regional per-capita GDP in 3rd quartile | | | | -0.013 (0.0119) |
| Initial department size | <0.001 (<0.0001) | <0.001 (<0.0001) | | |
| Initial dept. size in 1st quartile | | | -0.014 (0.0128) | -0.013 (0.0128) |
| Initial dept. size in 2nd quartile | | | -0.006 (0.0108) | -0.006 (0.0108) |
| Initial dept. size in 3rd quartile | | | -0.002 (0.0103) | -0.002 (0.0103) |
| Absolute change of the number of rival departments | 0.003 (0.0023) | 0.003 (0.0023) | | |
| The number of rival departments is unchanged | | | -0.015 (0.0167) | -0.015 (0.0167) |
| The number of rival departments has grown | | | -0.009 (0.0079) | -0.009 (0.0079) |
| Region main city | -0.004 (0.0080) | -0.004 (0.0080) | -0.004 (0.0079) | -0.005 (0.0079) |
| Constant | **-0.070*** (0.0244)** | **-0.079*** (0.0252)** | **-0.050* (0.0274)** | -0.042 (0.0288) |

**Source:** own estimations on USTAT data. Notes: school fixed effects are not displayed; standard errors in second lines; ***99% significant, **95% significant, *90% significant

***Table 10 : Two-Year Retention Rate, Impact Estimates***

| | **Specification I** | **Specification II** | **Specification III** | **Specification IV** |
|---|---|---|---|---|
| Observations | 1486 | 1486 | 1486 | 1486 |
| Prob. > F | <0.0001 | <0.0001 | <0.0001 | <0.0001 |
| ***Treatment variables*** | | | | |
| First year of reform | **0.291*** (0.0403)** | **0.292*** (0.0403)** | **0.290*** (0.0403)** | **0.289*** (0.0403)** |
| Second year of reform | 0.027 (0.0571) | 0.028 (0.0571) | 0.024 (0.0572) | 0.025 (0.0573) |
| Third year of reform | 0.012 (0.0702) | 0.017 (0.0703) | 0.012 (0.0705) | 0.018 (0.0709) |
| Fourth year of reform | 0.044 (0.0820) | 0.047 (0.0820) | 0.044 (0.0824) | 0.051 (0.0826) |
| **$X_i$ and $\Delta X_{it}$ control variables** | | | | |
| Absolute change of the number of courses offered | -0.002 (0.0030) | -0.001 (0.0030) | -0.002 (0.0030) | -0.002 (0.0030) |
| Log-change of regional per-capita GDP | | 0.549 (0.4551) | | |
| Log-change of regional per-capita GDP in 1st quartile | | | | -0.005 (0.0229) |
| Log-change of regional per-capita GDP in 2nd quartile | | | | 0.014 (0.0205) |
| Log-change of regional per-capita GDP in 3rd quartile | | | | -0.008 (0.0158) |
| Initial department size | <0.001 (<0.0001) | <0.001 (<0.0001) | | |
| Initial dept. size in 1st quartile | | | -0.016 (0.0170) | -0.015 (0.0170) |
| Initial dept. size in 2nd quartile | | | -0.001 (0.0143) | -0.001 (0.0144) |
| Initial dept. size in 3rd quartile | | | -0.001 (0.0136) | -0.001 (0.0137) |
| Absolute change of the number of rival departments | -0.002 (0.0036) | -0.002 (0.0037) | | |
| The number of rival departments is unchanged | | | -0.002 (0.0201) | -0.002 (0.0202) |
| The number of rival departments has grown | | | -0.015 (0.0205) | -0.016 (0.0206) |
| Region main city | -0.002 (0.0106) | -0.002 (0.0106) | -0.003 (0.0104) | -0.003 (0.0105) |
| Constant | **-0.105*** (0.0347)** | **-0.117*** (0.0359)** | **-0.086** (0.0426)** | **-0.082* (0.0439)** |

**Source:** own estimations on USTAT data. Notes: school fixed effects are not displayed; standard errors in second lines; ***99% significant, **95% significant, *90% significant

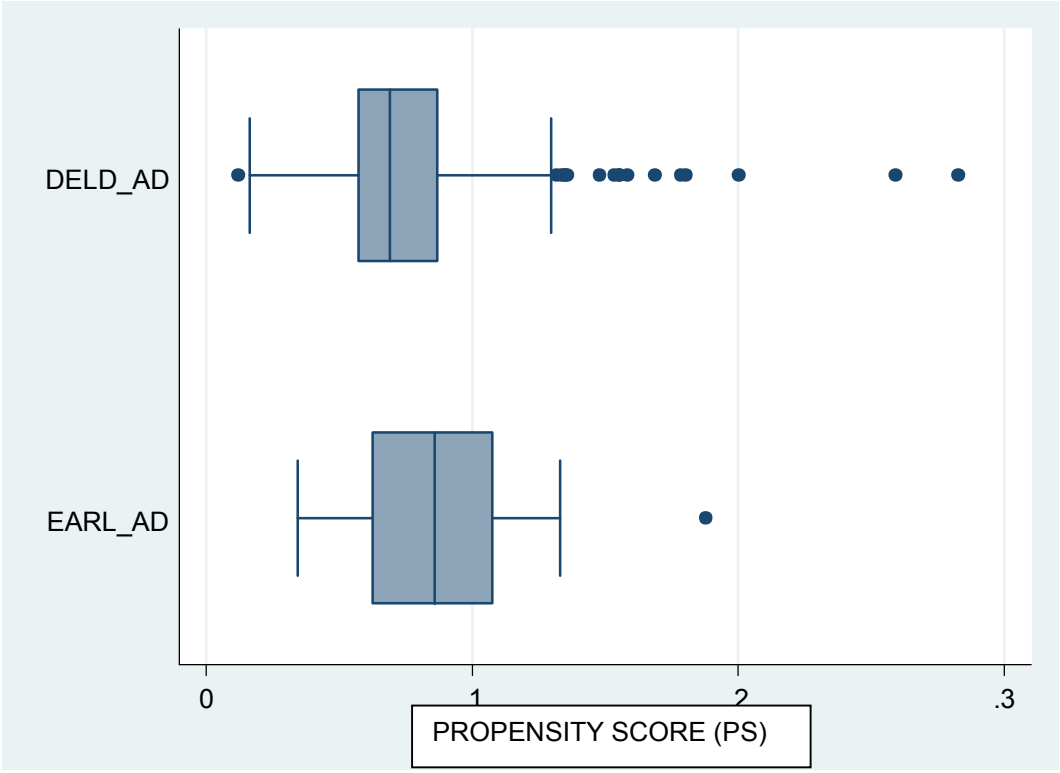
***Table 11: On-Time Graduation Rate, Estimate Results***

| | **Specification I** | **Specification II** | **Specification III** | **Specification IV** |
|---|---|---|---|---|
| Estimated impact | **0.0563*** ** | **0.0634*** ** | **0.1171*** ** | **0.1767*** ** |
| $\chi^2$ | 636.34 | 1652.16 | 6679.86 | 17062.05 |
| Yates-adjusted $\chi^2$ | 636.10 | 1651.89 | 6679.35 | 17061.25 |

**Source:** own estimations on USTAT data.

Notes: ***99% significant, **95% significant, *90% significant

*Figure 1: PS Distributions for Early-Adopter and Delayed-Adopter Departments*



[i] See the White House official web-site: http://www.whitehouse.gov/issues/education/higher-education.

[ii] With the exclusion of ISEF ("Higher Institutes for Physical Education") and the Universities for Foreign Aliens..

[iii] The other countries were: Albania, Bosnia-Herzegovina, Estonia, Hungary, Norway, Portugal, Romania, Spain and The Netherlands.

[iv] Until the end of the Eighties, all Italian universities offered solely a one-tier degree system composed exclusively by highly specialized degree curricula (called "Laurea"). These "Laurea" degrees were offered in all fields of study and they were accessible right after high school. Their typical statutory duration was four years, with the exception of the medical degrees (six years), and veterinarian, engineering and pharmacy studies (five years). The changes provisioned by the Bologna Process can indeed be considered an actual duration shortening of the first-tier university degrees because such one-tier system changed gradually, from the second half of the Eighties onward, thanks to the introduction of a large number of master degrees and Ph.D. programs. By the time the reform was introduced, at the end of the Nineties, the Italian university system was indeed composed, de facto, by a two-tier system, with the (still at least four-year long) "Laurea" degrees resembling first-tier undergraduate studies topped by an abundant offer of Master degrees available in a vast majority of fields of study. On the European and North-American job markets, the Italian four-year long "laurea" degree was indeed considered equivalent to a BA degree in most disciplines.

[v] As also detailed in the next section, the reform left unchanged the duration of only the six-year long medical degrees and the five-year long degrees of Veterinarian Medicine, and Pharmaceutical

Studies. For the degrees in Law, the actual statutory duration shifted from the pre-reform four years to a total of five years in the post-reform period.

[vi] Ph.D. programs were maintained at a statutory duration of three years.

[vii] With the exclusion of ISEF ("Higher Institutes for Physical Education") and the Universities for Foreign Aliens.

[viii] This choice is dictated by the fact that the single degree curricula changed denominations between the pre-reform and the post-reform years. The Departments unaffected by the reform (i.e. that ceased operating before the academic year 2001/02 or that started operating after the academic year 2002/03) were excluded from the analysis. However, these departments were taken into consideration for constructing the control variables that capture changes in the competition faced by each unit of observation in attracting first-year enrollees. Since a number of Italian *Departments* offered separate degree curricula in one or more peripheral campuses, in order to maintain a more precise geographical allocation of the data, we opted to code as separate units of observation those secondary peripheral campuses of a same *Department* that met the following criteria: a) continuous and uninterrupted existence over the period 1998/99 -2004/05; b) a total number of registered students, in the same period, above the 5% threshold of the distribution of Italian departments/schools. The secondary campuses that did not meet these criteria were not considered as separate units of observations and their data were aggregated to those of the respective main campuses.

[ix] The statutory duration of the five- or six-year long degrees of the Departments of Medicine, Veterinarian Medicine, and Pharmaceutical Studies was maintained unchanged by the reform. The Departments of Law were excluded as their four-year long pre-reform degrees were substituted by one single five-year long degree for students willing to achieve the same legal eligibility to practice law in the Italian court system and sustain the bar examination.

[x] 23 Departments are not included as units of observation because they did not operate in the period across the introduction of the reform (by either ceasing before the academic year 2001/02 or starting after 2002/03). 128 additional departments are excluded because they offered exclusively Law, Medicine, Veterinarian Medicine, or Pharmaceutical Studies degrees that were unaffected by the reform.

[xi] For ease of interpretation of the coefficients, the first differenced variables $\Delta\rho_t$ are rescaled to resume the familiar binary configuration $\rho_t \in \{0,1\}$. The pre-reform period (1998-1999) is the baseline time embedded in the intercept.

[xii] For ease of coefficient estimate interpretation, the first-differencing transformed variables ($\Delta Tc_{it}$) are rescaled to resume a usual binary configuration $Tc_{it} \in \{0,1\}$.

[xiii] The 24 fields of study coded for the analysis are as follows: mathematical, natural and physical sciences, maritime sciences; industrial chemistry, pharmaceutical studies; medicine, engineering, architecture, agricultural studies, veterinarian medicine, sociology, political sciences, law, Italian studies, preservation of cultural goods, psychology, environmental sciences, economics, educational sciences, modern languages and foreign literature, foreign languages for interpreters, paleography and philology, communication and show business studies, statistics, business administration, banking and insurance sciences, cultural goods, biotechnology, technological sciences, philosophy, oriental studies, human sciences, musicology, arts and design, social sciences.

[xiv] Italian students can graduate in most universities in three different graduation sessions throughout the academic year. These graduation sessions are traditionally set in the Summer of the last academic year in which the students are enrolled and in the subsequent Fall and Winter/Spring. E.g. for the first-year students newly enrolled in academic year t=2000/01, the available graduation sessions for completing on time a three-year degree are the following: Summer03, Fall03, Winter04. Because the USTAT data contains information on the calendar year in which students graduate, we operationalized on-time graduation to include all the graduating sessions of the

academic year t+n (e.g. Summer03, Fall03, Winter/Spring04, Summer04 and Fall04 would be the on-time graduation sessions for the students of a $n$ = 3-year degree curriculum who were new enrollees in year t = 2000/01).

[xv] Nevertheless, we also estimated the reform impact on the percentage of first-year newly enrolled students who graduate on time. For the sake of brevity, these results (which are comparable to the ones estimated on the OGR variable) are omitted from this paper and available upon request to the corresponding author.

[xvi] Such omitted variable bias would be in the event of major changes systematically taking places within each same department between the 2001-2003 period and the 2003-2005 period. These changes would have to involve factors such as the degree of difficulty of the final exams or the quality of the teaching and teaching assistance services that could affect the likelihood of on-time graduation independently from the introduction of the reform. However, since there is no evidence suggesting that any of such possible changes took place in the Italian departments sharply across the first two triennium of the last decade, we feel confident that the omitted variable bias risk is very small also when choosing to minimize the potential for attrition bias.

[xvii] As mentioned in section 2, the data available for the analysis cover the academic years from 1998/99 to 2004/05. For this reason the impact estimates for the two-year retention rates cannot include the fifth year of introduction of the reform.

[xviii] See www.almalaurea.it.

[xix] Under the assumption that the distribution of the students' skills remain unchanged, a reduction of the educational costs, such as the one implied by the Italian reform, is expected to affect retention rate and on-time graduation as follows. For the retention rate outcomes, assuming time-consistency of decisions, students would drop out from universities only if some unexpected negative shock (some "bad news") occurs. For any given amount of time elapsed since first-year enrolment, the remaining time until graduation is shorter after the reform. Any "bad news" is therefore easier to

face after the reform, as it must be dealt with for a shorter period: following a negative budget shock, for instance, students and their families have to rely upon savings for a shorter period, which means that they need a smaller amount of savings in order to survive within tertiary education. For on-time graduation outcomes, the implications of the educational cost reductions brought by the reform derive from the fact that in the Italian university system, students are allowed to be formally enrolled in their next year toward graduation even without having successfully passed all the required courses of the previous years. Assuming that students accrue an average of $k$ uncompleted courses per academic year, their "debt" at the expected date of graduation amounts to $4k$ or even $5k$ courses in the pre-reform situation, and to just $3k$ courses after the reform is implemented. This makes the actual time to graduation shorter after the reform, and eases the capability of students and their families to survive to a negative shock.