

Backward Spillovers and Equalized Access: The Bologna Process across Italy’s Education System

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Abstract

This paper estimates the causal effects of the Bologna Process reform in Italy, analyzing the impact of the introduction of the “3+2” degree structure at different stages of the educational system. We implement multiple Difference-in-Differences designs on a nationally representative dataset composed of more than 140,000 high school graduates and 220,000 university graduates. We find that the reform increased graduations from the academic-oriented high school track by +5.50 p.p., revealing a “backward spillover effect” on high school completion. For university-related outcomes, we estimate a positive effect on enrollments (+5.53 p.p.) and in-time graduations (+7.22 p.p.) and a negative effect on dropout (−2.70 p.p.). The effects are always larger for students coming from the most disadvantaged parental backgrounds, particularly when combined with high ability. These findings suggest that the Bologna Process was effective in reducing barriers to educational investment, especially for talented students with financial constraints, thus challenging the narrative of the reform attracting low ability students. Finally, descriptive labor-market analyses show a convergence of performances for graduates with different parental backgrounds, but only among Bachelor’s graduates.

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1 Introduction

The Italian university system entered the 2000s in a critical state: low enrollment rates, high dropout rates, a concerning length of study, and low completion rate. These elements explain why only about 9% of Italians aged between 25 and 64 had tertiary education in 1999 (OECD, 2002), less than half of the OECD average (20.5%). In this context, in 2001, the Bologna Process reform completely reshaped the structure of the Italian university system with the objective of harmonizing it to the other European systems both in the characteristics and in the performance. The main innovation introduced by the reform was the new “3+2” degree structure, explicitly inspired by the university systems of the Anglo-Saxon countries. In 2001, the old long one-tier degree with a four- or five-year length (the *Laurea*) was replaced at the national level by a two-tier degree structure formed by a three-year Bachelor’s degree, followed by a two-year Master’s degree. Whether, in what way, and who was affected by this historic reform is still an open empirical question. Although a well-established literature on the topic analyzed the Italian case, prior research answers only part of the questions, while the public debate remains polarized and often skeptical on the reform, suggesting that the available evidence has yet to settle the issue, especially when discussing who was affected by the reform.

We use a Difference-in-Differences design that leverages the parental background of high school and university graduates to identify variations in exposure to the reform. We apply this method to nationally representative surveys and we provide causal evidence that the Bologna Process reform significantly affected the Italian educational system at different stages. At the university level, the reform increased enrollment by 5.53 p.p., a substantial increase considering the pre-reform enrollment rate of 44.44%. The reform also reduced the dropout rate by −2.71 p.p. (from a pre-reform value of 16.14%), and increased the share of in time graduation by 7.22 p.p. (which averaged 17.42% before the reform). But the effects of the Bologna Process were not limited to the university. For the first time, we provide evidence that the reform had a “backward spillover effect” on the choice and completion of the high school track, significantly increasing by 5.50 p.p. the graduations from the academic-oriented high school track (*Liceo*), which before the reform accounted for 27.51% of all high school graduates.

Moreover, when analyzing how the effects of the Bologna Process unfolded among the treated population, we find that the effects were systematically greater for students from the most disadvantaged parental backgrounds: +7.81 p.p. increase in graduations from the academic-oriented track; while for university, we report a +6.61 p.p. increase in enrollment; −3.22 p.p. reduction in dropout; and +10.88 p.p. increase in in-time graduation. We also observe that within the treated group, cohorts who had yet to choose the high school track at the time of the implementation of the reform consistently experienced larger effects than the cohorts who had already chosen the high school track when the policy was enacted, emphasizing again the relevance of the early tracking system and its link to higher education. Gains increase monotonically in the ability for both high school track and university enrollment, with high ability students benefiting the most from the reform. The interaction of parental background and ability reveals a clear pattern: the effects were the largest among students with high ability from the most disadvantaged backgrounds, providing evidence that the reform successfully attracted highly talented students who were

lacking resources.

For what concerns the labor market outcomes, we provide descriptive evidence that, among Bachelor’s graduates, those coming from low parental backgrounds closed the existing gap in wage and unemployment rate with those from a high parental background. However, such convergence did not happen among Master’s graduates, for which a significant gap still exists in both dimensions.

The existing literature on the effects of the Bologna Process in Italy mainly focuses on university-related outcomes, reaching only partial agreement. At the same time, research on the effects of the Bologna Process on labor market outcomes is very scarce, and evidence of the effect on other stages of the educational system does not exist. In terms of enrollment, most studies document the existence of a positive and significant effect (Kroher et al., 2021). Using survey data representative at the national level, Cappellari and Lucifora (2009) estimate an increase between 9% and 15% using a before/after comparison, while Di Pietro (2012) estimates it between 4.9% and 7.5% through a Difference-in-Differences. Finally, Bondonio and Berton (2018) analyzing administrative panels at the department-level with a triple Difference-in-Differences report a positive effect between 14.5 and 17.3 p.p.. For what concerns inequality in accessing higher education, the evidence is mixed in direction and persistence of the effect: Argentin and Triventi (2011) and Brunori et al. (2012) find that the reform successfully reduced inequality in access but only temporarily, while Triventi et al. (2017) find an increase in social stratification between majors. Research on student’s performance is extremely limited, but generally finds positive results on in-time graduations (Bondonio and Berton, 2018), while evidence for dropout is mixed: Cappellari and Lucifora (2009) do not find significant effects of the reform, while other authors find either a small decline in dropout rate (d’Hombres, 2007; Di Pietro and Cuttillo, 2008) or an increase in completion probability (Chies et al., 2019). The literature on the labor market performance of the new degrees is very limited, mainly descriptive and heterogeneous in its findings. Descriptive studies report negative impacts on employment (Sciulli and Signorelli, 2011; Angeloni, 2019), while Bosio and Leonardi (2010) find improvements and Scandolo (2025) reports no significant effects. The results for wages are similarly inconsistent, with Bosio and Leonardi (2010) finding a reduction in graduate premium, and Scandolo (2025) estimating an increase of 1.5%–2.5%, but only for female graduates.

Comparative analyses with other countries that made similar changes reveal that the evidence around the reform is heterogeneous and often context-specific. The literature on Germany does not find any effect of the Bologna Process on enrollment (Horstschräer and Sprietsma, 2015), nor does it find any significant heterogeneity by students’ background (Neugebauer, 2015). Horstschräer and Sprietsma (2015) and Lerche (2016) estimate a significant reduction in dropout, driven mainly by high-achieving students (Enzi and Siegler, 2016), while an increase is estimated for in-time graduations (Lerche, 2016; Hahm and Kluve, 2019), and mixed results are found for performance (Hahm and Kluve, 2019; Mühlenweg, 2010). For Portugal, the results are closer to those found for the Italian case: Cardoso et al. (2007) and Portela et al. (2008) find a positive and significant effect on increasing demand for Bachelor’s degrees, heterogeneous by field of study. Outside of Italy, labor market evidence is again very limited and largely descriptive. In Germany, Neugebauer and Weiss (2018) report higher occupational prestige and entry

earnings of post-reform Bachelor’s graduates, who nevertheless face higher early unemployment risks. For Portugal, a descriptive study by [Suleman and Figueiredo \(2020\)](#) highlights the emergence of a significant wage and quality gap once Bachelor’s and Master’s degrees began competing in the labor market. A relevant exception is [Avdeev \(2025\)](#), who estimates the causal effects of the Bologna Process reform in Russia on the return to the new degrees. The author finds no significant average effect but substantial gender heterogeneity, with females experiencing higher labor market returns.

Although previous studies on the Italian case cover many dimensions of the Bologna Process reform, several crucial gaps remain. Based on the results we present, this paper makes two explicit contributions to the existing literature on the effects of the Bologna Process, also bridging it to the strands of the literature on early tracking, intergenerational mobility in education, and access to higher education.

Our first contribution is a systematic analysis of the impact of the Bologna Process in Italy within a coherent and unified framework. We apply a single identification strategy, using the same treatment definition to nationally representative data on high school and university graduates to estimate the effects of the reform throughout the educational pathway: from the choice and completion of high school track, through university participation and performance, to early labor market outcomes. Furthermore, the fragmented analyses of different segments of the educational pathway done in the literature ignores the fact that in a rigid early-tracking system like the Italian one, changing the incentives of university access may feed back into high school track choice and completion. At the age of 14, students - but most likely their parents - are required to choose between three high school tracks: academic-oriented (*Liceo*), technical (*Istituto Tecnico*), and vocational (*Istituto Professionale*). When making the choice, they know that only the academic-oriented track (*Liceo*) has a clear predetermined pathway to higher education, given that vocational education at the tertiary level is practically non-existent in the Italian system ([Di Pietro, 2004](#); [Dustmann, 2004](#); [Brunello and Checchi, 2007](#); [Checchi and Flabbi, 2007](#)). In a framework like the one described, a reform that makes university more accessible is likely to affect the choice and retention behavior in high school tracks. Through our analysis, we provide for the first time causal evidence that the Bologna Process generated a significant “backward spillover effect” on earlier stages of the Italian educational system, specifically affecting the probability of choosing and graduating from an academic-oriented high school track.

Our second contribution is to assess the medium-term impact of the Bologna Process reform using nationally representative surveys of high school and university graduates up to ten years after the reform, which have only been partially exploited so far. Moreover, when looking at the medium-term effects of the reform, we answer the crucial question of who has been affected the most by the Bologna Process. Specifically, we analyze the heterogeneous impact of the reform along two central dimensions of inequality: parental background, for which previous work provides only mixed and inconsistent evidence on unequal access ([Argentin and Triventi, 2011](#); [Brunori et al., 2012](#)); and student ability, for which, to our knowledge, no causal evidence exists. Finally, for labor market outcomes, we provide descriptive results that take into account the difference between Bachelor’s and Master’s graduates. This distinction, which has always been overlooked in the existing literature, allows us to address and, for the first time, shed

light on the selection issue that arises when comparing graduates from the new system with those from the old one.

The remainder of the paper is organized as follows. Section 2 describes the Italian education system before 2001 and the features of the Bologna Process reform. Section 3 introduces the ISTAT datasets used and presents the variables. Section 4 sets out the identification strategy and its features. Section 5 presents the results for the outcomes of interest, together with the heterogeneity and robustness analyses. Section 6 presents the conclusion.

2 Institutional background

The Italian educational system is composed of three cycles. The first is compulsory, begins at age 6 and consists of five years of primary school and three years of middle school.¹ This cycle - common for all students - usually ends at the age of 14, is free of charge, and is regulated at the national level. After the first compulsory cycle, the transition from middle school to high school is the most relevant educational investment decision in the rigid Italian early tracking system. The choice of the high school track for the 14-year-old student (but most likely for their families) is threefold: the academic-oriented track (*Liceo*), explicitly designed to prepare for university; the technical track (*Istituto Tecnico*), a labor market oriented program which prepares for white-collar jobs; and the vocational track (*Istituto Professionale*), also labor market oriented, which prepares students for blue-collar jobs. Each of these tracks lasts five years, is completely free, and is regulated at the national level. What is particularly relevant in this transition is that the choice of high school track is completely unrestricted: there are no limitations based on previous performance, there are no entry tests, and teacher recommendations are non-binding. A well-established literature highlights how in this unregulated early tracking environment the choice made at such a young age strongly amplifies the role of parental background in track allocation (Brunello and Checchi, 2007). This happens through a “diversion effect” (Hanushek and Wößmann, 2006), for which students coming from a high parental background tend to enroll in academic-oriented tracks, while students coming from a low parental background tend to enroll in vocational tracks, regardless of their ability (Checchi and Flabbi, 2007; Triventi and Trivellato, 2009; Carlana et al., 2022).

After completing the chosen five-year high school track and passing the high school final exam (usually at the age of 19), the student is awarded a high school diploma which has legal value and allows one to enroll in university. Before the implementation of the Bologna Process reform in 2001, the university degree was a long one-tier program (the *Laurea*) with a length of four or five years depending on the field of study. Access to university was (and still is) essentially unrestricted: no restriction on performance,

¹ For the cohorts we analyze, compulsory schooling is 8 years (5 of primary and 3 of middle school), thus generally from 6 to 14 years old. A short-lived attempt to extend it in 1999 (Law 9/1999) was repealed in 2003 (Law 53/2003), and the first effective change came only in 2007 (D.M. 139/2007) when compulsory schooling was raised to 10 years. However, this change only affected cohorts graduating from high school after 2012, and therefore does not concern our sample.

no entry tests, and no restriction on the high school track attended. Selective entry exists only for a small set of programs with *numerus clausus* (e.g. Medicine) and very few private institutions. However, despite open access, existing studies show that the high school track of origin is the dominant predictor of university participation, as attending the academic-oriented track (*Liceo*) constitutes *de facto* the main and most direct path to access higher education (Dustmann, 2004; Brunello and Checchi, 2007). At the beginning of the 2000s, more than 88% of students who graduated from *Liceo* enrolled in university, compared to only 17.8% of students who came from vocational tracks (Checchi and Flabbi, 2007). A pattern that reflected in the population of university enrollees: graduates from *Istituto Professionale* and *Istituto Tecnico* represented 33.7% of university students, despite being almost 60% of the high school graduates population. The reason is structural: the Italian higher education system is almost entirely university-based and tertiary level vocational programs are nearly absent: in 1999, less than 1% of high-school graduates enrolled in tertiary vocational programs (OECD, 2000). Therefore, the only clear predetermined path to pursue tertiary education is to enroll in the academic-oriented track.² The combination of all these elements explains the strong intergenerational persistence in educational attainment, which also operates through the crucial role of parental background in the choice of high school track (Checchi et al., 1999; Brunello and Checchi, 2007; Argentin and Triventi, 2011; Aina, 2013). Financing also matters. Although most universities are public, pursuing tertiary education is not free while the financial support provided to students is very limited and student loans are practically nonexistent. Consistent with these institutional features, the period was characterized by low tertiary attainment, a considerably high dropout rate, and long times to degree, causing delay in entry into the labor market (OECD, 2002, 2025). Detailed information about the institutional background is provided in the Appendix.

In this context, Law 509/1999 implemented the Bologna Process nationwide from the academic year 2001/2002, implementing the principles of the Bologna Declaration signed in 1999. The reform aligned with the objective of establishing the European Higher Education Area (EHEA), a harmonized and common structure for European higher education systems, to be reached through a common degree structure, a unified credit system, and cooperation in quality assurance. The core institutional change in the Italian system was to replace the traditional long one-tier program (*Laurea*) of four or five years, with a two-tier system formed by a first-level Bachelor’s degree of three years (*Laurea Triennale*) followed by a second-level Master’s degree of two years (*Laurea Magistrale*). Implementation was mandatory at the national level across universities and fields, with only limited exceptions. Medicine and Law, for instance, kept the old long-cycle programs alongside the new two-tier degrees, with the former remaining by far the more commonly chosen option.

The reform automatically determined two direct consequences. First, the reduction in the costs of get-

² More recently, some steps have been taken to develop a competitive tertiary level vocational path, such as the reorganization of the ITS (*Istituti Tecnici Superiori*) in 2008 and the launch of the ITS Academy in 2022. However, this path remains marginal in students’ educational choices: only about 2% of high school graduates enrolled in these programs in 2021 (OECD, 2023), and in 2024 graduates from tertiary level vocational programs accounted for just 0.3% of the 25-34 population (OECD, 2025).

ting a degree. The reform reduced the legal duration of tertiary degrees, therefore automatically reducing the costs (direct, indirect, and non-monetary) of the educational investment. Second, the reform gave universities more autonomy. Each institution could create, reorganize, or eliminate programs more flexibly, thereby expanding and diversifying the course offer and improving the match between the demand for and supply of university courses (d’Hombres, 2007; Di Pietro and Cutillo, 2008). Both these channels should lead to an increase in participation in higher education as the cost of the educational investment is reduced and, *ceteris paribus*, the investment is more productive. The expected effects on dropout, retention, and performance are more ambiguous *ex ante*, as they depend on the characteristics of those who comply with the change in incentives. On the one hand, a shorter program can mechanically reduce the dropout rate by converting some dropouts occurring in the fourth or fifth year into completions, but it can also induce a compliance effect, creating higher incentives to stay in university and reducing psychological cost (Bondonio and Berton, 2018; Chies et al., 2019; Hahm and Kluve, 2019). Moreover, a richer offer of university courses due to higher autonomy may further reduce mismatch-driven dropouts (d’Hombres, 2007; Di Pietro and Cutillo, 2008). On the other hand, reducing the non monetary costs of higher education could attract low ability students, possibly increasing dropout due to the reduction in the quality of the new enrollees.

3 Data

In order to analyze the effect of the Bologna Process reform in Italy, we use two ISTAT survey series: the “Surveys on Educational and Professional paths of Upper Secondary School graduates” and the “Survey on Employment outcomes of University graduates”. Both are nationally representative and are composed of six waves of students who graduated between 1995 and 2011 (1995, 1998, 2001, 2004, 2007, and 2011), allowing us to observe the composition and characteristics of high school and university graduates before and after the implementation of reform in 2001. All graduates are interviewed three years after their graduation and provide information about their parental background, educational choices, and professional experiences after graduation. Each wave of the dataset on high school graduates covers between 4% and 6% of the total number of graduates in the respective year, and the complete dataset of high school graduates, after the cleaning process, consists of 141,016 observations (Table A1 in the Appendix). For university graduates, each survey wave covers between 16% and 21% of the total population of university graduates in the corresponding year, with the complete dataset consisting of 228,652 observations (Table A2 in the Appendix). Detailed information about the cleaning process of both datasets is provided in the Appendix.

The ISTAT surveys we use allow us to observe nationally representative samples of high school and university graduates before and after the reform was implemented. Both sets of surveys report information on gender, age at graduation, residence, high school and university location, score in middle school and high school final exam, high school track attended, if ever rejected, if ever enrolled in university

and in which course, if ever dropped out, if graduated in time or the years of delay. In addition, the university graduates dataset includes information about previous labor market experiences and current situation: employment status, wage, hours worked, job mobility, type of contract, vertical and horizontal mismatch. And, most importantly for our research question, the surveys contain a lot of information about the parental background of the graduate. For both the graduates' parents, the surveys provide educational level, employment status, job category, and if they are employee or self-employed. Detailed descriptive statistics about the sample used are in the Appendix (Table A3 for high school graduates and Table A4 for university graduates in the Appendix). All survey variables are reclassified to the most detailed level of aggregation common across all survey years, thereby producing harmonized variables across all surveys on both the types of graduates and eliminating cross-survey discrepancies. In addition, some new variables are created. First, we aggregate the different *curricula* in which students graduate from high school in three macro categories (namely *Istituto Professionale*, *Istituto Tecnico*, and *Liceo*) to have comparable high school tracks across surveys. This categorization does not include high school *curricula* specifically designed to train primary teachers and artists (*Istituti Magistrali* and *Istituti d'Arte*, respectively). The Field of Study of the program attended in the university is categorized into the four standard groups used in the literature: STEM; Health and Psychology; Social Sciences and Law; and Humanities. The occupation of the parents is reclassified into the common four classes we usually find in the literature: entrepreneur, high-skilled, white-collar, and blue-collar.

4 Identification strategy

The purpose of this study is to estimate the causal effects of the implementation of the Bologna Process in Italy. To do so, we use as part of our identification strategy the Difference-in-Differences method on the dataset of high school and university graduates covering the period before and after the introduction of the reform described in the previous section.

From the previous section, we know that the exposure to the reform was universal for all high school students graduating from 2001 onward, since the new “3+2” degree structure was implemented nationwide in the academic year 2001/2002, and the adoption was mandatory for all universities. Hence, all high school students graduating in 2001 or later are treated by the reform, which does not directly lend itself to spatial variation of the treatment. To address this issue, we follow the approach of other papers in the literature on the economics of education (Belot et al., 2007; Di Pietro, 2012), which consists in assigning the treatment with respect to the heterogeneity of expected effects. As previously stated, the Bologna Process determined a reduction of the legal duration (from four/five years to three), and thus a reduction of the cost of getting a tertiary level degree. The idea underlying our identification strategy is that the cost reduction induced by the reform affected students differently depending on their parental background. Specifically, we expect the cost reduction to significantly affect the educational investment decision of students from a disadvantaged parental background, as relaxing the financial constraints to

access higher education is likely to be relevant in changing their enrollment behavior. Conversely, for students who come from a more privileged parental background, the same cost reduction related to the new university degree is unlikely to affect their enrollment decision. Actually, given the high intergenerational persistence of education in Italy, we can imagine that they would have enrolled in university anyway, regardless of the degree structure. For this reason, in our identification strategy, students coming from disadvantaged parental background will be defined as treated by the reform while students coming from more advantaged parental background will be used as controls. The reason behind the choice of the parental background as the relevant dimension of heterogeneity in expected effects comes naturally from the literature. An extensive descriptive and causal literature indicates parental background as a crucial determinant of educational investment, operating through many channels such as liquidity constraints, risk aversion, peers, and information access.³ Moreover, a substantial body of evidence indicates that *ceteris paribus* low parental background individuals tend to invest less in education than their high parental background peers, regardless of their ability. In particular, in our analysis we use parental education as a proxy for parental background, a choice that follows the example of several studies in the literature and which is particularly suited for the Italian context, given the high persistence in educational attainment across generations and low upward educational mobility. Therefore, the treatment is defined as follows: individuals who do not have any of their parents with a university degree (low parental background) are defined as treated; while individuals who have at least one parent with university degree (high parental background) are defined as controls. The same treatment definition is used for the analysis of high school graduates and university graduates.

For what concerns the time dimension of the Difference-in-Differences, our analysis is two-fold as we deal with two different definitions of exposures to the reform for high school graduates and university graduates. For the time dimension of high school graduates, we consider as exposed to the reform the students who graduated from high school in 2001 or later, since they faced the new “3+2” degree structure when making their enrollment choice after high school graduation. Therefore, for high school graduates, we define as pre-reform the cohorts of graduates in 1995 and 1998, while we define as post-reform the cohorts of graduates in 2001, 2004, 2007, and 2011. However, for the time dimension of university graduates, we define as exposed to the reform the students who graduated in 2004 or later, as this is the first available cohort of students who enrolled after the reform and graduated with a “new” degree established by the reform. For this reason, the time dimension of university graduates is shifted by one wave: we define as pre-reform the cohorts of graduates in 1995, 1998, and 2001; while we define as post-reform the cohorts of graduates in 2004, 2007, and 2011. Finally, to avoid issues related to wrong categorization of pre- and post-reform graduates in the analysis of university graduates, we keep as pre-reform graduates only those who enrolled before the reform and graduated in a “old” one-tier degree and

³ See Björklund and Salvanes (2011) for a synthesis of the literature on the role of parental background in educational investments. A non-exhaustive list of studies on the mechanisms includes Carneiro and Heckman (2002) for liquidity and credit constraints, Dohmen et al. (2011) for risk preferences, Chetty et al. (2016) for peer and neighborhood effects, and Bettinger et al. (2012) for information frictions.

we keep as post-reform graduates only those who enrolled after the reform and graduated in a “new” two-tier degree.⁴

We exploit three complementary Difference-in-Differences designs to observe the short- and long-term effects of the reform: a two-period, a three-period, and a flexible year-by-year Difference-in-Differences design. The two-period Difference-in-Differences has the following form:

$$Y_i = \alpha + \beta D_i + \gamma T_i + \delta D_i T_i + \mu \mathbf{X}_i + \phi_{g(i)} + \lambda_{t(i)} + \varepsilon_i \quad (1)$$

with Y_i denoting the outcome of interest for individual i ; D_i being the treatment dummy, which takes the value 1 for those individuals who do not have any of their parents holding a university degree, and 0 for those who have at least one parent with university education; T_i being the post reform dummy, which is equal to 1 if the graduate was exposed to the reform; $\mathbf{X}_i$ being the vector of control variables; $\phi_{g(i)}$ represents the group fixed effects with the groups defined as the different combinations of parental education,⁵ which allow us to account for group-specific time-invariant differences; $\lambda_{t(i)}$ represents the survey fixed effects, which allow us to account for the cohort-specific variations common between the groups. In this model, δ provides an estimate of the causal effect of the reform on the outcome variable, under the Difference-in-Differences assumptions discussed later.

In the three-period Difference-in-Differences (which for reason of data limitation can be estimated only for high school graduates and not for university graduates) we consider one pre-reform period (Pre) and we distinguish between two post-reform periods (Post 1 and Post 2). This design allows us to observe if there is a difference in the effects between those who graduate in the Post 1 period (2001, 2004), which we define treated “surprised”; and those who graduate in the Post 2 period, which we define treated “announced” (2007, 2011). The reason for this distinction within the treated group lies in the moment in which the reform was implemented with respect to the choice of the high school track. For the treated “surprised” (Post 1 period), the reform took place when they were already in high school and, therefore, when they had already chosen the high school track; while for the treated “announced” (Post 2 period), the reform was implemented before the choice of the school track. In order to allow the two groups to have two different estimates of the causal effect of the reform, we modified the previous model in the

⁴ In our sample of university graduates we deal with two dimensions of treatment: enrolling before or after the introduction of the reform and graduating in an “old” or “new” type of degree. In order to avoid a wrong categorization of individuals, we choose to consider only the “normal” and most common paths: those who enrolled before the reform and graduated in an “old” degree (thus excluding those who enrolled before the reform and then changed type of degree, graduating in a new course); and those who enrolled after the reform and graduated in a “new” degree (thus excluding those who enrolled after the reform and graduated in an old degree, i.e. the few courses that kept the old structure).

⁵ The two variables measuring mother’s and father’s education have four modes: no education/primary school, middle school, high school, and university or more. We interact these two variables to create 16 groups of parental education, each corresponding to one possible combination of parental education and control for group fixed effects.

following way:

$$Y_i = \alpha + \beta D_i + \gamma_1 T_i^{(1)} + \gamma_2 T_i^{(2)} + \delta_1 D_i T_i^{(1)} + \delta_2 D_i T_i^{(2)} + \mu \mathbf{X}_i + \phi_{g(i)} + \lambda_{t(i)} + \varepsilon_i \quad (2)$$

with D_i , $\mathbf{X}_i$, $\phi_{g(i)}$, and $\lambda_{t(i)}$ defined as above; $T_i^{(1)}$ being the Post 1 dummy, which takes value 1 for treated “surprised”; $T_i^{(2)}$ being the Post 2 dummy, which takes the value 1 for treated “announced”. In this specification, δ_1 provides an estimate of the causal effect of the reform on the selected outcome for treated “surprised” with respect to the pre-reform graduates and δ_2 provides an estimate of the same causal effect for treated “announced” with respect to the pre-reform graduates.

Finally, a flexible year-by-year Difference-in-Differences is implemented to estimate the cohort-specific effects of the reform on the outcomes of interest and to observe the evolution of the causal effects in the different cohorts. The flexible Difference-in-Differences design uses as reference the last available cohort before the reform: so the cohort of 1998 for the high school graduates and the cohort of 2001 for university graduates. The regression used for the estimation is as follows:

$$Y_i = \alpha + \beta D_i + \sum_{s \neq -1}^T \gamma_s T_i^{(s)} + \sum_{s \neq -1}^T \delta_s D_i T_i^{(s)} + \mu \mathbf{X}_i + \phi_{g(i)} + \varepsilon_i \quad (3)$$

with D_i , $\mathbf{X}_i$, $\phi_{g(i)}$ defined as above; $T_i^{(s)}$ being the cohort-specific dummy variable, and δ_s the respective estimates of the causal effect of the reform for each cohort with respect to the reference cohort.

For all of the models presented, to examine how the effects of the reform unfold within the treated population with respect to the parental background dimension, we consider an additional specification that allows for heterogeneous effects within the treated group. Starting from the definition of treated individuals (graduates who do not have any of their parents with university education), we define two subgroups: those with the lowest parental education (Lowest PE) and those with medium parental education (Medium PE). Individuals with the lowest parental education are graduates who have both parents without high school education, while graduates with medium parental education have at least one parent who completed high school, but none of the parents with a university degree. The models presented above are modified accordingly to include an estimation of the specific effect on each of the two subgroups.

We use the models described on a set of outcomes: high school track, enrollment in university, dropout from university, enrollment mobility, in time university graduation, wage, hourly wage, and unemployment. The first four variables are retrieved from the dataset on high school graduates, which provides information on the high school attended and the details of the enrollment in university; the last four variables are retrieved from the dataset on university graduates, which provides information on the details of the university graduation as well as on early labor market outcomes. High school track is a variable that can take three different values (*Istituto Professionale*, *Istituto Tecnico*, and *Liceo*) and is the track from which the student *graduated* in high school; thus, it is a direct measure of completion of the high school track and only indirectly a measure of the choice of the track. Enrollment in university is a binary variable that takes the value one if the high school graduate ever enrolled in university in the three years

between the high school graduation and the interview. Dropout is a binary variable that takes the value one if the high school graduate after the enrollment ever dropped out of a university program in the same period. Enrollment mobility is a binary variable that takes the value one if the high school graduate enrolled in a university located in a different region than the one of the high school. In time graduation is a binary variable that takes value of one whenever the university graduate finished the program within the legal duration declared by the university. For what concerns wages, we consider the net monthly wage expressed in 2015 euros, and using hours worked, we retrieve the hourly wage expressed in the same units. Finally, unemployment is a binary variable that takes the value one if the university graduate is unemployed at the time of the interview.

We consider as our main specification the model including only group fixed effects and cohort fixed effects, without any control variable. The reason for this choice is to avoid bad controls, since many variables related to educational choices and performance, as well as professional experiences, are very likely to be outcomes of the same treatment that we are analyzing. To ensure the robustness of our results, we also estimate two different specifications of our models with control variables. One specification (which is labeled “Var. pred.” in the tables in the Appendix) includes only variables that are “predetermined” with respect to the reform, i.e. those variables that cannot be influenced by the reform or by the parental education (gender, age, region of school/university). The other specification (labeled “All controls” in the tables in the Appendix) includes also the other covariates, which are likely to be bad controls (such as: high school track attended, middle school and high school final mark, rejection in high school, mother and father working conditions and occupation, etc.).

All specifications presented are logit models, and the estimates presented represent the average marginal effect. In addition, we present the Average Treatment Effect (ATE) as our primary result. This choice reflects the nationwide and universal nature of the Bologna Process reform, which affected the entire population rather than just the treated individuals. The comprehensive scope of the reform, coupled with our policy evaluation objective, led us to prioritize ATE over Average Treatment Effect on the Treated (ATT). To ensure additional robustness, we also estimated the ATT for all designs on all outcomes, which yield almost the same estimates. This close correspondence is consistent with the treatment being as good as randomly allocated and further supports the choice to focus on the ATE in the discussion of the results.

The design we are using requires a series of assumptions to correctly estimate the causal effect of the reform. First, the Parallel Trend Assumption (PTA) must be satisfied for all outcomes. To prove the credibility of this assumption, we provide the time series of the outcomes of interest by treatment status in Figures A1 – A4 in the Appendix, in which it is possible to see that, for the available data, the PTA holds in the pre-treatment period for all the outcomes. Second, agents must not anticipate the treatment by reacting to their expectations. We provide evidence that the agents did not anticipate the reform by performing a placebo test in which we estimate the effects of a fictitious treatment that occurred in the period before the actual treatment. As shown in Table A5 in the Appendix, for all outcomes we obtain non-significant estimates, which corroborate the assumption of no anticipation. Third, the composition

of the sample is stable as, by treatment definition, it is not possible for the observed individuals to move from the treated to the control group and vice versa. Fourth, the treatment timing is exogenous and well defined; and the treatment itself is irreversible (once treated, always treated). Fifth, there are no concurrent policies or shocks relevant to our identification that align with the treatment definition. Finally, all treated units are exposed to the reform at the same time, ruling out problems of staggered treatment or differential adoption across groups or cohorts.

5 Results

5.1 Educational outcomes

The upper panel of Table 1 reports the estimates of the causal effects of the Bologna Process using the two-period Difference-in-Differences. In the first column, we see that the reform had a statistically significant effect on increasing graduations from the academic-oriented high school track (*Liceo*) by +5.50 p.p., which is a sizable increase considering the pre-reform average to be 27.51%. The existence of this “spillover effect” of the reform on earlier stages of the educational system is a novel contribution to the literature on the Bologna process, as we provide evidence for the existence of a “backward spillover effect” of the reform on the high school track completion. This result can be interpreted as a combination of two mechanisms: increase in retention and direct choice. On the one hand, the reform made university degrees more accessible and less expensive and, therefore, increased the retention for students already enrolled in the academic-oriented track. On the other hand, the reform directly influenced the decision-making process for the choice of the high school track, pushing more students to choose the academic track. In this sense, we can imagine that families anticipated the decision of enrolling their children in university already at the moment of choosing the high school track, with the ultimate result of increasing the choice for the academic-oriented, which is the clear predetermined path to later access tertiary education.

The other columns report the estimates of the effects on university-related outcomes: enrollment in university, dropout from university, and in time graduation. In line with the existing literature, we find positive and significant effects of the reform on university enrollments, which are estimated to have increased by 5.53 p.p. (from a pre-reform enrollment rate of 44.44%). The positive result is consistent with what we could expect: the new degree structure reduced the legal duration and the costs associated with the university degree, therefore allowing more individuals to enroll into university. For what concerns the dropout rate (which averaged 16.14% in the pre-reform sample), we estimate a statistically significant reduction of −2.71 p.p. causally determined by the reform, which can be explained by arguments already presented in previous sections like the mechanical shortening of the course length, the compliance effect, and the better matching. This result is also informative given the already discussed *a priori* ambiguity on the expected effects: shorter programs and lower costs could either reduce dropout through a compliance and reduction of length channel or raise dropout if the reform attracted less prepared students. The

negative estimate suggests that the reform did not determine a sizable negative selection of low skilled students (who would have been more likely to dropout, therefore increasing the dropout rate on average) and did not deteriorate the quality of the incoming students. More evidence on this will be provided later. Finally, the last column shows the effect of the reform on the share of university students graduating within the legal duration of the degree. For this comparison to be as accurate as possible, we consider only Bachelor’s degree graduates for the post-reform period, so to compare productivity of first-level university degree graduates: “old” one-tier degrees before the reform and Bachelor’s degree after. The estimated effect of the Bologna process is an increase in the share of in time graduation of +7.22 p.p., which is a very significant increase considering that the share of in time graduations before the reform was only 17.42%. The positive effect we find can be explained using similar arguments that apply to the dropout rate reduction, such as the mechanical shortening of the degree’s length, the compliance effect, and the better matching.

Table 1: Two-period DiD on High School Track of graduation, and University Enrollment, Dropout and In time graduation

2-period Difference-in-Differences		High School	University		
		Track = Liceo	Enrollment	Dropout	In time grad.
Treatment	Treated x Post	0.0550*** (0.0128)	0.0553*** (0.0155)	-0.0271*** (0.0075)	0.0722*** (0.0184)
Treatment	Lowest PE	0.0781*** (0.0071)	0.0661*** (0.0143)	-0.0322*** (0.0069)	0.1088*** (0.0249)
Intensity	Medium PE	0.0312*** (0.0077)	0.0402*** (0.0146)	-0.0216*** (0.0065)	0.0468*** (0.0118)
Pre-reform average		27.51%	44.44%	16.14%	17.42%
Observations		134,730	141,016	73,392	166,976

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

To better understand how the effects of the reform unfold within the treated group, we present in the bottom part of Table 1 the results of the two-period Difference-in-Differences using the Treatment Intensity specification we presented in Section 4. In this version of the model, we divide the treated population into two subgroups: those with the lowest parental education (Lowest PE, neither of the parents ever completed high school) and those with medium parental education (Medium PE, at least one of the parents finished high school but neither of the parents has university education). The results obtained using this model exhibit the same pattern for all outcomes of interest: students with the lowest parental education are always affected the most by the reform. Indeed, this heterogeneous distribution

of the effect reveal a clear socioeconomic gradient, with students from more disadvantaged backgrounds being the ones most responsive to the incentives created by the reform. These estimates speak directly to the debate on who was affected the most by such reform, providing evidence in favor of the view that the Bologna Process benefited students coming from disadvantaged backgrounds. For the high school track of graduation, the overall effect of +5.50 p.p. on the treated group can be separated into +7.81 p.p. increase for those with the lowest parental education versus +3.12 p.p. for the medium parental education group, a remarkable 4.69 p.p. differential. University participation follows the same pattern. Enrollment increases by +6.61 p.p. for the Lowest PE group compared to +4.02 p.p. for the Medium PE group, while dropout is reduced by -3.22 p.p. for the Lowest PE group and by -2.16 p.p. for the Medium PE group. A considerable difference is also observable for the in time graduations: those with the Lowest PE experience an increase of +10.88 p.p., much larger than the +4.68 p.p. increase of the Medium PE.

In order to understand the effect of the reform on students' enrollment mobility, we apply the same Difference-in-Differences design considering as an outcome the change of the region to enroll in university (Table A6 in the Appendix). In the two-period specification, we find a small and statistically insignificant effect of the reform on the share of high school graduates who change region to enroll in university. The treatment intensity analysis reveals a stronger effect for individuals with the Lowest PE, who experience a -2.49 p.p. reduction in enrollment mobility, while the effect for the Medium PE group is not statistically significant (from a pre-reform share of mobile students of 18.77%). This pattern is consistent with our interpretation of the reform: by reducing the cost of university education, the reform attracted financially constrained students from disadvantaged backgrounds, who are more likely to enroll in local universities given the high costs of moving to another region to study.

In the analysis of treatment intensity, we consider two mutually exclusive subgroups of the treated population (Lowest PE and Medium PE) and estimate both effects jointly in a pooled specification. In this setting, if the two subgroups are characterized by different underlying trends or are exposed to subgroup-specific shocks, the joint estimation can mechanically affect the estimated effect for each intensity group. Therefore, as a robustness check, we re-estimate the model separately for each treated subgroup against the same control group using two different regressions: one regression comparing Lowest PE versus controls, excluding the Medium PE group; and the other regression comparing Medium PE versus controls, excluding the Lowest PE group. The results of this estimation in Tables A7 – A10 in the Appendix are almost identical to the standard specification of Table 1, suggesting that the findings are robust whether the effects are computed jointly or through separate subsample regressions.

All results presented in Table 1 are also robust to the inclusion of different sets of control variables as shown in Tables A11 – A14 in the Appendix. The estimations obtained including the control variables exhibit similar patterns in all the outcomes of interest except for in time graduation. Conditioning on predetermined covariates (i.e. those not affected by the reform) yields coefficients that are comparable but slightly smaller than the baseline without controls. Adding the complete set of controls, including the “bad” controls, typically reduces the estimated effects. This consistency in the results indicates robust-

ness to the inclusion of appropriate controls, increasing the credibility of the results presented, and aligns with the expected reduction of the effects when adding bad controls, which are likely to be outcomes of the same process and therefore partially absorb the effect of the reform on the outcomes.

As we said before, all the results of Table 1 are estimates of the Average Treatment Effect (ATE). As a robustness check we also compute the Average Treatment Effect on Treated (ATT) for all the outcomes of interest in Table A15 – A18 in the Appendix. What we observe when comparing the two is that the ATT estimates are almost identical to the ATE, with the ATT being slightly higher but not statistically different from the ATE. This close similarity of the two estimates suggests that in this setting the treatment can be considered as good as randomly allocated, and further supports our decision to focus on the ATE, consistent with the universal nature of the Bologna Process reform.

In Table 2 we use the three-period Difference-in-Differences to investigate how the effect of the reform unfolded over time. Specifically, we distinguish the post-reform cohorts in two periods: the treated “surprised”, who graduated in Post 1 and had already chosen the high school track when the reform was implemented; and the treated “announced”, who graduated in Post 2 period and thus had not chosen the high school track yet when the reform was rolled out.

Table 2: Three-period DiD on High School Track of graduation, University Enrollment and University Dropout

3-period Difference-in-Differences	High School	University	
	Track = Liceo	Enrollment	Dropout
Treated x Post 1 (Surprised)	0.0312*** (0.0088)	0.0380*** (0.0115)	-0.0203*** (0.0075)
Treated x Post 2 (Announced)	0.0739*** (0.0184)	0.0680*** (0.0210)	-0.0331*** (0.0060)
Pre-reform average	27.51%	44.44%	17.42%
Observations	134,730	141,016	73,392

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

This model specification reveals that the effects of the reform are systematically larger for the treated “announced” for all the outcomes. For the high school track, the probability of graduating from a *Liceo* increases by +7.39 p.p. for the treated “announced” and by +3.12 for the treated “surprised”. The University-related outcomes follow the same pattern: the enrollment rate increases by +6.80 p.p. for the treated “announced” and by +3.80 for the “surprised”; while the dropout rate shows a reduction of –3.31 p.p. for the treated “announced” and –2.03 for the “surprised”. For the high school track attended, we can interpret the higher effect for treated “surprised” with respect to the “announced” to partially disen-

tangle the two mechanisms: retention and direct choice. For the treated “surprised”, the implementation of the reform occurs after the choice of the high school track has been made, so the positive effect we estimate in graduating from the academic-oriented track (+3.12 p.p.) has to be attributed exclusively to the increased retention caused by the reform. In contrast, for the treated “announced”, the policy is implemented before the choice of the high school track, so the positive effect of the reform (+7.39 p.p.) is to be attributed also to the increased share of individuals directly choosing the academic track instead of the vocational one. Therefore, the difference between the effect of the treated “announced” and the treated “surprised” can be interpreted as the direct choice component of the effect and specifically as a measure of the increased share of students directly choosing the academic-oriented school track.

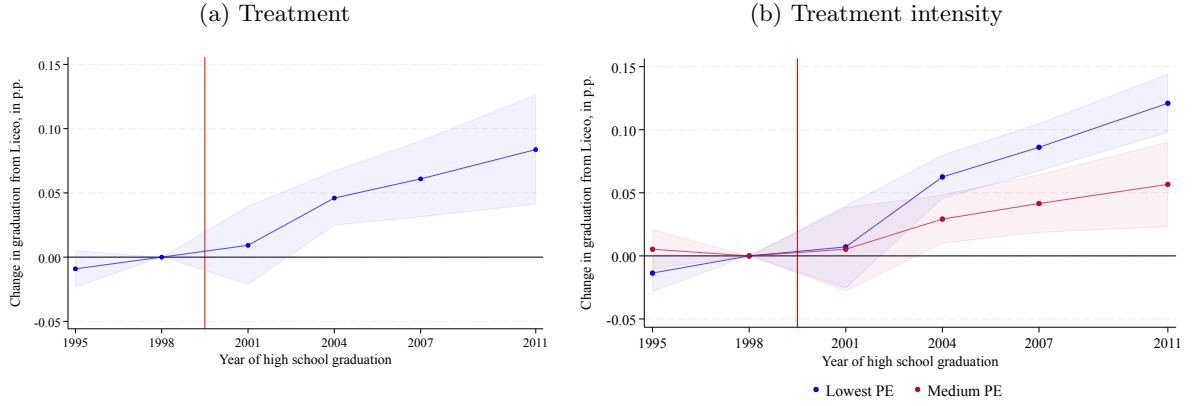
The treatment intensity by parental education (Lowest PE and Medium PE) using the three-period Difference-in-Differences specification is presented in Table A19 in the Appendix. For all outcomes, the estimates are consistent with the results presented before: within each period, the effects of the reform are always larger for students with the lowest parental education; and within each subgroup of treated individuals, the treated “announced” always experience larger effects.

A slightly different pattern emerges when looking at the three-period Difference-in-Differences design applied to students’ enrollment mobility (Table A6 in the Appendix). What we observe is that, in contrast to all the other outcomes, for enrollment mobility there is a stronger effect for the treated “surprised” (−3.22 p.p.) with respect to the “announced” (−0.85 p.p.) and that this effect is mainly driven by the “surprised” graduates with the Lowest PE (−4.35 p.p.). An explanation for these results is that the reform arrives after the high school track choice has already been made, leaving less time to plan and finance the relocation to a different region for study in university. As a result, additional entrants, who are financially constrained students from disadvantaged backgrounds, disproportionately choose local universities, mechanically reducing mobility. By contrast, cohorts exposed to the reform earlier in their educational path can adjust their plans and resources, so that the compositional shift toward low-mobility entrants is weaker and the reduction in mobility effect becomes non-significant.

The flexible year-by-year Difference-in-Differences specification allows us to see the evolution of the effects over time and observe the effects of the reform by cohorts. In Figures 1 and 2 the results of this model are plotted for two outcomes: graduating from the academic-oriented school track (*Liceo*) and enrolling in university, respectively. For both Figures, on the left (Figures 1a and 2a) we can see the estimated treatment effect on the entire treated population, while on the right (Figures 1b and 2b) we can observe the treatment intensity, with the blue line being the Lowest PE subgroup and the red line the Medium PE. For the other outcomes, the results are reported in Figures A5 and A6 in the Appendix. For all outcomes of interest, the estimations corroborate what was previously said. Under both specifications, post-reform coefficients move in the expected directions: the probability of graduating from the academic-oriented high school track rises, university enrollment increases, dropout from university falls, and the rate of in time graduations grows. These dynamics are consistent with the mechanisms discussed above and hold across outcomes. Moreover, the heterogeneity by parental background persists in the plots: effects for the Lowest PE subgroup are systematically larger than for the Medium PE subgroup,

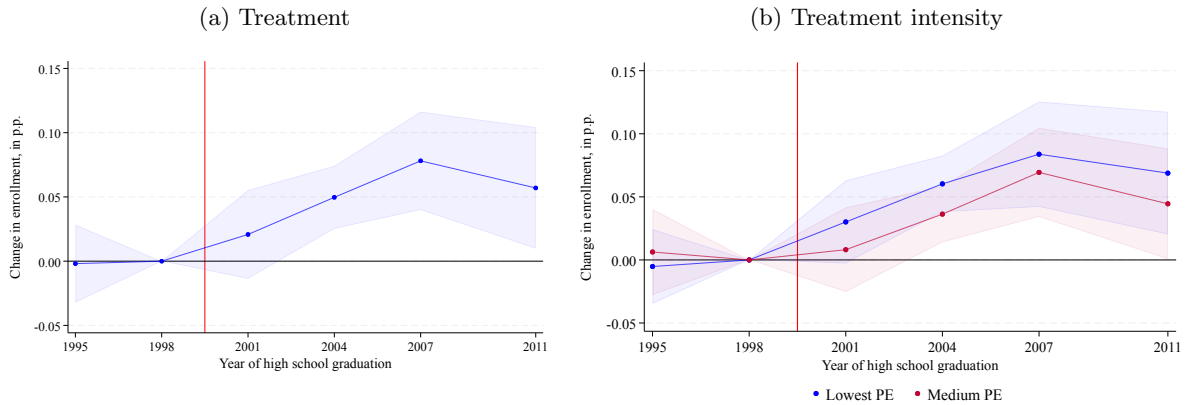
although year-to-year variations are not always individually significant. Finally, for all the outcomes, the pre-treatment estimated coefficients are not significant, providing additional support to the Parallel Trend Assumption.

Figure 1: Flexible year-by-year DiD on High School Track of graduation: Treatment and Treatment Intensity



Note: Survey of graduates in 1998 (last one before the reform) is considered as reference year for high school graduates. The shaded region denotes 95% confidence intervals constructed using standard errors clustered at the level of parental education. No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Figure 2: Flexible year-by-year DiD on University Enrollment: Treatment and Treatment Intensity



Note: Survey of graduates in 1998 (last one before the reform) is considered as reference year for high school graduates. The shaded region denotes 95% confidence intervals constructed using standard errors clustered at the level of parental education. No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

We have provided evidence supporting the thesis that the Bologna Process was effective in attracting students of low parental background, but we have yet to show which type of student in terms of ability was affected by the reform. To analyze this, we use as a measure of ability the score in the final exam of the previous educational stage. Therefore, for the high school track we consider the score of the middle school final exam, which has four categories ordered as follows: Sufficient, Good, Very Good, and Excel-

lent; while for all university-related outcomes we consider the score of the high school final exam, which ranges from 60 to 100.

In Table 3 we observe the heterogeneity in graduating from an academic-oriented school track (*Liceo*) by the score in the middle school final exam. All groups experience a statistically significant increase, but the effect is the largest for students who got ‘Very Good’ as a score on the final exam (+8.92 p.p.), while the other subgroups exhibit effects that are in line with the average effect on the full sample or significantly lower. When we consider the treatment intensity analysis, for each subgroup, the effect of the reform is always larger for students with the lowest parental education: a pattern consistent with previous results. As previously stated, forward-looking selection of high school tracks by graduates and their families is “backwardly” affected by the reform, and those who respond with stronger increases are high ability students coming from the lowest parental backgrounds. When expressed in relative terms with respect to the subgroup pre-reform average, the increase is of the same size for the lower part of the distribution (about +25% for ‘Sufficient’, +24% for ‘Good’, and +21% ‘Very Good’), while is smaller for ‘Excellent’ (about +6% in relative terms), consistent with a ceiling effect at high baseline graduation rates.

Table 3: Heterogeneity on graduating from the academic High School Track (*Liceo*) by Middle School Final Mark

Two-period DiD		Sufficient	Good	Very Good	Excellent	Full sample
Treatment	Treatment x Post	0.0276*** (0.0084)	0.0564*** (0.0189)	0.0892*** (0.0236)	0.0385** (0.0186)	0.0550*** (0.0128)
Treatment	Lowest PE	0.0378*** (0.0062)	0.0885*** (0.0120)	0.1171*** (0.0180)	0.0590*** (0.0161)	0.0781*** (0.0071)
Intensity	Medium PE	0.0160** (0.0080)	0.0205 (0.0138)	0.0577*** (0.0216)	0.0141 (0.0207)	0.0312*** (0.0077)
Pre-reform average		10.98%	23.47%	42.16%	65.24%	27.51%
Observations		47,460	40,865	27,323	25,158	141,106

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

We perform the same analysis on university enrollment in Table 4 to observe who benefits the most from the reform in terms of ability, sub-sampling by score bins in the high school final exam (60–69, 70–79, 80–89, 90–100). We observe a positive effect on all the final score distributions, as the reform causally increases the enrollment rates in all the score bins. But what we observe is that the estimated effect of the reform increases monotonically with the ability: the higher the score, the larger and more statistically significant the effect on university enrollment. Those who got the lowest high school score (60–69) experience a +2.73 p.p. increase, only significant at the 10% level, while those with the highest score (90–100) experience a +9.15 p.p. increase, significant at the 1% level. Expressed relative to sub-

group pre-reform enrollment rates, these increases correspond to about +10% in the bottom half of the score distribution (60–69 and 70–79) and around +13% to +15% in the top half (80–89 and 90–100), reinforcing that higher-ability students experience larger effects both in absolute and in relative terms despite their higher baseline enrollment.

When we perform the treatment intensity analysis, even within the same final score bin, we observe that the effects of the reform are always larger for graduates with the lowest parental education, corroborating the previous results. So also for what concerns university enrollment, the individuals who benefit the most are those with highest ability coming from the most disadvantaged parental backgrounds. Specifically, the subgroup of graduates with the highest final score (90–100) and the lowest parental education is the one that experiences the largest effect of the reform: +10.15 p.p., which is almost double than the baseline of +5.53 p.p..

Table 4: Heterogeneity on Enrollment in University by High School Final Mark

Two-period DiD		60–69	70–79	80–89	90–100	Full sample
Treatment	Treatment x Post	0.0273*	0.0405*	0.0840***	0.0915***	0.0553***
		(0.0157)	(0.0228)	(0.0290)	(0.0203)	(0.0155)
Treatment Intensity	Lowest PE	0.0372**	0.0569***	0.0872***	0.1015***	0.0661***
		(0.0152)	(0.0206)	(0.0289)	(0.0205)	(0.0143)
	Medium PE	0.0157	0.0184	0.0792**	0.0727***	0.0402***
		(0.0152)	(0.0209)	(0.0309)	(0.0180)	(0.0146)
Pre-reform average		28.58%	42.31%	55.97%	72.76%	44.44%
Observations		47,460	40,865	27,323	25,158	141,106

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

We also analyze the heterogeneity of the effect of the reform on university enrollment by school track of origin in Table A20 in the Appendix. What we obtain is a significant increase in enrollments among those who graduated in the academic-oriented school track (*Liceo*), while non-significant effects are registered for students from the vocational tracks (*Istituto Professionale* and *Istituto Tecnico*).

However, when in Table 5 we examine the heterogeneity of the effects on university enrollment by final score in high school and high school track, we observe a different picture. On the one hand, we observe that the positive and significant effect for academic-oriented graduates (*Liceo*) is driven by high ability students (+6.86 p.p. for the High Final Mark and *Liceo* combination), while the effect for low ability student from *Liceo* is positive but not significant. On the other hand, we observe that the overall non-significant effect for the “blue-collar vocational track” graduates (*Istituto Professionale*) hides a very large positive effect of +10.81 p.p. for those who obtained a high mark in the final exam and a negative

effect of -2.15 p.p. for those who obtained a low mark.

Table 5: Heterogeneity on Enrollment in University by High School track and High School Final Mark

Two-period DiD		Professionale		Tecnico		Liceo		Full sample
		Low FM	High FM	Low FM	High FM	Low FM	High FM	
Treatment	Treatment x Post	-0.0215 (0.0180)	0.1081* (0.0599)	0.0150 (0.0205)	-0.0046 (0.0393)	0.0251 (0.0163)	0.0686*** (0.0117)	0.0553*** (0.0155)
Treatment	Lowest PE	-0.0164 (0.0188)	0.1234** (0.0591)	0.0154 (0.0204)	-0.0179 (0.0425)	0.0471*** (0.0123)	0.0772*** (0.0119)	0.0661*** (0.0143)
Intensity	Medium PE	-0.0309* (0.0173)	0.0728 (0.0608)	0.0146 (0.0215)	0.0160 (0.0384)	0.0014 (0.0151)	0.0581*** (0.0129)	0.0402*** (0.0146)
Observations		29,404	12,449	31,113	15,818	24,355	21,591	141,016

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

The heterogeneity on university dropout by high school final mark reveals that the overall -2.71 p.p. reduction in the full sample is essentially driven by students with low to medium ability, which experience large and very significant reductions in dropout (-6.22 p.p. and -4.03 p.p.), while the effects for students with medium to high ability are small and not statistically significant (Table A21 in the Appendix). This effect could be interpreted as the result of the reduction of the effort necessary to complete the degree, which significantly helped students with low/medium ability preventing them from dropping out and did not affect the dropout behavior of high ability students. For what concerns the heterogeneity on in-time graduation by ability (Table A22 in the Appendix), we observe positive and significant effects on all levels of ability. The overall effect of the reform on this variable is rather homogeneous across all the ability levels with slightly higher effects for medium/high final scores (80–90 bin). As for the other outcomes, in time graduation exhibits the same pattern for what concerns treatment intensity: students from the lowest parental backgrounds experience significantly larger increases in graduating within the legal time, even when considering each final score bin.

Finally, in Table A23 we examine heterogeneity in enrollment mobility (changing region to attend university) by high school track and final mark. The overall effect is small and statistically insignificant (-1.99 p.p.), but it masks substantial heterogeneity: the reduction in mobility is concentrated among high-ability students graduating from *Liceo*, who experience a significant -3.38 p.p. effect. Moreover, the treatment intensity by parental education shows that this decline is strongest for high ability *Liceo* graduates with the Lowest PE (-4.80 p.p., significant), while the corresponding effect for the Medium PE group is smaller (-2.62 p.p.) and less significant. For all other subgroups, the estimates are imprecise and do not differ statistically from zero. This heterogeneity is consistent with the mechanism outlined above: by lowering the cost of higher education, the reform induced an inflow of high-ability students

from disadvantaged backgrounds; yet, because these students face tighter financial constraints, they are more likely to enroll in local universities due to substantial additional costs linked to relocation, therefore mechanically reducing the mobility of enrollment outside the region of origin.

The heterogeneity analysis of the effects of the reform by gender reveals a clear pattern: females experience larger effects than males in all outcomes of interest. For what concerns graduation from the academic-oriented high school track (*Liceo*), in Table A24 in the Appendix we can see that females experience on average a larger effect of the reform (+6.35 p.p.) with respect to males (+2.53 p.p.). When analyzing the intensity of the treatment, distinguishing between Lowest PE and Medium PE, previous results are confirmed: within each gender, individuals with the lowest parental education experience the larger effects; while within each parental education subgroup (Lowest PE and Medium PE), females always experience a larger and more significant effect. For this reason, females with the Lowest PE are the subgroup that actually increases *Liceo* graduations the most (+8.17 p.p.), while males with Medium PE increase the least (+1.03 p.p., not statistically significant). The same pattern is observed when we consider university enrollment as an outcome (Table A25 in the Appendix): a higher effect for females (+6.59 p.p.) than males (+3.73 p.p.), always higher effect for Lowest PE with respect to Medium PE within each gender, and again the largest increase in university enrollments is experienced by females from the lowest parental background (+7.75 p.p.). Finally, for what concerns university dropout, the results are partially different as females experience only a slightly larger reduction in dropouts as we can see in Table A26 in the Appendix (−2.66 p.p. for females against −2.19 p.p. for males). Moreover, the intensity of the effect between Medium PE and Lowest PE is different within each gender: for males, it is heavily driven by the Lowest PE subgroup (−3.15 p.p., very significant) with Medium PE experiencing non-significant effect; while for females both the subgroups experience almost the same effect (−2.68 p.p. for Lowest PE, −2.63 p.p. for Medium PE).

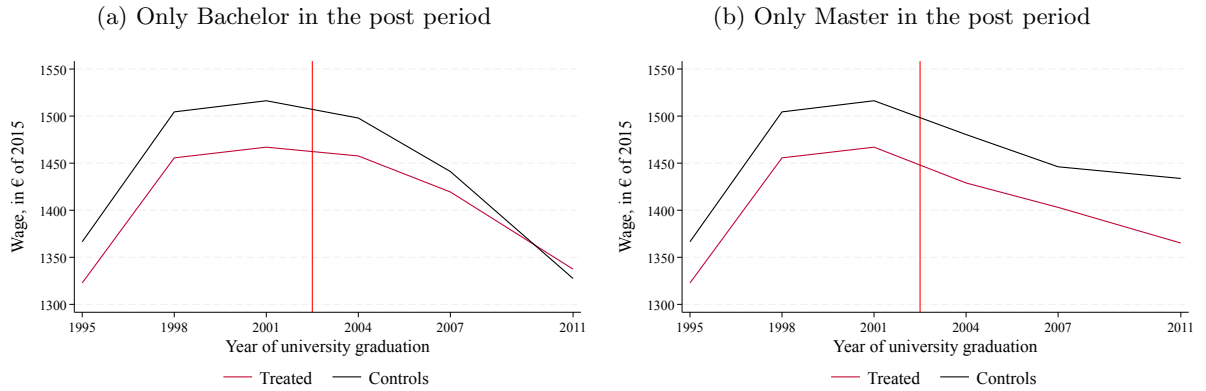
5.2 Labor market outcomes

In this section, we present the findings obtained using our identification strategy to examine early labor market outcomes. Before presenting the results, it is essential to acknowledge that the results of this section cannot be interpreted as causal estimates of the reform due to the specific characteristics of the reform and the structure of the dataset used. In the analysis of labor market outcomes, it is crucial to consider for each individual the highest degree obtained, as this will be the predominant educational factor that influences their performance. However, the reform, by changing the degree structure, introduced a new source of endogeneity: the choice to enroll in the Master’s degree after completing the Bachelor’s degree. This new choice is only in place in the post-reform period and therefore can potentially introduce a selection bias in the analysis of the labor market when we try to compare the “old” degrees with the “new” ones. If we compare the pre-reform degrees against the Master’s degree, we can obtain a biased estimate because Master’s graduates directly self-selected into and then completed an additional degree. But we can encounter this problem even if we compare pre-reform degrees against Bachelor’s degrees, as

Bachelor's graduates are just the counterpart of those who chose to do the Master's degree.

Acknowledging the existence of a potential selection bias, we apply the same identification strategy to the outcomes of the labor market and hereby present the results for descriptive purposes, without any claims of causality. We implement the same two-period Difference-in-Differences method, using the same treatment definition based on parental education, and for every outcome, we present pairwise comparisons: Old degrees vs Bachelor's degrees, or Old degrees vs only Master's degrees.⁶ We present in Figure 3 the time series of wage⁷ by treatment status: Figure 3a shows the time series considering only Bachelor's graduates in the post-reform period, while in Figure 3b only Master's graduates are considered in the post-reform period. In both figures, the pre-reform data are the same, as they refer to the old one-tier degrees.

Figure 3: Time Series of Wage by Treatment status: Old and Bachelor, Old and Master



When considering only the Bachelor's graduates in the post-reform period, what we can observe descriptively is that there is a clear convergence in wages between low parental background graduates (Treated) and high parental background graduates (Controls). A phenomenon that is evidently not in place when considering Master's graduates, for whom we observe a slight divergence in wages toward the end. This intuition is actually confirmed by the results of the two-period Difference-in-Differences on wage and hourly wage in Table 6.

When considering only Bachelor's graduates in the post-reform period, we observe a positive and significant value of +7.64% increase in wage among the treated with respect to the controls, meaning that there is a significant convergence in wages and therefore a tendency to close the existing wage gap between graduates from low and high parental background. However, when we consider only the Master's graduates in the post reform period, the effect is slightly negative and not significant, implying that there is no convergence between graduates with different parental backgrounds. As a robustness check,

⁶ In this way our work differs from the studies of [Bosio and Leonardi \(2010\)](#) and [Scandolo \(2025\)](#) that pool the two types of graduates into a single "new graduates" category.

⁷ Net monthly wage measured in 2015 euros, values adjusted using the FOI index by ISTAT.

we estimate the same model using the hourly wage as an outcome. The estimates present the same pattern (positive and strong effect for Bachelor's, non-significant effects for Master's), showing that the differences in the trends of the wage gaps are not driven by the amount of hours worked.

Table 6: Two-period DiD on Wage and Hourly Wage (Pairwise comparisons)

2-period DiD	Outcome = Wage (ln)		Outcome = Hourly Wage (ln)	
	Old and Bachelor	Old and Master	Old and Bachelor	Old and Master
Treated x Post	0.0764*** (0.0228)	-0.0144 (0.0128)	0.0541*** (0.0132)	0.0098 (0.0108)
Observations	79,710	90,407	79,710	90,407

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included. Dependent variable is log of monthly net wage expressed in 2015€ (adjusted using FOI index by ISTAT).

In Table 7 we analyze the intensity of the treatment effect on the wage between the two subgroups of Lowest PE and Medium PE, keeping the usual setting of pairwise comparisons described above. When restricting the post-reform period only to Bachelor's graduates, we observe that the overall convergence documented above (+7.64%) hides a clear gradient by parental education. The estimated effects are positive and statistically significant for both subgroups, but significantly larger for graduates with the Lowest PE (+10.81%) than for those with Medium PE (+4.90%), suggesting that the closing of the wage gap among Bachelor's graduates is primarily driven by the most disadvantaged group.

Table 7: Two-period DiD on Wage: Treatment and Treatment Intensity (Pairwise comparisons)

2-period DiD	Old and Bachelor			Old and Master		
	Treatment	Treatment Intensity		Treatment	Treatment Intensity	
		Lowest PE	Medium PE		Lowest PE	Medium PE
Treated x Post	0.0764*** (0.0228)	0.1081*** (0.0292)	0.0490** (0.0172)	-0.0144 (0.0128)	0.0040 (0.0272)	-0.0258** (0.0090)
Observations	79,710	79,710	79,710	90,407	90,407	90,407

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included. Dependent variable is log of monthly net wage expressed in 2015€ (adjusted using FOI index by ISTAT).

Similarly, when restricting the post-reform period to Master's graduates, we observe heterogeneous

effects between the two treated subgroups. For the Lowest PE, the effect is close to zero and is not significant (+0.40%), implying that the wage gap in place before the reform remains essentially unchanged; while for the Medium PE subgroup it is negative and statistically significant (−2.58%), suggesting a widening of the wage gap in the post-reform period for this subgroup of Master’s graduates. The results presented in Tables 6 and 7 are robust to the inclusion of different sets of control variables (Tables A27 and A29 in the Appendix) as well as to the inclusion of controls for labor market experience (Table A28 in the Appendix).

In Table 8 we present the results of applying the same Difference-in-Differences design to the unemployed variable.⁸ When restricting the post-reform period to Bachelor’s graduates, the treated individuals experience a statistically significant reduction in the unemployment probability of −2.92 p.p. and, consistent with previous results, the effect is larger and more significant for graduates coming from the most disadvantaged subgroup (−4.17 p.p. for Lowest PE versus −1.91 p.p. for Medium PE).

Table 8: Two-period DiD on Unemployment: Treatment and Treatment Intensity (Pairwise comparisons)

2-period DiD	Old and Bachelor			Old and Master		
	Treatment	Treatment Intensity		Treatment	Treatment Intensity	
		Lowest PE	Medium PE		Lowest PE	Medium PE
Treated x Post	-0.0292** (0.0126)	-0.0417** (0.0162)	-0.0191* (0.0106)	-0.0073 (0.0048)	-0.0133 (0.0109)	-0.0033 (0.0034)
Observations	127,215	127,215	127,215	145,938	145,938	145,938

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

By contrast, when restricting the post-reform period to Master’s graduates, the estimated effect is very close to zero and not statistically significant both in aggregate (−0.73 p.p.) and with respect to the intensity of treatment (−1.33 p.p. for Lowest PE and −0.33 p.p. for Medium PE), again suggesting that there is no convergence in unemployment at the Master’s level. In general, the unemployment results reinforce the narrative emerging from wage and hourly wage: we observe a convergence in labor market outcomes for Bachelor’s graduates, which is stronger for those with the lowest parental education, while we do not observe any convergence among Master’s graduates.

An interpretation consistent with these findings is to read the results as the outcomes of a positive self-selection mechanism enacted by the reform. We showed that the reform causally determined a significant increase in university enrollments and that this increase was mainly driven by high ability students

⁸ As discussed above, the Parallel Trend Assumption holds as shown in Figure A4 in the Appendix, and the results have to be interpreted descriptively due to the potential selection bias.

coming from a disadvantaged background. But it is relevant to note that the reform reduced the legal duration, and consequently the monetary and non-monetary cost, only of the first tier of the new structure. Therefore, the reform allowed marginal students to access higher education, but it was successful only for students who were marginal to the first-tier enrollment decision. In fact, the length and the costs of doing both Bachelor’s and Master’s degrees in the new system have not been reduced by the reform, as the “3+2” path can be even longer than the old degrees. Therefore, the reform determined a positive selection of high ability students from lower parental background but only in Bachelor’s courses, as this group of talented individuals is unlikely to have reached the Master’s degree. In this sense, the positive selection of high ability students from disadvantaged backgrounds could explain the labor market patterns we document for both wages and unemployment. This group of highly talented students coming from a low parental background started a Bachelor’s degree thanks to the cost-reduction induced by the Bologna Process, their high ability allowed them to complete the degree, and when they finally entered the labor market, they successfully closed the existing wage gap with their high parental background peers because of their high ability. However, for what concerns Master’s degrees, the Bologna Process is unlikely to have affected the student composition the same way it did with Bachelor’s degree. Therefore, the reform did not increase the intake of high ability students from low parental background in Master’s enrollees, which could explain the persistence of the wage gap in time between low and high parental background graduates.

6 Conclusion

The Bologna Process has undoubtedly been the most relevant and impactful reform of the Italian higher education system. It completely reshaped the Italian university system, and particularly its degree structure, with the aim of harmonizing it to the other European systems and contributing to the creation of a common European Higher Education Area. This paper estimated the causal effects of the Bologna Process reform in Italy on a wide range of educational and labor market outcomes, addressing the crucial question of who benefited most from the implementation of the reform.

Taken together, the evidence indicates that the Bologna Process generated sizable effects throughout the entire educational pathway. Specifically, the reform determined “backward spillover effects” on earlier stages of the educational system, increasing by +5.50 percentage points the share of students graduating from the academic-oriented high school track (*Liceo*). For university-related outcomes, the reform increased enrollment by +5.53 percentage points, reduced dropouts by −2.71 percentage points, and increased in-time graduation by +7.22 percentage points. Heterogeneity analyses reveal a clear socioeconomic and ability pattern: across all outcomes, the effects were systematically larger among students with the lowest parental education and among those with high ability. The “announced” cohorts always experienced stronger effects of the reform compared to the “surprised” cohorts, suggesting that receiving the reform before or after the choice of the high school track was relevant in determining the

response. Descriptively, we showed that after the reform was implemented, among Bachelor’s graduates, those coming from low parental backgrounds closed the existing gap in wage and unemployment rate with those from a high parental background. In contrast, such convergence did not happen among Master’s graduates, for which a significant gap still exists in both dimensions.

A natural interpretation is that, by reducing the legal duration of the university degrees, the reform directly lowered the direct, indirect, and non-monetary costs of getting a degree, thereby reducing barriers to entry and completion. Consistent with theoretical expectations, these changes directly affected university-related outcomes, increasing enrollments, reducing dropouts, and increasing in-time graduations. At the same time, the reform also affected earlier choices through a “backward spillover effect”: in a rigid early-tracking system, families and students appear to have anticipated the university enrollment decision already at the moment of high school track selection, shifting toward the academic-oriented track and increasing completion in *Liceo*. Most importantly, this paper clarifies the open question of who was affected by the reform. The evidence we provided supports the interpretation of the Bologna Process as a reform that increased equality of opportunity in higher education: the effects of the reform were stronger and more statistically significant among students with high ability, and specifically for those coming from the most disadvantaged backgrounds. Therefore, the results presented challenge the narrative of the reform mainly attracting low ability students not suited for the academic path. Instead, they support an interpretation in which shorter programs and lower costs relaxed financial constraints that were binding for talented students with limited resources, improving both efficiency in educational sorting and equity in access to higher education.

Online Appendix

A Additional Results

Tables

Table A1: Description of the “Surveys on Educational and Professional paths of Upper Secondary School graduates”

Year of survey	Year of HS graduation	N. of observations	N. of total graduates	Share of total	Survey technique
1998	1995	18,841	490,348	3.84%	Postal
2001	1998	23,253	478,904	4.86%	CATI
2004	2001	20,220	454,903	4.44%	CATI
2007	2004	25,880	452,726	5.72%	CATI
2011	2007	26,588	449,543	5.91%	CATI
2015	2011	26,234	450,869	5.82%	CAWI/CATI

Table A2: Description of the “Survey on Employment outcomes of University graduates”

Year of survey	Year of Uni graduation	N. of observations	N. of total graduates	Share of total	Survey technique
1998	1995	16,870	105,097	16.05%	Postal
2001	1998	20,529	129,307	15.88%	CATI
2004	2001	25,770	155,664	16.55%	CATI
2007	2004	46,856	260,070	18.02%	CATI
2011	2007	61,420	300,338	20.45%	CATI
2015	2011	57,207	299,449	19.10%	CAWI/CATI

Descriptive Tables of High school graduates

Table A3: High School graduates: Summary Statistics by Treatment Status

Variable	Controls	Treated	Total
Female	0.4753 (0.4994)	0.4502 (0.4975)	0.4543 (0.4979)
Age			
18 or less	0.0889 (0.2846)	0.0402 (0.1963)	0.0482 (0.2141)
19	0.6722 (0.4694)	0.6548 (0.4754)	0.6577 (0.4745)
20 or more	0.2389 (0.4264)	0.3050 (0.4604)	0.2942 (0.4557)
Area of High School			
North-West	0.2311 (0.4207)	0.2084 (0.4060)	0.2122 (0.4084)
North-East	0.2306 (0.4204)	0.2166 (0.4116)	0.2190 (0.4131)
Center	0.2220 (0.4148)	0.1958 (0.3965)	0.2001 (0.3997)
South	0.2125 (0.4083)	0.2532 (0.4345)	0.2465 (0.4306)
Islands	0.1039 (0.3043)	0.1261 (0.3316)	0.1224 (0.3274)
High School Track			
Istituto Professionale	0.1873 (0.3842)	0.3348 (0.4664)	0.3107 (0.4568)
Istituto Tecnico	0.2190 (0.4076)	0.3739 (0.4791)	0.3484 (0.4712)
Liceo	0.5937 (0.4950)	0.2913 (0.4479)	0.3409 (0.4687)
Middle School Final mark			
Sufficient	0.1771 (0.3786)	0.3113 (0.4613)	0.2907 (0.4515)
Good	0.2642 (0.4379)	0.3204 (0.4650)	0.3127 (0.4611)
Very good	0.2304 (0.4225)	0.2059 (0.3980)	0.2063 (0.4024)
Excellent	0.3283 (0.4670)	0.1625 (0.3669)	0.1904 (0.3897)
High School Final mark			
60-69	0.2899 (0.4537)	0.3475 (0.4762)	0.3380 (0.4730)
70-79	0.2807 (0.4493)	0.2916 (0.4545)	0.2898 (0.4537)
80-89	0.2042 (0.4031)	0.1917 (0.3936)	0.1938 (0.3952)
90-100	0.2252 (0.4177)	0.1692 (0.3749)	0.1784 (0.3829)
Rejected	0.1888 (0.3914)	0.2235 (0.4166)	0.2178 (0.4127)
External candidate	0.0368 (0.1884)	0.0371 (0.1891)	0.0371 (0.1890)
Enrollment	0.7133 (0.4522)	0.4826 (0.4997)	0.5205 (0.4996)
Dropout	0.1032 (0.3043)	0.1769 (0.3816)	0.1603 (0.3669)
Survived	0.6396 (0.4801)	0.3972 (0.4893)	0.4370 (0.4960)
Attainment	0.1142 (0.3180)	0.0596 (0.2368)	0.0686 (0.2527)

Table A3: High School graduates: Summary Statistics by Treatment Status

Variable	Controls	Treated	Total
Field of Study			
STEM	0.2463 (0.3717)	0.2343 (0.2989)	0.2371 (0.3130)
Health	0.1566 (0.3070)	0.1497 (0.2437)	0.1514 (0.2556)
Social Sciences/Law	0.3382 (0.4191)	0.3325 (0.3478)	0.3338 (0.3619)
Humanities	0.2589 (0.3791)	0.2835 (0.3249)	0.2777 (0.3350)
Two Tier	0.7129 (0.4996)	0.7692 (0.4686)	0.7559 (0.4773)
Father education			
No education/Elementary school	0.0146 (0.1062)	0.1748 (0.3798)	0.1535 (0.3551)
Middle School	0.0750 (0.2346)	0.4700 (0.4991)	0.4175 (0.4904)
High School	0.2332 (0.3857)	0.3552 (0.4786)	0.3390 (0.4690)
University	0.6772 (0.4992)	/	0.0899 (0.2815)
Mother education			
No education/Elementary school	0.0205 (0.1297)	0.1851 (0.3884)	0.1618 (0.3643)
Middle School	0.1038 (0.2815)	0.4651 (0.4988)	0.4141 (0.4905)
High School	0.3160 (0.4409)	0.3498 (0.4769)	0.3451 (0.4722)
University	0.5597 (0.4990)	/	0.0790 (0.2665)
Unemployed father	0.0070 (0.0834)	0.0174 (0.1306)	0.0157 (0.1241)
Father Occupation			
Entrepreneur	0.0529 (0.2137)	0.0747 (0.2594)	0.0713 (0.2526)
High skilled	0.5009 (0.4300)	0.0869 (0.2789)	0.1511 (0.3484)
White collar	0.2436 (0.4150)	0.2659 (0.4376)	0.2624 (0.4342)
Blue collar	0.2026 (0.3874)	0.5726 (0.4969)	0.5152 (0.5000)
Housewife mother	0.2720 (0.4450)	0.5271 (0.4993)	0.4852 (0.4998)
Mother Occupation			
Entrepreneur	0.0217 (0.1198)	0.0452 (0.1422)	0.0399 (0.1388)
High skilled	0.4396 (0.4567)	0.0738 (0.1810)	0.1558 (0.2665)
White collar	0.4212 (0.4509)	0.4656 (0.4097)	0.4557 (0.4176)
Blue collar	0.1175 (0.2701)	0.4154 (0.3926)	0.3486 (0.3775)
Observations	23,155	117,861	141,016

Notes: The table reports sample means with standard deviations in parentheses. Treatment definition: highest parental educational attainment is high school or lower.

Descriptive Tables of University graduates

Table A4: University graduates: Summary Statistics by Treatment status

Variable	Controls	Treated	Total
Female	0.5070 (0.5000)	0.5510 (0.4974)	0.5387 (0.4985)
Age			
24 or less	0.3229 (0.4676)	0.2782 (0.4481)	0.2907 (0.4541)
25–29	0.5501 (0.4975)	0.5112 (0.4999)	0.5221 (0.4995)
30 or more	0.1270 (0.3330)	0.2106 (0.4077)	0.1872 (0.3901)
Foreign citizen	0.0269 (0.1619)	0.0118 (0.1079)	0.0160 (0.1256)
Married	0.2107 (0.4078)	0.2844 (0.4511)	0.2638 (0.4407)
High School track			
Istituto Professionale	0.0120 (0.1087)	0.0489 (0.2157)	0.0386 (0.1926)
Istituto Tecnico	0.1004 (0.3006)	0.3237 (0.4679)	0.2612 (0.4393)
Liceo	0.8788 (0.3263)	0.6113 (0.4875)	0.6661 (0.4724)
High School Final mark (out of 100)			
60–69	0.1318 (0.3383)	0.1609 (0.3674)	0.1527 (0.3597)
70–79	0.2088 (0.4064)	0.2478 (0.4317)	0.2369 (0.4252)
80–89	0.2391 (0.4266)	0.2435 (0.4295)	0.2464 (0.4309)
90–100	0.4149 (0.4927)	0.3405 (0.4739)	0.3614 (0.4804)
Degree of the interview			
Old degree	0.5473 (0.4978)	0.4901 (0.4999)	0.5061 (0.5000)
Bachelor's degree	0.2857 (0.4518)	0.3616 (0.4615)	0.3404 (0.4584)
Master's degree	0.1669 (0.3729)	0.1483 (0.3554)	0.1535 (0.3605)
Highest degree attained			
Old degree	0.4926 (0.4999)	0.4539 (0.4980)	0.4647 (0.4988)
Bachelor's degree	0.1206 (0.3256)	0.2171 (0.4123)	0.1901 (0.3924)
Master's degree (2 y.)	0.2669 (0.4423)	0.2367 (0.4250)	0.2451 (0.4302)
Postgraduate Master (1 y.)	0.1036 (0.1036)	0.0822 (0.2747)	0.0882 (0.2836)
PhD	0.0163 (0.1268)	0.0102 (0.1003)	0.0119 (0.1084)
Area of university			
North-West	0.2541 (0.4353)	0.2502 (0.4332)	0.2513 (0.4338)
North-East	0.2047 (0.4035)	0.2188 (0.4138)	0.2149 (0.4109)
Center	0.2688 (0.4433)	0.2351 (0.4241)	0.2445 (0.4298)
South	0.1768 (0.3815)	0.2054 (0.4040)	0.1974 (0.3980)
Islands	0.0956 (0.2940)	0.0905 (0.2868)	0.0919 (0.2889)

Table A4: University graduates: Summary Statistics by Treatment status

Variable	Controls	Treated	Total
Changed province for university	0.4920 (0.4999)	0.5196 (0.4996)	0.5119 (0.4999)
Changed region for university	0.2470 (0.4313)	0.2177 (0.4127)	0.2259 (0.4182)
Changed area for university	0.1808 (0.3849)	0.1442 (0.3512)	0.1544 (0.3613)
Field of study			
STEM	0.2954 (0.4562)	0.2763 (0.4472)	0.2817 (0.4498)
Health	0.2292 (0.4203)	0.2178 (0.4128)	0.2210 (0.4149)
Social sciences/Law	0.3077 (0.4615)	0.3174 (0.4655)	0.3147 (0.4644)
Humanities	0.1677 (0.3736)	0.1884 (0.3911)	0.1826 (0.3864)
University Final mark (out of 110)			
69–90	0.0601 (0.2377)	0.0713 (0.2574)	0.0682 (0.2521)
91–100	0.2288 (0.4201)	0.2710 (0.4445)	0.2592 (0.4382)
101–110	0.4193 (0.4934)	0.4398 (0.4964)	0.4341 (0.4956)
110 cum laude	0.2906 (0.4540)	0.2168 (0.4120)	0.2374 (0.4255)
Delay in graduation			
In time	0.4181 (0.4932)	0.3857 (0.4868)	0.3948 (0.4888)
1 year	0.2397 (0.4269)	0.2227 (0.4161)	0.2275 (0.4192)
2 years	0.1446 (0.3517)	0.1455 (0.3526)	0.1452 (0.3523)
3 or more years	0.1964 (0.3973)	0.2449 (0.4300)	0.2314 (0.4217)
Father education			
No education/Elementary school	0.0035 (0.0595)	0.1894 (0.3918)	0.1374 (0.3443)
Middle School	0.0236 (0.1518)	0.3584 (0.4795)	0.2647 (0.4412)
High School	0.1711 (0.3766)	0.4522 (0.4977)	0.3736 (0.4838)
University	0.8017 (0.3987)	/	0.2242 (0.4171)
Mother education			
No education/Elementary school	0.0104 (0.1014)	0.2305 (0.4212)	0.1690 (0.3747)
Middle School	0.0576 (0.2331)	0.3587 (0.4796)	0.2745 (0.4462)
High School	0.3131 (0.4638)	0.4108 (0.4920)	0.3835 (0.4862)
University	0.6189 (0.4857)	/	0.1731 (0.3783)
Father unemployed	0.0030 (0.0546)	0.0078 (0.0880)	0.0065 (0.0801)
Father entrepreneur	0.2798 (0.4489)	0.2964 (0.4567)	0.2918 (0.4546)
Father blue collar	0.0451 (0.2076)	0.3027 (0.4594)	0.2307 (0.4213)
Mother housewife	0.2351 (0.4241)	0.4914 (0.4914)	0.4197 (0.4935)
Mother entrepreneur	0.0683 (0.2523)	0.0982 (0.2976)	0.0898 (0.2860)
Mother blue collar	0.0134 (0.1149)	0.1056 (0.3073)	0.0798 (0.2709)

Table A4: University graduates: Summary Statistics by Treatment status

Variable	Controls	Treated	Total
Employed	0.3461 (0.4757)	0.2540 (0.4353)	0.2798 (0.4489)
Unemployed	0.8591 (0.3480)	0.8588 (0.3483)	0.8588 (0.3482)
Inactive	0.7790 (0.4149)	0.8718 (0.3343)	0.8459 (0.3611)
Neet	0.7683 (0.4219)	0.6913 (0.4620)	0.7128 (0.4525)
Wage	1437.03 (743.69)	1406.32 (640.79)	1414.12 (668.55)
Hourly wage	9.51 (7.24)	9.43 (6.08)	9.45 (6.40)
Labor market experience (months)	31.15 (13.67)	31.73 (13.55)	31.56 (13.59)
Labor market experience (years)	3.04 (1.19)	3.09 (1.17)	3.08 (1.18)
Changed province for job	0.5243 (0.4994)	0.5427 (0.4982)	0.5379 (0.4986)
Changed region for job	0.3076 (0.4615)	0.2672 (0.4425)	0.2778 (0.4479)
Changed area for job	0.2353 (0.4242)	0.1899 (0.3922)	0.2018 (0.4014)
Permanent contract	0.1800 (0.3842)	0.1955 (0.3966)	0.1912 (0.3932)
Part-time contract	0.5064 (0.5000)	0.5878 (0.4922)	0.5650 (0.4958)
Vertical mismatch (requirements)	0.4003 (0.4900)	0.4505 (0.4975)	0.4372 (0.4960)
Vertical mismatch (self-reported)	0.2652 (0.4414)	0.3034 (0.4597)	0.2937 (0.4554)
Observations	63,955	164,697	228,652

Placebo test

Table A5: Placebo test on High School track of graduation, Enrollment, Dropout, and In time graduation

2-period Difference-in-Differences	High School	University		
	Track = Liceo	Enrollment	Dropout	In time grad.
Treatment x Placebo Post	0.0053 (0.0076)	0.0012 (0.0154)	-0.0187 (0.0222)	-0.0009 (0.0048)
Observations	42,094	42,094	18,706	62,413

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Two-period and Three-period DiD on Change region

Table A6: Two-period and Three-period DiD on Change region: Treatment and Treatment Intensity

Difference-in-Differences models		Treatment	Treatment Intensity	
			Lowest PE	Medium PE
2-period DiD	Treated x Post	-0.0199 (0.0124)	-0.0249* (0.0138)	-0.0158 (0.0124)
3-period DiD	Treated x Post 1	-0.0322** (0.0157)	-0.0435*** (0.0167)	-0.0245 (0.0164)
	Treated x Post 2	-0.0085 (0.0103)	-0.0053 (0.0107)	-0.0081 (0.0100)
	Observations	68,301	68,301	68,301

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Robustness with respect to Treatment Intensity

Table A7: Two-period DiD on High School track of graduation: Robustness check on Treatment Intensity

Two-period DiD		Single equation	Double equation	
			Only Lowest PE	Only Medium PE
Treatment Intensity	Lowest PE x Post	0.0781*** (0.0071)	0.0676*** (0.0058)	
	Medium PE x Post	0.0312*** (0.0077)		0.0367*** (0.0093)
Control variables		NO	NO	NO
Group FE, Year FE		YES	YES	YES
Observations		134,730	80,258	76,723

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Table A8: Two-period DiD on Enrollment: Robustness check on Treatment Intensity

Two-period DiD		Single equation	Double equation	
			Only Lowest PE	Only Medium PE
Treatment Intensity	Lowest PE x Post	0.0661*** (0.0143)	0.0627*** (0.0142)	
	Medium PE x Post	0.0402*** (0.0146)		0.0386*** (0.0142)
Control variables		NO	NO	NO
Group FE, Year FE		YES	YES	YES
Observations		141,016	83,994	80,177

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Table A9: Two-period DiD on Dropout: Robustness check on Treatment Intensity

Two-period DiD		Single equation	Double equation	
			Only Lowest PE	Only Medium PE
Treatment Intensity	Lowest PE x Post	-0.0322*** (0.0069)	-0.0317*** (0.0065)	
	Medium PE x Post	-0.0216*** (0.0065)		-0.0191*** (0.0059)
Control variables		NO	NO	NO
Group FE, Year FE		YES	YES	YES
Observations		73,392	38,749	51,159

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Table A10: Two-period DiD on In time graduation: Robustness check on Treatment Intensity

Old degrees and Bachelor		Single equation	Double equation	
			Only Lowest PE	Only Medium PE
Treatment Intensity	Lowest PE x Post	0.1088*** (0.0249)	0.1055*** (0.0241)	
	Medium PE x Post	0.0468*** (0.0118)		0.0499*** (0.0127)
Control variables		NO	NO	NO
Group FE, Year FE		YES	YES	YES
Observations		166,976	100,113	111,805

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Robustness with respect to control variables (Educational outcomes)

Table A11: Two-period DiD on High School track of graduation: Robustness check with different sets of control variables

2-period Difference-in-Differences		No controls	Predet. controls	All controls
Treatment	Treated x Post	0.0550*** (0.0128)	0.0515*** (0.0127)	0.0487*** (0.0130)
Treatment Intensity	Lowest PE	0.0781*** (0.0071)	0.0739*** (0.0060)	0.0665*** (0.0093)
	Medium PE	0.0312*** (0.0077)	0.0274*** (0.0073)	0.0295*** (0.0106)
Control variables	Personal attributes	NO	YES	YES
	Prev. educ. attainment	NO	NO	YES
	Parental background	NO	NO	YES
Fixed Effects	Group FE, Year FE	YES	YES	YES
Observations		134,730	134,730	134,730

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Baseline of control variables: male, 19 years old at graduation, region of the school = Piedmont, middle school final mark = sufficient, high school final mark = 60/69, not external candidate, never rejected, school track = *Istituto Professionale*, mother and father are blue collar employees. Cohort Fixed Effects and Group Fixed Effects are included.

Table A12: Two-period DiD on Enrollment: Robustness check with different sets of control variables

2-period Difference-in-Differences		No controls	Predet. controls	All controls
Treatment	Treated x Post	0.0553*** (0.0155)	0.0497*** (0.0136)	0.0372*** (0.0095)
Treatment Intensity	Lowest PE	0.0661*** (0.0143)	0.0612*** (0.0117)	0.0421*** (0.0090)
	Medium PE	0.0402*** (0.0146)	0.0335*** (0.0123)	0.0301*** (0.0096)
Control variables	Personal attributes	NO	YES	YES
	Prev. educ. attainment	NO	NO	YES
	Parental background	NO	NO	YES
Fixed Effects	Group FE, Year FE	YES	YES	YES
Observations		141,016	141,016	141,016

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Baseline of control variables: male, 19 years old at graduation, region of the school = Piedmont, middle school final mark = sufficient, high school final mark = 60/69, not external candidate, never rejected, school track = *Istituto Professionale*, mother and father are blue collar employees. Cohort Fixed Effects and Group Fixed Effects are included.

Table A13: Two-period DiD on Dropout: Robustness check with different sets of control variables

2-period Difference-in-Differences		No controls	Predet. controls	All controls
Treatment	Treated x Post	-0.0271*** (0.0057)	-0.0241*** (0.0065)	-0.0184*** (0.0062)
Treatment Intensity	Lowest PE	-0.0322*** (0.0069)	-0.0297*** (0.0079)	-0.0214*** (0.0075)
	Medium PE	-0.0216*** (0.0065)	-0.0181*** (0.0068)	-0.0151** (0.0068)
Control variables	Personal attributes	NO	YES	YES
	Prev. educ. attainment	NO	NO	YES
	Parental background	NO	NO	YES
Fixed Effects	Group FE, Year FE	YES	YES	YES
Observations		73,392	73,392	73,392

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Baseline of control variables: male, 19 years old at graduation, region of the school = Piedmont, middle school final mark = sufficient, high school final mark = 60/69, not external candidate, never rejected, school track = *Istituto Professionale*, mother and father are blue collar employees. Cohort Fixed Effects and Group Fixed Effects are included.

Table A14: Two-period DiD on In time graduation: Robustness check with different sets of control variables

2-period Difference-in-Differences		No controls	Predet. controls	All controls
Treatment	Treated x Post	0.0722*** (0.0184)	0.0835*** (0.0231)	0.0820*** (0.0226)
Treatment Intensity	Lowest PE	0.1088*** (0.0249)	0.1342*** (0.0350)	0.1323*** (0.0343)
	Medium PE	0.0468*** (0.0118)	0.0482*** (0.0133)	0.0474*** (0.0128)
Control variables	Personal attributes	NO	YES	YES
	Prev. educ. attainment	NO	NO	YES
	Parental background	NO	NO	YES
Fixed Effects	Group FE, Year FE	YES	YES	YES
Observations		166,976	166,976	166,976

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Baseline of control variables: male, 25-29 years old at graduation, Italian citizen, not married, region of university = Piedmont, school track = *Istituto Professionale*, high school final mark = 60/69, mother and father are blue collar employees. Cohort Fixed Effects and Group Fixed Effects are included.

Robustness with respect to ATE and ATT

Table A15: Two-period DiD on High School Track of graduation: ATE and ATT estimates

2-period DiD	Treatment		Treatment Intensity			
	ATE	ATT	Lowest PE		Medium PE	
			ATE	ATT	ATE	ATT
Treated x Post	0.0550*** (0.0128)	0.0581*** (0.0138)	0.0781*** (0.0071)	0.0830*** (0.0074)	0.0312*** (0.0077)	0.0330*** (0.0081)
Observations	134,730	77,246	134,730	77,246	134,730	77,246

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Table A16: Two-period DiD on University Enrollment: ATE and ATT estimates

2-period DiD	Treatment		Treatment Intensity			
	ATE	ATT	Lowest PE		Medium PE	
			ATE	ATT	ATE	ATT
Treated x Post	0.0553*** (0.0155)	0.0592*** (0.0166)	0.0661*** (0.0143)	0.0711*** (0.0155)	0.0402*** (0.0146)	0.0432*** (0.0157)
Observations	141,016	81,148	141,016	81,148	141,016	81,148

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Table A17: Two-period DiD on University Dropout: ATE and ATT estimates

2-period DiD	Treatment		Treatment Intensity			
	ATE	ATT	Lowest PE		Medium PE	
			ATE	ATT	ATE	ATT
Treated x Post	-0.0271*** (0.0075)	-0.0296*** (0.0061)	-0.0322*** (0.0069)	-0.0351*** (0.0074)	-0.0216*** (0.0065)	-0.0236*** (0.0071)
Observations	73,392	41,985	73,392	41,985	73,392	41,985

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Table A18: Two-period DiD on In time graduation: ATE and ATT estimates

2-period DiD	Treatment		Treatment Intensity			
	ATE	ATT	Lowest PE		Medium PE	
			ATE	ATT	ATE	ATT
Treated x Post	0.0722*** (0.0184)	0.0898*** (0.0233)	0.1088*** (0.0249)	0.1354*** (0.0314)	0.0468*** (0.0118)	0.0582*** (0.0147)
Observations	166,976	48,458	166,976	48,458	166,976	48,458

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Three-period DiD with Treatment Intensity

Table A19: Three-period DiD with Treatment Intensity on High School Track of graduation, Enrollment in University and Dropout from University

3-period Difference-in-Differences		High School	University	
		Track = Liceo	Enrollment	Dropout
Lowest PE	Post 1 (Surprised)	0.0430***	0.0496***	-0.0273***
		(0.0077)	(0.0094)	(0.0092)
	Post 2 (Announced)	0.1082***	0.0780***	-0.0358***
		(0.0086)	(0.0217)	(0.0063)
Medium PE	Post 1 (Surprised)	0.0139**	0.0215***	-0.0132*
		(0.0070)	(0.0108)	(0.0075)
	Post 2 (Announced)	0.0445***	0.0539***	-0.0289***
		(0.0121)	(0.0200)	(0.0075)
	Pre-reform average	27.51%	44.44%	17.42%
	Observations	134,730	141,016	73,392

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Heterogeneity

Table A20: Heterogeneity on Enrollment in University by High School track

Two-period DiD		Professionale	Tecnico	Liceo	Total
Treatment	Treatment x Post	0.0127 (0.0208)	0.0165 (0.0174)	0.0514*** (0.0110)	0.0553*** (0.0155)
Treatment Intensity	Lowest PE	0.0213 (0.0211)	0.0132 (0.0182)	0.0674*** (0.0079)	0.0661*** (0.0143)
	Medium PE	-0.0048 (0.0188)	0.0209 (0.0190)	0.0332*** (0.0110)	0.0402*** (0.0146)
Pre-reform average		19.17%	39.78%	79.82%	44.44%
Observations		41,853	46,931	45,946	141,016

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Table A21: Heterogeneity on Dropout from University by High School Final Mark

Two-period DiD		60–69	70–79	80–89	90–100	Full sample
Treatment	Treatment x Post	-0.0622*** (0.0182)	-0.0403** (0.0164)	0.0159 (0.0327)	0.0017 (0.0149)	-0.0271*** (0.0057)
Treatment Intensity	Lowest PE	-0.0607*** (0.0192)	-0.0515** (0.0218)	0.0105 (0.0329)	-0.0028 (0.0151)	-0.0322*** (0.0069)
	Medium PE	-0.0636*** (0.0187)	-0.0287* (0.0162)	0.0229 (0.0326)	0.0077 (0.0155)	-0.0216*** (0.0065)
Pre-reform average		23.89%	18.23%	13.23%	9.04%	16.14%
Observations		16,084	20,180	17,271	19,857	73,392

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Table A22: Heterogeneity on In time graduation by High School Final Mark

Two-period DiD		60–69	70–79	80–89	90–100	Full sample
Treatment	Treatment x Post	0.0448** (0.0193)	0.0661*** (0.0171)	0.0755*** (0.0184)	0.0596*** (0.0218)	0.0722*** (0.0184)
Treatment	Lowest PE	0.0880*** (0.0226)	0.1011*** (0.0267)	0.1194*** (0.0191)	0.0826*** (0.0286)	0.1088*** (0.0249)
Intensity	Medium PE	0.0101 (0.0072)	0.0404*** (0.0084)	0.0459*** (0.0107)	0.0448** (0.0196)	0.0468*** (0.0118)
Pre-reform average		10.58%	13.51%	17.22%	24.01%	17.42%
Observations		26,755	41,284	42,213	56,404	166,976

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Table A23: Heterogeneity on Enrollment mobility by High School Track and High School Final Mark

Two-period DiD		Professionale		Tecnico		Liceo		Full sample
		Low FM	High FM	Low FM	High FM	Low FM	High FM	
Treatment	Treatment x Post	0.0260 (0.0589)	-0.0371 (0.0364)	0.0117 (0.0221)	-0.0171 (0.0301)	0.0015 (0.0213)	-0.0338** (0.0161)	-0.0199 (0.0124)
Treatment	Lowest PE	0.0105 (0.0594)	-0.0312 (0.0380)	0.0068 (0.0259)	-0.0030 (0.0295)	-0.0156 (0.0193)	-0.0480** (0.0233)	-0.0249* (0.0138)
Intensity	Medium PE	0.0526 (0.0617)	-0.0485 (0.0362)	0.0164 (0.0246)	-0.0311 (0.0303)	0.0123 (0.0208)	-0.0262* (0.0155)	-0.0158 (0.0124)
Observations		4,167	4,521	9,871	9,980	17,799	19,445	68,301

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Table A24: Heterogeneity on High School Track of graduation by Gender

Two-period DiD		Male	Female	Total
Treatment	Treatment x Post	0.0253** (0.0122)	0.0635*** (0.0152)	0.0550*** (0.0128)
Treatment	Lowest PE x Post	0.0477*** (0.0094)	0.0817*** (0.0118)	0.0781*** (0.0071)
Intensity	Medium PE x Post	0.0103 (0.0102)	0.0400*** (0.0132)	0.0312*** (0.0077)
Pre-reform average		18.87%	34.68%	27.51%
Observations		62,497	71,861	134,730

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Table A25: Heterogeneity on Enrollment in University by Gender

Two-period DiD		Male	Female	Total
Treatment	Treatment x Post	0.0373** (0.0151)	0.0659*** (0.0228)	0.0553*** (0.0155)
Treatment	Lowest PE x Post	0.0435*** (0.0148)	0.0775*** (0.0215)	0.0661*** (0.0143)
Intensity	Medium PE x Post	0.0301* (0.0155)	0.0471** (0.0237)	0.0402*** (0.0146)
Pre-reform average		40.03%	49.10%	44.44%
Observations		64,063	76,562	141,016

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Table A26: Heterogeneity on Dropout from University by Gender

Two-period DiD		Male	Female	Total
Treatment	Treatment x Post	-0.0219** (0.0094)	-0.0266*** (0.0086)	-0.0271*** (0.0057)
Treatment	Lowest PE x Post	-0.0315*** (0.0092)	-0.0268** (0.0106)	-0.0322*** (0.0069)
Intensity	Medium PE x Post	-0.0128 (0.0094)	-0.0263*** (0.0081)	-0.0216*** (0.0065)
Pre-reform average		19.04%	14.15%	16.14%
Observations		29,324	43,788	73,392

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Robustness with respect to control variables (Labor market outcomes)

Table A27: Two-period DiD on Wage (ln): Robustness check with different sets of control variables

2-period Difference-in-Differences	Old and Bachelor			Old and Master		
	No controls	Predet. controls	All controls	No controls	Predet. controls	All controls
Treatment						
Treatment x Post	0.0764*** (0.0228)	0.0896*** (0.0225)	0.0756*** (0.0201)	-0.0144 (0.0128)	-0.0131 (0.0115)	-0.0153 (0.0100)
Treatment Intensity						
Lowest PE	0.1081*** (0.0292)	0.1174*** (0.0233)	0.0974*** (0.0184)	0.0040 (0.0272)	0.0003 (0.0220)	-0.0060 (0.0181)
Medium PE	0.0490** (0.0172)	0.0659*** (0.0208)	0.0576** (0.0196)	-0.0258** (0.0090)	-0.0212** (0.0092)	-0.0209** (0.0090)
Control variables						
Personal attributes	NO	YES	YES	NO	YES	YES
Educ. attainment	NO	NO	YES	NO	NO	YES
Parental background	NO	NO	YES	NO	NO	YES
Observations	79,710	79,710	79,710	90,407	90,407	90,407

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Baseline of control variables: male, 25–29 years old at graduation, region of the school = Piedmont, middle school final mark = sufficient, high school final mark = 60/69, not external candidate, never rejected, school track = *Istituto Professionale*, mother and father are blue collar employees. Cohort Fixed Effects and Group Fixed Effects are included.

Table A28: Two-period DiD on Wage (ln): Robustness check with labor market experience

2-period DiD	Old and Bachelor		Old and Master	
	No controls	Labor mkt exp.	No controls	With controls
Treated x Post	0.0764*** (0.0228)	0.0717*** (0.0205)	0.0144 (0.0128)	-0.0165 (0.0097)
Labor market experience	NO	YES	NO	YES
Observations	79,710	79,710	90,407	90,407

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

No control variables included. Cohort Fixed Effects and Group Fixed Effects are included. Labor market experience is measured as number of years worked between the graduation and the interview.

Table A29: Two-period DiD on Hourly Wage (ln): Robustness check with different sets of control variables

2-period Difference-in-Differences	Old and Bachelor			Old and Master		
	No controls	Predet. controls	All controls	No controls	Predet. controls	All controls
Treatment						
Treatment x Post	0.0541*** (0.0132)	0.0532*** (0.0101)	0.0441*** (0.0089)	0.0098 (0.0108)	0.0064 (0.0080)	0.0043 (0.0071)
Treatment Intensity						
Lowest PE	0.0712*** (0.0177)	0.0623*** (0.0104)	0.0459*** (0.0077)	0.0244 (0.0245)	0.0126 (0.0166)	0.0056 (0.0132)
Medium PE	0.0392*** (0.0101)	0.0455*** (0.0112)	0.0427*** (0.0109)	0.0009 (0.0057)	0.0026 (0.0064)	0.0036 (0.0065)
Control variables						
Personal attributes	NO	YES	YES	NO	YES	YES
Educ. attainment	NO	NO	YES	NO	NO	YES
Parental background	NO	NO	YES	NO	NO	YES
Observations	79,710	79,710	79,710	90,407	90,407	90,407

Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Baseline of control variables: male, 25–29 years old at graduation, region of the school = Piedmont, middle school final mark = sufficient, high school final mark = 60/69, not external candidate, never rejected, school track = *Istituto Professionale*, mother and father are blue collar employees. Cohort Fixed Effects and Group Fixed Effects are included.

Figures

Parallel Trend Assumption

Figure A1: Time series plot for the outcomes of interest, by treatment status

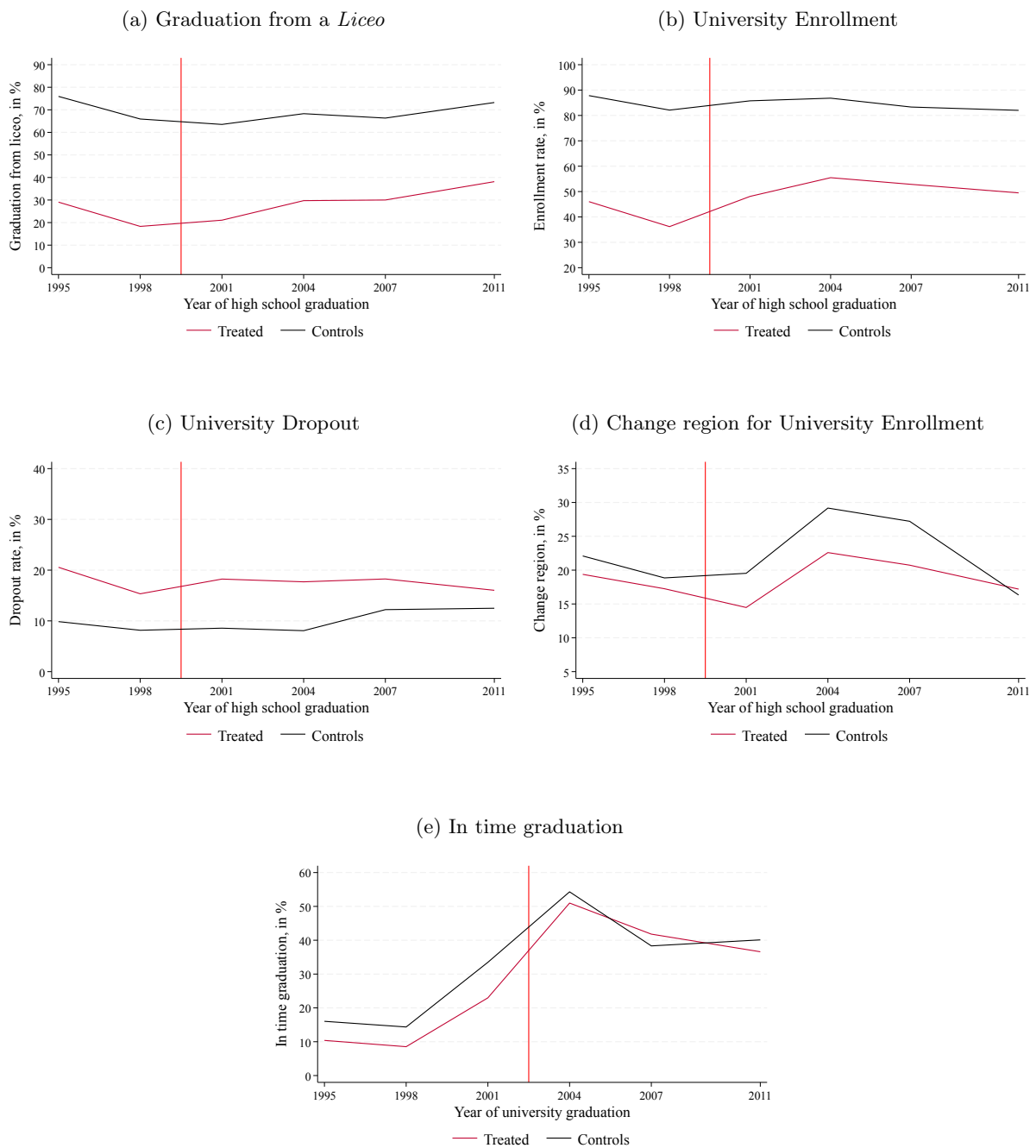


Figure A2: Time series plot of Wage: pairwise comparisons (net monthly wage in 2015€)

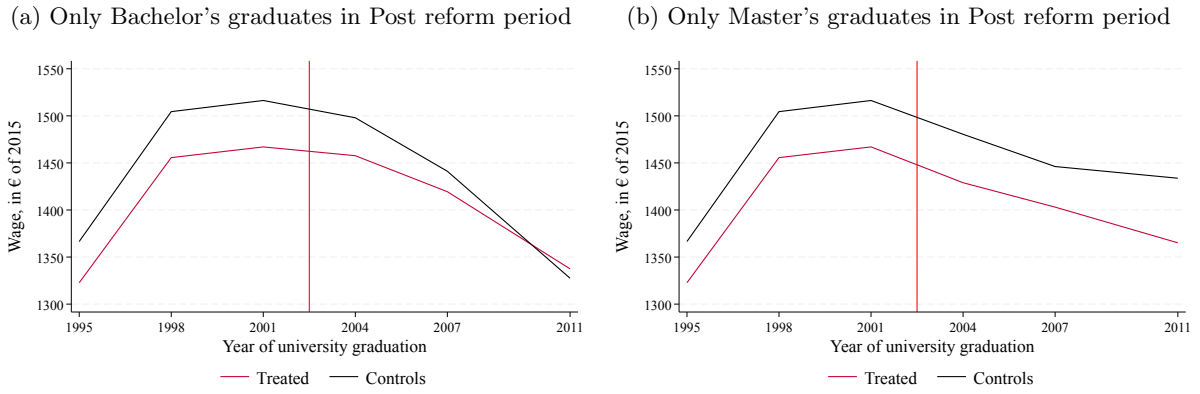


Figure A3: Time series plot of Hourly Wage: pairwise comparisons (in 2015€)

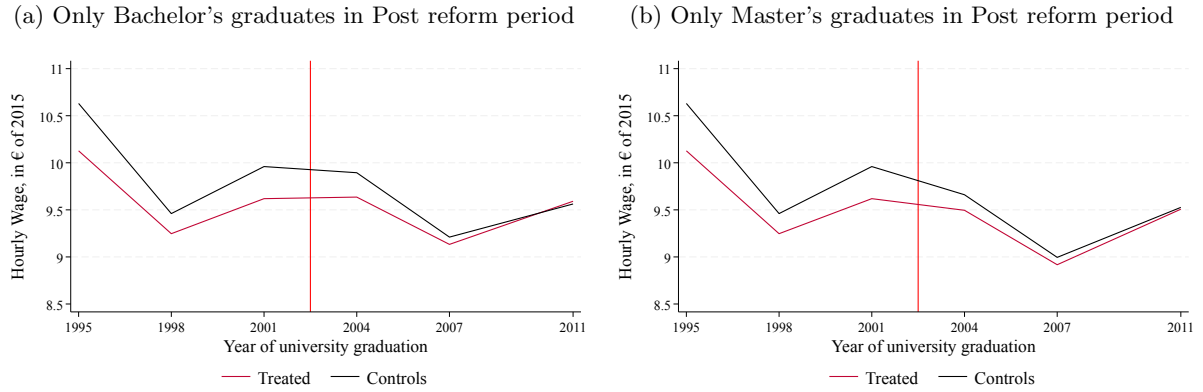
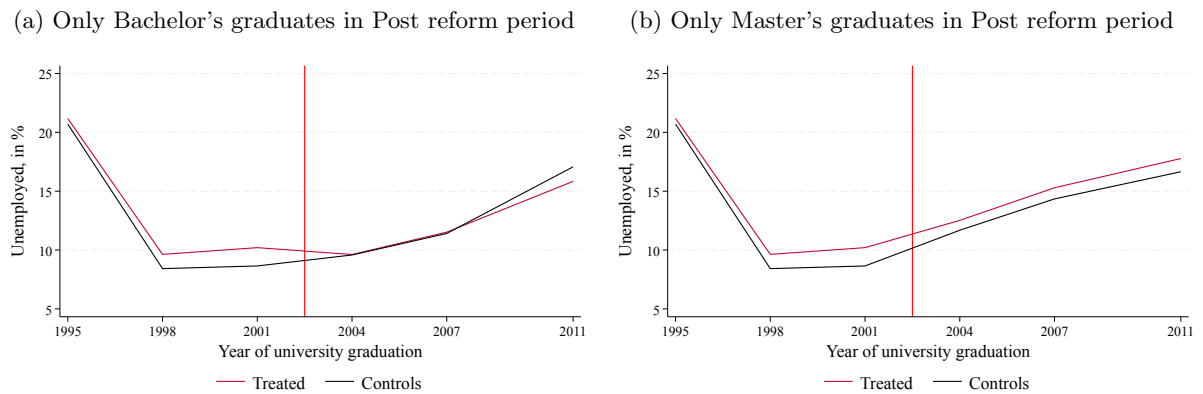
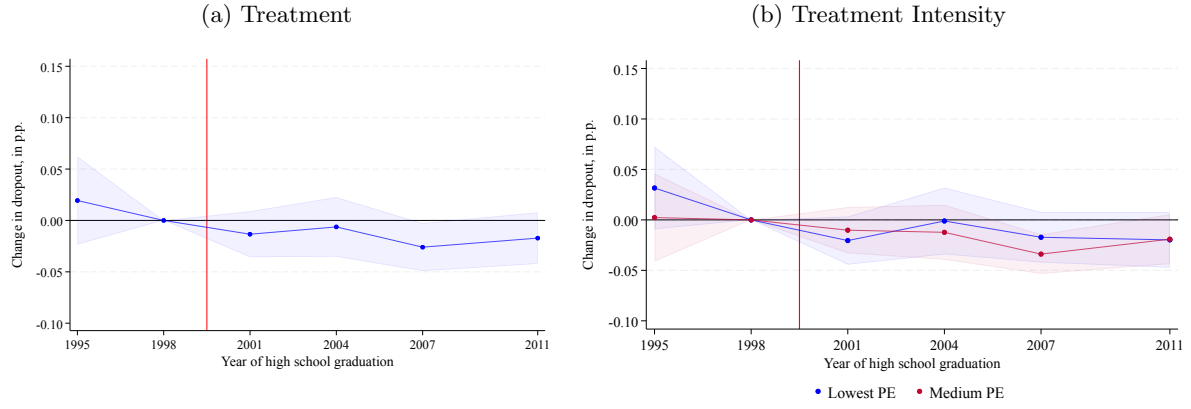


Figure A4: Time series plot of Unemployed: pairwise comparisons



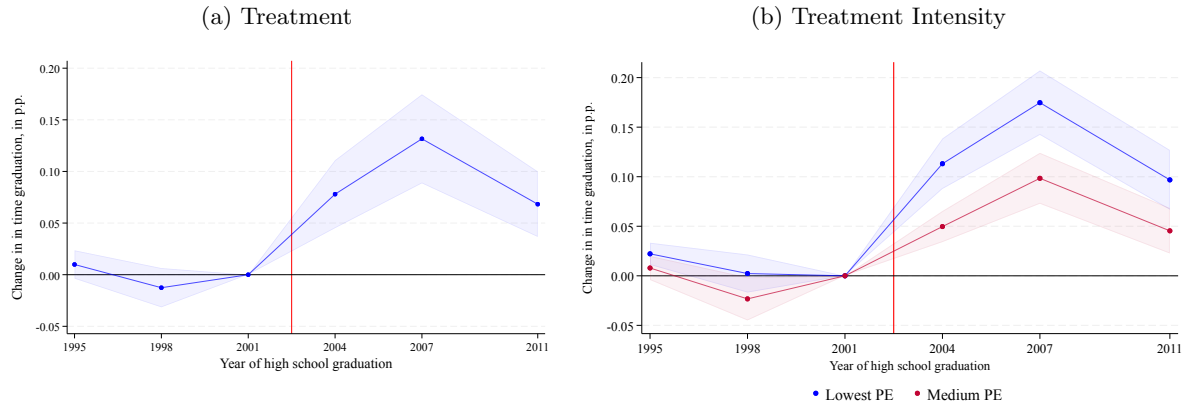
Flexible year-by-year Difference-in-Differences

Figure A5: Flexible year-by-year DiD on University Dropout: Treatment and Treatment Intensity



Note: Survey of graduates in 1998 (last one before the reform) is considered as reference year for high school graduates. The shaded region denotes 95% confidence intervals constructed using standard errors clustered at the level of parental education. No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

Figure A6: Flexible year-by-year DiD on In time graduation: Treatment and Treatment Intensity



Note: Survey of graduates in 2001 (last one before the reform) is considered as reference year for university graduates. The shaded region denotes 95% confidence intervals constructed using standard errors clustered at the level of parental education. No control variables included. Cohort Fixed Effects and Group Fixed Effects are included.

B Institutional Background

The cost of investing in tertiary education is a channel through which the unrestricted access of the Italian university system does not lead to equal access to higher education. Although Italian universities were predominantly public and largely funded by transfers from the central government, a relevant share of the cost of attendance still fell on the students' families. Moreover, Italy was classified by Eurydice (1999) as a low-support country in terms of direct financial aid to tertiary students, and fee exemptions were reserved for low-income families (only around 10% of students in 1999 benefited from a fee exemption (Agasisti and Catalano, 2007)). At the same time, only a small share of students received need- or merit-based scholarships (only 4.72% in 1998 (Agasisti and Catalano, 2007)), and student loans are practically nonexistent. Importantly, this implies that the effective cost of higher education is not limited to tuition fees, but also includes living costs, commuting, and study-related expenses, as well as the opportunity cost of time spent in education. In a context where financial support instruments are weak, household resources become a key margin shaping both entry and attainment behaviors. Moreover, the university system was also not flexible for students who would need to combine study and work: before the Bologna Process reform, there were no dedicated part-time study programs for adults or working students, making it harder to reconcile labor market participation with regular progress in university careers (Triventi and Trivellato, 2009).

Performance indicators at the end of the 1990's confirm a system in critical conditions. Despite having a first-time university entry rate of 43% (close to the OECD average of 45% (OECD, 2002)), in 1999 only 9.27% of Italians aged 25-64 had tertiary education, which was less than half of the OECD average of 20.5% (OECD, 2025). The situation was even worse if we consider the age group of 25-34 years, as in the same year only about 10% of Italians had a tertiary education, compared to an OECD average of 24% (OECD, 2025). The university system also suffered from high dropout and low completion: first-year dropout was 20.3% in 1999/2000, in line with the 22-25% range of the previous years (Chies et al., 2019), the gross graduation rate was 18.1% in 2000 (compared to the OECD average of 27.5%), while the survival rate was 42% (compared to the OECD average of 70%) (OECD, 2002). Moreover, Italy showed concerning values in terms of delay in graduation: in 1999, only 6.5% of graduates completed their degree within the legal duration, while more than 40% graduated with a delay of four years or more (CNVSU, 2005). As a result, the graduation time for a four-year course averaged 7.5 years (Aina, 2013), with the consequent effect of obtaining the degree later in time and further delaying entry into the labor market.

Crucially, these outcomes were facilitated by some peculiar features of the institutional setting of the Italian university that weakened incentives for timely progression. For instance, there was no binding limit on the number of years a student could remain enrolled, progression was not conditioned on performance, and students could repeatedly refuse exam results without formal repercussions. In such a framework, dropout coexisted with a large area of "stagnation", where students remained enrolled but accumulated credits slowly, contributing to very long durations and high share of delays in graduation. Moreover, university *curricula* were rigid and highly standardized at the national level and, in a governance framework

marked by strong central regulation and limited institutional autonomy, universities had few incentives and reduced capacity to innovate in teaching organization or student support, reinforcing inefficiencies as the system expanded and the student population became more heterogeneous (Ballarino and Perotti, 2012).

C Cleaning Process

“Surveys on Educational and Professional paths of Upper Secondary School graduates”

The original surveys were cleaned and harmonized across waves to remove discrepancies and ensure comparability. For each variable, we retained the most detailed level consistently available and generally kept observations with missing values, later including a specific missing value dummy in the regressions. This rule was applied systematically, with a few exceptions where missingness affected particularly important variables, in which case we dropped the corresponding observations. We report these exceptions below by year of high school graduation:

- **Graduates in 1995:** we removed 2 observations that had inconsistent answers about enrollment in university (shrinking the original size of 18,843 to 18,841 observations)
- **Graduates in 1998:** we removed 9 observations with missing values in enrollment in university, gender, and other variables (from 23,261 to 23,253 observations)
- **Graduates in 2001:** we removed 188 observations that had missing values in father education, mother education, gender, and other variables (from 20,408 to 20,220 observations)
- **Graduates in 2004, 2007, 2011:** no observations were removed

In total, we removed 199 observations: from 141,215 to the final size of 141,016 observations (0.14%).

“Surveys on Employment outcomes of University graduates”

The same cleaning procedure was applied to these surveys: they were cleaned and harmonized over years with the objective of keeping as many observations as possible. We only dropped observations when they presented missing values in education for both parents (which is used to define treatment) and in other relevant variables such as gender, age, employment status, years to graduate and wage. Below, we report the number of observations that were dropped in each survey by year of graduation from university:

- **1998:** we removed 456 observations (from 17,326 to 16,870 observations)
- **2001:** we removed 315 observations (20,844 to 20,529 observations)
- **2004:** we removed 236 observations (26,006 to 25,770 observations)
- **2007:** we removed 444 observations (47,300 to 46,856 observations)
- **2011:** we removed 580 observations (62,000 to 61,420 observations)
- **2015:** we removed 1,193 observations (58,400 to 57,207 observations)

In total, we removed 3,224 observations: from 231,876 to 228,652 observations (1.39%).

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