ISSN 2704-7407

UNIVERSITÀ CATTOLICA DEL SACRO CUORE
Dipartimento di Economia e Finanza

Working Paper Series

Backward Spillovers and Equalized Access: The Effects of the Bologna Process across Italy's Education System

Lorenzo Cappellari, Marco Morelli

Working Paper n. 149

Febbraio 2026

UNIVERSITÀ
CATTOLICA
del Sacro Cuore

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

Lorenzo Cappellari
Università Cattolica del Sacro Cuore

Marco Morelli
University of Luxembourg

Working Paper n. 149
Febbraio 2026

Dipartimento di Economia e Finanza
Università Cattolica del Sacro Cuore
Largo Gemelli 1 - 20123 Milano – Italy
tel: +39.02.7234.2976
e-mail: dip.economiaefinanza@unicatt.it

Backward Spillovers and Equalized Access: The Effects of the Bologna Process across Italy's Education System

Lorenzo Cappellari[1] and Marco Morelli[2]

[1] *Università Cattolica del Sacro Cuore, Milan*

[2] *University of Luxembourg, Luxembourg*

February 2026

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 systematically larger for students 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.

JEL Classification: I23, I24, I28

Keywords: Higher education reform, University enrollment and dropout, Backward spillovers

Acknowledgments: The data were accessed and analyzed at Laboratorio ADELE (ISTAT) in Florence and Milan. Access to the data was provided under the standard ISTAT procedures. The opinions expressed and any remaining errors are solely the responsibility of the authors. We acknowledge funding from the Italian Ministry of Research (PRIN grant 2022-J53D23004660008 "STEP-BY-STEP").

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 rates. 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 has analyzed the Italian case, prior research answers only part of the questions, while the public debate remains polarized and often skeptical of 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 the share of 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.

In terms of 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 has primarily focused on university-related outcomes, reaching only partial agreement. At the same time, research on the labor market effects of the Bologna Process remains very limited, and evidence on its effects on other stages of the educational system is, to our knowledge, absent. 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.. Regarding inequality in access to 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 Cutillo, 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 approach in the existing literature overlooks an important feature of the Italian institutional setting. In a rigid early-tracking system like the Italian one, changes in university access incentives can 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 this framework, a reform that makes university more accessible is likely to affect both 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 documents 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, potentially increasing dropout through a decline in the average quality of 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.

As described in Section 2, 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. Indeed, given the high intergenerational persistence of education in Italy, these students 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 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 we keep as post-reform

[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.

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:

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

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 following way:

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

---

[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.

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. As for 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. We interpret this result as the combination of two mechanisms: an increase in retention and a direct choice effect. 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. Regarding 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 induce 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***	0.0553***	-0.0271***	0.0722***
		(0.0128)	(0.0155)	(0.0075)	(0.0184)
Treatment Intensity	Lowest PE	0.0781***	0.0661***	-0.0322***	0.1088***
		(0.0071)	(0.0143)	(0.0069)	(0.0249)
	Medium PE	0.0312***	0.0402***	-0.0216***	0.0468***
		(0.0077)	(0.0146)	(0.0065)	(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 robustness 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***	0.0380***	-0.0203***
		(0.0088)	(0.0115)	(0.0075)
Treated x Post 2	(Announced)	0.0739***	0.0680***	-0.0331***
		(0.0184)	(0.0210)	(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 disentangle 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.

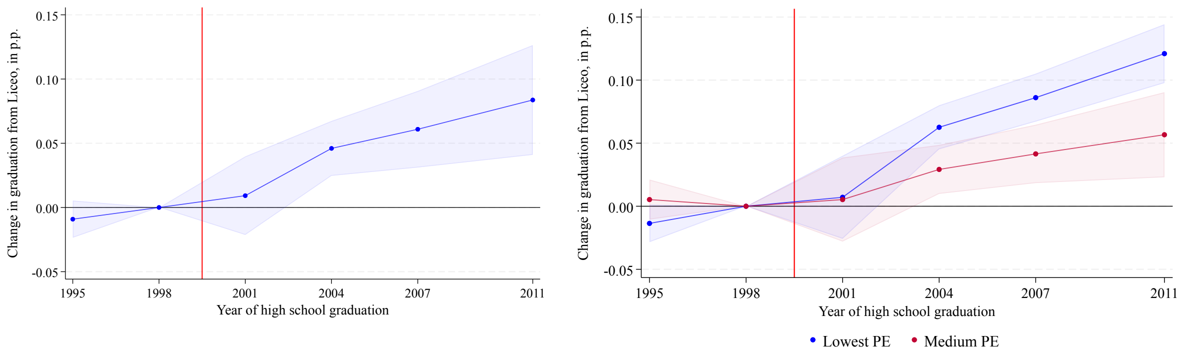
The treatment intensity by parental education (Lowest PE and Medium PE) using the three-period Difference-in-Differences specification is presented in Table A19 in the Appendix. For all outcomes, the estimates are consistent with the results presented before: within each period, the effects of the reform 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

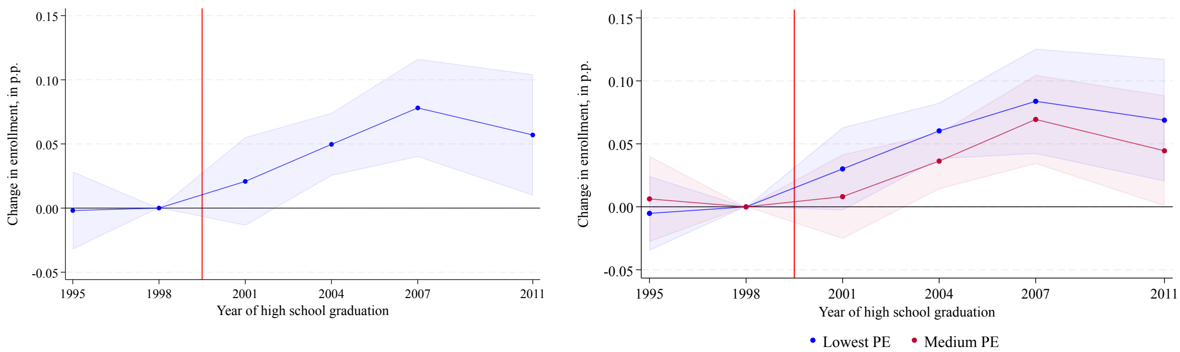
(a) Treatment (b) Treatment intensity



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

(a) Treatment (b) 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 Excellent; 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 on 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***	0.0564***	0.0892***	0.0385**	0.0550***
		(0.0084)	(0.0189)	(0.0236)	(0.0186)	(0.0128)
Treatment Intensity	Lowest PE	0.0378***	0.0885***	0.1171***	0.0590***	0.0781***
		(0.0062)	(0.0120)	(0.0180)	(0.0161)	(0.0071)
	Medium PE	0.0160**	0.0205	0.0577***	0.0141	0.0312***
		(0.0080)	(0.0138)	(0.0216)	(0.0207)	(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 subgroup 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 in terms of 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	0.1081*	0.0150	-0.0046	0.0251	0.0686***	0.0553***
		(0.0180)	(0.0599)	(0.0205)	(0.0393)	(0.0163)	(0.0117)	(0.0155)
Treatment Intensity	Lowest PE	-0.0164	0.1234**	0.0154	-0.0179	0.0471***	0.0772***	0.0661***
		(0.0188)	(0.0591)	(0.0204)	(0.0425)	(0.0123)	(0.0119)	(0.0143)
	Medium PE	-0.0309*	0.0728	0.0146	0.0160	0.0014	0.0581***	0.0402***
		(0.0173)	(0.0608)	(0.0215)	(0.0384)	(0.0151)	(0.0129)	(0.0146)
	Pre-reform average	13.94%	32.90%	30.50%	60.00%	72.42%	89.76%	44.44%
	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. When examining 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 in terms of 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 on 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- bility 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.

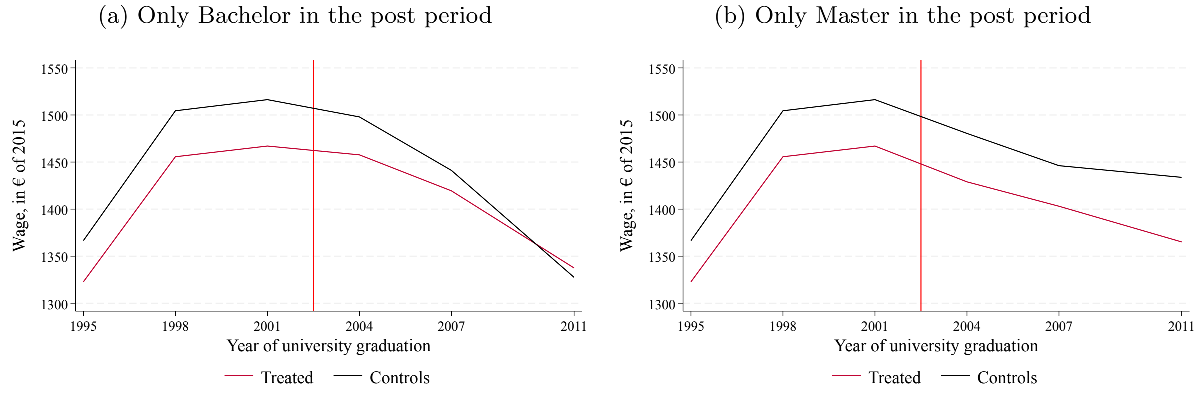
The heterogeneity analysis of the effects of the reform by gender reveals a clear pattern: females experience larger effects than males in all outcomes of interest. When examining graduation from the academic-oriented high school track (*Liceo*), in Table A24 in the Appendix we can see that females experience 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, turning to 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 potential selection bias, we apply the same identification strategy to labor market outcomes and present the results for descriptive purposes, without claims of causality. We implement the same two-period Difference-in-Differences using the same treatment definition based on parental education, and for every outcome we present two pairwise comparisons: Old degrees against Bachelor's degrees, and Old degrees against Master's degrees.[6] We present in Figure 3 the time series of wage[7] 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, we observe a clear convergence in wages between low parental background graduates (Treated) and high parental background graduates (Controls). A phenomenon that is not in place when considering Master's graduates, for whom wages slightly diverge toward the end of the period. The estimates of the two-period Difference-in-Differences on wage and hourly wage in Table 6 confirm this descriptive picture.

When considering only Bachelor's graduates in the post-reform period, we observe a positive and significant +7.64% increase in wage among the treated with respect to the controls, indicating a significant convergence in wages and a closing of the pre-reform wage gap between graduates from low and high parental backgrounds. By contrast, when only Master's graduates are considered in the post-reform period, the effect is slightly negative and not statistically significant, implying that the pre-reform wage gap between graduates from different parental backgrounds is not reduced. As a robustness check, we estimate the same model using the hourly wage as an outcome. The estimates present the same pattern (positive and strong effect among Bachelor's, non-significant effects among Master's), showing that the

[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.

differences in the trends of the wage gaps are not driven by the amount of hours worked.[8]

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***	-0.0144	0.0541***	0.0098
	(0.0228)	(0.0128)	(0.0132)	(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***	0.1081***	0.0490**	-0.0144	0.0040	-0.0258**
	(0.0228)	(0.0292)	(0.0172)	(0.0128)	(0.0272)	(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).

[8] The Parallel Trend Assumption holds for both variables as shown in Figure A2 and Figure A3 in the Appendix.

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.[9] 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**	-0.0417**	-0.0191*	-0.0073	-0.0133	-0.0033
	(0.0126)	(0.0162)	(0.0106)	(0.0048)	(0.0109)	(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 of these findings is that they reflect differences in the types of job to which Bachelor's and Master's graduates have access, combined with the differential influence of parental background

[9] 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.

plays across the occupational categories. In Table 9 it is possible to observe two patterns in the distribution of the job types of university graduates. First, the occupational distribution of Master's graduates closely mirrors that of graduates from the old one-tier degrees: Master's graduates enter high-skilled professional jobs in proportions comparable to old degree graduates, suggesting that the reform left the top of the occupational distribution essentially unchanged. Second, the occupational distributions of Bachelor's and Master's graduates appear to be significantly different, with Bachelor's graduates predominantly absorbed into medium-skilled occupations and Master's graduate employed in highly skilled jobs. In fact, only 12.29% of Bachelor's graduates are employed as Professionals (against 44.28% among Master's graduates), while 64.40% work as Technicians and Associate Professionals and 8.01% as Service and Sales Workers (against 35.32% and 4.83%, respectively among Master's graduates).

Table 9: Distribution of Job Type of University graduates by Highest Degree attained, in %

Job type	Old degree	Bachelor's degree	Master's degree
Legislators, Senior Officials and Managers	2.65	1.73	1.52
Professionals	46.64	12.29	44.28
Technicians and Associate Professionals	36.53	64.40	35.32
Clerical Support Workers	9.05	10.64	11.78
Service and Sales Workers	3.08	8.01	4.83
Skilled Manual Workers	0.51	0.95	0.51
Plant and Machine Operators, Assemblers	0.25	0.44	0.17
Elementary Occupations	0.28	0.55	0.36
Armed Forces Occupations	1.00	0.99	1.22
Total	100.00	100.00	100.00
Number of Observations	66,124	26,012	35,704

Notes: The 9 categories are retrieved from the ISTAT CP2011 classification, which replicates the ISCO-08 major groups.

These compositional differences in the job types distribution can explain the different convergence behaviors of the labor market outcomes between Bachelor's and Master's graduates. Bachelor's graduates are predominantly employed in medium-skilled occupations, in which hiring and remuneration depend largely on the formal credential and on the technical competences required for the role, whereas the role of family networks and parental background in shaping wages and employment prospects is comparatively limited. In this segment of the labor market, Bachelor's graduates from low parental backgrounds can effectively close the existing gap with their high parental background peers. By contrast, Master's graduates are employed in high-skilled professional occupations, in which a substantial body of literature indicates that family networks and parental connections retain a significant role in shaping hiring opportunities, career progression, and earnings (Corak and Piraino, 2011; Mocetti, 2016; Aina and Nicoletti, 2018). In this segment of the labor market, the formal degree appears not to be sufficient to offset the advantage of graduates from high parental backgrounds, which is consistent with the persistence of the wage and unemployment gaps we document among Master's graduates.

In this sense, the labor market evidence we document is consistent with the broader narrative emerging from our analysis: by reducing the cost of the first tier of the new structure, the Bologna Process expanded access to the labor market through Bachelor's degrees, particularly for students from disadvantaged backgrounds. Once in the labor market, however, the extent to which graduates from different parental backgrounds converge in their outcomes appears to be bounded by the type of jobs to which each degree gives access, and by the persistent role that family background plays in the high-skilled occupations that Master's graduates predominantly enter.

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. 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*.

Regarding labor market outcomes, we showed descriptively 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 whom a significant gap still exists in both dimensions. This asymmetry is consistent with the differential occupational composition of the two new degrees. Bachelor's graduates are predominantly absorbed into medium-skilled occupations, in which outcomes depend largely on the formal credential and on technical competences, while family networks play a more limited role. By contrast, Master's graduates are concentrated in high-skilled professional occupations, in which parental background and family connections retain a substantial role, thereby preserving the observed gap.

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 are stronger and more statistically significant among high ability students, and especially among those 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	0.0012	-0.0187	-0.0009
	(0.0076)	(0.0154)	(0.0222)	(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	-0.0249*	-0.0158
		(0.0124)	(0.0138)	(0.0124)
3-period DiD	Treated x Post 1	-0.0322**	-0.0435***	-0.0245
		(0.0157)	(0.0167)	(0.0164)
	Treated x Post 2	-0.0085	-0.0053	-0.0081
		(0.0103)	(0.0107)	(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***	0.0676***	
		(0.0071)	(0.0058)	
	Medium PE x Post	0.0312***		0.0367***
		(0.0077)		(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***	0.0627***	
		(0.0143)	(0.0142)	
	Medium PE x Post	0.0402***		0.0386***
		(0.0146)		(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***	-0.0317***	
		(0.0069)	(0.0065)	
	Medium PE x Post	-0.0216***		-0.0191***
		(0.0065)		(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***	0.1055***	
		(0.0249)	(0.0241)	
	Medium PE x Post	0.0468***		0.0499***
		(0.0118)		(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***	0.0515***	0.0487***
		(0.0128)	(0.0127)	(0.0130)
Treatment Intensity	Lowest PE	0.0781***	0.0739***	0.0665***
		(0.0071)	(0.0060)	(0.0093)
	Medium PE	0.0312***	0.0274***	0.0295***
		(0.0077)	(0.0073)	(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***	0.0497***	0.0372***
		(0.0155)	(0.0136)	(0.0095)
Treatment Intensity	Lowest PE	0.0661***	0.0612***	0.0421***
		(0.0143)	(0.0117)	(0.0090)
	Medium PE	0.0402***	0.0335***	0.0301***
		(0.0146)	(0.0123)	(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***	-0.0241***	-0.0184***
		(0.0057)	(0.0065)	(0.0062)
Treatment Intensity	Lowest PE	-0.0322***	-0.0297***	-0.0214***
		(0.0069)	(0.0079)	(0.0075)
	Medium PE	-0.0216***	-0.0181***	-0.0151**
		(0.0065)	(0.0068)	(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***	0.0835***	0.0820***
		(0.0184)	(0.0231)	(0.0226)
Treatment Intensity	Lowest PE	0.1088***	0.1342***	0.1323***
		(0.0249)	(0.0350)	(0.0343)
	Medium PE	0.0468***	0.0482***	0.0474***
		(0.0118)	(0.0133)	(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**			
			Lowest PE		**Medium PE**	
	ATE	ATT	ATE	ATT	ATE	ATT
Treated x Post	0.0550***	0.0581***	0.0781***	0.0830***	0.0312***	0.0330***
	(0.0128)	(0.0138)	(0.0071)	(0.0074)	(0.0077)	(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**			
			Lowest PE		**Medium PE**	
	ATE	ATT	ATE	ATT	ATE	ATT
Treated x Post	0.0553***	0.0592***	0.0661***	0.0711***	0.0402***	0.0432***
	(0.0155)	(0.0166)	(0.0143)	(0.0155)	(0.0146)	(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**			
			Lowest PE		**Medium PE**	
	ATE	ATT	ATE	ATT	ATE	ATT
Treated x Post	-0.0271***	-0.0296***	-0.0322***	-0.0351***	-0.0216***	-0.0236***
	(0.0075)	(0.0061)	(0.0069)	(0.0074)	(0.0065)	(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**			
			Lowest PE		**Medium PE**	
	ATE	ATT	ATE	ATT	ATE	ATT
Treated x Post	0.0722***	0.0898***	0.1088***	0.1354***	0.0468***	0.0582***
	(0.0184)	(0.0233)	(0.0249)	(0.0314)	(0.0118)	(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	0.0165	0.0514***	0.0553***
		(0.0208)	(0.0174)	(0.0110)	(0.0155)
Treatment Intensity	Lowest PE	0.0213	0.0132	0.0674***	0.0661***
		(0.0211)	(0.0182)	(0.0079)	(0.0143)
	Medium PE	-0.0048	0.0209	0.0332***	0.0402***
		(0.0188)	(0.0190)	(0.0110)	(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***	-0.0403**	0.0159	0.0017	-0.0271***
		(0.0182)	(0.0164)	(0.0327)	(0.0149)	(0.0057)
Treatment Intensity	Lowest PE	-0.0607***	-0.0515**	0.0105	-0.0028	-0.0322***
		(0.0192)	(0.0218)	(0.0329)	(0.0151)	(0.0069)
	Medium PE	-0.0636***	-0.0287*	0.0229	0.0077	-0.0216***
		(0.0187)	(0.0162)	(0.0326)	(0.0155)	(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**	0.0661***	0.0755***	0.0596***	0.0722***
		(0.0193)	(0.0171)	(0.0184)	(0.0218)	(0.0184)
Treatment Intensity	Lowest PE	0.0880***	0.1011***	0.1194***	0.0826***	0.1088***
		(0.0226)	(0.0267)	(0.0191)	(0.0286)	(0.0249)
	Medium PE	0.0101	0.0404***	0.0459***	0.0448**	0.0468***
		(0.0072)	(0.0084)	(0.0107)	(0.0196)	(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	-0.0371	0.0117	-0.0171	0.0015	-0.0338**	-0.0199
		(0.0589)	(0.0364)	(0.0221)	(0.0301)	(0.0213)	(0.0161)	(0.0124)
Treatment Intensity	Lowest PE	0.0105	-0.0312	0.0068	-0.0030	-0.0156	-0.0480**	-0.0249*
		(0.0594)	(0.0380)	(0.0259)	(0.0295)	(0.0193)	(0.0233)	(0.0138)
	Medium PE	0.0526	-0.0485	0.0164	-0.0311	0.0123	-0.0262*	-0.0158
		(0.0617)	(0.0362)	(0.0246)	(0.0303)	(0.0208)	(0.0155)	(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**	0.0635***	0.0550***
		(0.0122)	(0.0152)	(0.0128)
Treatment	Lowest PE x Post	0.0477***	0.0817***	0.0781***
Intensity		(0.0094)	(0.0118)	(0.0071)
	Medium PE x Post	0.0103	0.0400***	0.0312***
		(0.0102)	(0.0132)	(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**	0.0659***	0.0553***
		(0.0151)	(0.0228)	(0.0155)
Treatment	Lowest PE x Post	0.0435***	0.0775***	0.0661***
Intensity		(0.0148)	(0.0215)	(0.0143)
	Medium PE x Post	0.0301*	0.0471**	0.0402***
		(0.0155)	(0.0237)	(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**	-0.0266***	-0.0271***
		(0.0094)	(0.0086)	(0.0057)
Treatment Intensity	Lowest PE x Post	-0.0315***	-0.0268**	-0.0322***
		(0.0092)	(0.0106)	(0.0069)
	Medium PE x Post	-0.0128	-0.0263***	-0.0216***
		(0.0094)	(0.0081)	(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***	0.0896***	0.0756***	-0.0144	-0.0131	-0.0153
	(0.0228)	(0.0225)	(0.0201)	(0.0128)	(0.0115)	(0.0100)
Treatment Intensity						
Lowest PE	0.1081***	0.1174***	0.0974***	0.0040	0.0003	-0.0060
	(0.0292)	(0.0233)	(0.0184)	(0.0272)	(0.0220)	(0.0181)
Medium PE	0.0490**	0.0659***	0.0576**	-0.0258**	-0.0212**	-0.0209**
	(0.0172)	(0.0208)	(0.0196)	(0.0090)	(0.0092)	(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***	0.0717***	0.0144	-0.0165
	(0.0228)	(0.0205)	(0.0128)	(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***	0.0532***	0.0441***	0.0098	0.0064	0.0043
	(0.0132)	(0.0101)	(0.0089)	(0.0108)	(0.0080)	(0.0071)
Treatment Intensity						
Lowest PE	0.0712***	0.0623***	0.0459***	0.0244	0.0126	0.0056
	(0.0177)	(0.0104)	(0.0077)	(0.0245)	(0.0166)	(0.0132)
Medium PE	0.0392***	0.0455***	0.0427***	0.0009	0.0026	0.0036
	(0.0101)	(0.0112)	(0.0109)	(0.0057)	(0.0064)	(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

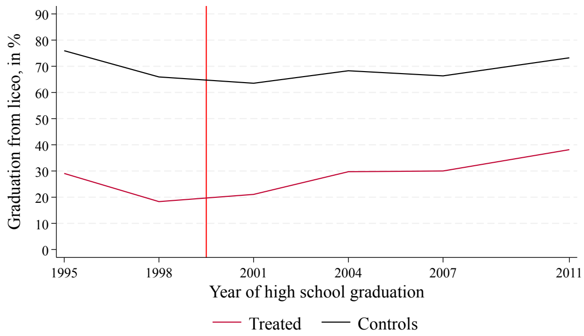
Notes: Standard errors clustered at parental education level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Baseline of control variables: male, 25–29 years old at graduation, region of the school = Piedmont, middle school final mark = sufficient, high school final mark = 60/69, not external candidate, never rejected, school track = *Istituto Professionale*, mother and father are blue collar employees. Cohort Fixed Effects and Group Fixed Effects are included.

Figures

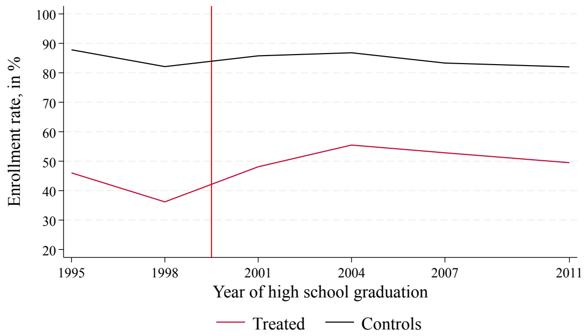
Parallel Trend Assumption

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

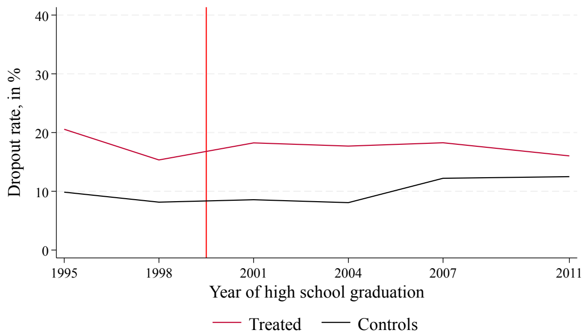
(a) Graduation from a *Liceo*



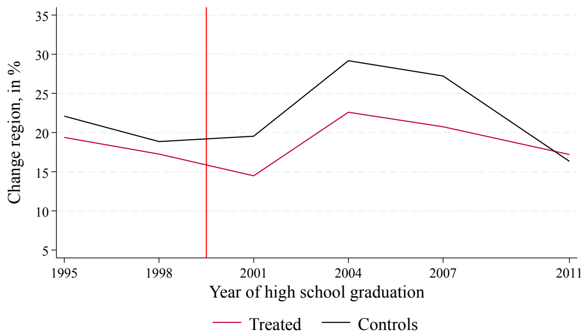
(b) University Enrollment



(c) University Dropout



(d) Change region for University Enrollment



(e) In time graduation

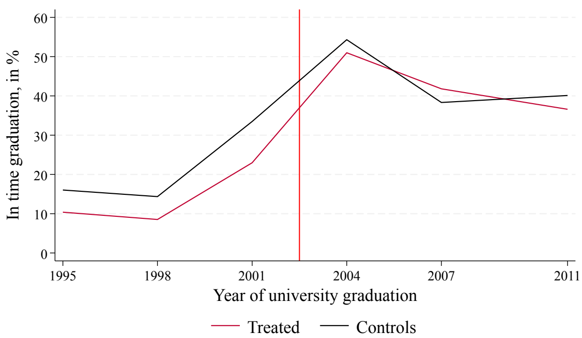
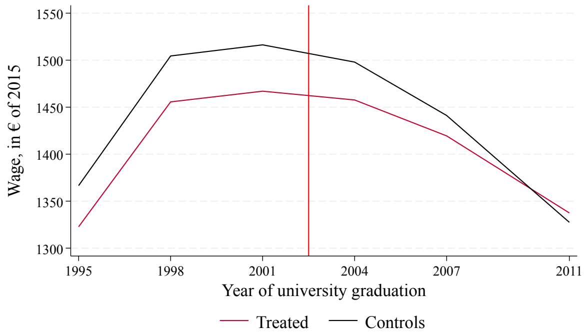


Figure A2: Time series plot of Wage: pairwise comparisons (net monthly wage in 2015€)

(a) Only Bachelor's graduates in Post reform period



(b) Only Master's graduates in Post reform period

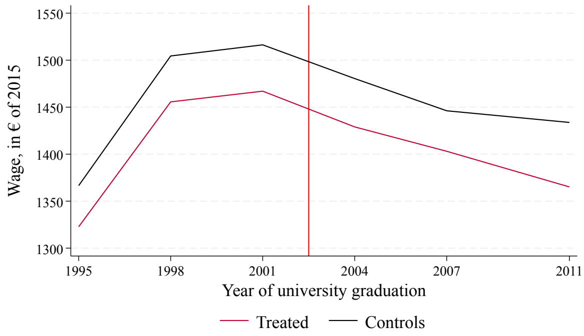
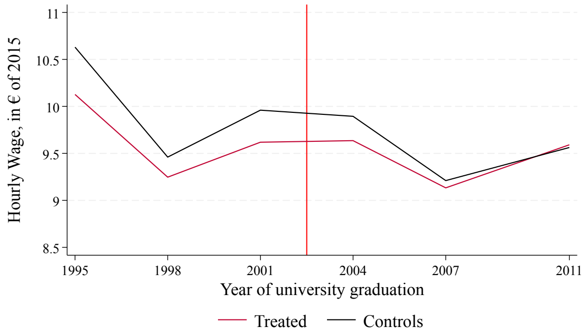


Figure A3: Time series plot of Hourly Wage: pairwise comparisons (in 2015€)

(a) Only Bachelor's graduates in Post reform period



(b) Only Master's graduates in Post reform period

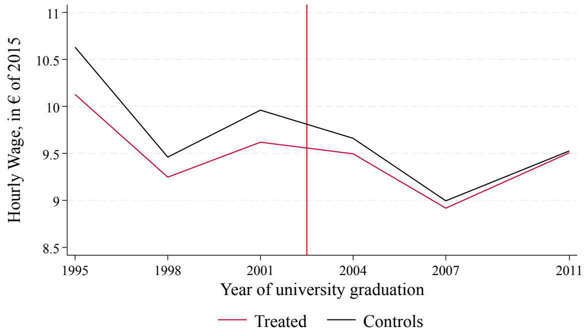
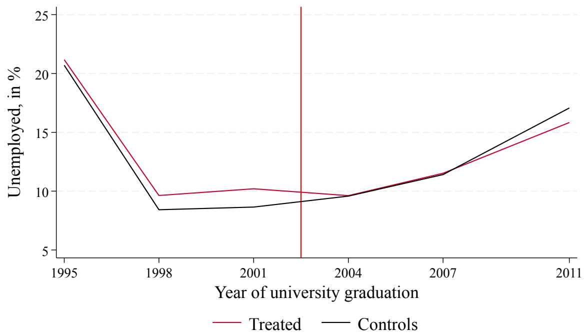
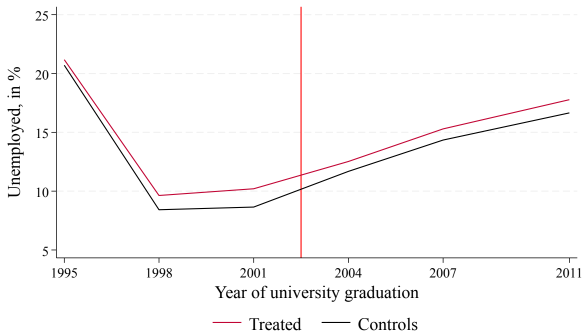


Figure A4: Time series plot of Unemployed: pairwise comparisons

(a) Only Bachelor's graduates in Post reform period



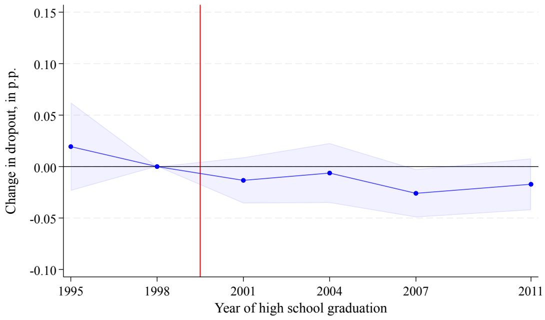
(b) Only Master's graduates in Post reform period



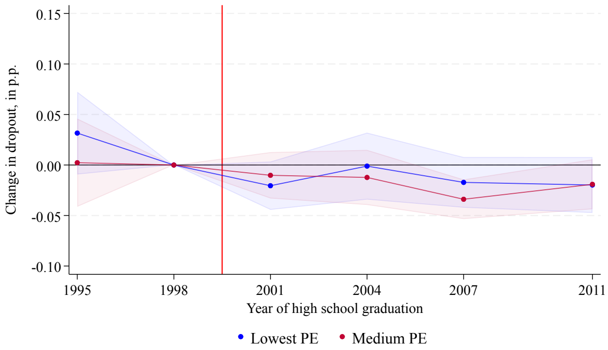
Flexible year-by-year Difference-in-Differences

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

(a) Treatment



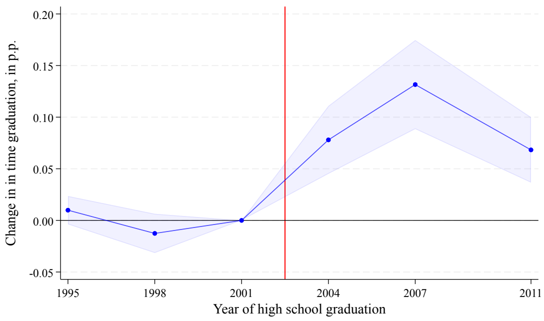
(b) Treatment Intensity



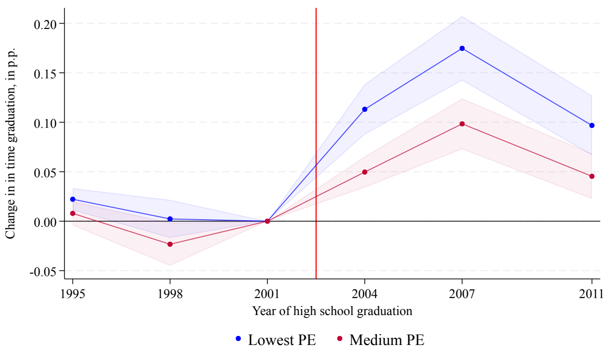
Note: Survey of graduates in 1998 (last one before the reform) is considered as reference year for high school graduates. The shaded region denotes 95% confidence intervals constructed using standard errors clustered at the level of parental education. No control variables included. Cohort Fixed Effects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

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

(a) Treatment



(b) 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%).

References

Agasisti, T. and Catalano, G. (2007). Efficienza ed equità nel sistema universitario italiano: gli effetti di quindici anni di riforme. Technical report, Società Italiana di Economia Pubblica (SIEP), Pavia. Conferenza SIEP, Economia del capitale umano, 13–14 settembre 2007.

Aina, C. (2013). Parental background and university dropout in Italy. *Higher Education*, 65:437–456.

Aina, C. and Nicoletti, C. (2018). The intergenerational transmission of liberal professions. *Labour Economics*, 51:108–120.

Angeloni, S. (2019). A policy reform aimed at improving the job quality of graduates. *Higher Education Policy*, 34:1–20.

Argentin, G. and Triventi, M. (2011). Social inequality in higher education and labour market in a period of institutional reforms: Italy, 1992–2007. *Higher Education*, 61:309–323.

Avdeev, S. (2025). Balancing study and work: Heterogeneous impact of the Bologna reform on the labour market. *Oxford Bulletin of Economics and Statistics*, 87:252–286.

Ballarino, G. and Perotti, L. (2012). The Bologna Process in Italy. *European Journal of Education*, 47(3):348–363.

Belot, M., Canton, E., and Webbink, D. (2007). Does reducing student support affect scholastic performance? Evidence from a Dutch reform. *Empirical Economics*, 32:261–275.

Bettinger, E. P., Long, B. T., Oreopoulos, P., and Sanbonmatsu, L. (2012). The role of application assistance and information in college decisions: Results from the H&R block FAFSA experiment. *The Quarterly Journal of Economics*, 127(3):1205–1242.

Björklund, A. and Salvanes, K. G. (2011). Education and family background: Mechanisms and policies. In Hanushek, E. A., Machin, S., and Woessmann, L., editors, *Handbook of the Economics of Education*, volume 3, pages 201–247. Elsevier.

Bondonio, D. and Berton, F. (2018). The impact of degree duration on higher education participation: Evidence from a large-scale natural experiment. *Oxford Bulletin of Economics and Statistics*, 80(5):905–930.

Bosio, G. and Leonardi, M. (2010). The impact of Bologna process on the graduate labour market: Demand and supply. *Giornale degli Economisti e Annali di Economia*, 69(3):29–65.

Brunello, G. and Checchi, D. (2007). Does school tracking affect equality of opportunity? New international evidence. *Economic Policy*, 22(52).

Brunori, P., Peragine, V., and Serlenga, L. (2012). Fairness in education: The Italian university before and after the reform. *Economics of Education Review*, 31:764–777.

Cappellari, L. and Lucifora, C. (2009). The Bologna Process and college enrollment decisions. *Labour Economics*, 16:638–647.

Cardoso, A. R., Portela, M., Sá, C., and Alexandre, F. (2007). Demand for higher education programs: The impact of the Bologna Process. *CESifo Economic Studies*, (CESifo Working Paper No. 2081).

Carlana, M., La Ferrara, E., and Pinotti, P. (2022). Implicit stereotypes in teachers' track recommendations. *AEA Papers and Proceedings*, 112:409–414.

Carneiro, P. and Heckman, J. J. (2002). The evidence on credit constraints in post-secondary schooling. *The Economic Journal*, 112(482):705–734.

Checchi, D. and Flabbi, L. (2007). Intergenerational mobility and schooling decisions in Germany and Italy: The impact of secondary school tracks. *IZA Discussion Papers*, (2876).

Checchi, D., Ichino, A., and Rustichini, A. (1999). More equal but less mobile? education financing and intergenerational mobility in Italy and in the US. *Journal of Public Economics*, 74:351–393.

Chetty, R., Hendren, N., and Katz, L. F. (2016). The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment. *American Economic Review*, 106(4):855–902.

Chies, L., Graziosi, G., and Pauli, F. (2019). The impact of the Bologna Process on graduation: New evidence from Italy. *Research in Higher Education*, 60(2):203–218.

CNVSU (2005). Sesto rapporto sullo stato del sistema universitario. *Ministero dell'Istruzione, dell'Università e della Ricerca (MIUR)*, (VI).

Corak, M. and Piraino, P. (2011). The intergenerational transmission of employers. *Journal of Labor Economics*, 29(1):37–68.

d'Hombres, B. (2007). The impact of university reforms on dropout rates and students' status: Evidence from Italy. *EUR Scientific and Technical Research series, European Commission Joint Research Centre*, (JRC 40507):1–30.

Di Pietro, G. (2004). The determinants of university dropout in Italy: a bivariate probability model with sample selection. *Applied Economics Letters*, 11(3):187–191.

Di Pietro, G. (2012). The Bologna Process and widening participation in university education: New evidence from Italy. *Empirica*, 39:357–374.

Di Pietro, G. and Cutillo, A. (2008). Degree flexibility and university drop-out: The Italian experience. *Economics of Education Review*, 27:546–555.

Dohmen, T., Falk, A., Huffman, D., Sunde, U., Schupp, J., and Wagner, G. G. (2011). Individual risk attitudes: Measurement, determinants, and behavioral consequences. *Journal of the European Economic Association*, 9(3):522–550.

Dustmann, C. (2004). Parental background, secondary school track choice, and wages. *Oxford Economic Papers*, 56(2):209–230.

Enzi, B. and Siegler, B. (2016). The impact of the Bologna reform on student outcomes: Evidence from exogenous variation in regional supply of Bachelor programs in Germany. *Ifo Working Papers*, (Ifo Working Paper No. 225).

Eurydice (1999). Financial support for students in higher education in Europe: Trends and debates. Technical report, EURYDICE European Unit, Brussels. Key Topics in Education, Volume I. ISBN 92-828-5163-X.

Hahm, S. and Kluve, J. (2019). Better with Bologna? Tertiary education reform and student outcomes. *Education Economics*, 27(4):425–449.

Hanushek, E. A. and Wößmann, L. (2006). Does educational tracking affect performance and inequality? Differences-in-Differences evidence across countries. *The Economic Journal*, 116:C63–C76.

Horstschräer, J. and Sprietsma, M. (2015). The effects of the introduction of Bachelor degrees on college enrollment and dropout rates. *Education Economics*, 23:296–317.

Kroher, M., Leuze, K., Thomsen, S. L., and Trunzer, J. (2021). Did the Bologna Process achieve its goals? 20 years of empirical evidence on student enrolment, study success and labour market outcomes. *IZA Discussion Papers*, (IZA Discussion Paper No. 14757).

Lerche, K. (2016). The effect of the Bologna process on the duration of studies. *CEGE Discussion papers*, (287).

Mocetti, S. (2016). Dynasties in professions and the role of rents and regulation: Evidence from italian pharmacies. *Journal of Public Economics*, 133:1–10.

Mühlenweg, A. (2010). Teaching, organization, and personal problems: Evidence from reforming tertiary education in Germany. Technical Report ZEW Discussion Paper No. 10-040, ZEW – Centre for European Economic Research, Mannheim.

Neugebauer, M. (2015). The introduction of Bachelor degrees and the under-representation of students from low social origin in higher education in Germany: A pseudo-panel approach. *European Sociological Review*, 31:591–602.

Neugebauer, M. and Weiss, F. (2018). A transition without tradition: Earnings and unemployment risks of academic versus vocational education after the Bologna Process. *Zeitschrift für Soziologie*, 47:349–363.

OECD (2000). *Education at a Glance 2000: OECD Indicators*. OECD Publishing, Paris.

OECD (2002). *Education at a Glance 2002: OECD Indicators*. OECD Publishing, Paris.

OECD (2023). *Education at a Glance 2023: OECD Indicators*. OECD Publishing, Paris.

OECD (2025). Educational Attainment in Italy, Adults 25–34 and 25–64, ISCED 2011 levels 5–8. `https://data-explorer.oecd.org/vis?fs[0]=Topic%2C1%7CEducation%20and%20skills%23EDU%23%7CEducation%20attainment%23EDU_ATT%23&pg=0&fc=Topic&bp=true&snb=6&df[ds]=dsDisseminateFinalDMZ&df[id]=DSD_EAG_LSO_EA%40DF_LSO_NEAC_DISTR_EA&df[ag]=OECD.EDU.IMEP&df[vs]=1.0&dq=ITA._T.Y25T34%2BY25T64.ISCED11A_5%2BISCED11A_6T8%2BISCED11A_5T8..........OBS...A&pd=1999%2C&to[TIME_PERIOD]=true&vw=tb`. Accessed 24 Oct 2025.

Portela, M., Sá, C., Alexandre, F., and Cardoso, A. R. (2008). Perceptions of the Bologna process: what do students' choices reveal? Working paper, September 2008.

Scandolo, E. (2025). Students' reactions to more flexible university systems: the Bologna Process in Italy. Unpublished manuscript, February 28, 2025.

Sciulli, D. and Signorelli, M. (2011). University-to-work transitions: an empirical analysis on Perugia graduates. *European Journal of Higher Education*, 1:39–65.

Suleman, F. and Figueiredo, M. D. C. (2020). Entering the labour market in the context of higher education reform and economic recession: Young Bachelor and Master graduates in Portugal. *Journal of Youth Studies*, 23:1396–1417.

Triventi, M. and Trivellato, P. (2009). Participation, performance and inequality in Italian higher education in the 20th century: Evidence from the Italian Longitudinal Household Survey. *Higher Education*, 57(6):681–702.

Triventi, M., Vergolini, L., and Zanini, N. (2017). Do individuals with high social background graduate from more rewarding fields of study? Changing patterns before and after the Bologna process. *Research in Social Stratification and Mobility*, 51:28–40.

Working Paper del Dipartimento di Economia e Finanza

1. L. Colombo, H. Dawid, *Strategic Location Choice under Dynamic Oligopolistic Competition and Spillovers*, novembre 2013.
2. M. Bordignon, M. Gamalerio, G. Turati, *Decentralization, Vertical Fiscal Imbalance, and Political Selection*, novembre 2013.
3. M. Guerini, *Is the Friedman Rule Stabilizing? Some Unpleasant Results in a Heterogeneous Expectations Framework*, novembre 2013.
4. E. Brenna, C. Di Novi, *Is caring for elderly parents detrimental to women's mental health? The influence of the European North-South gradient*, novembre 2013.
5. F. Sobbrio, *Citizen-Editors' Endogenous Information Acquisition and News Accuracy*, novembre 2013.
6. P. Bingley, L. Cappellari, *Correlation of Brothers Earnings and Intergenerational Transmission*, novembre 2013.
7. T. Assenza, W. A. Brock, C. H. Hommes, *Animal Spirits, Heterogeneous Expectations and the Emergence of Booms and Busts*, dicembre 2013.
8. D. Parisi, *Is There Room for 'Fear' as a Human Passion in the Work by Adam Smith?*, gennaio 2014.
9. E. Brenna, F. Spandonaro, *Does federalism induce patients' mobility across regions? Evidence from the Italian experience*, febbraio 2014.
10. A. Monticini, F. Ravazzolo, *Forecasting the intraday market price of money*, febbraio 2014.
11. Tiziana Assenza, Jakob Grazzini, Cars Hommes, Domenico Massaro, *PQ Strategies in Monopolistic Competition: Some Insights from the Lab*, marzo 2014.
12. R. Davidson, A. Monticini, *Heteroskedasticity-and-Autocorrelation-Consistent Bootstrapping*, marzo 2014.
13. C. Lucifora, S. Moriconi, *Policy Myopia and Labour Market Institutions*, giugno 2014.
14. N. Pecora, A. Spelta, *Shareholding Network in the Euro Area Banking Market*, giugno 2014.
15. G. Mazzolini, *The economic consequences of accidents at work*, giugno 2014.
16. M. Ambrosanio, P. Balduzzi, M. Bordignon, *Economic crisis and fiscal federalism in Italy*, settembre 2014.
17. P. Bingley, L. Cappellari, K. Tatsiramos, *Family, Community and Long-Term Earnings Inequality*, ottobre 2014.
18. S. Frazzoni, M. L. Mancusi, Z. Rotondi, M. Sobrero, A. Vezzulli, *Innovation and export in SMEs: the role of relationship banking*, novembre 2014.
19. H. Gnutzmann, *Price Discrimination in Asymmetric Industries: Implications for Competition and Welfare*, novembre 2014.
20. A. Baglioni, A. Boitani, M. Bordignon, *Labor mobility and fiscal policy in a currency union*, novembre 2014.
21. C. Nielsen, *Rational Overconfidence and Social Security*, dicembre 2014.
22. M. Kurz, M. Motolese, G. Piccillo, H. Wu, *Monetary Policy with Diverse Private Expectations*, febbraio 2015.
23. S. Piccolo, P. Tedeschi, G. Ursino, *How Limiting Deceptive Practices Harms Consumers*, maggio 2015.
24. A.K.S. Chand, S. Currarini, G. Ursino, *Cheap Talk with Correlated Signals*, maggio 2015.
25. S. Piccolo, P. Tedeschi, G. Ursino, *Deceptive Advertising with Rational Buyers*, giugno 2015.

26. S. Piccolo, E. Tarantino, G. Ursino, *The Value of Transparency in Multidivisional Firms*, giugno 2015.
27. G. Ursino, *Supply Chain Control: a Theory of Vertical Integration*, giugno 2015.
28. I. Aldasoro, D. Delli Gatti, E. Faia, *Bank Networks: Contagion, Systemic Risk and Prudential Policy,* luglio 2015.
29. S. Moriconi, G. Peri, *Country-Specific Preferences and Employment Rates in Europe*, settembre 2015.
30. R. Crinò, L. Ogliari, *Financial Frictions, Product Quality, and International Trade*, settembre 2015.
31. J. Grazzini, A. Spelta, *An empirical analysis of the global input-output network and its evolution*, ottobre 2015.
32. L. Cappellari, A. Di Paolo, *Bilingual Schooling and Earnings: Evidence from a Language-in-Education Reform*, novembre 2015.
33. A. Litina, S. Moriconi, S. Zanaj, *The Cultural Transmission of Environmental Preferences: Evidence from International Migration*, novembre 2015.
34. S. Moriconi, P. M. Picard, S. Zanaj, *Commodity Taxation and Regulatory Competition*, novembre 2015.
35. M. Bordignon, V. Grembi, S. Piazza, *Who do you blame in local finance? An analysis of municipal financing in Italy*, dicembre 2015.
36. A. Spelta, *A unified view of systemic risk: detecting SIFIs and forecasting the financial cycle via EWSs*, gennaio 2016.
37. N. Pecora, A. Spelta, *Discovering SIFIs in interbank communities*, febbraio 2016.
38. M. Botta, L. Colombo, *Macroeconomic and Institutional Determinants of Capital Structure Decisions*, aprile 2016.
39. A. Gamba, G. Immordino, S. Piccolo, *Organized Crime and the Bright Side of Subversion of Law*, maggio 2016.
40. L. Corno, N. Hildebrandt, A. Voena, *Weather Shocks, Age of Marriage and the Direction of Marriage Payments*, maggio 2016.
41. A. Spelta, *Stock prices prediction via tensor decomposition and links forecast*, maggio 2016.
42. T. Assenza, D. Delli Gatti, J. Grazzini, G. Ricchiuti, *Heterogeneous Firms and International Trade: The role of productivity and financial fragility*, giugno 2016.
43. S. Moriconi, *Taxation, industry integration and production efficiency*, giugno 2016.
44. L. Fiorito, C. Orsi, *Survival Value and a Robust, Practical, Joyless Individualism: Thomas Nixon Carver, Social Justice, and Eugenics*, luglio 2016.
45. E. Cottini, P. Ghinetti, *Employment insecurity and employees' health in Denmark,* settembre 2016.
46. G. Cecere, N. Corrocher, M. L. Mancusi, *Financial constraints and public funding for eco-innovation: Empirical evidence on European SMEs,* settembre 2016.
47. E. Brenna, L. Gitto, *Financing elderly care in Italy and Europe. Is there a common vision?*, settembre 2016.
48. D. G. C. Britto, *Unemployment Insurance and the Duration of Employment: Theory and Evidence from a Regression Kink Design*, settembre 2016.
49. E. Caroli, C.Lucifora, D. Vigani, *Is there a Retirement-Health Care utilization puzzle? Evidence from SHARE data in Europe*, ottobre 2016.
50. G. Femminis, *From simple growth to numerical simulations: A primer in dynamic programming*, ottobre 2016.
51. C. Lucifora, M. Tonello, *Monitoring and sanctioning cheating at school: What works? Evidence from a national evaluation program*, ottobre 2016.

52. A. Baglioni, M. Esposito, *Modigliani-Miller Doesn't Hold in a "Bailinable" World: A New Capital Structure to Reduce the Banks' Funding Cost*, novembre 2016.
53. L. Cappellari, P. Castelnovo, D. Checchi, M. Leonardi, *Skilled or educated? Educational reforms, human capital and earnings*, novembre 2016.
54. D. Britto, S. Fiorin, *Corruption and Legislature Size: Evidence from Brazil*, dicembre 2016.
55. F. Andreoli, E. Peluso, *So close yet so unequal: Reconsidering spatial inequality in U.S. cities*, febbraio 2017.
56. E. Cottini, P. Ghinetti, *Is it the way you live or the job you have? Health effects of lifestyles and working conditions*, marzo 2017.
57. A. Albanese, L. Cappellari, M. Leonardi, *The Effects of Youth Labor Market Reforms: Evidence from Italian Apprenticeships*; maggio 2017.
58. S. Perdichizzi, *Estimating Fiscal multipliers in the Eurozone. A Nonlinear Panel Data Approach*, maggio 2017.
59. S. Perdichizzi, *The impact of ECBs conventional and unconventional monetary policies on European banking indexes returns*, maggio 2017.
60. E. Brenna, *Healthcare tax credits: financial help to taxpayers or support to higher income and better educated patients? Evidence from Italy*, giugno 2017.
61. G. Gokmen, T. Nannicini, M. G. Onorato, C. Papageorgiou, *Policies in Hard Times: Assessing the Impact of Financial Crises on Structural Reforms*, settembre 2017.
62. M. Tettamanzi, *E Many Pluribus Unum: A Behavioural Macro-Economic Agent Based Model*, novembre 2017.
63. A. Boitani, C. Punzo, *Banks' leverage behaviour in a two-agent New Keynesian model*, gennaio 2018.
64. M. Bertoni, G. Brunello, L. Cappellari, *Parents, Siblings and Schoolmates. The Effects of Family-School Interactions on Educational Achievement and Long-term Labor Market Outcomes*, gennaio 2018.
65. G. P. Barbetta, G. Sorrenti, G. Turati, *Multigrading and Child Achievement*, gennaio 2018.
66. S. Gagliarducci, M. G. Onorato, F. Sobbrio, G. Tabellini, *War of the Waves: Radio and Resistance During World War II*, febbraio 2018.
67. P. Bingley, L. Cappellari, *Workers, Firms and Life-Cycle Wage Dynamics*, marzo 2018.
68. A. Boitani, S. Perdichizzi, *Public Expenditure Multipliers in recessions. Evidence from the Eurozone*, marzo 2018.
69. M. Le Moglie, G. Turati, *Electoral Cycle Bias in the Media Coverage of Corruption News*, aprile 2018.
70. R. Davidson, A. Monticini, *Improvements in Bootstrap Inference*, aprile 2018.
71. R. Crinò, G. Immordino, S. Piccolo, *Fighting Mobile Crime*, giugno 2018.
72. R. Caminal, L. Cappellari, A. Di Paolo, *Linguistic skills and the intergenerational transmission of language*, agosto 2018.
73. E. Brenna, L. Gitto, *Adult education, the use of Information and Communication Technologies and the impact on quality of life: a case study*, settembre 2018.
74. M. Bordignon, Y. Deng, J. Huang, J. Yang, *Plunging into the Sea: Ideological Change, Institutional Environments and Private Entrepreneurship in China*, settembre 2018.
75. M. Bordignon, D. Xiang, L. Zhan, *Predicting the Effects of a Sugar Sweetened Beverage Tax in a Household Production Model*, settembre 2018.
76. C. Punzo, L. Rossi, *The Redistributive Effects of a Money-Financed Fiscal Stimulus*, gennaio 2019.
77. A. Baglioni, L. Colombo, P. Rossi, *Debt restructuring with multiple bank relationships*, gennaio 2019.

78. E. Cottini, P. Ghinetti, S. Moriconi, *Higher Education Supply, Neighbourhood effects and Economic Welfare*, febbraio 2019.
79. S. Della Lena, F. Panebianco, *Cultural Transmission with Incomplete Information: Parental Perceived Efficacy and Group Misrepresentation*, marzo 2019.
80. T. Colussi, Ingo E. Isphording, Nico Pestel, *Minority Salience and Political Extremism*, marzo 2019.
81. G. P. Barbetta, P. Canino, S. Cima, *Let's tweet again? The impact of social networks on literature achievement in high school students: Evidence from a randomized controlled trial*, maggio 2019.
82. Y. Brilli, C. Lucifora, A. Russo, M. Tonello, *Vaccination take-up and health: evidence from a flu vaccination program for the elderly*, giugno 2019.
83. C. Di Novi, M. Piacenza, S. Robone, G. Turati, *Does fiscal decentralization affect regional disparities in health? Quasi-experimental evidence from Italy*, luglio 2019.
84. L. Abrardi, L. Colombo, P. Tedeschi, *The Gains of Ignoring Risk: Insurance with Better Informed Principals*, luglio 2019.
85. A. Garnero, C. Lucifora, *Turning a Blind Eye? Compliance to Minimum Wages and Employment*, gennaio 2020.
86. M. Bordignon, M. Gamalerio, E. Slerca, G. Turati, *Stop invasion! The electoral tipping point in anti-immigrant voting*, marzo 2020.
87. D. Vigani, C. Lucifora, *Losing control? Unions' Representativeness, "Pirate" Collective Agreements and Wages*, marzo 2020.
88. S. L. Comi, E. Cottini, C. Lucifora, *The effect of retirement on social relationships: new evidence from SHARE*, maggio 2020.
89. A. Boitani, S. Perdichizzi, C. Punzo, *Nonlinearities and expenditure multipliers in the Eurozone*, giugno 2020.
90. R. A. Ramos, F. Bassi, D. Lang, *Bet against the trend and cash in profits*, ottobre 2020.
91. F. Bassi, *Chronic Excess Capacity and Unemployment Hysteresis in EU Countries. A Structural Approach*, ottobre 2020.
92. M. Bordignon, T. Colussi, *Dancing with the Populist. New Parties, Electoral Rules and Italian Municipal Elections*, ottobre 2020.
93. E. Cottini, C. Lucifora, G. Turati, D. Vigani, *Children Use of Emergency Care: Differences Between Natives and Migrants in Italy*, ottobre 2020.
94. B. Fanfani, *Tastes for Discrimination in Monopsonistic Labour Markets*, ottobre 2020.
95. B. Fanfani, *The Employment Effects of Collective Bargaining*, ottobre 2020.
96. O. Giuntella, J. Lonsky, F. Mazzonna, L. Stella, *Immigration Policy and Immigrants' Sleep. Evidence from DACA*, dicembre 2020.
97. E. Cottini, P. Ghinetti, E. Iossa, P. Sacco, *Stress and Incentives at Work*, gennaio 2021.
98. L. Pieroni, M. R. Roig, L. Salmasi, *Italy: immigration and the evolution of populism*, gennaio 2021.
99. L. Corno, E. La Ferrara, A. Voena, *Female Genital Cutting and the Slave Trade*, febbraio 2021.
100. O. Giuntella, L. Rotunno, L. Stella, *Trade Shocks, Fertility, and Marital Behavior* , marzo 2021.
101. P. Bingley, L. Cappellari, K. Tatsiramos, *Parental Assortative Mating and the Intergenerational Transmission of Human Capital*, aprile 2021.
102. F. Devicienti, B. Fanfani, *Firms' Margins of Adjustment to Wage Growth. The Case of Italian Collective Bargaining*; aprile 2021.
103. C. Lucifora, A. Russo, D. Vigani, *Does prescribing appropriateness reduce health expenditures? Main e_ects and unintended outcomes*, maggio 2021.

104. T. Colussi, *The Political Effects of Threats to the Nation: Evidence from the Cuban Missile Crisis*, giugno 2021.
105. M. Bordignon, N. Gatti, M. G. Onorato, *Getting closer or falling apart? Euro countries after the Euro crisis*, giugno 2021.
106. E. Battistin, M. Ovidi, *Rising Stars*, giugno 2021.
107. D. Checchi, A. Fenizia, C. Lucifora, PUBLIC SECTOR JOBS: *Working in the public sector in Europe and the US*, giugno 2021.
108. K. Aktas, G. Argentin, G. P. Barbetta, G. Barbieri, L. V. A. Colombo, *High School Choices by Immigrant Students in Italy: Evidence from Administrative Data*, luglio 2021.
109. B. Fanfani, C. Lucifora, D. Vigani, *Employer Association in Italy. Trends and Economic Outcomes*, luglio 2021.
110. F. Bassi, A. Boitani, *Monetary and macroprudential policy: The multiplier effects of cooperation,* settembre 2021.
111. S. Basiglio, A. Foresta, G. Turati, *Impatience and crime. Evidence from the NLSY97,* settembre 2021.
112. A. Baglioni, A. Monticini, D. Peel, *The Impact of the ECB Banking Supervision Announcements on the EU Stock Market*, novembre 2021.
113. E. Facchetti, L. Neri, M. Ovidi, *Should you Meet The Parents? The impact of information on non-test score attributes on school choice*, dicembre 2021.
114. M. Bratti, E. Cottini, P. Ghinetti, *Education, health and health-related behaviors: Evidence from higher education expansion*, febbraio 2022.
115. A. Boitani, C. Dragomirescu-Gaina, *News and narratives: A cointegration analysis of Russian economic policy uncertainty*, aprile 2022.
116. D. Delli Gatti, J. Grazzini, D. Massaro, F. Panebianco, *The Impact of Growth on the Transmission of Patience*, luglio 2022.
117. I. Torrini, C. Lucifora, A. Russo, *The Long-Term Effects of Hospitalization on Health Care Expenditures: An Empirical Analysis for the Young-Old Population*, luglio 2022.
118. T. Colussi, M. Romagnoli, E. Villar, *The Intended and Unintended Consequences of Taxing Waste*, settembre 2022.
119. D. Delli Gatti, G. Iannotta, *Behavioural credit cycles*, settembre 2022.
120. C. Punzo, G. Rivolta, *Money versus debt financed regime: Evidence from an estimated DSGE model*, novembre 2022.
121. M. Ovidi, *Parents Know Better: Sorting on Match Effects in Primary School*, novembre 2022.
122. L. Cappellari, D. Checchi, M. Ovidi, *The effects of schooling on cognitive skills: evidence from education expansions*, dicembre 2022.
123. M. Bertoni, G. Brunello, L. Cappellari, M. De Paola, *The long-run earnings effects of winning a mayoral election*, gennaio 2023.
124. M. Bordignon, F. Franzoni, M. Gamalerio, *Is Populism reversible? Evidence from Italian local elections during the pandemic*, gennaio 2023.
125. L. Colombo, G. Femminis, A. Pavan, *Subsidies to Technology Adoption when Firms' Information is Endogenous*, gennaio 2023.
126. L. Pieroni, M. Rosselló Roig, L. Salmasi, G. Turati, *Legal status and voluntary abortions by immigrants*, gennaio 2023.
127. F. Di Pace, G. Mangiante, R. Masolo, *Do firm expectations respond to Monetary Policy announcements?*, febbraio 2023.
128. R. Masolo, *Heterogeneity and the Equitable Rate of Interest*, febbraio 2023.
129. D. Delli Gatti, R.Terranova, E. M. Turco, *Mind the knowledge gap! The origins of declining business dynamism in a macro agent-based model*, ottobre 2023.

130. R. Davidson, A. Monticini, *Bootstrap Performance with Heteroskedasticity*, novembre 2023.
131. V. Colombo, A. Paccagnini, *Uncertainty and the Federal Reserve's Balance Sheet Monetary Policy*, febbraio 2024.
132. M. Bordignon, T. Colussi, F. Porcelli, *Populists at work. Italian municipal finance under M5s governments*, febbraio 2024.
133. A. Boitani, L. Di Domenico, S. Ricchiuti, *Monetary policy and inequality: an heterogenous agents' approach*, settembre 2024.
134. M. Di Giacomo, M. Piacenza, L. Salmasi, G. Turati, *Understanding productivity in maternity wards*, ottobre 2024.
135. D. Bonciani, R. Masolo, S. Sarpietro, *How Food Prices Shape Inflation Expectations and the Monetary Policy Response*, ottobre 2024.
136. G. Battiston, L. Corno, E. La Ferrara, *Informing Risky Migration: Evidence from a field experiment in Guinea*, dicembre 2024.
137. F. Di Pace, G. Mangiante, R. Masolo, *Monetary Policy Rules: the Market's View*, dicembre 2024.
138. L. Corno, E. La Ferrara, A. Voena, *Female Genital Cutting and the Slave Trade*, maggio 2025.
139. M. Carlana, L. Corno, *Peer influence in Educational Choices: Social Image Concerns and Same-Gender Interactions*, maggio 2025.
140. R. Disney, J. Gathergood, S. Machin, M. Sandi, *Human Capital from Childhood Exposure to Homeownership:Evidence from Right-to-Buy*, giugno 2025.
141. J. Boshoff, S. Machin, M. Sandi, *Youth Crime and Delinquency In And Out Of School*, giugno 2025.
142. A. Boitani, C. Dragomirescu-Gaina, A. Monticini, *Managing the chaos: policy challenges in a hyperinflationary environment*, giugno 2025.
143. E. Cottini, L. Popoescu, L. Salmasi, G. Turati, *Poisoned Air, Shortened Lives: PM2.5 Exposure and Premature Mortality in Southern European Cities*, luglio 2025.
144. M. Bordignon, N. Gatti, G. Turati, *Are citizens willing to reduce public debt? Beliefs, information and policy preferences*, luglio 2025.
145. J. Costa-Fon, N. Gatti, G. Turati, D. Wiesen, *A prosocial legacy of COVID-19 among healthcare professionals?,* ottobre 2025.
146. A. Ariu, G. Rivolta, *The Micro and Macro of a Large and Sudden Devaluation*, ottobre 2025.
147. F. Di Pace, G. Mangiante, R. Masolo, *Brexit and the cost of living: a tale of two phases,* dicembre 2025.
148. A. Albanese, L. Cappellari, M. Ovidi, *Earning ability over the life cycle*, dicembre 2025.
149. L. Cappellari, M. Morelli, *Backward Spillovers and Equalized Access: The Effects of the Bologna Process across Italy's Education System*, febbraio 2026.