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Essays on education reforms

Anna De Paoli

Prof. Daniele CHECCHI
Prof. Massimiliano BRATTI

Prof. Michele SANTONI

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Introduction

*Education enhances one's ability to receive, decode,
and understand information,
and that information processing and interpretation
is important for performing or learning to perform many jobs.
(Richard R. Nelson and Edmund S. Phelps, 1966).*

There is a wide consensus on the role played by education in stimulating growth processes, according to the perspective that social returns to education extend beyond private returns. This work analyses empirically the effects of different reforms in education on attainment, fertility and labor market. We show that policies that help access to schooling and increase educational attainment have an impact on fertility choices and on employment.

The first part is dedicated to explore how educational attainment varies after reforms in compulsory education and can be seen as a starting point for further analysis that make use of these kind of laws to instrument for education. In order to test reforms' efficacy we collect data and background information on thirty countries, so to include both developed and developing countries, doing our best to extend the analysis as much as possible. So we select those countries where the reforms have been implemented during the last fifty years of the last century and for which we have reliable individual level data. We first perform a country study that helps us to find the most suitable way for identifying people's exposure to the reforms, solving problems posed by migration, mobility and by low quality of data. Then, using a standard linear model, we analyze reform's average effect on the full set of countries. Overall the reforms had a positive effect on educational attainment, even if there is great variability among the countries considered.

The question on whether the distribution of education is altered after the reforms is also addressed in a quantile regression framework. Our results show that the reforms had a bigger impact on the first quartile, consistently with the change in minimum leaving age caused by the reforms. After this first introductory part about the characteristics of the effects of reforms in compulsory education, we focus on a single country and we investigate whether education, instrumented by a reform in compulsory education, affects teenage fertility and women labor market participation (see Chapter 2).

This study makes use of a representative sample of Ecuadorian women taken from two waves of censuses. The reform, implemented in 1977, increased compulsory education by three years and made the completion of lower secondary education mandatory.

The case of Ecuador is relevant because teenage fertility rate is high and women average educational level low, so that women are frequently unskilled and often unemployed. Early fertility hampers women to complete even basic schooling and is associated to subsequent poor mothers' and children's outcomes. So according to this perspective, a reform in compulsory education is promoting investment in human capital and can potentially help breaking poverty traps.

Results show that education, which is positively affected by the reform, is endogenous with respect to teenage childbearing. So using the reform as instrument is a way to solve endogeneity problems and get unbiased estimates of the impact of fertility on education. There are many reasons why education can be endogenous to teenage fertility: women with strong labor market preferences can decide to invest more in education and less in fertility, or vice versa, or they can drop out of school because of childbearing or the other way round.

Results show that completing lower secondary school lowers the likelihood of early motherhood by 7%. We then extend the analysis to include women's labor supply in a model that takes into account simultaneously also education and early motherhood. As expected, education, while decreases teenage childbearing, increases women's labor force participation; in turn, teenage motherhood has negative effects of labor supply. The last section concludes stressing the potential intergenerational effects of changes in the age at first birth, showing that firstborn children born to older mothers have better educational outcomes than those born to younger ones.

From a policy perspective a reform in compulsory education can be considered not only as a way for increasing average educational level, but also as a tool to promote better women's outcomes, that in this case are represented by lowered early motherhood rate and by increased labor market participation.

The last chapter takes into account a reform that reorganized the Italian university system in 2001. So we explore labor market outcomes of such a reform using a survey that collects employers' preferences over prospective workers. This is a relevant issue because this process as part of the Bologna Process, aimed at creating a common and harmonized educational system in Europe, and its labor market effects haven't been explored yet.

Before 2001 students attended a unitary system that lasted four to five years, while with the reform a two-tier system is introduced. A two-years MA equivalent degree now follows a three-years BA degree, and students can decide whether to take only the BA degree or to continue for five years and specialize. While scholars showed that the reform increased access to tertiary education and decreased drop out ratio and age at degree, it's not yet clear what happens when students start looking for a job.

So first we explore whether graduates are more likely to be hired after the reform or not, focusing on the type of jobs for which they are required. Results show that in fact the expected demand for high skilled human capital has slightly increased in the post reform era, especially for technical and clerical professions. Even if not precisely estimated, the increase in the demand for graduates grew at a lower rate than its supply. We also investigate how employers chose between BA or MA graduates, but this preference is not clearly defined yet.

Data also show that there are differences on how preferences are shaped over the disciplines in which people are specialized: in the post reform era there is an increase in the share of those with a scientific and humanities degrees, and a decline of those that come from science-political studies.

We present and discuss the potential confounding factors that could alter the increase in the demand for graduates that we observe: the reform in the employment protection regulation, the 2008 crisis and a general growing trend. Evidence suggests that those factor should not lead to overestimate the demand for skilled personnel.

1 Chapter 1: Fifty years of compulsory school reforms: evidence from a cross-section of countries

1.1 Abstract

In the second half of the twentieth century reforms in compulsory education laws aimed at increasing educational attainment in many countries. Using individual-level data from a cross-section of country, the question on whether these reforms had the expected effect is addressed. In a regression discontinuity design framework, reforms effects are controlled by mean of a standard linear model, focusing on the average effect, and of a quantile regression model. An introductory part based on a single country study will give advices on the best strategy to identify individuals' exposure to the reform, taking into account migration and internal mobility, and problems related to low quality of data and discrepancies among them. Then the overall effect of the reforms is considered on a cross section of countries and the results show that the average effect is positive, even if there are substantial differences among the countries considered. Reforms play a role in reducing inequality of educational attainment, having a positive impact on people who belong to the lower quantiles of the distribution of education.

1.2 Introduction

Endogeneity is a problem common to many empirical studies that make use of human capital as an explanatory variable. Thus education, which usually proxies human capital, has been instrumented in various way in order to get consistent estimates of the effect of human capital on different outcomes: wages (Angrist and Krueger, 1991 Duflo 2001, Meghir and Palme, 2005, among others), fertility (Monstad et al., 2008, Black et al. 2008) and health (Behrman and Wolfe, 1989, Glewwe, 1999, among others).

This chapter is aimed at finding out if reforms in compulsory education laws have an impact on educational attainment, so to provide further evidence in support of the use of the reforms as an instrument for education. Not only we show how reforms are effective, but also we suggest strategies that are useful for tackling the issue of identifying correctly people that are exposed to reform or, more in general, to programs. In our sample individuals are targeted by compulsory education reforms according to their age and so we analyse reforms' impact by mean of the research discontinuity design method. This strategy can be pursued to study causal effects of programs (here reforms in compulsory education), to which individuals are assigned if an observable characteristic (here age) is below/above an exogenously given threshold. This method is valid if people can not modify their characteristic so to be voluntarily be assigned to the program. The idea behind the impact assessment using RD is that the sample in the neighborhood of the cutoff point represents features of randomized design, because individuals vary only in their treatment status.

In the empirical literature the reforms in compulsory education are a very frequent instrument for schooling and so here we provide a set of 'guidelines' for correctly measuring the variation in educational attainment caused by reforms. A precise estimation of a reform impact is crucial for having an instrument strongly correlated with the potential endogenous regressor.

An introductory part will be dedicated to explore how to identify people's exposure to reforms and how to measure schooling level at best. This is done considering a single country only, Canada.

In particular the focus will be: the way in which educational attainment is defined (schooling years, detailed level, general level); the identification of individuals by place of residence or by

place of birth (also foreign born people will be treated); how broad should be set the cohort of treated and not treated group (1, 2 or more years before and after the reform) and finally it will be considered a last case where the exact date of reform is not known, but only the decade in which it was implemented. In general, this first part will be devoted to deal with problems of poor information about the institutional changes and lack of details about identification of the group of people involved in those changes; we show how reforms' impact measurement varies as individuals' exposure to reform is determined differently, so to define the best model to fit the available data.

Then we extend the analysis to a larger dataset and we explore reforms' effects on average educational attainment and on quantiles of the education distribution different from the mean.

We will take into account not only average treatment effect, but also variations in the distribution of educational attainment, to see whether the dispersion of education increases or decreases after a reform. A policy change on the minimum leaving age can affect average attainment, but can also reduce inequality of education, bringing to school those who belong to lower quantiles (as Eide and Showalter, 1998, and Brunello, Fort and Weber, 2009, show). To investigate this point a simultaneous quantile regression model will be used.

The fact that reforms affect more people at lower quantiles can be seen as an argument in favour of the human capital against sorting hypothesis (Lang and Kropp, 1986). If education is just a signal on the labour market, after a reform educational attainment should increase uniformly across all individuals, while if human capital hypothesis holds reform should affect only targeted people. For the human capital hypothesis to hold, as we are considering reforms in compulsory education, it must be that people mostly affected are those that belong to the lower quantiles of education's distribution.¹

This work makes use of individual level data, from both censuses and surveys, and about 30 countries are taken into account; in the next section we will start exploring the data sources that will be used in the whole chapter.

1.3 Data

1.3.1 The reforms on compulsory schooling

Information about reforms are collected using the *Eurydice Information Network on Education*, which contains an exhaustive source for all European countries (including new entrants), and from *Unesco project Education for All* (EFA), which instead provides reports for the rest of the World. When the information about the reform on compulsory schooling was not clearly specified or even missing EFA has been considered jointly with specific country sources, such as single country studies. If necessary the relevant laws have been looked up. All the sources used and the countries considered are listed in Table A.1 in the Appendix.

For the purpose of this work only reforms implemented after the Second World War have been taken into account so that people in the treated, in the not treated and in the control groups are not too old when surveyed. To consider reforms further back in time would be problematic, since the surveys and censuses, the sources of individual data used here, were made at the end of the last century. Including old aged people can potentially lead to bias estimates if death is not a random event (for instance, if it is correlated with income and education). As a consequence some countries, such as US, Japan and South Korea, for which data were available but reforms were implemented before 1950, were excluded. For the opposite reason even too recent reforms have been left out, since pupils in the treated group would be too young and, thus, the effect of the reform would be underestimated (we might truncate the distribution of education because people are still student). To overcome the restrictions posed by some recent reforms so to include as many countries as possible,

¹ We would expect opposite results if reforms on higher education we would have been taken into account.

years of schooling, which is our outcome variable, will be constrained to be lower or equal to 17 in the case of Canada and to 15 when the full set of countries is considered.

1.3.2 Individual data

Individual-level data come from two sources as they are collected by two different institutions. IPUMSI (*Integrated Public Use Microdata Series, International*) data are censuses from different countries harmonized by the Minnesota Population Centre. IPUMSI covers all the non-European countries in this study, plus Greece and Romania.

ISSP (*International Social Survey Programme*) data are collected from different surveys taken in various years (from 1985 to 2006) and they cover sociological topics; even if topics change every year, it's still possible to retrieve some basic information uniformly across years, such as age, gender, years of schooling and maximum educational attainment. Since the number of people surveyed each year in each country is around 1500, many years have been pooled to build each country's dataset. Moreover, since every single survey reflects a specific topic, it can be biased in some direction and so using different years we can prevent the data from being biased.

The two sources differ in the way people can be identified. In general we can say that IPUMSI, as it's a collection of harmonized censuses, is more detailed and potentially reliable than ISSP. Having a quick look at the countries included in the two groups, one can note that quality of data are inversely related to income, which is quite unusual. So almost all Latin American countries (except Uruguay), African countries, plus Greece, will be treated with a greater level of accuracy than the European countries (see again Table A.1).

Going into further details, given the specific aims of this work, the main difference is that while in the IPUMSI data is possible to identify people according to the nation of birth, in the ISSP data this is not possible, since the only information we have is country of residence. To know the place of birth is important when trying to determine the place where the person received education and, even if we can not be fully confident about where he or she was educated, it's better than just knowing residence when adult. So in the ISSP data we can not exclude foreign born people and in the case of Germany we will only use province of residence, while we should identify people by province of birth (*länder*).²

Another point is that while in the ISSP data is never possible to control for race, in IPUMSI for Brazil, Canada, Costa Rica, Colombia and Ecuador, we can check for differences in the attainment among minorities. In Brazil the non-white group includes indigenous, black and Asian people, in Canada blacks and Chinese, in Colombia blacks and indigenous, in Costa Rica blacks, indigenous and Asian and in Ecuador blacks, mestizos, mulattos and indigenous.

The last important difference is the way in which educational attainment is expressed, when levels are considered; since ISSP is not harmonized, there are discrepancies in the codes and in the definitions used. So, when information about the level attained is needed, it will be only taken into account a broad definition which is uniform across sources (such as primary schooling) and all the other existing definitions won't be considered (such as some primary, lower secondary, incomplete secondary, some college, and so on...).

1.4 Methodological issue

1.4.1 The case of Canada: the institutional framework

Since our purpose is to study the effect of compulsory education reforms on educational attainment, we dedicate this introductory part to investigate potential confounding factors in the data that could blur a reform's effects. The case of Canada will be helpful in providing some indications on how to better identify people's exposure to reforms. Canada has been considered in

² In Germany and Canada reforms have been implemented in different years in each province, but since for Canada the IPUMSI data were used it was possible to distinguish the province of birth and not only province of residence.

this preliminary study, because of two specific features: compulsory schooling legislation differs across provinces and by time of implementation and people are quite likely to move across provinces during their lifetime or to migrate to Canada from abroad. Moreover, different definitions of schooling attainment are available. So the case of Canada on one side poses some problems in terms of identification, but on the other allows us to tackle these problems because data source for Canada – IPUMSI – is detailed and reliable.

In Canada the implementation of reforms in compulsory education shows time and territorial variation (see Table 1) and minimum school leaving age (MLA) after the reform is 16. In all provinces, but two, compulsory schooling was increased by 1 year, so from the age of 15 to 16. In Manitoba and in New Brunswick instead MLA before the reform was 14 years.³

Province of birth, Canada	year	Reform after	MLA before	MLA	Treated age in 2001 (<i>age</i>)
Newfoundland	1987	16	15	35	
New Brunswick	1966	16	14	48	
Quebec	1988	16	15	27	
Ontario	1954	16	16	52	
Manitoba	1962	16	14	52	
Saskatchewan	1965	16	15	50	
Alberta	1970	16	15	45	
British Columbia	1990	16	15	25	
North-western Territories, Prince Edward Island	1980	16	15	35	

Table 1 Reform in compulsory education in the Canadian Provinces. Source: Oreopoulos (2005).

Individual data are taken from the 2001 census and, according to the reforms' setting, the first treated cohort is composed by those that are 14 years old (or 13 for Manitoba and New Brunswick) when the reform was implemented.⁴ Those that are already 15 when the reform was enacted instead compose the not-treated group. In the next section we will give the details of the model used for the analysis.

1.4.2 RD design method

Both in this introductory part and in the rest of this chapter, in order to estimate the effect of the reform we will follow a regression discontinuity (RD) designs framework that was first introduced by Thistlethwaite and Campbell (1960). This approach is very popular whenever scholars want to estimate programs' effect on various economic outcomes, such as in Angrist and Lavy (1999) and in Black (1999), among others. This method is particularly useful in all the non-experimental settings where individuals are assigned to the treated or non-treated group, depending on the value taken by a specific variable over which they do not have control (see Lee 2008 and Lee and Lemieux 2010).

The RD designs requires that a threshold in an observed characteristic defines the treated and the non-treated groups and assumes that individuals close to the cut-off point (just above or just below) are identical in all the characteristics but the assignment (observable) variable.

The outcome variable of interest is represented by Y_i , years of schooling of individual i . Assignment to the treatment is determined by individuals' age so that the dummy variable R_i is

³ An exhaustive report on the history of the Canadian schooling legislation throughout the last century can be found in the work done by Oreopoulos (2005).

⁴ We decide not to include those that are 15 years old in the year of the reform, since the law was enforced from the subsequent year onward.

equal to 1 if individual is younger than a cut-off age and 0 otherwise, $R_i \equiv I(\text{age}_i \leq \overline{\text{age}_j})$. Let $Y_i(1)$ and $Y_i(0)$ denote the potential outcomes for each individual being exposed and not exposed to the reform respectively, so that the outcome that we want to estimate is $E[Y_i(1)] - E[Y_i(0)]$.

According to the RD design method we look at the discontinuity in the conditional expectation of our outcome given age, so to find out the average causal effect of the reform. So in our case, the impact of the intervention is given by the difference in mean outcome (schooling) of the treated and the non treated groups in the neighborhood of the cutoff point. We assume that all other individual's characteristics do evolve smoothly around the cut-off age, so that the average outcome of those just above the threshold age could be used as a counterfactual for the one of those just below (that are treated by the reform). We choose to use individuals that are two years older and younger than the cut-off age determined by the year of reform's implementation, so to be as close as possible to the threshold age and at the same time have enough observation to perform the analysis.

In order to have more precise estimates of the reform's effect we also control for sex, race, residence, province of birth. So we fit the following linear regression model and we estimate the average reform's effect:

$$Y_i = \beta_0 + \beta_1 R_i + X_i' \Gamma + \varepsilon_i \quad (\text{equation 1})$$

where X represents the set of control covariates. This setting is identical to the one that we will be using when dealing with the full set of countries.

Since years of schooling are not available in the IPUMSI dataset for Canada, the levels in their detailed version have been converted into years, according to Table 1 in the Appendix. To do this not only the IPUMSI data set has been taken into account, but also the ISSP survey, where both the levels and the years of schooling are available. In the ISSP dataset, the definition of levels is quite different from the IPUMSI definition, but we managed to get anyway some supplementary information about the average number of years necessary to complete primary, lower secondary, secondary and tertiary education. In this way we could transform IPUMSI levels of education into years. Different codings have been tried and results obtained in the regressions do not vary much; so the coding in Table 1 in the Appendix will be used in this part and in the rest of the study.

Note that, since in some provinces the reform affects people that are 23 years old, we decide to include those with a post-graduate degree in the same category of those with a university (BA) degree, thus censoring years of schooling at 17.

Now the following sections will take into account the confounding factors one by one, starting from people's mobility, and the reference model is the one that we just introduced.

1.4.3 Mobility - Province of birth vs. province of residence

Since 38% of the sample doesn't live in the province of birth and reforms were implemented at different times in different provinces, it is relevant to investigate whether the reforms' effects vary according to the definition of people's exposure to reforms, if by place of residence or by place of birth. While in Canada people's mobility is a relevant phenomenon, we assume that in other European countries, for which the information about place of birth is not available, but would be important for identifying the treated group, internal migration flows are relevant to a lesser extent at the time of reforms.

So two models are compared: one in which the exposure to the law is defined by place of birth and another where it is defined by the place of residence and the simple ordinary least square model of eq. 1 has been performed.

Results (see columns (1) and (2) in Table 2 in the Appendix) show that the coefficient for the treatment differs in the two cases, so, as expected, mobility is a relevant issue when dealing with problems related to people exposure to laws.⁵

The resulting reform's coefficient is slightly higher in the case in which individuals are identified according place of birth, surely because, in order to determine the exact place where people were living when the reforms were implemented, place of birth is a better proxy than place of residence. So, from now we will identify individuals on the basis of their province of birth.

Unfortunately only the IPUMSI samples will allow this distinction, while ISSP is based on province of residence. For example Germany will be treated according to people residence, even if laws were implemented at different times in each province (*länder*). Brunello et al (2009) use the reforms in compulsory education as the instrument for schooling in a wage equation and identify people's exposure to reform using the ISSP data set, where the exact place of birth is not available; so in that case the estimation of the reforms' effect in the case of Germany could be imprecise.

1.4.4 Foreign born people

Since almost 20% of the sample is foreign born, it could be interesting to include in the analysis foreign born people that migrate to Canada before the reform and that belong to the age cohort treated or to the control group. So a new dummy variable for treatment has been created in order to include also foreign born people that migrate to Canada early in their lifetime. Since the identification based on the place of birth gives more precise estimation, the new dummy variable include foreign born people according to their residence, but native Canadians are still identified by their place of birth.⁶ Results are shown in column (3) of Table 2.

Including foreign born people bring almost the same results we had excluding them. The reform's coefficient is smaller (0.17 against 0.2) and two explanations are possible: strangers are not affected by the laws as citizens, if for some reason they don't respect the law or laws are enforced to a lesser extent. Also it could be possible that the educational attainment of migrants is on average higher then the level that became mandatory with the Canadians reforms considered, so that the treatment is not a treatment for them. Looking at the descriptive statistics on the level of education of foreign born people we notice that first of all they are on average more educated then native Canadians, and then that they are not affected by the laws, since the average years of schooling shows no variation between the non treated and the treated group (see Table 3 in the Appendix). On average migrants in the treated and non treated cohort have an educational level which is higher than the one set by Canadians reforms.

In fact running a regression separately for foreign born people, we find out that the reform is completely ineffective (results shown in column (4) of Table 2). The reader should consider that there is evidence that Canada is one of the six largest skilled migrants receiver country (Defoort, 2006), so it's not surprising that foreign born people are not affected by Canadians reforms.

This is to say that including people that were not born in the country of study must be done with caution, even if they belong to the treated group and if they migrate before the laws were put in act. We cannot assume that the distribution of the educational levels is the same between the

⁵ The reader should note that in the regression showed in column 2 we excluded all individuals that live in the Atlantic Provinces and Territories (New Brunswick, Nova Scotia, Prince Edward Island, Newfoundland and Labrador, Yukon, Northwestern Territories and Nunavut), since the universe of definition of province of birth does not include the people born in these provinces. So province of birth is not reported for individuals that were born in one of these regions, but we only know province of birth of people not originally from these territories. So for the case for which the province of birth is considered, we will concentrate only on Quebec, Ontario, Manitoba, Saskatchewan, Alberta and British Columbia.

⁶ So we are assuming that the foreign born people don't move anymore after migration and this is reasonable since only 2% of the foreign born people changed province of residence – which is our unit of interest - in the last 5 years. This does not mean that they didn't move before, but nevertheless we can use this information to somehow reinforce the assumption.

Canadians and foreign born people. So whenever it will be possible migrants will be excluded from the sample.

1.4.5 *Schooling years vs. general level attained (less than primary or no school at all, primary completed, secondary completed, tertiary completed).*

In the Canadian dataset two different definitions of educational attainment are possible: one detailed and one general (only the maximum level reached). This latter way potentially downward biases the effect of the reform since only the complete level is reported. Moreover if the reform is not targeted to the completion of a specific level, it might be the case that reform's effect is not evident at all. However, since our purpose is to collect advices so to perform the analysis on a broader dataset, we prefer to rely on the general definition of education, as it's more likely to be comparable across different contexts.

Here we test whether the reforms had an impact on the probability of completing primary, secondary and tertiary education simultaneously.

In the Table 4 in the Appendix there are the descriptive statistics for educational attainment for the cohort affected by reform and the cohort not affected. In both cases the exposed cohort has a higher mean with smaller variance than the not exposed, anyway when levels are considered the difference is very small. In this case levels considered are primary, secondary and tertiary education attained. Even if in some cases it's available also some in-between levels (such as some primary, lower secondary, and so on), this broad definition has been preferred here as it's fully comparable across samples; using a more detailed level definition would be meaningless for the final purpose of this preliminary part where we are defining the standards to identify people's exposure to reforms.

In order to estimate if the reform altered the probability to complete one of the three levels considered we perform an ordered probit model, using the same regressors as before. Reform's effect is not statistically different from zero (results not reported). So using only the maximum level attained as the outcome to test for reform's effects don't lead to any relevant results and might be problematic when more detailed information about people's schooling level is not available. In this case, if years of schooling were not available, we might have inferred that the reforms didn't have an impact at all. So if we want to investigate the variations on educational attainment after a reform in compulsory education, the outcome should be education expressed in years and not in levels. Nevertheless, the use of education expressed in general levels can be fruitful whenever the reform had a strong impact, or if a specific level is the target itself.

1.4.6 *Length of the exposure to the treatment considered: 1 year after and before, or more?*

To extend the breadth of age cohorts in which the effect of reforms is observed can be problematic if the reform is recent because we include people that potentially have not completed their studies yet (thus underestimating the effect of the reform) and if other major reforms are enforced in the meanwhile (the larger is the span considered the more likely there could be other reforms or other events going over). Moreover, a general increasing trend can lead to overestimate the reform's effect. The main advantage instead is to increase the number of observations and to capture the impact of a law even if there is some delay in its enforcement. Whenever IPUMSI data will be used, the number of observations will never be problematic since those data are collected from censuses; however, in the next section for many countries another database will be used and it might not be enough one or two years. So we first test how our baseline model reacts to increases in the age cohorts considered and then we take into account the largest possible cohort-model introducing a smoothing polynomial in age, which is a standard practice for a RD design strategy where the cohorts are wide enough to allow for a trend.

So first the regression in eq. 1 has been performed with different age cohorts before and after the reform: ± 1 , ± 2 , ± 3 , ± 4 , ± 5 , ± 6 and ± 7 years wide. In Table 5 we report the values of the reform's coefficient in all these cases. The reform's effect is almost stable to increases in the

cohorts' wideness from ± 6 onwards and there is evidence of a positive general trend, as the coefficient is increasing as cohorts are widening.

In order to take into account the growing general trend problem we use the ± 7 years cohort and we include a smoothing polynomial in age of different orders (see results in Table 6 in the Appendix). With a polynomial of order 2 or 3 the variance of the model (sum of the residuals corrected for degree of freedom) is stable and the reform's effect (+0.17) is consistent to what we found in the first paragraph (+0.2), even if slightly smaller. In this last case the whole time span considered is 15 years, and since some reforms are very recent, it's not reasonable to increase it any further, because people in the sample are too young and could be still at school.⁷ This is probably also the reason why the reform's effect is smaller than in the case where we compared cohorts 2-years wide.

So according to these results, using a sample which is very close to the cut-off-age that assigns people to the treatment or using a larger age cohort, with a smoothing polynomial, are both valid strategies. Probably in the case of Canada, for which we have a large data set, but a bias due to the inclusion of young people, it's more appropriate to rely on a comparison of cohorts close to the threshold age, while in other cases, where data sources are scarcer, it might be better to use the full sample with the polynomial.

1.4.7 *Generic exposure to the treatment: what happens if we ignore the year of the reform but we know the decade in which the reform was implemented.*

This exercise is meant to deal with the uncertainty about the precise date in which the reform was passed (as in Angrist and Krueger, 1991, for United States). So let's suppose the only information we have is the minimum leaving age in every decade, without knowing exactly the year in which the reform was implemented. Table 7 in the Appendix summarizes the real year of the reform and the relative decade that will be used in this exercise.

The decade variable (Table 7) will tell the next closest decade to the year of reform. For Quebec we are assuming that during all the eighties the minimum leaving age was 15, while from the nineties 16. Ontario will be an exception, because the cut-off decade should be 1950, but then the two groups that will be compared are people that are 15 during the forties and during the fifties. This might be problematic because the control group is composed by people that are too old when the census was made (2001) and so the next decade was considered. In this exercise the control group is composed by people that were in the treated age during the decade before the reform, while the treated group by people in the treated age in the decade after the treatment, so that the total time span is 20 years. So we perform the regression that includes also the polynomial in age so to correct for general trend (since we are using a large time span) and results show that the coefficient for the treatment is equal to +0.11 (see Table 8). So in this case the information about the precise date of the reform is crucial to observe correctly its impact. Using decades instead of the cut-off year leads to imprecise estimations, even if the general effect of the reform is preserved. Angrist and Krueger (1991) use reforms in compulsory schooling to instrument education in a wage equation, but their IV strategy lead to results that are not different from those obtained with OLS. Their information about the date in which laws were implemented is incomplete and they use, as in this exercise, the decades. In their case relying on more precise instruments might have lead to different result, as in the case of Canada that we are presenting.

So, given the suggestions offered in this introduction, we can now precede the analysis with in a more robust way the effect of reforms.

⁷ Having young people in the treated group lead to underestimate the reform's effect since they could be still at school and thus the maximum level that they could have reached can never be the highest possible (so the distribution of education for the treated group would be left truncated).

1.5 The model: a cross-countries analysis

The introduction about Canada gives some advices to correctly identify people when all the countries are considered jointly; people should be classified according to birth place, rather than looking at residence; strangers should be excluded from the dataset; the breadth of treatment cohort should be of 2 years before and after the treatment if the dataset is large and the reforms implemented recently; if observation for each country are scarce also wider age cohorts can be used, provided general trends are controlled with smoothing polynomials; educational attainment should be proxied by years of schooling, rather than by general levels; if the exact date of the reform is not known, approximating it with decades in which it took place (if known) lead to imprecise estimates. Table A.2 in the Appendix summarizes statistics about each countries' years of schooling.

In order to test for changes in educational attainment in the post reform period, we divide people into treated and not treated group according to their age when the reform was implemented, following the same RD designs method that we used for Canada. In order to include as many countries as possible we censor years of schooling at 15, given that in many the reform has been implemented quite recently.⁸ All the countries included in the analysis and the reforms are listed in Table A.1 in the Appendix. As in the Canadian example, we decide to use the 2 years wide cohorts, so that we use individuals that are close to each countries cut-off date, and also the 7 years wide cohorts with the smoothing polynomial in age.

1.5.1 Average effect of the reforms a linear model.

In this section we present the result of the estimations of reforms' average effect considering the full set of countries. We first find out what is the general reforms effect and then we detail the results exploring differences across countries.

We replicate the regression modelled by equation (1) including in the matrix X for controls countries fixed effects (results are in column (1) of Table A.3 in the Appendix). Unfortunately we can not control whether school quality changed over time. An indicator that could potentially have been used is the pupil-teacher ratio (Barro and Lee, 2001, and UN dataset), but it's available in an exhaustive way only some countries and years.⁹

Then the same model of equation 1 is taken into account, but the cohorts are 7 years wide with a smoothing polynomial in age (see column (3) of the same table).¹⁰

We also consider a richer specification where we interact the dummy for the treatment with each country fixed effect, so to explore cross countries variations of the impact of reform:

$$Y_i = \beta_0 + \beta_1 R_i + (R_i * C)' \beta + X_i' \Gamma + \varepsilon_i \quad (\text{equation 2})$$

where C is the matrix for countries fixed effects which is interacted with the reforms' dummy. Errors are clustered at country level.

Results are reported in column (3) of Table A.3 in the Appendix.

⁸ This corresponds to the completion of at least some tertiary education, since in the majority of countries considered secondary schooling is completed after 12 years. Using this 'recoding' for educational level should not be troublesome since it corresponds to a level well above the target of the reforms taken into account.

⁹ By the way, it is still an open question if having more teaches relative to pupils is a signal of improved quality, especially if one looks to relationship between investment in education (pupil teacher ratio) and achievements, measured with standardized tests (Hanusheck, 1998). The assumption that policy makers have to allocate more money education to improve students' performance is still quite controversial.

¹⁰ In this specification we also try to include controls for macro shocks that occurred around the same time of the reforms and that involved at least some people in the treated or the not treated group. The shocks considered are Second World War, the genocides in Rwanda, the Soviet regime and the Latin American regimes. In all these case the dummy is equal to 1 during the war, the regimes and the genocides, while it's equal to 0 otherwise. So there are four different dummy variables, assuming that all the Latin American regimes had the same effect on those countries; the same reasoning holds for all the countries under the Soviet influence. All the information on macro shocks are retrieved from the Economist Intelligence Unit, various years. Since the results show that those events didn't impact average schooling level in our specification, we decide to exclude them from the final regressions.

The results from the regression that refers to equation 1 show that the coefficient for treatment is equal to 0.14 ($p < 0.05$). This means that in the five years after the reform average schooling increased by 0.14 years. This result is not explicative about the different reform's impact between countries, but give a general idea of the effectiveness of reforms.

Results also show that being a female and white is associated with higher educational level, which is coherent with the fact that in many countries considered general ethnical minorities are disadvantaged and women have higher educational level. This last result does not generally hold for developing countries, but those countries are less represented in our sample.

This model is telling that all the countries experienced a growth in educational attainment just after the reform, provided that no major other events happened in the meanwhile. Even if we consider wider age cohort (7 years before and after) and we include a polynomial in age, the reforms' impact is still of a comparable magnitude (+0.14 vs. +0.187). This difference in the two models can probably be explained by the fact that some reforms might be enforced with some delays, even if we don't know precisely why this happened.

When the parameters of the reforms are free to vary across countries, we can observe that the impact of reforms differs across countries. So now we explore the reform's impact in every country using the interaction terms and the procedure clearly described by Suits (1984). This method allows to determine what is each country reform's effect with respect to an average level.¹¹ We rank countries according to the reform's strength in Table A.4 in the Appendix.

For 22 countries out of 35 the parameter for treatment is positive and significant, while for 13 it is negative. First of all the reform's coefficient is not explicative of the reform's impact in absolute terms but only with respect to an average of the countries considered here. The reform's coefficients computed following the Suits method shows what is the impact of reform in each country with respect to the average effect of all the reforms considered. For Argentina, Panama, and the Philippines the problem might be that those that are in the treated group are very young (between 18 and 19 years old), so that their schooling level is severely censored even if we decided that in this exercise the maximum years of schooling is 15. For the Czech Republic instead the negative results could be due to the fact that in 1984 the MLA was set to 16 years of compulsory schooling, but in 1990 it was decreased to 15. This second subsequent reform might have altered our results. In fact if we exclude these four countries and we compare years of schooling before and after the reform for the other 'problematic' 9, we get that educational level actually increased in the post reform period.¹² So for 9 countries the reforms simply were weakly enforced with respect to the others in this study, even if problems about number of observations and about historical events might have hampered reforms' efficacy as well.¹³

Overall, this linear model is telling that the majority of the countries experienced a growth in educational attainment. When the parameters of the reforms are free to vary across countries, we can observe that the impact of reform shows great variability: the reforms' effect ranges from +0.016 of Slovenia to +0.62 of Bulgaria. This shed some light on the average effect of laws, showing relevant differences across countries. In the next section it will be analysed if along with variability across countries there is also variability within countries, comparing people at different point of the distribution of education.

¹¹ This average level includes all the countries analyzed.

¹² In the pre reform period average years of schooling for Romania, Slovak Republic, Venezuela, Denmark, Hungary, Mexico, Ecuador, Cyprus and Costa Rica was 9,2, while in the post reform it is 9,3.

¹³ For Slovak Republic and Cyprus, given that some observation are retrieved from old ISSP surveys, we can have the aforementioned censoring problem; for Romania the reform was implemented just before the Ceausescu regime and this concurrent event could have depress people's educational investment.

1.5.2 The effects on the distribution of educational attainment with quantile regressions

This part will be dedicated to exploring the effect of reforms on different points of the distribution of education. Do increases in minimum leaving age raise everyone's educational attainment or they affect differently each quantile?

This exercise can also be useful to tackle the argument according to which reforms are not really exogenous. If reforms were implemented in those countries where schooling attendance had already been increasing before the reform, the law is potentially endogenous. Landes and Salomon (1972) argue that laws that reform compulsory schooling are in fact endogenous; analysing the US at the end of the XIX century they conclude that the laws were not the cause of the increased attendance, as schooling levels were already increasing before the reforms. Their explanation is that the cost of passing a law is a positively related to the potential violators, so that no policy maker would ever enforce a law if costs were bigger than benefits (that is if the target of the reform is too far from the current level). So following this reasoning a compulsory school reform is enacted where schooling level is already increasing, otherwise no politicians would maintain consensus.

Lleras-Munay (2001) performs the same analysis and concludes that reforms were not endogenous (and of course effective) for two reasons; first because the laws considered did not affect all the distribution of educational attainment in the same way, that is to say that the reforms increased the attainment of the lower percentiles of the distribution of education, thus reducing inequality; then she checked if future reforms can be used to predict current schooling level and findings confirms that future laws are not related to current attainment.

So if all the distribution of education (people on every point of the distribution) shifts after a reform, it can be a signal that the reform is not really effective since what's increasing the educational attainment is a trend. So here a quantile regression model has been performed (as Lleras-Munay, 2001, and Brunello et al. (2009) did among others). With this approach it's possible to see at what quantiles of the distribution of education the reform was more effective, given that we already know that on average it is.

The model uses exactly the 7 years cohorts with the polynomial in age.¹⁴ To be more formal the quantile of interest is defined as q , and for each observation i , r_i is the residual as in standard linear model:

$$r_i = y_i - \sum_k \beta_k x_{ik}$$

Where y_i is the dependent variable and x_i reassumes all the explanatory variables. k ($\kappa = 1 \dots K$) are the regressors used before.

Let's define a multiplier h_{ik} which is equal to $2q$ if $r_i > 0$ and equal to $2(1-q)$, otherwise. So the problem is:

$$\min_{\beta_k} \sum_i |r_i| h_i$$

so the estimation procedure is a minimization of weighted residuals with weight h_i . Consider also that it's possible with the same technique to perform the regression at different quantiles simultaneously. The results coincide with the case of separate estimation but with the simultaneous equations model it's possible to compare the coefficients across equations (the variance-covariance matrix is estimated entirely). This last approach has been followed and a simultaneous regression has been performed at different quantiles (10th, 25th, 50th, 75th and 90th), only in the general case (mean across country).

The results, reported in Tables A.5 in the Appendix, show that the reforms were effective at the first quartile (coefficient equal to 0.013, $p < 0.1$), while the effect is 0 at all the other point of the distribution that we tested. This result means that reforms on MLA successfully increase educational attainment of the targeted population. Since most of the reforms aimed at increasing

¹⁴ Here we prefer to use a large time span rather the 2 years cohorts, since we are comparing different quantiles and so it's crucial to have many observations in every point of the distribution of education.

total years of schooling to 9 years (even if with differences among countries), we can expect that basic education (the first stages of primary education) is not changing after this kind of reform (that is to say that literacy goals have already been reached). At the same time compulsory schooling reforms do not involve higher levels of education (such as secondary and tertiary). This means that the decision to complete secondary education or to go to college does not depend on laws that rise minimum leaving age. In principle we could have had also this kind of result if reforms changed the perceived importance of education increasing attainment even among people not directly constrained by the law; the effect of reforms could have propagated to all higher levels of education. This result instead is telling that schooling increased mainly among people that without the reform would have left school before the new minimum leaving age. This is consistent with the human capital hypothesis as opposed to the sorting hypothesis (see Lang and Kropp, 1986).

So the fact that the a lower quantile is more affected than higher means that reforms were effective and, moreover, we can see this as evidence of the fact that those reforms are not endogenous, since they do not homogeneously affect the distribution of education.

The next section will present an analysis similar to the quantile approach, but with a more descriptive power. Instead of looking at quantiles, that we don't know exactly what they stand for in terms of years of schooling, we will be studying the impact of reforms on primary schooling.

1.5.3 Basic attainment and secondary schooling

Here we perform a discrete choice model (probit) aimed at testing if the probability to complete primary schools is affected by the reform. So the dependent variable is equal to 1 if the individual completed primary schooling and to 0 if he didn't. The model includes the explanatory variables of equation (1) and errors are clustered at country level. The probability of completing lower secondary schooling which would be a more appropriate test, given the reforms considered, could not be checked because data are collected from different sources that on this specific point present strong discrepancies.

This approach is similar to the quantile regression model used before, but this time we the results will be easier to interpret since they are referred to a specific level achieved, while quantiles does not correspond to a precise level. As before, results will be presented in the general form (for all the countries) and in the detailed one. In the section about Canada we found a similar not encouraging result.

On average reforms increase the probability to complete primary by 1,5% ($p < 0.001$), even if the reforms didn't target specifically these specific levels. The full set of marginal effects is listed in Table A.5 in the Appendix.

So when the probability to complete primary school is considered, there might be positive reform's effects in those country where the elementary level needs still to be reached by a significant number of people. Results obtained with the probit model tell that the reforms probably play a role in helping some countries in reaching basic attainment, represented by primary school.

We also checked if the probability of completing secondary education is affected, but unfortunately this is not the case. Given the countries and reforms considered this would be probably a too ambitious outcome, since the reforms on average targeted a level which is below the completion of secondary school.

1.6 Conclusions

This analysis shows the effects of reforms on compulsory schooling that were implemented in the second half of the last century in various countries. The impact is studied on educational attainment, using a RD design framework. Given the increasing importance of reforms in education as a way for instrumenting individuals' schooling, it's relevant to analyze how to better measure

reforms' impact. All the reforms considered here raised minimum school leaving age. We first give advices on the strategies that can be followed for determining reform's impact on education using a single country as a reference. Then we perform the analysis on the full set of countries for which information about individuals and reforms are available.

A linear regression model shows that reforms are effective on increasing average years of schooling by 1.4%. If in the same model the parameters for the effect of reform are let free to vary across countries, we obtain great variability between countries.

A quantile regression approach takes into account variations on the distribution of education. We observe that reforms are most effective at the first quartile (coefficient equal to 0.014) leaving unaltered the other quantiles tested.

The effect of reform is also studied in a probit model, where the impact of laws on the probability to complete primary schooling is taken into account. The reforms are found to have positive effects.

Policies aimed at increasing compulsory schooling are successful on average and promote schooling among those at the lower tails of the distribution of education.

Here we stress the importance of the circumstances under which educational level is influenced by reforms in compulsory education and of the empirical strategies that can be pursued for identifying correctly people assigned to the treatment, so that reforms can be used fruitfully whenever an instrument for schooling is needed.

1.7 Appendix

<i>Educational attainment</i>	<i>Years of schooling</i>
Below grade 5	3
Grades 5-8	7
Lower secondary general completed	9
Secondary completed	12
Post-secondary technical education	14
Some college completed	15
University or more	17

Table 1 Years of schooling and corresponding codes

	(1)	(2)	(3)	(4)
Different definitions of exposure to reform:	Prov Resid	Prov Birth(restr)	Migrants included	Only Migrants
Reform, def by prov. of resid	0.19***			
Reform, def by prov. of birth§		0.2***		
Reform, def by prov. of birth (migrants included)			0.17***	
Reform, def by prov. of resid (migrants only)				-0.105
White	-0.55***	-0.1***	-0.14	0.78***
Female	0.3***	0.25***	0.2***	-0.14
Prov of resid fx eff	yes	yes	yes	yes
Prov of birth fx eff	yes	yes	yes	no
Observations	7751	6856	7640	784
R-squared	0.033	0.026	0.025	0.040

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

§ Here we include only Ontario, Manitoba, Saskatchewan, Alberta, Quebec and British Columbia provinces

Table 2: comparisons among different strategies for identifying the correct target group. Column (1) refers the identification by province of residence, column (2) to province of birth. Columns (3) and (4) refer to migration status: column (4) is the same regression as the one showed in column (2), but migrants are included, while in the regression of column (4) we included only foreign born people.

	Average years of schooling	
	Non Migrants	Migrants
Before the reform	12.76	15
After the reform	13	15

Table 3 Summary statistics for years of schooling: group of migrants and non-migrants

Average educational attainment, before and after the reform					
Before the reform			After the reform		
obs	mean	st dev	obs	mean	st dev
3169	2.04	0.707	3279	2.07	0.69

Note: 2 corresponds to secondary education completed.

Table 4 Summary statistics for educational level, before and after the reform

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Cohort ± 1 year	Cohort ± 2 years	Cohort ± 3 years	Cohort ± 4 years	Cohort ± 5 years	Cohort ± 6 years	Cohort ± 7 years
Reform, def by prov. of birth	0.131	0.199***	0.212***	0.257***	0.306***	0.323***	0.318***
Observations	3414	6856	10235	13700	17232	20917	24518
R-squared	0.021	0.026	0.027	0.027	0.028	0.027	0.025

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

Table 5 Different coefficients obtained widening the cohorts considered.

	(1)	(2)	(3)	(4)
	Polynomial order 4	Polynomial order 3	Polynomial order 2	Polynomial order 1
Cohort ± 7 years				
Reform, def by prov. of birth	0.15*	0.17**	0.17**	0.19**
Observations	24518	24518	24518	24518
R-squared	0,032	0,031	0,031	0,025
Res sum squares	185103	185387	185404	186450
Degree of freedom	24500	24501	24502	24503
Variance of the model	7,56	7,57	7,57	7,61

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

Table 6: Test for reforms' efficacy using a cohort 7 years wide and different order polynomials in age.

	True year	Decade	Minimum leaving age	Total years of compulsory schooling
Quebec	1988	1990	15→16	9→10
Ontario	1954	1960	16	8→10
Manitoba	1962	1960	14→16	7→9
Saskatchewan	1965	1970	15→16	8→9
Alberta	1970	1970	15→16	8→10
British Columbia	1990	1990	15→16	8→9

Table 7 Date transformed into decade

Def by decade	
Reform ,def by prov. of birth and by decades	0.11**
White	-0.79***
Female	0.32***
Observations	41501
R-squared	0.077

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

Table 8 Regression with the uncertainty about reforms implementation year

Country	Reform	Min leaving age after the reform	Min leaving age before the reform	Years of compulsory schooling after the reform	Years of compulsory schooling before the reform	Ind data sources	Info on reforms
Argentina	1993	15	12	10	7	ipumsi	Unesco, Ley 24.195
Austria	1962	15	14	9	8	issp	Brunello et al (2009)
Belgium	1983	18	14	12	8	issp	Brunello et al (2009)
Brazil	1971	14	11	8	5	ipumsi	Unesco, EFA
Canada:						ipumsi	Unesco, Oreopoulos (2005)
<i>Quebec</i>	1988	16	15	10	9		
<i>Ontario</i>	1954	16	16	10	8		
<i>Manitoba</i>	1962	16	14	9	7		
<i>Saskatchewan</i>	1965	16	15	9	8		
<i>Alberta</i>	1970	16	15	10	8		
<i>British Columbia</i>	1990	16	15	9	8		
Chile	1966	13	11	8	6	ipumsi	Unesco, www.countrystudies.us
Colombia	1991	15	10	9	4	ipumsi	http://education.stateuniversity.com
Costa Rica	1973	15	12	9	6	ipumsi	Unesco, www.oei.es
Cyprus	1985	15	12	10	7	issp	Unesco, EFA, Eurydice
Czech Republic	1984	16	15	10	9	issp	Eurydice
Denmark	1971	16	14	9	7	issp	Brunello et al (2009), Eurydice http://www.tau.ac.il/eial/X_1/ ,
Ecuador	1977	15	12	9	6	ipumsi	Unesco
Finland	1972-1977	16	13	9	6	issp	Brunello et al (2009) unesco,
Ghana	1985	15	12	9		ipumsi	http://www.educatejournal.org
Germany-West						issp	Brunello et al (2009), Eurydice
<i>Schleswig-Holstein</i>	1956	15	14	9	8		
<i>Hamburg</i>	1949	15	14	9	8		
<i>Niedersachsen</i>	1962	15	14	9	8		
<i>Bremen</i>	1958	15	14	9	8		
<i>Nordrhein-Westfalen</i>	1967	15	14	9	8		
<i>Hessen</i>	1967	15	14	9	8		
<i>Rheinland-Pfalz</i>	1967	15	14	9	8		
<i>Baden-Wuerttemberg</i>	1967	15	14	9	8		
<i>Bayern</i>	1969	15	14	9	8		
<i>Saarland</i>	1964	15	14	9	8		
Great Britain	1973	16	15	11	10	issp	Eurydice
Greece	1975	15	12	9	6	ipumsi	Eurydice
Hungary	1993	16	14	10	8	issp	Eurydice, http://www.inca.org.uk/
Ireland	1972	15	14	9	8	issp	Brunello et al (2009), Eurydice
Italy	1963	14	11	8	5	issp	Brunello et al (2009), Eurydice
Mexico	1993	15	12	9	6	ipumsi	Unesco, http://pcmi.ias.edu/
Netherlands	1973	16	15	10	9	issp	Brunello et al (2009), Eurydice
Panama	1995	15	15	11	10	ipumsi	www.oei.es , Gaceta Oficial
Philippines	1987	12	0	6	0	ipumsi	Unesco
Portugal	1986	15	12	9	6	issp	Eurydice
Rwanda	1985	15	13	6	4	ipumsi	Unesco
Slovak Republic	1984	16	15	10	9	issp	Eurydice
Slovenia	1969	14	12	8	6	issp	Eurydice

Spain	1970	14	12	8	6	issp	Brunello et al (2009), Eurydice
Sweden	1962	16	15	9	8	issp	Brunello et al (2009), Eurydice
Switzerland	1970	15	12	9	6	issp	Unesco
Uruguay	1986	15	12	9	6	issp	Unesco
Venezuela	1980	14	11	9	6	ipumsi	Unesco

Table A.1 Summary of countries considered, reforms and sources used

Country	Obs	Years of schooling			
		Mean	Std. Dev.	Min	Max
Argentina	3614	10.05	3.19	0	17
Austria	3828	10.35	2.67	0	24
Belgium	1142	13.44	3.01	0	24
Brazil	26575	6.04	4.53	0	17
Canada	25070	13.10	3.05	3	23
Chile	4893	9.36	4.48	0	18
Colombia	8197	7.97	4.52	0	17
Costa Rica	12230	8.20	4.27	0	18
Cyprus	1031	13.49	2.24	6	20
Czech Republic	3858	12.86	2.24	4	23
Denmark	3267	12.76	3.75	6	24
Ecuador	18263	8.38	5.24	0	18
Finland	2647	12.43	3.93	0	24
Germany	4975	10.51	3.25	0	24
Ghana	5013	5.65	6.29	0	18
Great Britain	5688	11.86	2.08	6	24
Greece	5237	10.79	4.16	0	22
Hungary	235	12.44	2.88	3	22
Ireland	3923	12.16	3.04	0	24
Italy	3045	11.21	4.42	0	24
Mexico	4601	9.49	4.17	0	18
Netherlands	7314	13.28	3.72	0	24
Panama	7005	9.75	4.08	0	18
Philippines	5638	9.39	3.21	0	14
Portugal	2330	10.45	4.64	0	24
Rwanda	6592	3.56	3.22	0	18
Slovak Republic	2177	12.77	2.45	0	23
Slovenia	5281	11.16	3.09	0	24
Spain	4406	10.78	4.34	0	24
Sweden	3917	11.12	3.88	0	24
Switzerland	4304	11.46	3.21	0	24
Uruguay	938	10.67	3.99	0	24
Venezuela	7222	8.29	3.51	0	18

Table A.2 Summary statistics for years of schooling

	(1) OLS 2 yrs wide	(2) OLS Countries' effect	(3) OLS 7 yrs wide
Reform	0.14**	0.0529***	0.187***
Reform*at		0.290***	
Reform*be		0.138***	
Reform*bg		0.719***	
Reform*ch		-0.00471	
Reform*cy		0.0927***	
Reform*cz		-0.269***	
Reform*de		0.269***	
Reform*dk		0.0576***	
Reform*es		0.473***	
Reform*fr		0.349***	
Reform*gb		0.162***	
Reform*hu		0.0635***	
Reform*ie		0.530***	
Reform*ita		0.278***	
Reform*nl		0.281***	
Reform*pl		0.130***	
Reform*pt		0.606***	
Reform*ru		0.136***	
Reform*se		0.554***	
Reform*sk		-0.0229***	
Reform*slo		0.111***	
Reform*uy		0.388***	
Reform*ar		-0.697***	
Reform*br		0.190***	
Reform*ca		0.139***	
Reform*cl		0.450***	
Reform*co		0.137***	
Reform*cr		0.0938***	
Reform*ec		0.0686***	
Reform*gh		0.457***	
Reform*gr		0.475***	
Reform*mx		0.0681***	
Reform*pa		-0.432***	
Reform*ph		-0.514***	
Reform*ro		-0.0661***	
Reform*rw		0.201***	
Age			0.0462*
Age^2			-0.001***
Female	0.180**	0.180**	0.118
White	2.268***	2.268***	2.189***
Countries fixed effects	yes	yes	yes
Constant	5.831***	5.873***	5.512***
Observations	75810	75810	209501
R-squared	0.343	0.344	0.353

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

Table A.3: general and detailed effect of treatment

Ranking of countries			
Argentina	-0.792	Canada	0.044
Philippines	-0.609	Great Britain	0.067
Panama	-0.527	Brazil	0.095
Czech Republic	-0.364	Rwanda	0.106
Romania	-0.161	Germany	0.174
Slovak Republic	-0.118	Italy	0.183
Venezuela	-0.095	Netherlands	0.186
Denmark	-0.037	Austria	0.195
Hungary	-0.031	France	0.254
Mexico	-0.027	Uruguay	0.293
Ecuador	-0.026	Chile	0.355
Cyprus	-0.002	Ghana	0.362
Costa Rica	-0.001	Spain	0.378
Slovenia	0.016	Greece	0.380
Poland	0.035	Ireland	0.435
Russia	0.041	Sweden	0.459
Colombia	0.042	Portugal	0.511
Belgium	0.043	Bulgaria	0.624

Table A.4: Ranking of countries according to the reforms' impact

	Quantile:				
	10°	25°	50°	75°	90°
Reform	0**	0.013*	0	0	0
Female	0***	0	0	0***	0***
White	2***	2***	3***	3***	3***
Age	0***	-0.18***	0***	0***	0***
Age^2	0***	0.006***	0	0***	0*
Age^3	-0***	-0.00006***	-0	-0***	0*
Countries fixed effects	yes	yes	yes	yes	yes
Constant	1**	5.8***	6***	8***	9***
Observations	211169	211169	211169	211169	211169
Pseudo R-squared	0.356	0.31	0.2	0.17	0.073

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

Table A.5 Simultaneous quantile regression: general effect of the reforms

Marginal effects for the probability of completing primary schooling	
Reform	0.0149***
Female	0.00314
White	0.116***
Countries fixed effects	yes
Observations	75810
Pseudo R-squared	0.291

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

Table A.6 Probit model for the completion of primary school.

2 The effect of schooling on fertility, labor market participation and children's outcomes, evidence from Ecuador

2.1 Abstract

Using a representative sample of Ecuadorian young women's households, this paper focuses on the role played by education in shaping fertility choices, labor market participation rate and future children's outcomes. Education, which is found to be endogenous with respect to teenage childbearing, is instrumented by a reform that took place in 1977. Estimates show that the completion of lower secondary school decreases the probability of early motherhood by 7%. Then, after controlling for labor market preferences in a model where the choices to be a mother and to be in the labor force are considered simultaneously, we find evidence that schooling is positively related to women's labor market participation rate and negatively to early motherhood. The last section concludes stressing the potential intergenerational effects of changes in the age at first birth, showing that firstborn children born to older mothers have better educational outcomes than those born to younger ones. So policies aimed at increasing women educational attainment are found to be positively related to better women's outcomes, expressed by lowered teenage motherhood rate and by increased labor market attachment, and also to improved children's conditions, represented by their schooling attendance.

2.2 Introduction

This work will consider the effect of education on fertility and on labor market participation rate in the case of Ecuador, where fertility rate is high and women average educational level low. As a consequence women are unskilled and underpaid or even unemployed. In 1982, 24% of teenagers were mothers and on average 19 years old mothers completed less than 6 years of schooling, while their non-mothers counterparts 8 years.¹⁵ Teenage childbearing is commonly associated with subsequent poor mothers' and children's outcomes (see for example Ribar, 1999 and 1994, and Krueger and Berthelon, 2009) and in Ecuador children's conditions are deprived and sometimes miserable. In fact, along with academic works, there is huge evidence that in Ecuador investment in human capital is low, especially among poor families. Children drop out of school and work in order to increase family income (ILO, 2004). They are exploited in the formal and informal labor market, working for a pay which is far below the minimum wage on plantations or in farms (*ibidem*). Governmental commitment to prevent minors' abuse and exploitation is not efficacely pursued and much of the effort for promoting policies in favor of children is left to NGOs.

Graphs of Figures 1 clearly suggest the correlation between education, teen fertility and participation in the labor market. Education here expressed in levels (no schooling or some, primary completed and some secondary, secondary completed and above) is positively related to

¹⁵ According to 1982 census, data provided by IPUMSI.

participation in the labor market rates and inversely to early fertility, proxied by being a mother before age 18.

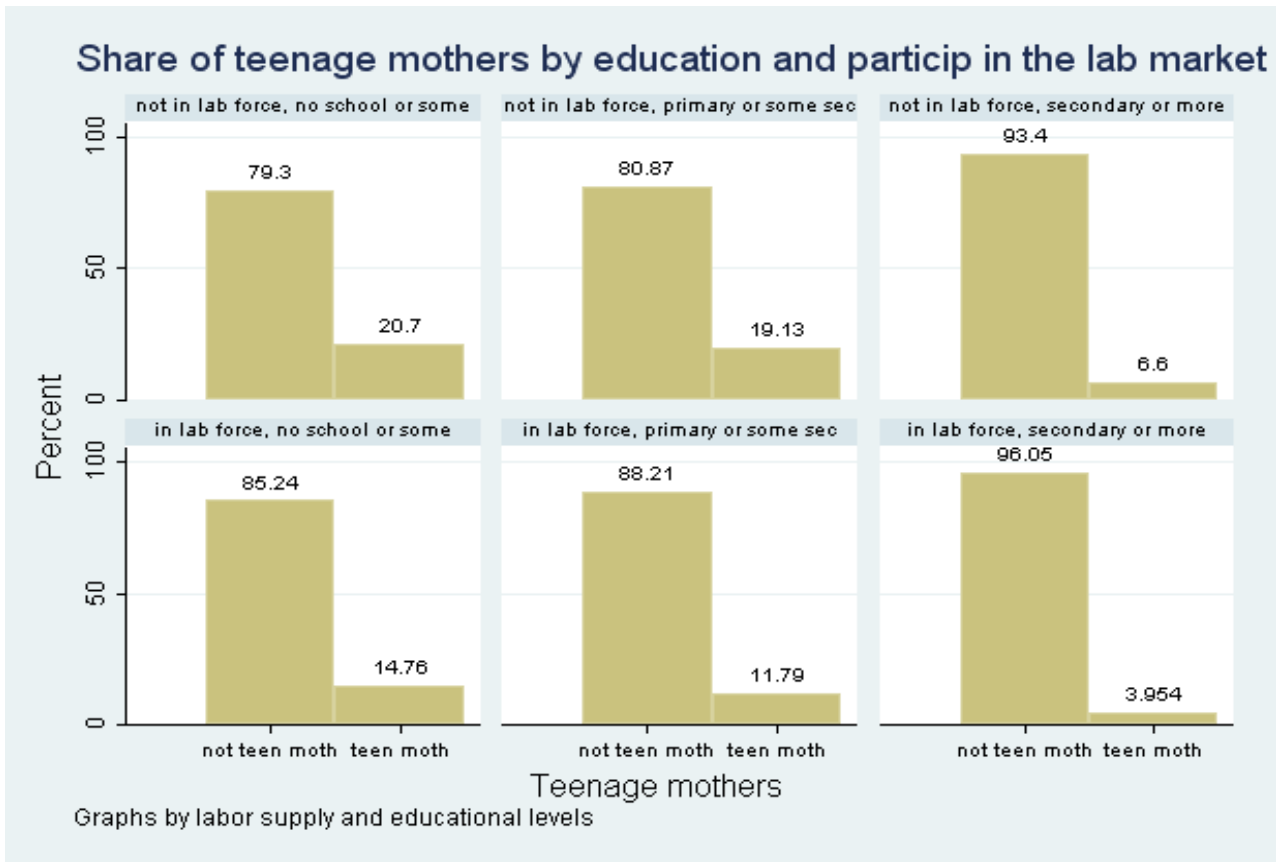


Figure 1 Proportion of teenage mothers in 1990 and 1982, by educational level and participation in the labor market; source: IPUMSI.

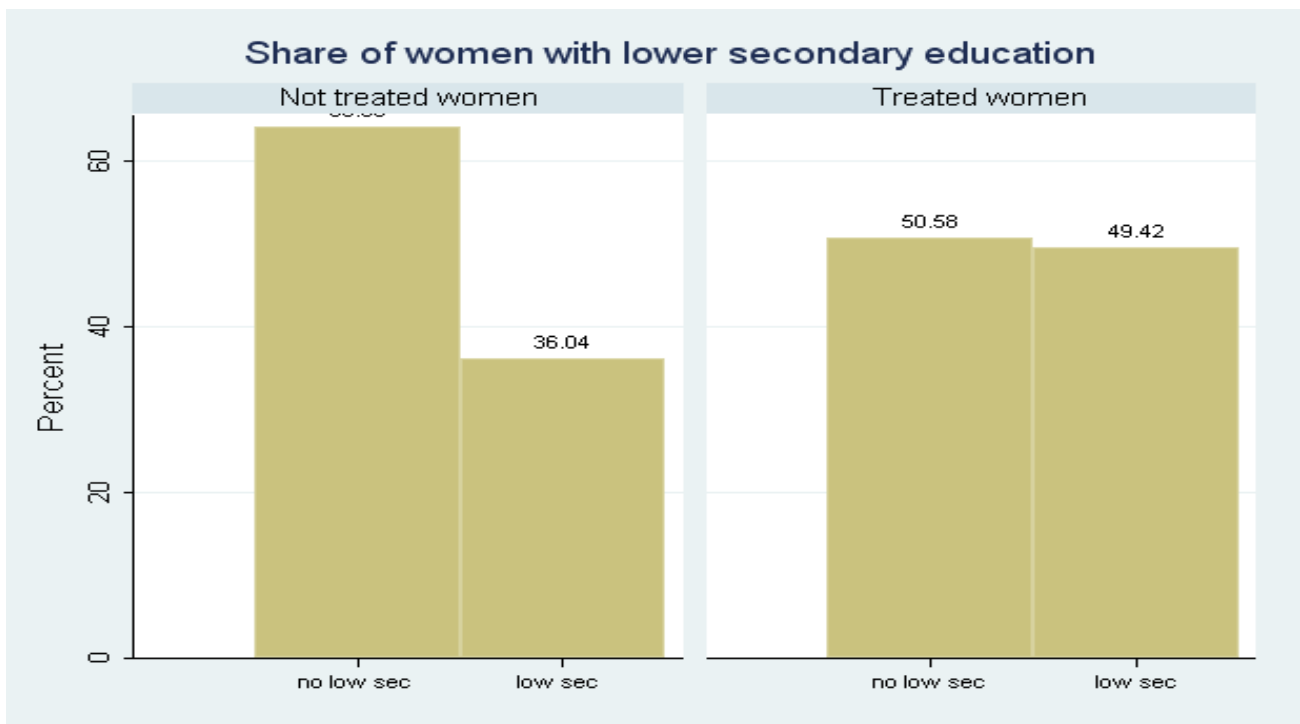


Figure 2 Proportion of women with lower secondary education. Sample of women aged 24-31, in 1982 and 1990; source IPUMSI.

What needs to be uncovered is the causal relation between education and teenage motherhood, and if fertility choices affects also the labor market participation and vice versa.

Since the relationship between education and (early) fertility might be endogenous we exploit an exogenous variation in compulsory education to instrument women's educational attainment. The graphs in Figure 2 shows the shift in the educational level associated with the reform.

In this context, from a policy perspective, promoting education is relevant because benefits from an increased educational attainment include a reduction in fertility rates among teenagers and labor opportunities; this in turn will positively affect further future generations, helping to break self-sustaining poor conditions. So here we will also stress the importance of schooling on future outcomes, exploiting the variation in women educational attainment caused by a reform.

So a first part will focus on the relationship between teenage motherhood and education, which is treated as potentially endogenous. This problem has been solved using an instrumental variable approach, by means of a reform of compulsory education that took place in Ecuador in 1977. Intuitively education can be endogenous to teenage fertility because women with a stronger preference for participating in the labor market could simultaneously invest more in education and less in children, and vice versa; moreover it's not clear if the decision to quit school is a cause or an effect of fertility decisions, for example if teenagers get pregnant and they drop out of school (Glewwe, 2002, and Ribar, 1994). So empirically it's difficult to estimate the effect of education on fertility, because it depends on unobserved characteristics (ability and preferences) that determine simultaneously fertility choices and investment in human capital. Acemoglu and Angrist (2000) and Angrist and Krueger (1991) refer to reforms in compulsory education law as to the ideal instrument for education. So education is instrumented and a differences-in-differences approach is applied to assess the effects of schooling on early fertility. Education has also been instrumented and found negatively related to fertility in the works by Black et al. for the US and Norway (2008), by Monstad et al. (2008) for Norway, by León (2004) for US, by Fort (2009) for Italy and by Tayfur et al. (2008) for Turkey. Osili and Long (2007) test the efficacy of a different Nigerian policy program to boost girls' schooling on fertility. In this work we won't use data on completed fertility but we could only take into account when young women gave birth for the first time, since all information are retrieved from censuses; so, our outcome variable for early fertility will indicate if women got their first child before the age of 18. In this study education is found to be endogenous with respect to teen motherhood and instrumental variables estimates suggest that education is negatively affecting early motherhood, as expected.

A second part is then dedicated to understanding whether the effect of a higher educational level on fertility passes through the labor market, even if there might be also non-market outcomes stemming from education (changed values, aspirations and attitude toward childhood). So basically in this section we will try to understand why women who get more schooling delay the age at first birth, so that education has negative effects on early fertility. Heckman and Walker (1990) showed that women wages are negatively related to fertility: more educated women have higher wages, delay first conceptions and reduce total number of kids. The idea that will be tested, as in Bratti (2003), Happel et al. (1984), is that the increased level of education makes more profitable working and thus the opportunity cost of a child rises and their 'consumption' falls. Since the decision to have a baby when teenager and to work is interdependent, and education is potentially still endogenous a trivariate probit model is considered. In fact a simple regression model where the decision to participate on the labor market is regressed against an indicator for teenage fertility can be spurious because it's the result of an interdependent decision where each woman chooses to invest in early fertility given her labor market expectations. Results clearly support that education while diminishes the probability to be a mother before age 18, increases the probability to be in the labor force.

In the last part we will explore if the age at first birth influences children's outcomes. Here we only have information on current household's members, so each mother's fertility timing is associated with her children's reported educational level. The analysis shows that avoiding teenage

motherhood has positive effects on children educational outcomes. So there is an indication for positive intergenerational effects of schooling, even if the analysis can not show a proper causal relation.

2.3 Education, fertility and labor market outcomes in the literature

As already mentioned scholars showed that there is a causal relationship that goes from maternal education to fertility, passing through labor market. An increase in the schooling level of women raises their expected wages and their participation in the labor market as well. This in turn cause an upward shift in the opportunity cost of having a baby, expressed in missed wages during pregnancy and afterwards (Happel 1984 et al., Heckman and Walker, 1990). On this market mechanism we will return later on.

However, an increase in educational level has an impact on fertility that goes beyond the labor market outcomes cited above. There are many channels through which education influences reproductive behavior and moreover this relationship is not static over the demographic transition. Education improves women's ability to process information regarding contraceptive technologies, healthy behavior (see for example Behrman and Wolfe, 1989) and it strengthens their role inside the couple. So here we will briefly review all the non-market outcomes associated with an increased schooling level and fertility.

Glewwe (1999) and Schultz (1993) find that mothers' education has positive effect on the health status of their offspring so that child mortality is reduced. Chou et al. (2007) find evidence of decreasing child mortality with increasing parents' educational level, instrumented with a reform on compulsory education in Taiwan. A recent work by Krueger and Berthelohn (2009) analyzes the effect of a school reform that increased hours spent in school in Chile and show that teen motherhood is reduced by access to full-day education. Note that increased education does not always imply a reduction in fertility rates; in fact in a country at the beginning of the demographic transition, education can be associated with an increase in the number of children, because of their health improvement and higher survival rate.¹⁶ Notwithstanding, empirical analysis, as in Schultz (1993), finds that when infant mortality goes down also fertility is reduced because there is a quality/quantity trade off (consistently with Becker and Lewis, 1973). In fact, education is associated with an increase in the number of children if fertility is not controlled, but as soon as it gets fully controlled and desired family size is smaller than the actual one, those aforementioned effects boosting fertility are offset by the use of contraceptives. Rosenzweig and Schultz (1985), Rosenzweig and Seiver (1982), Ainsworth et al. (1996), Florez and Nuñez (2002) provide evidence that education have a positive effect on the use of contraceptives.

Even if birth controls methods are diffused and relatively cheap, husband disapproval can hamper women to use them, as shown by Kamal (2000) in a study on Bangladesh. So another important aspect that is taken into account is if education affects the decision making processes inside the family in favor of the women. In the works done by Jejeeboy (1995), women are shown to have a stronger role in family planning and more bargaining power within the household, the more they are educated. Moreover, cultural values, beliefs and tradition play an important role in shaping desired family size, and it has been shown that whenever they are modified there is a response in the fertility rates. So the availability of contraception by itself is not a sufficient condition for reducing the number of children (La Ferrara et al., 2008, study how values diffused by a soap opera reduced the desired family size Brazil).

Women's education does not only affect women's attitude toward fertility decisions, but it has been shown to influence also children's outcomes other than their survival rate.

¹⁶ And also as shown in Bratti (2003) for developed countries, for high level of education the relationship turns positive, because high-income women have access to childcare.

If mothers' education makes less expensive to spend time with children in terms of effort (as shown by Black et al., 2005) and if there is a tradeoff between quality and quantity of children, improvement in parental education is transmitted to their offspring (Glewwe 2002, Hanushek, 1992). Rosenzweig (1990) shows empirically that when investment in children's human capital is higher than the number of children is lower, sustaining the hypothesis of an underlying trade off.

Francesconi (2008) and Ribar (1999, 1994) show that children born to teenage mothers have worse outcomes in terms of education, earnings, health and inactivity. Hunt (2003) analyzing US finds that people born to teenage mothers are more likely to be criminals, because of their deprived childhood. Levine and Painter (2003) find that teenagers out of wedlock mothers in US have lower education than peers that have children later on. This is mainly due to preexisting disadvantages and not to the birth itself, nevertheless the estimates indicate that the persistence of those disadvantage is due to becoming mother when teen. Black et al (2005) find instead limited support for intergenerational effects between educational level of parents and their offsprings, while in another paper (2008) they find that education delays teenage childbearing and this attitude is transmitted to their children (in Norway and US). McCrary and Royer (2009) instead find that in California and Texas the effect of mothers' education on timing of fertility and children's health is not significant.

On developing countries many studies concentrate on health problems of children born to teenagers (Woldemicael for Eritrea, 2005, LeGrand et al. for Sahel region, 1993, Florez and Nuñez for six Latin American countries, 2003) and results are clearer than those of developed countries. In Indonesia mothers' and fathers' increased educational level plays a role in decreasing the number of early births and in increasing children health, as showed by Duflo and Breierova (2004). Overall there is the common agreement that too young mothers are more prone to invest less in their children than older mothers with respect to both education and health. This fact is particularly severe in countries where poverty is widespread so that children are subject to high mortality and morbidity rates. So mothers' education can have an impact on children's well being through delaying early pregnancies.

While for developing countries scholars study the effect of education and fertility on human capital outcomes and only marginally on labor market perspectives, for developed countries most of the current research is focused on the latter in a dynamic perspective and, in particular, on how women emancipation causes a postponement of fertility and generally its decline.¹⁷ In this work the issue is not fertility postponement, but to see if education can promote more equal conditions for women and more investment in human capital, which is commonly viewed as a crucial goal to be pursued for growth.

The contributions to theoretical models of fertility stem from two streams of literature: one in which Becker (1960) and Becker and Lewis (1973) stress the importance of the cost of raising a child and earning potential, proxied respectively by female wages and family income. Becker (1960) finds that the elasticity of demand for children quality to income is larger than the elasticity of demand for children quantity. Heckman and Walker (1990) estimate a reduced-form neoclassical life cycle model for fertility in which lower fertility rates and a delayed times to all conceptions are associated to higher female wages. In an empirical model Behrman and Rosenzweig (2002) point out that assortative mating effect amplifies the income effect of an increased educational level on children quantity, because more educated women chose more educated partners further increasing the household's income.

On the other hand, Easterlin (1966) proposed a model where changes in relative cohort size affect fertility through their impact on relative income. The idea is that after baby booms there are 'crowding mechanisms' in families, schools and labor market that deteriorate a cohort income expectations with respect to their parents'. This leads to downward revision of opportunities, that cause reduction in fertility rates and an increase in female labor force participation, as the young

¹⁷ With the exception of Italy, where participation in the labor market rate is still relatively low but at the same time fertility rates are one of the lowest among European countries (see Di Tommaso, 1999).

cohort tries to maintain their relative economic status (there is the underlying hypothesis that each cohort tends to keep the status in which it grew up). Jeon and Shields (2008) test this relationship in a cointegrated model on US and find general support for the Easterlin hypothesis. Macunovich (2000) extends the analyses to developing countries and shed some light on a phase of the demographic transition where fertility control becomes accepted and so trade off between quantity and quality of children takes place.

Many scholars test empirically life cycle models of fertility where education plays a crucial role in explaining both fertility *quantum* and/or *tempo*. In these models children enter the utility function of a household together with other consumption goods. The price associated to children are expressed by the time spent with the child, which is generally the opportunity cost represented by female wages, the direct cost of rearing a child and the investments in human capital that a household forgoes (Walker 1995).

Gustaffson (2001) reviews many empirical contributions to the fertility patterns observed in Europe. He points out that the main factor driving the slow down of the observed population growth is the women's career costs: those include the wage loss and the lower human capital investment caused by time spent out of work.¹⁸

2.4 Women's education and teenage motherhood

2.4.1 The effect of the reform on educational attainment

Here we will take into account the effect of education on risk of being a teen mother, exploiting the variation caused by the reform on compulsory schooling, given the potential endogeneity of education on fertility decisions. Before explaining the effects of education on fertility we will concentrate on first stage schooling equation, then after some considerations about the effect of the reform on educational level we will use the instrumented education in a fertility equation.

Schooling is endogenous to fertility if there are some unobservable women's characteristics that are correlated with both the investment in education and fertility decisions, like strong labor market preferences; in fact if a woman wants to be in the labor force she will invest more in education and less in fertility. The reform that will be taken into account to instrument education is a law passed in 1977 (*Ley de Educación y Cultura*) that extended compulsory education from six years to nine years, making it freely available to all.¹⁹ Education before 1977 was compulsory until age 12, so that this law extended compulsory education by 3 years: from age 12 to age 15.

Data are taken from 1982 and 1990 censuses, provided by *Instituto Nacional de Estadística y Censos*, and harmonized by IPUMSI and contain information on households' composition and on the dwellings' general characteristics. Therefore we can link each woman present in the sample to her children and to all the people currently living in the household. Unfortunately it's not possible to keep track of those family members that had left the household (for example children when grown up). So data provide two kinds of information: a) on the house's status: number of rooms, availability of the kitchen, phone line, sewage system, water supply, and b) on individuals that live in: parental rule, link to household head, age, sex, province of birth, place of residence, educational level, employment status and occupation.

The analysis follows a differences-in-differences framework, so that women in the sample are divided into four groups. We know that the reform took place in 1977 and we have data from two census years, 1982 and 1990. Women that were younger than 27 years old in 1990 were targeted by the reform (treated group), as they were 14 years old or younger in 1977. Since people could have started school late when child and/or repeated a grade, we include in the treated group women that were older than 12. Instead those women that in 1977 were older than 14 were not involved by the

¹⁸ See also Gustaffson and Wetzel (2000).

¹⁹ http://www.tau.ac.il/eial/X_1/ossenbach.html

reform and form the not treated group. Since there might be secular trends that could increase schooling even without the reform, computing just the differences in educational attainment between these two cohorts is not sufficient to infer reform efficacy. To get rid of this possible growing trend, we include in the regression two more groups of women: those that were older and younger than 27 in 1982, mimicking the same reasoning as above, and then we control for age. In this way assuming that the trend if present is constant (i.e. the only thing that happened is the reform) we can be sure that what increased educational level was the reform. Note that this is a standard assumption in every study that makes use of difference-in-differences technique. Each group here defined is four-year wide: so the treated includes women aged from 24 to 27, not treated from 28 to 31 (for both the censuses year). The figure below reassumes the setting used for the analysis according to the description just given.

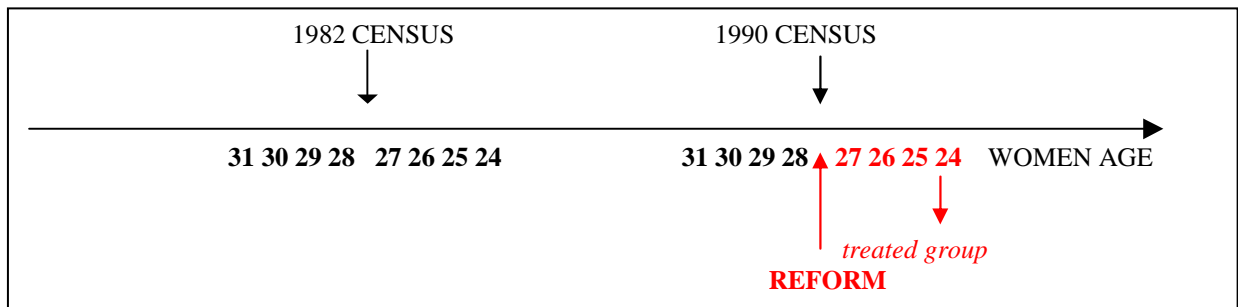


Figure 3: scheme used in the DID structure; in bold the age groups considered, in red the cohort affected by reform

We analyze the effect of compulsory schooling reform on different educational outcomes but the DID scheme remains the same, so as the other regressors.

The explanatory variables include two sets of regressors: those that have the purpose to instrument education, vector z_i , and those that will be used also in the fertility and in the labor regression (x_i and w_i variables).

So the instruments z_i include: a dummy variable for the reform, equal to 1 if women are in the treated group, and equal to 0 otherwise; the share of the number of teachers per in each *canton* in each census year (the *canton* is the smallest territorial unit of analysis) and the average level of education of people older than 28 in each *canton* and census year; this last variable aims to capture the ‘socio-cultural environment’ in which each women live in and that could potentially influence women educational attainment. We include only educational attainment of people older than 28 to prevent the reform having influences on that group’s schooling level. The share of teachers, which includes teachers of any type of school, is a ratio on the school aged population and proxies the amount of resources available for education.²⁰

AGE RANGES				
	28-31 in 1982	24-27 in 1982	28-31 in 1990	24-27 in 1990
<i>REFORM</i>	0	0	0	1
<i>TREND</i>	0	0	1	1

Table 2: scheme for the dummy variables setting used in the DID framework.

Then we have two groups of variables: the first one is meant to control for factor that are related to fertility, while the last one includes generic controls. The x_i group of variables includes: a dummy variable controlling for the trend which is equal to 1 for people were surveyed in the 1990

²⁰ A further distinction between teachers in elementary, secondary and tertiary education does not improve the estimation results.

census year and equal to 0 otherwise; the share of married woman in the *canton* in each census year;²¹ the share of doctors and nurse in each *canton*, in each census year. This can control for the fact that where the density of doctors is higher information about contraceptive methods are easier to acquire. Then there is the w_i group of variablest: age; a household wealth index built with partial correlation analysis that contains information on the socio-economic conditions of the household and on the household's assets;²² a dummy variable equal to 1 if the woman live in Quito or Guayaquil, the two biggest cities in Ecuador (this controls for the fact that living in a big city can increase educational opportunities)²³.

Summary statistics of all the variables are in Table 1, in the Appendix.

So the aim of this first stage regression is to see the effect of the compulsory school reform on education and this is carried out according different specifications. Since the age of the women in the sample could have an impact on the completion of the highest level of education, thus confounding the effect of the reform, years of schooling are censored at 13 (this correspond to some tertiary education or more). The educational outcome s_i is represented by years of education (censored at 13), by the educational attainment expressed in levels (no schooling, some primary, primary completed, lower secondary, secondary and some tertiary or above), or by the completion of lower secondary school, as follows:

$$s_i = \beta_o + \beta_1 z_i + x_i \beta_2 + w_i \beta_3 + \varepsilon_i \quad (1)$$

where s_i is modeled with a Tobit model, with a multinomial logit and with a probit models respectively. Standard errors are clustered at canton level, allowing for spatial correlation. The error term ε_i is assumed to be independent from the reform, conditional on all the other covariates.

According to the reform setting one should expect a significant effect of the reform on the probability of completing lower secondary education, otherwise the reform impact would be confused with a general expansion of educational attainments. So first is performed a standard least square model in which the years of schooling are regressed against the variables listed before (column 1, Table 2a in the Appendix) and the reform had no effects. In this model two problems are ignored: we are including outliers with high leverage and we are ignoring the fact that the variable is censored. So performing a Tobit model on the same sample (column (2)) we get better results but still the reform is not significant. In the subsequent regression (column (3)) is then reported the results of the Tobit model excluding the outliers.²⁴ Here we see that the reform increased the educational attainment of women for which education is observed in the interval [0,13] by 0,1 years. Also a probit model, that estimate the probability of completing lower secondary school, confirms that the reform was effective in making people staying at school for 9 years (results in column (4)). The reform increased the probability of completing lower secondary school by 1,5%. Note that focusing on the precise target of the reform is a strategy that also was followed by Meghir and Palme (2005), in their study about the effect of a reform in Sweden.

To get further details on the effect of the reform on its specific target, we perform a multinomial logit model on each level, and results are reported in columns (1) to (6) of Table 2b. The result clearly shows that the reform increased only the probability of completing lower secondary education and then, since the marginal effect is equal to 0,0127, that the reform increased

²¹ Controlling for each woman marital status can potentially bias the estimation, since it's not clear if marriage is a consequence of a cause of pregnancy. In this way we have a more indirect measure of marital status. So this variable is meant to capture social norms that influences women's fertility behavior.

²² The variables included here are: the availability of electricity, of sewage system, of water supply, of kitchen, of toilet and number of rooms per person, similarly to what La Ferrara et al. (2008) do. Since these variable are discrete it has been used polychoric partial correlation analysis, following Kolenikov and Angeles (2004).

²³ It would have been better to control for residence in urban/rural area, but unfortunately this information is not available for ther 1982 sample.

²⁴ Only less than 3% of the observations is lost. Further investigations about the possible reasons why those observations should be excluded revealed that for most f them there were errors and mismatches (i.e. years of education that are not comparable with the declared educational attainment, no clear household's structure, etc...)

years of education by 0,114.²⁵ So all the models agree on the sign and on the magnitude of the effect of the reform. The standard assumption of Independence of Irrelevant Alternatives has been tested on the multinomial model with the Small-Hsiao test (Small and Hsiao, 1985) and it holds (see Table 3 in the Appendix).

One possible confounding factor would be if along with the reform other events that influences schooling attendance took place. In Ecuador different military dictatorships had been in power from the beginning of the sixties until 1979, when democratic elections were held, after a period of state-lead modernization and growing foreign debt. After that year governments were unable to promote serious structural reforms, so that poverty and unemployment condition worsened, with increasing popular discontent (The Economist Intelligence Unit, various years). So the political context was unstable, even if it didn't end up in guerrilla or severe violence episodes. According to the information available there is no reason to believe that the end of dictatorship caused an increase in educational attainment, since the country's general conditions worsened. So at most the effect of the reform is downward biased.

Since the next step will be to explore the relationship between education and fertility, an important notice should be made here: to be sure that the reform does not have a direct effect on fertility we check that the reform did not reduce the occurrence of early pregnancies. Even if it's very unlikely that a young girl gets pregnant before the end of lower secondary school, we run a probit regression to check if the probability to have a baby before 15 years old is influenced directly by the reform. The setting used to control for this effect is identical to the one used previously for the schooling equations and results confirm that there is no 'incarceration' effect of the reform on early fertility.²⁶ Moreover none of the z_i variables has a direct impact on early fertility (see results in Table 4, column (1)).

Since the age of first pregnancy that will be modeled in the next section is inferred by the age of the oldest child living with the mother, one concern might be the loss of information about children that had been abandoned or that start their own family soon in the lifetime. It's reassuring that, if education promotes investment and care for children, more educated mothers do separate from their young children less frequently, so that the coefficient of education in the second stage equation will be at most downward bias (if the probability to be abandoned is decreasing with education). Nevertheless it would be better not to compare women aged too differently, since data confirm that the probability to live without a mother is increasing with age (so a 17 years old individual is more likely to live without her own mother than a 12 years old). According to the setting depicted above we would have to compare the age of first pregnancy of a cohort 8 years wide, which might blur the effect of education on teenage motherhood, if children start living alone or even their own family soon in their lifetime. In order to avoid this problem we could exclude older cohorts and just focus on women aged 24, 25, 26 and 27, in the two census years, provided there is no trend going over between the two censuses in both schooling and fertility choices. Inferring the age of first pregnancy by using information about 27 years old women can be done safely, because analyzing average age of children in each household we discover that it drops from 18 years old onward. This is telling that on average children live with parents until they reach age 18 and thus women aged 27 should be still leaving their first born children which can be at most 15 (assuming that pregnancies before age 12 are very unlikely).²⁷ Before doing that we have to check that there is no trend going over between the two censuses.

Education is not increasing in time between the two censuses in every specifications of Tables 2a (see the marginal effect of the variable for the 1990 Census) and in Table 2b time effect is close

²⁵ Since the comparison category is no schooling, the effect of the reform expressed in years is equal to: $0,114 = 9 \cdot 0,0127$. Nine here refers to the years that are necessary to complete lower secondary education.

²⁶ Black et al. (2008) and Krueger and Berthelon (2009) show that spending more time at school reduces the time available to be involved in risky behavior, thus lowering early motherhood rate.

²⁷ The Statistical Office providing the census states that a woman can be considered a mother from the age of 12. So children born before that age are not considered as her own children.

to zero or negative. Splitting the sample into young (24-27 years old) and old women (28-31), we see that time variation occurs only when comparing the two groups of young women (Table 4, column (3)), while there is no increase in educational attainment if one looks only at the comparison between the two groups formed by older women (column (2)).

So, from now on we use only the sample composed by women aged 24-27, because we can reasonably assume that the only thing that affected education was the reform.²⁸ In this way the effect of schooling will be on a same aged group of women, so that the further study on teenage motherhood is not biased because of age.

Results of the Tobit regression on the sample of young women confirm the efficacy of the reform and years of schooling are increased by 2,8% (Column (3) of Table 4).²⁹ So the reform can be used to instrument education in a fertility equation.

2.4.2 The effect of education on teenage fertility

The model that will be presented here will try to analyze the effects of education on each woman's probability to become a teenage mother. The age at which each woman gave birth for the first time is derived from the age of the eldest own child living in the household and it is used to build the binary variable for teenage motherhood. Also Francesconi (2008) and Ribar (1999) use a dichotomous model for teenage fertility, but consider adolescent childbearing until the age of 20. This threshold is not appropriate for a developing country such as Ecuador at the time considered, and in fact in this study, teenage motherhood is defined until 18 years old. So the sample of women is divided into those that become mother before age 18 and those that didn't and so a probit model is considered.

The probability of being a teenage mother T_i is modeled as follows:

$$T_i^* = \beta_1 + \gamma_i + x_i' \beta_2 + w_i' \beta_3 + \eta_i \quad (6)$$

where $T_i = 1$ if $T_i^* > 0$, and $T_i = 0$ otherwise; T_i^* is normalized to 0 without consequences, since the equation includes an intercept.

We run a first regression in which education is not instrumented (see column (1) in Table 5) and the completion of lower secondary education s_i is negatively associated with teenage fertility, as expected. This simple model does not take into account that education might be endogenous, so we run a two-stage fertility equation where education is instrumented with the z_i variables, as explained in the previous paragraph. Even if years of schooling might be a more accurate measure for education, here a binary variable for lower secondary attainment is preferred: first of all because it's the precise target of the reform that is used as instrument, and secondly because it's easier to interpret the results, as years of schooling are censored and a Tobit model in the first stage would be needed. In this last case the computation would be burdensome and results not so easy to interpret, as we would have to consider also the probability of having education censored at 13.

So we run a first stage probit equation for schooling followed by a second stage probit equation for teenage motherhood, according the following framework.

In the first stage equation for schooling s_i is the probability to complete lower secondary education and $s_i = 1$ if $s_i^* > 0$, and $s_i = 0$ otherwise:

$$s_i^* = z_i' \beta_1 + x_i' \beta_2 + w_i' \beta_3 + \varepsilon_i$$

and then we have equation (6) for being a teenage mother as second stage.

Let's define:

$$v_{1i} = z_i' \beta_1 + x_i' \beta_2 + w_i' \beta_3$$

$$v_{2i} = \gamma_i + x_i' \beta_2 + w_i' \beta_3$$

²⁸ The trend is not even significant for fertility decisions once accounted for the effect of education.

²⁹ The partial R^2 of the regression of completion of primary schooling on the instruments is equal to 0,08.

so that the joint error term $v_i = (\varepsilon_i, \eta_i)' \sim N = (0, \Sigma)$, where $\Sigma = \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix}$ and ρ measures the endogeneity of schooling s_i in the fertility equation. If $\rho = 0$ then the relationship is not endogenous.

The resulting likelihood function that has to be maximized can be written as:

$$L_i(\beta_1, \beta_2, \gamma, \rho; s_i, T_i | z_i, x_i, w_i) = \Phi((-\vartheta_{1i}, -\vartheta_{2i})', \Sigma) \quad (7)$$

This function is maximized as a bivariate probit.

Results are reported in Table 5: column (2) shows the marginal effects for the final fertility equation, while in column (3) are reported marginal effects for the first stage schooling equation. Completing lower secondary education decrease the probability of being a teenage mother by 7,07%, meaning that education has a role in postponing early pregnancies.

ρ is statistically significant and negative, meaning that there is a latent factor that increases the probability to complete lower secondary schooling and decreases the probability of being a teenage mother (or viceversa), as expected.³⁰ So this model is appropriate and education can not be considered exogenous with respect to adolescent childbearing.

So the reform considered here, causing an increase in schooling level, decreases the probability of early motherhood. Since having children when adolescent could negatively affect their future development, this policy can be thought as a tool to promote education on the current generation with potential effects on the next one and this aspect will be explored in the last section.

Here one possible confounding factor would be if along with the reform there were significant changes in the availability of contraceptives. According to what we are aware of, we can not assume that there were dramatic changes in the birth controls used by Ecuadorian women caused by exogenous shocks, such as massive family planning campaigns that in fact in those years were reported as ‘weak’; so the use of contraceptives relies on women educational level and not on their availability (Weinberger et al., 1989, and Lapham and Mauldin, 1984). However, this would be properly controlled for if we had information about women family planning choices that instead here we can only assume. Other information such as each woman desired family size or her parental background (in terms of both education and fertility history) would surely increase the power of the model, but the data set available is very limited.

To gain further details into the effects of education on fertility we have to find out the reasons why women decide not to become teenage mothers, including in the analysis the labor market. Since the decision of having a baby and of working can be interdependent, the specification should take into account the simultaneity problem.

2.5 Education, fertility and labor market participation

As previously reviewed, there could be many channels through which education can affect the fertility choices. What we can measure with the available data is whether a more skilled woman participates in the labor market more frequently than a less educated.

So here it will be tested if a shock on the educational system has an effect on both the labor market participation rate and on teenage fertility, taking these decisions as interdependent. Several methods have been used before for this purpose: Ekert-Jaffe and Stier (2009) and Colombino and Di Tommaso (1996) use a bivariate probit model; Di Tommaso and Weeks (2000) and Bratti, (2003) a multinomial; Di Tommaso (1999) a trivariate model.

Alternatively one can instrument fertility in order to establish a causal relationship on female labor supply, as Cruces and Galiani (2007) do for Mexico and Argentina, following what Angrist and Evans (1998) do for US. Agüero and Marks (2008) instead, using as instrument for number of children infertility problems, find no effect of fertility on labor market participation for six Latin

³⁰ Note that if endogeneity problem is not taken into account and a standard probit model is considered (column (1)), the resulting schooling coefficient is bigger. This means that the effect of education would have been overestimated.

American countries. Piras and Ripani (2005) analyze the effect of motherhood on wages and results are mixed, meaning that for some Latin American countries there is a wage premium associated to motherhood. Here we won't test any effect on wages because this information is not available and moreover because we would exclude all the non-working women, which are half of the sample.

In this study the focus will be the role of education in changing women's probability to give birth before age 18 and then both education and teenage motherhood are considered to estimate labor supply. Our main concern is that teen pregnancy and labor market participation are interdependent and that education is potentially endogenous with respect to fertility and to labor supply. So we will use a framework that allows interdependence among the three choices.

Each woman in the sample first decides to complete lower secondary education and we showed that the 1977 law positively affects this educational attainment. So the schooling level, instrumented by the reform, is used in a early fertility equation and found negatively correlated with adolescent motherhood; we interpret this relationship as causal, since education was instrumented with an exogenous shock. In this part we want to explore one of the possible reasons why more educated women do not become mothers when teenager, that is if women with strong labor market preferences invest more in education and less in fertility. Since the data give information about the current employment status and don't contain information on working starting age, we analyze if fertility and education decisions taken in the past influence labor market participation at present. So the labor market participation equation is modeled as follows:

$$L_i^* = \gamma s_i + \delta T_i + w_i \beta_3 + \iota_i = \vartheta_{i3} + \iota_i \quad (8)$$

and $L_i = 1$ if $L_i^* > 0$, and $L_i = 0$ otherwise. The equation for labor market participation includes the dummy for lower secondary attainment (s_i) and the dummy for teenage motherhood (T_i). The x_i group of variables is not included here, since it only controls for characteristics that affect fertility decision, and are exclusion restrictions. So to the bivariate model explained in the previous section is added the labor equation, and the three equations are estimated simultaneously in a trivariate probit model framework. We choose not to estimate separately equation (8) because potentially there could be factors that are correlated with both early fertility and labor market participation, and with education and labor market participation. Since teenage fertility is defined early in the life cycle with respect to participation in the labor market (because of the data available), we choose to consider early fertility in the labor equation and not vice versa. Di Tommaso (1999) uses a trivariate model as well, but she includes a (predicted) wage equation, rather than a schooling equation. Here wages are not available, and the best predictor for wage in this context is in fact education.

We assume that the error terms $\varepsilon_i, \eta_i, \iota_i$ are distributed as a multivariate normal, with mean of zero and a variance-covariance matrix V with unitary variance and symmetric. The likelihood function that is maximized is the following:

$$L_i(\beta_1, \beta_2, \beta_3, \gamma, \delta, \tau; s_i, T_i, L_i | z_i, x_i, w_i) = \Phi((- \vartheta_{1i}, - \vartheta_{2i}, - \vartheta_{3i})', V)$$

$$\text{and the variance-covariance matrix is } V = \begin{bmatrix} 1 & \tau_{12} & \tau_{13} \\ \tau_{21} & 1 & \tau_{23} \\ \tau_{31} & \tau_{32} & 1 \end{bmatrix}, \text{ where } \tau_{jk} = \tau_{kj}.$$

The results of the trivariate model are shown in Table 6 in the Appendix.

Completing lower secondary schooling increases the probability to participate in the labor market by 35% and at the same time decreases the probability to be a teenage mother by 5,3%. The direct effect of teenage fertility on labor supply is negative and equal to 31%. Education has a stronger impact on the decision to participate in the labor market, as it can be seen as a proxy for expected wages.

The effect of education on teenage fertility is less strong than in the previous model, because here also labor market is considered and the two equations are positively related. In fact if we look at the estimated correlations, the *taus*, we see that there is a latent factor not captured by the model

that affects positively labor participation and negatively education; even if it might be counter-intuitive there could be cases of very low income where education is discouraged but at the same time labor supply necessary, and there are surely jobs for which education is not needed at all; or on the contrary high income women are well educated but don't need to work in order to survive. These two effects, also combined, help explaining why the *tau* between education and labor equation is negative. The same reasoning could be followed when interpreting the fact that also there is a latent factor that increases both teen fertility and labor market participation. Low income could prevent women to use birth controls, and at the same could force them to supply labor.³¹ Moreover, women in the sample are 24-27 years old and teenage fertility is limited to having a baby before 18: so teenage fertility cannot rule out completely labor market participation.

The correlation between the errors of the fertility and education equations has instead the expected sign, since education and teen fertility are always negatively correlated. We could not think of a hidden characteristic that could promote early fertility and education. Sometimes the relationship between education and fertility is inversely U-shaped, when for high-income level women fertility rates are high, because they can afford child care services. In this case we are only estimating the probability to become mother before age 18, and education can not be reasonably positively related to adolescent motherhood. Following the interpretation of Walker (1995) there are women that have high discount rate and receive utility from their child so that they have children early in the life time and forgo education. On the other hand, when the intertemporal discount rate is lower, children's 'opportunity cost' is high, so women delay their 'consumption' in order to get educated and participate in the labor market.

To sum up, we see that women with strong labor market preferences tend to invest more in education and less in (early) fertility. We don't know what are the fertility patterns for women when older, it might be the case that completed fertility levels would be the same for more and less educated women. In this context we only observe that the investment in education and not in adolescent motherhood are positively related to labor supply.

We could in principle give more complete results if we could use more detailed information about religion and about the ethnicity of people, but unfortunately those information are not collected. Also it would be relevant to know at what age the women in the sample were conceived, in order to test if there is intergenerational transmission of fertility behavior and if so this could be a stronger instrument for early fertility. In general any information about the original family would help to give more precise estimates, but with census data this is not possible, since the majority of women involved in the analysis are not living with their parents anymore.

Another aspect that is not taken into account is the kind of job: we don't know if the working woman is exploited in some informal labor market or if she has a fair wage. Promoting education because it generates skilled human capital and encourages female labor market participation would be pointless if all better educated women just end up exploited. So this model could be improved in various ways conditional on having more information about each household's characteristics. Anyway, the results obtained here are reasonable and consistent with standard interpretations.

2.6 Mothers' education, fertility and children's outcomes

The first empirical model presented in this work shows how women's educational attainment, increased by a compulsory schooling reform, delays fertility. From a policy perspective it's interesting to see whether these effects on women influence their children's outcomes. So the purpose here is to investigate the intergenerational effect of a postponement of first pregnancy on each woman children's educational attainment. Since universal education is a goal pursued by many

³¹ This interpretation helps explaining why the marginal effects for schooling on fertility is lower than in the previous model, some women even with low education can decide not to have a baby when teenagers because they have to work.

policies, it's relevant to understand if benefits of an increased educational level spill over on future generations.

Various papers in the literature showed the positive effects of avoiding early pregnancies on children education, morbidity and mortality as already reviewed. While for developed countries the evidence is sometimes limited and not significant, for developing countries benefits of parents' educational attainment on children outcomes are less blurred and definitely positive. So campaigns encouraging women's education by the World Bank or the UN for example find their support.³²

In this framework we want to explore if the delaying of early motherhood increases mothers' investment in their offspring, measured by years of schooling. Here we can not properly study the channel through which older mothers make their children attend school more frequently with respect to younger ones, because we don't have any kind of information about mothers' attitude towards their child. As a first attempt, we simply refer to changes in children's education associated with variation in mothers' age at first birth, so to get some insights about the sign of the correlation between mothers' age at birth and children's educational outcomes.

From the 1990 census data we retrieve information on all households' members and we can regress years of schooling of children against their mothers' age when at birth, so to observe if kids born to older mothers have better educational attainment with respect to those born to younger ones. Given that education is increasing with age, the dependent variable considered here refers to an index that counts years of school on schooling age as follows:

$$index_i = \frac{edu_i}{age_i - 6}$$

where edu_i indicates years of education of child i . This index is bounded between 0 and 1, where 1 means that a child has the maximum possible level of schooling given his age.

This model is estimated with ordinary least square and mothers' age at first birth is also interacted with a dummy indicating if the mother is in the labor force or not:

$$index_i = \beta_0 + \sum_{k=13}^K (L_i \times A_{ki}) \beta_{ki} + C_i' \beta + u_i \quad (5)$$

where A_i is mother age at birth, L_i is a dummy variable indicating if each child's mother is in the labor force or not and C_i is a matrix of other controls for child's family characteristics; C_i includes: sex, siblings currently living in the household, siblings younger than 5, number of family members (and its square to capture crowding effects), a dummy that indicates if the father lives in the same household or not, the educational level of the household's head, the number of teachers relative to school age people, a wealth index, a dummy for living in rural or urban area and controls for province of birth.

This first part includes two samples: first we see the effect of mother's age at first birth on the sample of firstborns and then the analysis is extended to the whole sample of children living with the mother. In both cases we include in the sample kids from age 7 to age 17.

So A_i is child i mother's age at birth and in the first estimation (columns 1 and 2 of Table 10 in the Appendix) K is equal to 26. This choice takes into account that mothers aged 34 and over could have children that already left the household, since in Ecuador, as shown at the beginning, on average people leave his parents' house at 18. So until age 33 we can assume that the eldest child in the household is actually the first born. Note that we can count schooling years only of people aged 7 and over (which is exactly why we choose age 26 as the maximum mother's age at first birth). Moreover, we exclude from the sample all the households where the declared number of children and the children that are currently living in the household diverge.³³ The results confirm that when a woman decides to postpone her first birth, there are positive effects on the educational outcome of

³² See also the review by Strauss and Thomas (1995).

³³ Note that even if kids leave their parental home on average when 18, there are cases of younger people living without a mother. This takes also into account children that did not survived, so that we exclude from the sample those households where the following equation is not satisfied: $declared_children = children_counted + children_not_alive$.

the firstborn. This effect reasonably becomes significant if childbearing is delayed from age 17 onwards, while after age 20 it flattens.³⁴ This is also consistent with our model for fertility, since we model the probability to become mother before age 18, considering that after that threshold problems related with teen fertility fade out. The explanation that lies behind this result is that women postpone their first birth so to invest more in their children. But here the two things are not causally linked.

In the second case (see columns 3 and 4 of Table 10 in the Appendix) the same analysis is performed over the full sample children present in the census, so we include all households without caring if there are children that already moved away. So here K is equal to 42, assuming that from 49 years old onwards it's very unlikely to give birth. The pattern of the coefficients attached to mother's age at birth is very similar to the one in the previous 'restricted' sample, with the exception that here there is a decline for the children born after age 30. This slightly fading effect could be explained with the fact that children conceived at that mothers' age are likely not to be firstborns and the presence of siblings might be negatively correlated with educational attainment.³⁵

In each of the two cases, the effect of mothers' age at birth is interacted with labor market participation. While in the first case the coefficient attached to mother's age at first birth is bigger when working mothers' effect is excluded, in the second one it is the opposite, but in the two cases this difference is not significantly different from zero.³⁶ The interpretation lies in the fact that it can be the case that working mothers have less control over their firstborns, with respect to non-working mothers, so that they tend to go to school less with respect to the more controlled ones of non-working mothers; however, when all children are considered probably a wage effect offsets the loss of control that working mothers can provide; we assume that in this full-sample case children of non-working mothers have to provide some income to the family and so that their schooling index is lower. Other explanations could be possible but we don't have enough information to provide more details and moreover the difference between working and non-working mothers is small and not significant. The graphs below (Figures 4 and 5) represent the coefficients of mother age at birth obtained by the estimation (complete results in the Appendix).

³⁴ The sudden drop at age 26 is not statistically significant and caused by the fact this age group is too small.

³⁵ 80% of the children born after age 30 have 2 siblings or more.

³⁶ All the coefficients of the interaction between age and labor market participation are positive but not statistically different from zero.

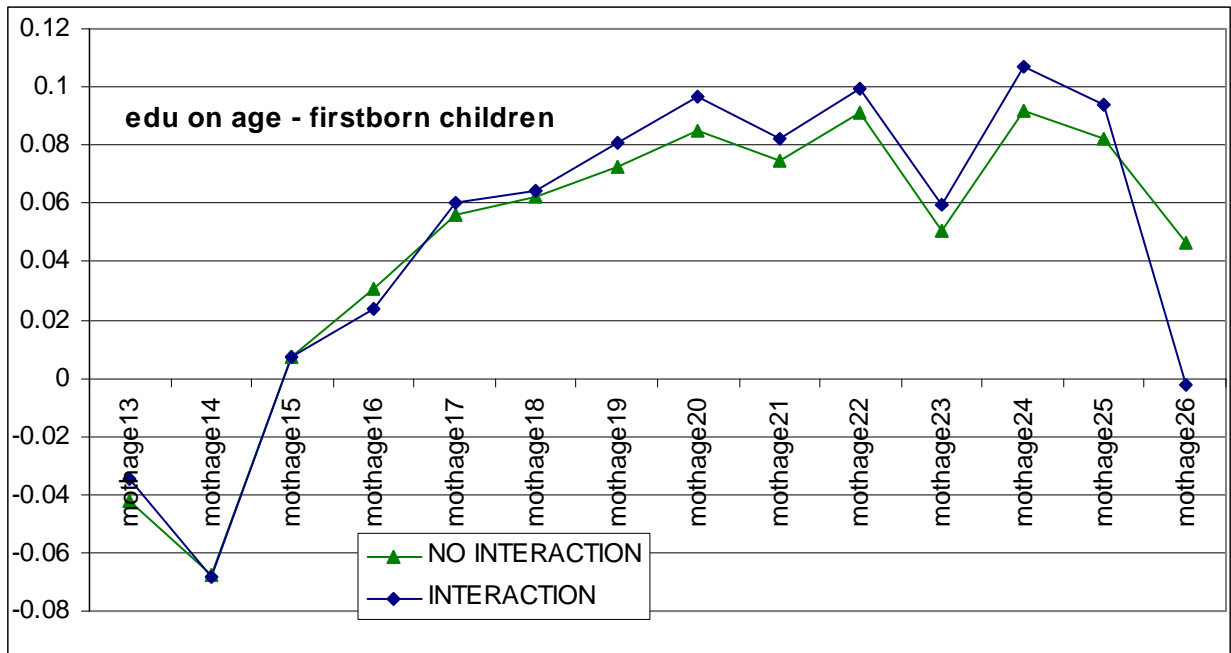


Figure 1 Estimated coefficients of mothers' age at birth on firstborns education index

The other variables included in the estimation have the expected sign, with the exception of the presence of the father that is always positive by never statistically significant. That probably is due to the fact the fathers are present in the household in most cases and moreover their effect is already captured by their educational level variable.

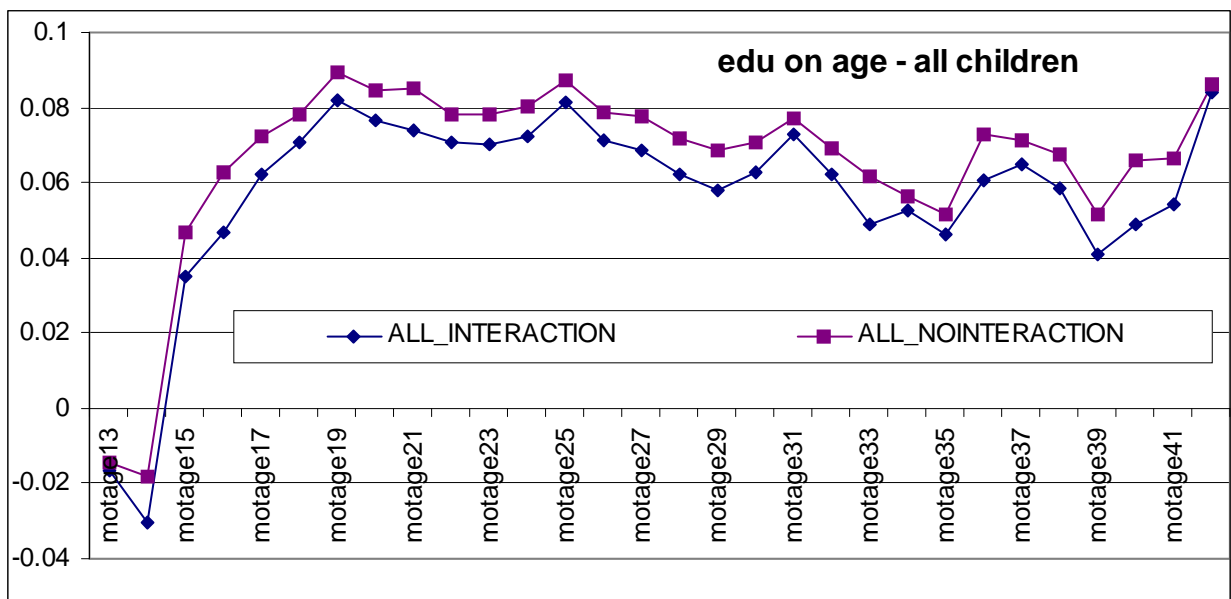


Figure 2 Estimated coefficients of mothers' age at birth on children's education index

This set of results confirms that there are positive effects on children outcomes if motherhood is shifted away from early adolescence: in fact in both cases the coefficient attached to age at first birth becomes significant after age 17 and grows until age 20. So for example giving birth at age 20 rather than at age 12 increases children educational level by 10%, no matter their current grade. This suggests that avoiding early childbearing is an important policy aim in order to promote children enrollment and literacy. Of course other information are needed if we want to generalize

this result to long run effects of teenage motherhood, such as the completed educational attainment of teenage mothers' children when adult.

2.7 Conclusions

This work sheds some light on the effects of a reform of mandatory education on women fertility choices, labor market participation rates and their children outcomes. Education is found to be endogenous with respect to fertility behavior and so it has been instrumented with a reform that increased women's probability of completing lower secondary schooling.

The results confirm that completion of lower secondary schooling affects women's risk of being teenage mothers and labor supply. Age at first birth is delayed after adolescence for more educated women and to older mothers are associated better children outcomes, in terms of schooling. However, further evidence is needed in order to estimate precisely the size and the duration of the intergenerational effects.

In a model where the decision of working is considered jointly with fertility choices, we found women that want to be in the labor force invest more in education and less in fertility. But this is only a possible explanation for changes in fertility behavior, because as pointed out by the literature, the effect of education on fertility doesn't shape only labor market choices (care for children, education, changes in aspirations, and so on).

So from a policy perspective it's evident that a reform on compulsory education is a good tool not only to promote schooling attendance itself, but also to increase women control over family planning choices and participation in the labor market. This is particularly relevant in a country where gender differences in both the labor market and educational attainment are pronounced, and where there is evidence that children enrollment is not yet satisfactory and their exploitation in the labor market still high.

2.8 Appendix

Variable:	Obs	Mean	Std. Dev.	Min	Max
Share of teachers per 100 pop	51404	2.914	1.188	.1422	10.57
Reform	51404	.309	.462	0	1
Average years of sch of people >28, by census year, by canton	51404	5.87	2.06	.9049	8.906
Age	51404	27.27	2.25	24	31
Census 1990	51404	.583	.494	0	1
Share of doctors and nurse by 1000 people	51404	3.835	2.69	0	14.19
Wealth index	51404	.274	1.372	-2.438	9.22
Share of married women by canton, by census year	51404	72.67	10.01	25	100
bigcity	51404	.372	.483	0	1

Table 1 Summary statistics of the variables

	(1)	(2)	(3)	(4)
<i>Model</i>	OLS	TOBIT	TOBIT	PROBIT
<i>Dependent variable</i>	Years of schooling	Years of schooling	Years of schooling	Lower secondary
Share of teachers per 1000 pop	0.149***	0.210***	0.216***	0.0227***
Reform	0.0731	0.0917	0.104*	0.0158*
Average years of sch of people >28, by census year, by canton	0.197***	0.176***	0.178***	0.0148***
Age	-0.118***	-0.128***	-0.123***	-0.0134***
Census 1990	0.123	0.177	0.142	0.0654
Share of doctors and nurse by 1000 people	-0.0632***	-0.0717***	-0.0796***	-0.00236
Wealth index	1.684***	1.906***	1.918***	0.225***
Share of married women by canton, by census year	-0.00478*	-0.00550**	-0.00615**	-0.000681
bigcity	0.0651	0.225*	0.229*	0.0153
Province of birth	yes	yes	yes	yes
Sigma		3.720***	3.733***	
Observations	51404	51404	49928	49928
R-squared	0.422	.	.	
Pseudo R-sq		0.0964	0.0959	

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

Table 2a Schooling equation: OLS, Tobit and probit models.

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Model</i>	MULTINOMIAL LOGIT MODEL: Marginal effects					
<i>Dependent variable</i>	no schooling	some prim	primary	lower sec	secondary	some tert or above
Share of teachers per 1000 pop	0.00393*	-0.0112***	-0.0122***	-0.000830	0.00793***	0.0124***
Reform	-0.00384	-0.00298	-0.0153*	0.0127***	0.00948	-0.00011
Average years of sch of people >28, by census year, by canton	-0.0179***	-0.00507	0.00758	0.0143***	-0.00541*	0.00653**
Age	0.0053***	0.0114***	-0.00322*	-0.00557***	-0.00440***	-0.004***
Census 1990	0.00957	-0.0793***	-0.000798	0.0115	0.0002**	0.0007*
Share of doctors and nurse by 1000 people	0.00364***	-0.00275	9.70e-05	0.00155	-0.00271	0.000173
Wealth index	-0.0554***	-0.110***	-0.0625***	0.0495***	0.0762***	0.103***
Share of married women by canton, by census year	0.000104	0.000621***	-2.32e-05	0.000253	-0.000144	-0.001***
bigcity	0.0209**	-0.0173*	-0.0167	-0.0127*	0.0252*	0.000625
Province of birth	yes	yes	yes	yes	yes	yes
Sigma						
Observations	49928	49928	49928	49928	49928	49928
R-squared						
Pseudo R-sq	0.1627	0.1627	0.1627	0.1627	0.1627	0.1627

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

Table 2b Schooling equation: marginal effects of the multinomial logit model.

Small-Hsiao tests of IIA assumption						
Ho: Odds(Outcome-J vs Outcome-K) are independent of other alternatives						
Omitted	lnL(full)	lnL(omit)	chi2	df	P>chi2	evidence
some_primary	-2.57e+04	-2.57e+04	106.082	96	0.226	for Ho
Primary	-2.17e+04	-2.17e+04	110.459	96	0.149	for Ho
lower_secondary	-2.70e+04	-2.70e+04	97.439	96	0.440	for Ho
Secondary	-2.82e+04	-2.82e+04	91.591	96	0.608	for Ho
some_tert or above	-2.71e+04	-2.71e+04	92.983	96	0.568	for Ho

Table 3 Small-Hsiao test for IIA assumption

<i>MODEL:</i>	(1) PROBIT	(2) TOBIT	(3) TOBIT
<i>TEST:</i>	Prob of being mother before the end of lower sec	Trend on the OLD group	Reform on the YOUNG group
Years of schooling	-0.00177***		
Share of teachers per 100 pop	-0.000674	0.163***	0.261***
Reform	-0.00119		0.286*
Average years of sch of people >28, by census year, by canton	0.000835	0.248***	0.111*
Age	-0.000359	-0.140***	-0.0368
Census 1990	0.00472**	0.100	
Share of doctors and nurse by 1000 people	0.000333	-0.0850***	-0.0729***
Wealth index	-0.00138***	1.938***	1.900***
Share of married women by canton, by census year	0.000133**	-0.000899	-0.0124***
bigcity	-0.00503**	0.233	0.215
Sigma		3.693***	3.763***
Observations	49928	23135	26793

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

Table 4 Different checks on schooling and fertility. The first model is meant to exclude the possibility that the reform affects directly fertility before age 15, while the other two columns present the schooling equation for the sample of young (aged 24-27) women and the sample of old (aged 28-31) separately.

<i>MODEL:</i>	(1)	(2)	(3)
<i>DEPENDENT VARIABLE:</i>	probit (MFX)	iv probit (MFX): second stage	iv probit (MFX): first stage
	BEING A MOTHER BEFORE 18	BEING A MOTHER BEFORE 18	PROB OF COMPLETING LOWER SEC
Share of teachers per 100 pop			0.0292***
Reform			0.0931***
Average years of sch of people >28, by census year, by canton			0.00655
Completion of lower secondary school	-0.104***	-0.0707***	
Age	-0.00288*	-0.00280*	-0.00605
Share of doctors and nurse by 1000 people	-0.00302***	-0.00262***	-0.000642
Wealth index	-0.0151***	-0.0215***	0.225***
Share of married women by canton, by census year	0.000975***	0.00103***	-0.00108**
bigcity	0.00563	0.00342	0.0130
Province of birth	yes	yes	yes
rho_12		-0.12***	
Observations	26793	26793	26793

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

Table 5 Marginal effects for the fertility equations: standard probit model and IV estimation.

	(1)	(2)	(3)	(4)	(5)
EQUATIONS:	LAB MKT PART	TEENAGE MOTHER	LOWER SEC	mfx_LAB EQUAT.	mfx_FERT EQ
REGRESSORS:					
Lower Secondary	0.949***	-0.290**		0.350***	-0.0529**
Teen fertility	-1.070***			-0.312***	
Share of teachers per 1000 pop			0.0779***		
Reform			0.241***		
Average years of sch of people older than 28, by census year, by canton			0.0144		
Share of doctors and nurse by 1000 people		-0.0113*	-0.000895		-0.00212*
Share of married women by canton, by census year		0.00755***	-0.00404***		0.00141***
Age	-0.0184**	-0.0213**	-0.0114	-0.00688**	-0.00398**
Wealth index	0.0967***	-0.129***	0.587***	0.0361***	-0.0240***
bigcity	0.185***	0.0127	0.0298	0.0696***	0.00238
Province of birth	yes	yes	yes	yes	yes
tau_12	0.527**	0.527**			
tau_13	-0.273***		-0.273***		
tau_23		-0.183**	-0.183**		
Observations	26793	26793	26793	26793	26793

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

Table 6: Trivariate probit model for education, fertility and labor market participation

	(1)	(2)	(3)	(4)
SAMPLE	education on age firstborns	education on age firstborns	education on age all children	education on age all children
moth age at birth 13	-0.0348	-0.0424	-0.0164	-0.0143
moth age at birth 14	-0.0683*	-0.0669**	-0.0304	-0.0183
moth age at birth 15	0.00719	0.00731	0.0351	0.0469**
moth age at birth 16	0.0236	0.0303	0.0467*	0.0628***
moth age at birth 17	0.0604*	0.0555**	0.0624**	0.0723***
moth age at birth 18	0.0647**	0.0621**	0.0708***	0.0783***
moth age at birth 19	0.0811**	0.0727***	0.0819***	0.0891***
moth age at birth 20	0.0964***	0.0846***	0.0764***	0.0848***
moth age at birth 21	0.0820**	0.0744***	0.0740***	0.0850***
moth age at birth 22	0.0993***	0.0913***	0.0705***	0.0781***
moth age at birth 23	0.0593*	0.0508*	0.0701***	0.0784***
moth age at birth 24	0.107**	0.0922***	0.0725***	0.0802***
moth age at birth 25	0.0936**	0.0822***	0.0812***	0.0871***
moth age at birth 26	-0.00226	0.0457	0.0711***	0.0788***
moth age at birth 27			0.0686***	0.0777***
moth age at birth 28			0.0624**	0.0718***
moth age at birth 29			0.0582**	0.0688***
moth age at birth 30			0.0629**	0.0706***
moth age at birth 31			0.0726***	0.0773***
moth age at birth 32			0.0622**	0.0691***
moth age at birth 33			0.0491*	0.0615***
moth age at birth 34			0.0528**	0.0561***
moth age at birth 35			0.0460*	0.0517**
moth age at birth 36			0.0606**	0.0726***
moth age at birth 37			0.0649***	0.0711***
moth age at birth 38			0.0586**	0.0673***
moth age at birth 39			0.0409	0.0514**
moth age at birth 40			0.0488*	0.0657***
labforce	0.00153	-0.0163**	-0.0422	-0.0131***
mothage_j*lab force	yes	no	yes	no
sex	not significant		not significant	
Num sib	0.0115**	0.0115**	0.0164***	0.0164***
Baby sib	-0.0130***	-0.0130***	-0.0117***	-0.0118***
Fam size	-0.0232***	-0.0232***	-0.0137***	-0.0137***
sqfamsize	0.00892	0.00912	0.00928***	0.00937***
Father in hh	-0.000458	-0.000468	-0.000487***	-0.000489***
Edu head HH	0.00872	0.00824	0.000490	0.000386
Share of teachers	0.00888***	0.00882***	0.0110***	0.0110***
Wealth index	0.00575**	0.00588**	0.00885***	0.00887***
urban	0.0318***	0.0316***	0.0422***	0.0422***
Province of birth	0.0127	0.0127	0.0152**	0.0152**
Observations	yes	yes	yse	yes
R-squared	12830	12830	97916	97916
Pseudo R-squared	0.145	0.144	0.173	0.173

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

Table 7 Intergenerational effects: columns (1) –(4) mother age at birth on children education.

3 The demand for skilled workers after the “3+2” university reform

3.1 Abstract

Based on a survey on employers' preferences over prospective workers hiring practices, this work focuses on the variations in the demand for skilled labor force after a reform of the Italian University system. This reform caused an increase in tertiary education attendance and a reduction in drop out rates. Whether graduates are more likely to be hired after the increase in the supply of skilled human capital or not is investigated, paying attention to the kind of occupation for which they are required. Since the reform modified both the length of degrees and course contents, this work also attempts to study employers' choice between the BA and the MA graduates.

Results show that the demand for skilled workers increased and this is mainly driven by the fact that employers are substituting less skilled workers with more skilled ones for clerical and technical occupations.

Factors that could have altered post-reform hiring decisions are presented and discussed and evidence suggests that they should not lead to overestimate the effect of the University reform.

3.2 Introduction

In 2001 the Italian university system underwent a radical reform that modified the length of studies as part of the Bologna process, which is aimed at creating a common and integrate European educational system.³⁷ Before 2001 students attended a unitary system that lasted four to five years, while with the reform a two-tier system is introduced.³⁸ A two-years 'master' degree now follows a three-years 'basic' one, and students can decide whether to take only the BA degree or to continue for five years. For those that decide to take only the BA (three years), the reform basically implied a reduction in the length of studies; so on one side we should be observing higher enrolment (because of lowered opportunity cost), lower drop out rates and shorter graduation times; on the other it's not clear how the new 'regime' met labor market needs in terms of skills and competencies.

Up to now most of the scholars analyzed the reform effects looking at the supply side of the labor market. Findings confirm the efficacy of the reform in terms of increasing access, lowering drop out and decreasing age at degree (see among others the work by Cappellari and Lucifora, 2009).

This paper instead is focused on the hiring choices of employers before and after the reform and investigates how workers' required characteristics changed. This is one of the first attempts of assessing the effects of the Italian University system and it is based directly on employers hiring

³⁷ The reform also introduced the credit system and gave each university the right to choose the curricula for the courses offered. Credits are given to students according to the specific workload associated to each subject in the curricula.

³⁸ Note that in Italy 'actual' time to degree differed a lot from the legal length of university courses. On average students graduate at 27 (see for example Moscati, 2000), so that even if the pre and post reform system should be diverging only by one year, the shortening in graduation times caused by reform could be even larger.

plans. From a policy perspective it's relevant to explore labor market outcomes of such a drastic institutional change.

The analysis makes use of six waves of Excelsior survey, collected by *Unioncamere* (Association of Italian Chambers of Commerce), which reports for each firm a) the required characteristics that future employees should have, in terms of skills, experience and age, b) the profession for which each worker is required and her type of employment contract and c) the general characteristics of the firm, such as dimension, industry, geographical area and whether it exports. So we will first explore whether the reform increased the probability that a graduate is hired, focusing on the jobs for which graduates are more required. Then only looking at the post reform period, the circumstances under which a BA graduate is preferred over a MA graduate are taken into account. In this way the kind of job and firms' characteristics that are best fitted by the new types of degrees are explored.

In the following section we will explore in more details the university reform that was implemented. Then we review the existing literature about this reform effects and in general about the Bologna Process in other countries. Next we present the data and the different models that explore reform's effects on employers' choice. A discussion on potential confounding factors follows and the last section concludes.

3.3 Institutional features of the university reform

In this section we will briefly describe the changes in the university system to which Italy had been exposed.

Before the reform, Italy was characterized by a unitary system of tertiary education. Students after secondary school could either enroll in university courses that should last from four to six years, or in another kind of short ones called 'diploma', that lasted only for three years. This last option was scarcely diffused, and most of the students attended a standard four years university course.³⁹ Note also that in Italy getting the degree is of primary importance, because it has a legal value (which means that a degree is often a prerequisite to enter public sector and all regulated jobs).⁴⁰ Under the previous regime, the Italian university system was characterized by several problems: low enrollment, high drop out rates and selectivity in the access by parental background, long lasting graduation times and rigidity of the curricula.

The 2001 reform meant both to tackle the aforementioned problems and to uniform Italian tertiary education system to the European one and preceded along two lines: it changed the curricula (simplifying course contents and creating new degrees of study) and it introduced a two levels system that replaced the unitary old 'regime'. Now a BA degree lasting three years forms the first step of tertiary education. It should provide students with general knowledge of scientific principles, but also with more practical skills (a training period is compulsory in many courses). After that, students can decide either to join the labor market or to attend two more years of university, thus getting a MA equivalent degree, during which they get advanced education and specialize. So basically legislators made easier the access to university and its completion, modifying both the contents and the legal duration of the BA level. Those that stop after three years get a lower qualification than those that completed the 'old' one tier system, but this should be compensated by the fact that they are younger and already with some labor market experience, as training became compulsory in most of the courses.

³⁹ This type of 'diploma' degree was implemented only in 1996 and so few students have had the chance to enroll by the time the reform of the university system was passed and the diploma was substituted by the BA level degree. Probably a few more years would have been needed in order for the 'diploma' to become a frequently required option.

⁴⁰ Many occupations in Italy are regulated: prospective architects, doctors, lawyers among others after the degree should pass a State exam in order to be fully entitled to practice.

Data collected by CNVSU (National Committee for Evaluating the University System) in Table 1 show that the total number of graduates increased in the post reform period. Before the reform the share of those that falls in the category BA degree/‘diploma’ is small, especially in the southern areas.

Total number of graduates, by type of degree 2000-2002					Total number of graduates, by type of degree 2006-2008				
	<i>old regime</i>	<i>BA - diploma</i>	<i>MA</i>	<i>tot</i>		<i>old regime</i>	<i>BA - diploma</i>	<i>MA</i>	<i>tot</i>
<i>north west</i>	113708	19426	0	133134	<i>north west</i>	38012	127316	47627	212955
<i>north east</i>	98051	16672	0	114723	<i>north east</i>	39472	100075	30988	170535
<i>centre</i>	117186	20518	0	137704	<i>centre</i>	61437	137191	36396	235024
<i>south</i>	134277	14480	0	148757	<i>south</i>	98978	146609	29415	275002
Share of graduates, by type of degree					Share of graduates, by type of degree				
<i>north west</i>	0.854	0.146	0	1	<i>north west</i>	0.178	0.598	0.224	1
<i>north east</i>	0.855	0.145	0	1	<i>north east</i>	0.231	0.587	0.182	1
<i>centre</i>	0.851	0.149	0	1	<i>centre</i>	0.261	0.584	0.155	1
<i>south</i>	0.903	0.097	0	1	<i>south</i>	0.360	0.533	0.107	1

Table 1 Graduates by type of degree before and after the reform. Old regime refers to the unitary tier old system graduates and to graduates from the courses whose length was not modified; BA degree or the old diploma refers either to the new ‘short’ degree or the ‘old’ diploma; MA degree includes those with the 5 years degree under the new regime. Source: CNVSU, Annual report on graduates, various years.

The group that refers to those with the BA degree and to the diploma includes, also in the pre reform period, those that were enrolled in the old ‘regime’ but decided to pass on to the new one, so that even in 2002 there are students that got the new 3 years BA degree. In most of the cases, those are students that were a way older than they should be in order to graduate on time and that decided for the ‘short’ degree in order to graduate faster. Remember that in Italy under the old regime people took on average 3 more years to graduate with respect to the legal duration of university. The group with a BA degree becomes consistent in the post reform era, even if there are still (late) graduates from the old unitary system and from those courses whose length was not modified by the reform. In fact, the reform didn’t change the duration and the structure of some courses and so we put graduates from those courses in the ‘old regime’ category.⁴¹ Those that took the MA degree are less than 20% in all areas except in the northwest. So according to this picture, on one side this reform played a role in increasing the supply of skilled human capital, but on the other almost 60% of the new graduates attended a shorter course than before, while less than 20% complete the new MA degree. It’s crucial to mention though that we don’t know if some of those with a ‘short’ degree will specialize in the future, so what we see here is just the number of graduates at the time when the employers, in the surveys focus of our analysis, have to make their choices.

	2000-2002	2006-2008
Graduates on time	67705	285116
Total number of graduates	534309	893516
Share of graduates on time on total number of grads	0,13	0,32

Table 2 Share of graduates that got their degree on time, according to the legal duration of their university course. Source: CNVSU, Annual report on graduates, various years.

⁴¹ The degrees whose length was not modified are: medicine, dentistry, construction engineering, pharmacology and law.

It's also worth noting that the share of those that graduated respecting the legal duration of their course (with respect to total number of graduates) shows a sharp increase in the post reform period: from 13% in the pre reform era to 32% (see Table 2 above); so after the reform not only the total number of graduates increased, but also graduation times clearly shortened. As a result the skilled supply of labor is larger and younger than before the reform.

According to the evidence just presented that documents a relevant shift in the skill composition of the labor force it is important to analyze what are labor market outcomes of the reform.

3.4 The literature

The existing literature about the 2001 Italian university reform is still limited, especially if one is interested to labor market outcomes of graduates from the new 'regime'.

Studies on tertiary education in other countries find that family income affects directly the decision to enroll in a tertiary education program. Carneiro and Heckman (2002) focus on the effects of credit constraint on higher education enrolment in the US, showing that financial limitations are a relevant matter. Vandenberghe (2007) explores the relationship between income and tertiary education participation in some European countries with an IV estimation and finds that there is a causal positive effect of income on higher education enrollment, with the exception of Belgium and Germany. In a paper about Britain, Blanden et al. (2004) find that family income has a causal impact on the decision to enroll in college. So those works agree on the possibility that whenever costs, both direct or indirect, are removed or lowered, there is an increase in access to higher educational systems.

The Italian tertiary education is analyzed by Cappellari and Lucifora (2009); they argue that the reform had a relevant impact on the likelihood to enroll of those students that come from unfavorable parental background, so that it can be considered as a process that removed at least some inequality in higher education attendance. It also reduced drop-out, even if with respect to this facet the effect is more limited. Di Pietro and Cutillo (2008) also point that the reform triggered a change in student behavior that caused a decrease in the drop-out ratio. The fall in drop-out is not caused by a change in students' characteristics, but by a change in their attitude and motivation. This is consistent with the finding that the reform favored those coming from poor parental background whose drop-out 'attitude' would have been probably higher. So the effect of the reform had been twofold: it increased access and motivation.

Bratti et al. (2010) in a paper about the Economics faculty at Marche Polytechnic University find out that the reform, reducing students' workload, increases the likelihood of passing exams. The paper suggests two possible explanations: either professors changed their 'grading standards', or university became more effective. Even if it can't be proved which one of these two hypothesis holds, there is evidence indicating that the reform caused grade inflation, so that it became less costly to get a degree.⁴² Boero et al. (2005), using a data set based upon administrative archives of the universities of Cagliari and Viterbo, analyze post-reform students' outcomes, in terms of progression and retentions. They find that gender, parental background and prior educational performance still play a significant role for progression and drop out. Despite the reform, withdrawal and age at degree remain high, even if they are lowered under the new regime; so they argue that other major reform are needed in order to improve Italian university system. By the way, the reader should keep in mind that those works are case studies, as they refer to specific universities that are not representative of the national university system.

So, in Italy, on one side the reform had an impact in promoting the access to higher education among the least advantaged, potentially favoring social mobility in a country where instead

⁴² By the way, we should keep in mind that this work is a case study and takes into account only the first year of a specific faculty, so we don't know if their argument can be generalized to the whole university system.

intergenerational mobility in terms of educational level is low, as pointed out by Checchi et al. (1999); on the other, this result has been achieved probably lowering quality so that exams are failed less frequently because professors decreased the required standards. This explains the observed decrease in the age at degree and in drop out. So consistently with other works on tertiary education, when costs of education are lowered, access increases.

It's not clear then what could be the effects of such a reform once students join the labor market, because the share of graduates in the labor force increased, but at the same time it seems that there is a decrease in the skill content of the new regime's degree.

Since other countries in Europe went through the same reform, there are attempts of assessing its effects; since the implementation is very recent the analysis is still limited. For Germany Horstschräer and Sprietsma (2010) instead find no significant effect of the Bologna process in terms of both enrollment and drop out ratio. Portela et al. (2009) analyze the effect of the Bologna Process in Portugal, where the implementation of the reform varied over institutions and curricula. They show that the demand for academic programs whose duration was reduced by the reform is in fact increased. Along with this result, their evidence suggests also that the reformed curricula are 'second best' choices and not first best.

The demand side had been never studied up to now with few exceptions, because the first new 'regime' graduates wave entered the labor market just in 2004. According a CNVSU report (2009) on Italian university system, the share of new 'regime' graduates became higher than that of the old 'regime' only from 2006, meaning that there were transition years during which the supply of young skilled human capital was composed by the two types of graduates.⁴³

Bosio and Leonardi (2010) look at the effect of the university reform on the labor market, using administrative data on the new 'regime' graduates and on the number of new courses opened by university, in order to proxy for the variation in the supply of skilled human capital. Their estimation takes also into account the timing of labor market reforms that are a proxy for variation in the demand for graduates. They find that there was an increase in the number of graduates hired, but a decrease in college wage premium and in the quality of the contracts (fixed term are used more extensively among graduates than before).

Villosio (2010) interviewed 226 firms' personnel managers about their opinions over the graduates from the new regime. She mainly focused about how workers with short and long degrees met market needs, in terms of skills acquired. Her results point out that the reform didn't succeed in creating flexible workers and that both BA and MA graduates are hired. With the exception of some sector in which MA graduates are preferred over the less specialized ones, the difference between the two types is not so sharp. She concludes saying that a BA graduate with two years experience is equivalent to a graduate with a MA degree.

In the next section we present the dataset that is used for assessing how the demand for graduates varied after the university reform.

3.5 Data description

In this analysis we use six waves of the Excelsior survey in order to determine the demand for skilled workers. Data taken from Excelsior have also been used by economists in other studies (Cappellari et al. 2010, Campa et al. 2009, Colombo and Stanca, 2008), but never for this purpose.

Every year the Association of Italian Chambers of Commerce by mean of that survey asks firms about the characteristics of future employees. The Excelsior survey is included in the official statistics produced on an annual basis within the Italian National Statistical System (SISTAN).

⁴³ CNVSU is the national center for university system evaluation and provides yearly detailed statistics and analysis on the characteristics of the Italian high educational system and depends directly from the Italian Ministry of Education and Research.

This survey is supported and promoted by the Labor Ministry and it's mandatory. The firms involved are all those with more than one employee and that are registered at the local Chambers of Commerce. Public enterprises, such as schools, universities and hospitals, are excluded, while professional offices, like those of lawyers and architects, are included. When information is missing or incomplete, it is integrated from sources other than the survey (National Institute of Social Security - INPS, Italian Workers' Compensation Authority – INAIL, etc...). After the information about all the firms is collected, a representative sample is drawn to form the Excelsior survey we use here.⁴⁴ So by mean of this survey we can estimate the expected labor demand for the current year (interviews are made each January and ask about intentions for the same year).

The pre reform period refers to 2001, 2002 and 2003, while the post reform to 2007, 2008 and 2009 and the same firms that answered the survey at least in one year in the pre reform period were interviewed again after the reform (please keep in mind that not all firms answered the survey in each year in each sub-period, if no new workers are wanted, see the Table 3). If they don't plan to hire anyone in any of the years after the reform, then they are recorded only in the pre reform era and, vice versa, they might be listed in the post reform period but not in the pre reform for the same reason. An analogous reasoning holds for firms that exit the market or for new entrants. So for 19620 private firms we can determine the demand for graduates before and after the reform (and if we include also those firms that are recorded in one period only, the number raises to 29378 and 30910 in the pre and post reform period respectively, see Tables 4 and 5 for further details).

Number of firms that are in:	2001-2003			2007-2009		
	<i>Freq.</i>	<i>Percent</i>	<i>Cum.</i>	<i>Freq.</i>	<i>Percent</i>	<i>Cum.</i>
<i>1 survey out of 3</i>	9165	31.2	31.2	6151	19.9	19.9
<i>2 surveys out of 3</i>	9129	31.07	62.27	8531	27.6	47.5
<i>3 surveys out of 3</i>	11084	37.73	100	16228	52.5	100
<i>Total</i>	29378	100		30910	100	

Table 3 Firms divided according to the number of surveys they are in, by each sub-period.

We don't have information on hiring practices for each single year, but we consider each sub-period as one, so we don't know if the information referred to the pre reform period is referred to 2001, 2002 or 2003. Anyway we know in what years each firm answered the questionnaire. The years between 2003 and 2006 have been excluded, because in that period the share of graduates from the old 'regime' was still greater than that from the new one and it would not be possible to assert anything about the effect of the reform.

Each questionnaire reports information on prospective workers and on the firm itself, and for every type of worker the required characteristics are listed, so that every cell in the data set corresponds to a worker.

The set of variables collected on the prospective workers are: the required educational level (lower secondary education, vocational training, secondary schooling, university) and the specific field, experience, training, sex and age (if lower than 25 or not). The questionnaire also specifies if a foreign language and IT knowledge are needed. For every worker the employer has to detail the type of contract, if permanent or temporary, and the type of occupation to which she is assigned.⁴⁵

The person that fills in the survey is also asked to give some details about the firm: number of employees and the expected turnover, specific industry, geographical area (North-east, north-west, center and south) and whether the firm exports (we only know if it does or not).⁴⁶

⁴⁴ Further details about how the representative sample is build can be found at:

<http://excelsior.unioncamere.net/web/notametodologica3.php> and in the following web pages about methodology.

⁴⁵ Occupation is classified following Istat-ISCO categories and in the analysis we will mainly refer to the 1-digit and 2-digits classifications.

⁴⁶ Industries are classified following ATECO 2002 categories, provided by the National Statistical Office.

Time	<i>Nr of required workers</i>	<i>Av nr workers,by firm</i>	<i>Nr of firms requiring workers</i>	<i>Nr of required grads</i>	<i>Av nr grads,by firm</i>	<i>Nr of firms requiring grads</i>
<i>All firms ('unbalanced')</i>						
2001-2002	623358	21.2	29378	72270	15.5	4657
2007-2009	675258	21.8	30910	101163	13.7	7358
<i>Total</i>	<i>1298616</i>		<i>60288</i>	<i>173433</i>		<i>12015</i>
<i>Firms in both time periods ('balanced')</i>						
2001-2002	585741	29.9	19620	69460	16.9	4105
2007-2009	619489	31.6	19620	96751	15.9	6074
<i>Total</i>	<i>1205230</i>		<i>39240</i>	<i>166211</i>		<i>10179</i>

Table 4 Number of firms in each period and number of required workers, total and graduates.

Some descriptive statistics are in Table 4 and report the number of new workers required by the firms in the sample, while the full set of variables is described in Table 8 in the Appendix. So in the first sub-period firms were looking for 623358 new workers (12% were graduates, at least with the diploma), while in the post reform period workers were 675258 (15% of graduates). While the average required number of workers by each firm increased over time, the average number of graduates required by firms is lower in the post reform era, meaning that there are more firms hiring graduates. This is telling that there were firms that before the reform didn't hire graduates and that start doing it after the reform.

From Table 8 in the Appendix, we notice that people younger than 25 are less required after the reform and this could be explained by the fact that workers have to be more educated, but also with more working experience (the share of required workers without experience drop from 46% to 32%) and training (the share of workers with training increases from 69% to 85%).

Unfortunately we don't have information about firms that don't hire in those years or that only fire. We don't have information about firms' innovativeness for the pre reform period, so we can't control if the demand for graduates is driven by firms that innovate or correspond to a general expansion in the educational level of each firms' human capital.⁴⁷

In order to determine the reform's effects, the 'ideal' data set should allow us to keep track of the individuals' placement both during university and afterwards. Alternatively we should have very detailed information about whom firms hire with respect to age, place of birth, current and past address (in the school years), name of the university that provided education and type of contract and wages. The ideal data set should take into account the spatial variation of the reforms and should cover a time span long enough to control for a general underlying trend.

In this way we could find out the precise extent to which the university reform impacts the demand for highly educated human capital, controlling also for the territorial variation in the implementation of the university and employment deregulation reforms. Unfortunately no such a data source exists, but we'll analyze the available data in order to retrieve from them all the possible information.

3.6 Models and results

⁴⁷ Firms are asked whether they produce goods/services new to the market only in the post reform period, so they are not comparable over time with respect to this issue.

3.6.1 *The before and after comparison in a probit model*

In this section we will present the results meant to find out if there are significant differences between the two periods that could be attributed to the ‘3+2’ reform.

First of all we will consider how the demand for skilled workers varies, taking the two periods separately (columns 1,2 of Table 9 in the Appendix). The outcome is a binary variable that is equal to 1 if the prospective worker is a graduate and 0 otherwise. In order to distinguish if the graduate got a BA degree (that is compared with the diploma in the first period) or a MA degree (5 years under the new system, standard degree in the old unitary system) we weight the each request by $\frac{3}{4}$ and $\frac{5}{4}$ respectively. This weight reflects the fact that a graduate that is specialized under the new ‘regime’ had to study for 5 years, while for the short degree only 3 years are needed. The denominator represents the number of years (officially) needed to complete university under the old unitary system. We weighted even the old system diploma by $\frac{3}{4}$ as it lasts 3 years. This is a simplification since the diploma is not really equivalent to the new BA three years degree, but is the best we could do in order to make a distinction from the university degree.

The two first models agree on the sharp increase of the proportion of graduates in the technical and clerical occupations. The use of a permanent contract is positively correlated with the demand for skilled human capital in the post reform era. From this viewpoint, it seems that employers, when demanding highly educated workers, don’t make use of temporary contracts in the post reform period more extensively than before, not at least in relative terms. We don’t find any significant result in terms of the required experience, but training in the pre reform period is significantly related to the demand for graduates. A gender preference is more marked in the first period, and it is reasonable to assume that for those occupations for which a graduate is needed, gender should be indifferent.⁴⁸ In this preliminary analysis it’s also worth noting that while in the pre-reform period the demand for graduates came from big firms, in the new era the impact of firms’ dimension is less strong. After the reform knowing a foreign language is less a distinctive characteristic of graduates, probably because it is becoming a requisite even for less qualified human capital.

The before and after comparison showed in column (3) of the same Table, reveals that there is in fact an increase in the demand for graduates; the time effect is highly significant: in the post reform period the demand for skilled workers increased by 0,68%. This model not only takes into account firms’ dimension, but also the prospective turnover of all personnel. In this way we can control for the fact that the firms that require more graduates could also be those that require more personnel in general (or vice versa). Since the marginal effect for the turnover is not different from zero we can exclude that the demand for highly skilled human capital is driven by ever-expanding firms. Firms’ dimension is anyway positively related to our outcome: bigger firms hire more graduates than smaller firms. Employers also require that the highly educated human capital be trained, even if in this context it’s not clear enough what is meant for training.

It’s important to notice that the old graduates are preferred over young ones. This in part can reflect that the new BA degree is not enough for employers, but also we should keep in mind that we are estimating the relative demand for graduates, so we are always comparing graduates with non-graduates. In this perspective it’s quite obvious that low skilled human capital can be relatively younger than highly skilled. And also note that the reference category here is that age is not an important characteristic to look at when choosing a worker, so it’s not clear at all how choices are shaped over age. Surprisingly, experience does not play a role here, but again this should be due in part to the fact that the we’re comparing graduates vs. non graduates, for which probably experience is more relevant (at least in relative terms). An aspect that should be important for evaluating the university reform is that employers report that they won’t have problems in finding prospective graduates because of a lack of educational facilities; however, for some jobs people are not qualified enough. Our interpretation is that universities courses supply responds to market needs,

⁴⁸ For example job for highly educated people should not require strength, so men should not be preferred over women.

but people choose not to enroll in some courses. So, there is a mismatch between the demand and the supply of labor due not to a lack of the educational system, but to people decisions.

However, the choices of each worker's education and of worker's other characteristics (occupation, type of contract, age, sex, experience, IT and foreign language knowledge, training) could be determined jointly, so that the variation in the demand for graduates could also include changes in all required workers' characteristics. In this model we control for workers' characteristics in order to test how the demand for high skilled human capital varied, *ceteribus paribus*. So keeping fix occupations, age, gender preferences and all the other characteristics, we investigate whether the required number of graduates changed or not in the post reform period.

Performing the same regression of column (3), but excluding the characteristics the could be determined potentially jointly with the educational level, we obtain that the demand for graduates increased by 3.5%, as shown in column (4) of Table 9 in the Appendix. This results show that the choice of the worker's educational level is done simultaneously to the choice of other workers' characteristics that are observed in this survey. This result is not conflicting with the previous one, but simply we have a model that allows that jointly with educational level other employees' characteristics vary.

The same probit model is then considered only on the sample of firms that are in both sub-periods; results are stronger in term of the impact that regressors have on the estimated probability (column 2 of Table 10 in the Appendix), so that in the post reform era the probability of hiring a graduate is increased by 0,73% (against 0.68%, so +11%). Marginal effects are in absolute value bigger than those of the 'unbalanced' case, meaning that those firms that are in both time periods are selected, even if this difference does not contradict our main result. If we think that in this regression we are excluding a) firms that join the survey only in the post reform era because new entrants, b) firms that are only in the pre reform period because they exit the market, c) firms that don't hire new personnel for long time periods, this result is plausible.

In order to investigate how firms are selected we analyze the characteristics of firms that are in both sub-periods and of those that appear just in one of them. We find that the always-respondent firms are on average bigger and located in the northwest, and conversely those that are just in the before reform era or in the post reform era are smaller and more frequently placed in the south of Italy. Differences at industry level are less clearly shaped. So, performing just a regression on the group of firms that are in both periods might be misleading and not representative. Anyway, our main result does not differ dramatically in the 'unbalanced' and in the 'balanced' case.

Again we consider the model excluding the characteristics that could be determined jointly with the prospective worker's educational level, focusing on the 'balanced' group of firms (column (3) of Table 10 in the Appendix). Consistently with what we have already found the likelihood of hiring a graduates is increased by 3.7%, because we are not taking into account characteristics that might have varied together with the required educational level and firms that are just in one sup-period. Anyway, this last set of results is in line with the 'unbalanced' case with the exclusion of workers characteristics.

3.6.2 *The increase of graduates: overall trend or growth in some occupations?*

As we already commented before, for some jobs (technicians and clerks) the share of graduates increased substantially and here we will provide evidence aimed at showing whether the change in the demand for graduates is driven by the expansion of the two occupations or it is triggered by a substitution between skilled and unskilled workers for the aforementioned jobs.

By mean of a shift-share analysis, we decompose the variation in the proportion of graduates along two dimensions: time and type of job.

Given the share of graduates in each occupation in the two periods and given the weight of each occupation in the whole economy, we can observe two things. First we analyze what would have happened to the share of graduates in each job, if their share was fixed at the level of the pre

reform period and only the overall weight of each occupation did vary. To be clearer: if the share of graduates had followed proportionally the expansion of each occupation how it would have increased/decreased? This tells how important is the effect of the expansion of some jobs in triggering the increase/decrease in the demand for graduates. Results are reported in column (10) of Table 12 in the Appendix and show that for the managerial occupations the share of graduates should have decreased by 0,2% (or by 1215 units) if it would have varied proportionally with the weight of that occupation in the economy. For the scientific and highly specialized occupation there was a decrease in their weight in the economy, so that if graduates would have followed this decline they should have decreased by 0.3%. Conversely graduates should have increased by 0,2% (or by 1608 units) if they had varied proportionally with the weight of the clerical jobs. This means that the share of clerks in the economy grew and this is a factor driving the increase in the demand for highly skilled human capital. For the other kind of jobs the change in the proportion of graduates followed the trend of each occupation.

Column (11) of the same Table, shows how the proportion of graduates would have varied if the weight of each occupation had stayed the same. This shows the change in the share of graduates that we would have observed even without the expansion of some specific occupations, pointing out the increase in the skill intensity of each job. So even if the highly specialized occupation didn't increase their weight in the economy, graduates increased by 0,1% in those jobs. The biggest increase is observed for technical occupations: keeping fixed the weight of these jobs, the share of graduates increased by 2,8%, so after the reform the required human capital is more skilled. If the weight of clerical occupations was constant, the share of graduates would have increased by 0,9%, meaning that even for that jobs the intensity of highly skilled human capital grew over time.

This evidence is suggesting that those occupations for which the skilled human capital intensity is higher showed a decline in the years taken into account. Highly specialized occupations category includes: mathematicians, physicists, architects and urban planners, doctors and nurses, managers, financial analysts and officers, social scientists, lawyers and judges, professors, researchers, teachers, educators and artists. Instead those occupations that traditionally showed a lower intensity of highly skilled human capital gained momentum. The substitution of less skilled workers with more skilled ones is the factor that mainly drives the increase in the demand for graduates for the technical and clerical jobs.

3.7 Discussion

It's clear that any before and after comparison should be taken with caution, especially in those cases where other events took place in the meanwhile the university reform was implemented. Here we will present and discuss the possible confounding factors that might have altered employers' hiring decisions and we will provide evidence that support the thesis that the increase in the demand for skilled human capital is driven by the university reform.

3.7.1 Reforms in employment protection regulation

Our main concern is if the reforms in the labor market regulation altered the demand for workers, as they could have reduced labor costs.⁴⁹ Even if regions and sectors implement the reform at different times, given the structure of our data set, that information can't be used fruitfully, as we can't locate firms at regional level. According the preliminary analysis done by Cappellari et al. (2010), the 2003 reform increased the turnover of workers, meaning that the apprenticeship made

⁴⁹ The first de-regulation measures that are relevant for our analysis were approved in 2001 and they loosen fixed term contracts, which liberalization was first introduced in 1997 with the 'Treu Laws'. The 2001 decree modified some aspects of the fixed term contracts, making their use more generic (see Cappellari et al., 2010). In 2003, the 'Biagi Laws' allowed new forms of apprenticeships and made easier to use temporary contracts. This reform increased age for apprenticeship from 26 to 30 years old and made easier for firms to accomplish the mandatory training that is basically the reason why those contracts exist. The implementation of the Biagi's measures started in late 2005 and by 2006 it had reached the most important sectors (see Bosio and Leonardi, 2010, for more details).

less costly hiring and firing. On the other hand the 2001 decree does seem to have increased the cost of workers. So with respect the changes in the cost of hiring, the two reforms seem to have had opposite effects. It's not yet clear though which one prevails and it's may be too soon to draw conclusions about the overall impact. Anyway, for the purpose of this paper employment protection reforms are only relevant if they altered the cost ratio of graduates with respect to non graduates, but not if both groups of workers' costs are influenced to the same extent. Here we will provide evidence showing that the relative cost of graduates should not have decreased with respect to non-graduates, thus the variation in the demand for skilled workers should be at most downward biased.

For sociologists flexibility refers to the insecurity that a worker has about the duration of the employment contract; they claim that unskilled workers are more exposed to insecure job positions (Maurin and Postel-Vinay, 2005), because they are easily replaced (Erikson and Goldthorpe, 1992). Marsden (1995) argues that skilled workers are more likely to enjoy long-term contracts, because they play a crucial role in promoting cooperation at firm level. So from a sociological perspective there is no reason to believe that graduates became less expensive with respect to unskilled workers, but, on the contrary, their claim is that they become relatively more costly.

According to SHIW survey, in 2000 15% of the workers with a temporary contract were graduates, while in 2008 they were 19% (see Table 5).⁵⁰ Among those employed permanently graduates were 12% and 16% in 2000 and 2008 respectively. So, the share of graduates has increased in both the groups, but it increased more in the group of permanent workers (+33% versus +26%). Note that in this data set it's not possible to distinguish further the type of temporary contract. This evidence agrees with the sociologists that claim that the use of temporary contract should not be diffusing faster among skilled workers, but among unskilled.

Share of graduates on workers, by type of contract					
Type of contract:	2000	2002	2004	2006	2008
Permanent	0,12	0,14	0,11	0,12	0,16
Temporary	0,15	0,12	0,21	0,23	0,19

Table 5 Share of graduates on total work force by type of contract, excluding those that are employed in the Public Administration; source: SHIW 2000-2008.

This evidence suggests that graduates are increasingly more required by employers and that the use of temporary contracts did not lower their relative cost, expressed by the type of contract.

3.7.2 General trend

Another problem is if the effect of the reform is confused with a general upgrade in the skills required to workers. If, for instance, over the same years there was a substantial reduction in those sectors for which the unskilled workers are needed, we would have observed an increase in the share of skilled workers not because more skilled human capital became available (supply push), but because the low skilled human capital intensity sectors are downsizing. In this case we would overestimate the effect of the reform. Looking at the distribution of graduates across sectors (defined at two digit level), we don't find any significant difference in the industries that underwent a reduction in the share of skilled human capital and those that didn't. Instead we find that the share of graduates increased across all sectors but 8 (out of 54), even if at different rates (see Table 11 in the Appendix). Moreover, in the manufacturing sectors that are expected to downsize, the share of graduates increased.

As a further control for testing possible trends in the upgrading of the labor force we run a regression including only graduates whose courses length was not modified. The reform in fact changed the legal duration of the majority of the degrees but left unaltered the duration of medicine, dentistry, architecture, law, construction engineering and pharmacology degrees. Notice that since

⁵⁰ The SHIW is the Survey of Household Income and Wealth and it is provided by the Bank of Italy from 1980 onwards. It's is freely available from: <http://www.bancaditalia.it/statistiche/indcamp/bilfait/dismicro/storico/stata>

in the survey public enterprises are excluded, the number of required doctors is anyway small. So we consider the same probit model for the probability of hiring a graduate, but we take into account only potential workers whose degree duration was not altered. Results are shown in Table 10 column (5). The time effect is very close to 0, even if it still retains statistical significance. This result suggests that there might be an increasing trend in the demand for skilled workers, as the probability of being hired increased also for the graduates with non-reformed degrees. However, the time effect is very close to zero and small if compared with the case in which all graduates are considered. So if the required number of graduates workers with a reformed degree did increase at the same rate as the number of workers with a non-reformed degree, the expected expansion in the demand for graduates would have been more limited. Two caveats prevent us from using as a proper control this group of degrees: a) the required workers with these degree are just a very small fraction of the required graduates (1% of all required graduates); b) the control group degrees are not equivalent to the group of reformed degree. So for this reform it does not exist a proper control group to take into account properly a time trend.

3.7.3 The 2008 crisis

Lastly, the effect of the reform could be hindered by the 2008 international crisis, when our last survey was taken.

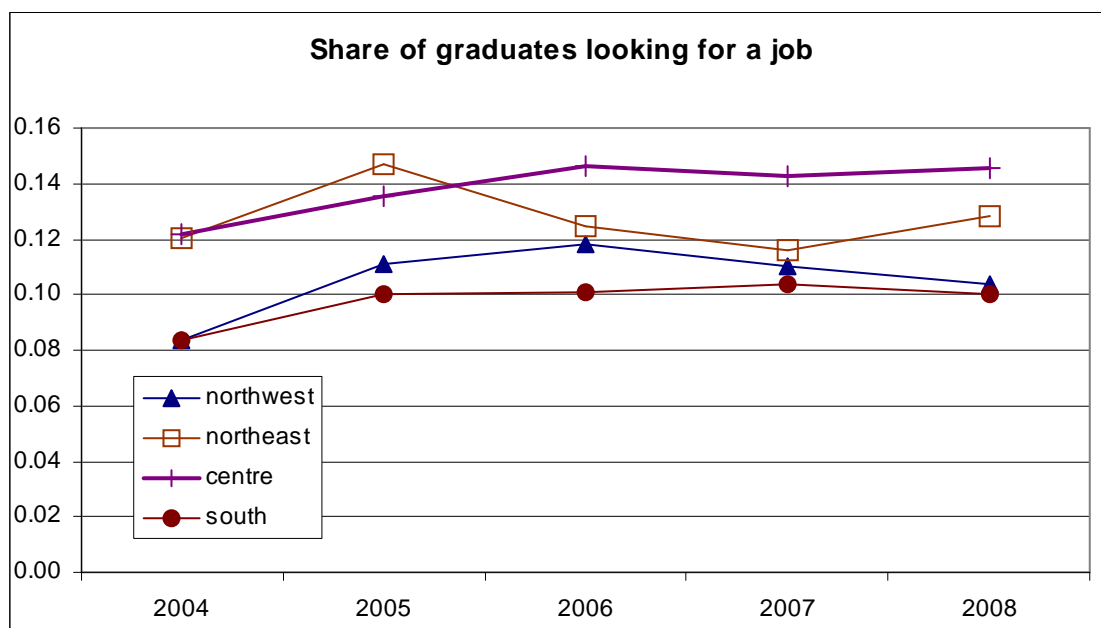


Figure 1 Share of graduates looking for a job among, by macro area. Source: Istat, Labor force survey, years: 2004-2008.

The reader should take into account that the post reform period collects surveys from 2007 to 2009, that predicts hiring practice the current years; unfortunately we don't have information on each year so we can't exclude from the analysis all the surveys taken in 2009, but only those that answer just in 2009.⁵¹ Anyway, there is evidence that the crisis was unexpected (at least in 2007 and 2008) so that the first two surveys of the post reform period can be used safely.

In order to control for the possible downward bias caused by the crisis, the analysis includes controls aimed at capturing the effect of reduction in total employment in each firm. In this way we purged the effect of the reform from firms' downsizing. The marginal effect of the expected turnover on the probability of hiring a graduate is not different from zero (see again Table 9 in the Appendix), meaning that there is no relationship between firms downsizing and the demand for

⁵¹ Since the crisis happened in September and firms are interviewed in January, we assume that the 2008 survey can be used without problems.

graduates. Anyway, unless there is evidence that the decrease in hiring had been sharper for unskilled human capital than for skilled, the ratio graduates vs. non graduates is not altered. In Table 10 in the Appendix we run the probit regression that estimates the probability of hiring graduates, excluding those firms that were in the survey just in 2009 (see column 4), but results do not differ significantly (compare column 1 and 4 in Table 10). Note that we can exclude the last survey only for firms that are in the survey only in 2009, so the number of excluded firms is small, as the majority was interviewed at least in another year during the post-reform sub-period.

Using data collected from the Istat (National Institute of Statistics) - Labor Force surveys, we monitored the share of graduates among people that are looking for a job from 2004. Figure 1 shows that there is no great variation in the last years, although the effect of the crisis should become evident during 2009, whose data are not available yet. However, this graph suggests that the share of unemployed graduates didn't increase dramatically in the last years. So the crisis didn't alter the graduate non-graduate ratio.

So ideally we would like to find a way that could tell us how firms would have behaved if the only event that happened between the two periods was the reform, that is we want to separate the effect of the reform from any macro trend and from the changes in labor cost caused by the introduction of the flexible contracts. Unfortunately up to now, no data source allows the researcher to control for all the changes in the environment firms face before and after the reform.

3.8 Do short graduates find a job?

To get more insights into the effect of the reform, we analyze, in the post reform era, how employers' preferences are revealed when choosing between a BA and a MA graduate. In the survey they also have the option to say that they are indifferent between the two. Since before the reform the distinction between BA and MA degree didn't exist, it is interesting to observe to what are the job characteristics associated to the two types of workers.

Type of degree	Freq.	Percent	Percent (only on grads)
<i>No degree</i>	579757	85.97	
<i>BA (3-years degree)</i>	16207	2.40	17.13
<i>MA (5-years degree)</i>	42146	6.25	44.54
<i>Indifferent</i>	36281	5.38	38.34
<i>Total</i>	674391	100.00	100

Table 6 Employers preferences over the different degrees in the post reform period

Table 6 above clearly shows that MA graduates are preferred over BA graduates, and that whenever an employer wants to hire a graduate she/he is indifferent between the two types 38% of the times. We could say that the newly created BA degree didn't meet market needs, as it is only a requirement for a small fraction of firms. Another worrying outcome is that the share of undetermined employers not expressing a preference for either of the two options is relevant. We could argue that this is because employers still don't know how well trained are new graduates, or because the job that the new worker is asked to do can be done by both of the types. So we could say that for 38% of the new jobs created, employers believe that there is no big difference between a BA and a MA graduate. 45% of the times instead employers evaluate not sufficient the BA degree. Overall it seems that on one side there is still uncertainty about reform's outcomes and that in order to increase the probability of being hired a student should take the MA degree. This last result partly invalidates the reform's aims. Anyway, these results should be interpreted with caution

as they show only employers' plans and we would be far more conclusive knowing who is actually hired.

In order to see how the choice of a type of graduate is correlated with the observable characteristics we perform a multinomial logit model.⁵² So first we model the choice of the employers over four possible outcomes: she/he can hire a non graduate, a BA graduate, a MA graduate or be indifferent between the two types. Then, in another model we concentrate only on graduates and so the choice is modeled over three possible outcomes: BA or MA or indifferent. In principle this last model should take into account that there is first a selection into the choice of hiring a graduate or not, and then one can choose the type of degree that best fits firm's needs. This would translate into a selection probit model followed by a multinomial one and given the number of observation it would be computationally too burdensome to perform.

Table 13 in the Appendix reports the estimated coefficients of the multinomial logit model in columns (1) to (3) and the marginal effects in columns (4) to (7).

Non-graduates are in general not required for the first four Istat categories, because they are mostly required for semi/non-qualified occupations that form the residual category. For graduates instead the employers' choice is equally distributed between indifference and MA degree for the two first occupation categories (managers and highly specialized jobs). BA graduates are not more attractive than MA graduates for any kind of job, while for clerks and technicians employers are indifferent. This is hard to interpret because we don't know why the employer is indifferent (lack of information about the reform? Or because the job can be done by any type of graduate? Or because there is no difference between them?). Permanent contracts are clearly more frequent for MA graduates, while temporary are for non graduates. For the two remaining categories there is no clear pattern. Young people are required among non graduates, while people older than 25 are preferred among MA graduates. Again a gender preference is expressed when choosing non graduates while employers are indifferent when choosing a MA graduate. This is consistent with the fact that for those jobs where a MA graduate is needed it would be unreasonable to prefer men over women or vice versa. Bigger firms tend to prefer graduates over non graduates, but they rarely choose a BA graduate. Overall we could say that the choice of MA graduate is clearly opposed to not choosing a graduate, but no obvious relationship emerges when dealing with BA graduate or with the indifference category. In this sense it seems that preferring a worker with a short degree is not yet characterized. Those with a short degree are only marginally preferred for qualified jobs. Employers do make a clear distinction between non graduates and MA graduates, but the other two options are not differentiated systematically.

Table 14 in the Appendix shows results from a similar multinomial logit model, with the exception that here non graduates are excluded. BA graduates are never preferred for any jobs, except when a young worker is wanted. Big firms seem to be those that more frequently choose the 'indifference' option. Permanent contracts are more frequent among those that clearly prefer a candidate with either the short or the long degree.

Probably in order to get some more insights we would need further information about firms' precise needs and the personnel actually hired. In this way our analysis wouldn't be puzzled by the indifference category.

3.9 Did the type of required skills change over time?

Since in the survey the employers are asked to indicate also the new worker's field of studies, we classified the different fields into three macro areas to investigate potential changes in employers' preferences.

So each new graduate worker can be classified into three mutually exclusive categories: as a science graduate (if he/she got a degree in architecture, agricultural studies, engineering, chemistry,

⁵² The purpose of this part of analysis is merely descriptive since the Independence of Irrelevant Alternatives property is not satisfied by the two models presented here.

biology, medicine, mathematics or physics), as a political scientist (if the degree is in economics, business & management, statistics, sociology or in political sciences) or as a literate (if the degree is in humanities: law, modern and ancient languages, philosophy or psychology). Table 7 shows required graduates classified by field of study, by occupation and by time, so that for each occupation we can observe the share of graduates' field of study. The last three columns show the growth rate of the graduates' field of study share.

Overall scientific degrees are required more frequently than the others, even if the share of scientific degrees is declining over time (-0,8%). The second most wanted degrees are the ones included in the political sciences category, and also in this case the same declining trend is observed (-0.9%). The humanities category which covers a wide range of subjects (from Italian, to History, including Communications), instead showed an increase (+216%), but it's still the less important in terms of share.

	2001-2003			2007-2009			Growth rate		
	Science	Pol-science	Humanities	Science	Pol-science	Humanities	Science	Pol-science	Humanities
<i>Legislators, senior officials and managers</i>	0,34	0,64	0,02	0,29	0,66	0,05	-0,12	0,02	1,16
<i>Professionals</i>	0,69	0,26	0,05	0,73	0,17	0,10	0,06	-0,34	0,91
<i>Technicians and associate professionals</i>	0,59	0,38	0,03	0,56	0,30	0,14	-0,04	-0,22	3,46
<i>Clerks</i>	0,01	0,98	0,01	0,02	0,90	0,08	1,65	-0,09	8,92
<i>Sales workers and semi-qualified, non -qualified occupations</i>	0,88	0,12	0,00	0,74	0,20	0,07	-0,17	0,72	15,34
<i>Total</i>	0,59	0,38	0,04	0,54	0,34	0,12	-0,08	-0,09	2,16

Table 7 Share of required field, by occupation (only for graduates)

Table 15 in the Appendix show the results of six probit models for the probability to choose each type of degree first against all possible workers (general case) and then only against other graduates. So, controlling for other confounding factors, we can observe how the university reform (and all the other events) affect employers' preferences over prospective workers' field of study.

The share of workers with a degree in humanities (in column 1 and 4) increased over time only with respect to other graduates. This is due to the fact that those with a degree in humanities are just a small fraction of workers if compared to the full set of required workers. Looking at results in column (4) we find that 'literate' workers are required for the managerial and clerical professions, that experience is a requisite and that there is preference for women. This group is hired on a temporary basis. Those with a degree in science are hired more frequently in the post reform era in both the general and the restricted case (the one where only graduates are compared); they are more likely to be hired with a permanent contract. Science graduate women are never preferred and training is a prerequisite, while experience not (even if it's not completely clear the difference between the two). Employers declare that finding a suitable worker with a science degree is problematic. If we look at the column (5), we find that the category of science graduates is always negatively associated to the professions categories in the analysis, because people with the other types of degree are required more frequently for more diffused occupations (especially those with a humanity degree).⁵³ Political-scientist category (columns 3 and 6) shows a little increase after the reform in the general case, but a declining one if we compare them only with graduates. In the general case no clear pattern emerges in the characteristics of firms and job that are more likely associated with those types of workers. In the comparison with other graduates, economists are not

⁵³ Note that the occupations, for which science graduates are mostly wanted, do not have a great weight in the whole economy (i.e. high professionals jobs are only a small fraction of the jobs in the whole system).

difficult to find. Overall, it seems that those with a scientific degree are difficult to find and this may suggest that universities don't meet market needs for those jobs that require scientific skills.

Anyway these problems are not related specifically with the reform. It would be interesting to analyze whether this decline in the preference for political-science graduates is caused by syllabuses simplifications that resulted from the reform. As we mentioned before, it could be the case that employers rather than choosing a graduate in business prefer an engineer, because the mathematics syllabuses in the business courses have been simplified too much under the new regime. At the same time people with a degree in humanities could have substituted those with a secondary school diploma. Anyway with the available data we can not go into further details and answer precisely these questions.

3.10 Conclusions

In 2001 the Italian university system went through a reform that modified the length of courses and simplified contents. The unitary-tier 'regime' that officially lasted for four to six years was substituted with a two-tier system, lasting 3 and 2 years respectively. Students that stop after the first three years get a BA degree, while those that continue for 2 more years specialize and get a MA equivalent degree. The reform is meant to uniform the Italian higher educational to the uniform European Educational System and to overcome some problems, such as: high age at degree, curricula rigidities and high drop out rate and low enrolment.

Up to now the empirical literature mostly focused on the effect of the reform on the enrollment decision, drop out and age at degree. There is a general agreement that the reform has been successful in increasing access to university and decreasing drop out, even if the aforementioned problems are not completely weed out.

In this study we explored what happened when graduates from the new 'regime' entered the labor market and substituted the old 'regime' graduates. So we paid attention to employers' choices and we tried to identify the demand for graduates from six surveys that ask employers about their intentions on prospective workers.

In the years after the reform the required share of graduates increased by 0.7%: the effect is small but significant. The effect is bigger (+ 3.5%) if we consider that together with educational level other workers' characteristics might have varied.

We present and discuss other concurrent events might have altered this result. Two reforms that liberalized the use of temporary contracts were implemented in the same years and they could have modified labor cost. However, it's not clear yet if the two reforms increased or decreased labor cost and more importantly if they altered the relative cost of graduates with respect to non graduates. Evidence provided shows that the relative cost of skilled workers should be at most increased, thus leading us to underestimate the variation in the demand for skilled workers caused by the university reform.

Any underlying increasing trend in the demand for skills could have also biased the effect of reform; so a) we perform a control experiment in which include only the required workers with degrees whose length was not modified by the reform and b) we checked whether there was a sudden expansion in highly technological sectors; both these checks do not reveal any general increasing trend in the demand for graduates.

The international 2008 crisis might have biased our relationship, so using data collected by Istat on labor forces, we show that at least the ratio between skilled and unskilled unemployed is stable over time, fact that attenuates our worries about the impact of the crisis. We also exclude from the regression model the firms that only answered the survey in 2009 and results do not change.

A part from the general increase in the share of graduates in the new labor force, we analyzed other features of the expected demand for workers.

First we focused on a decomposition of the demand for graduates looking at each occupation. We found that the demand for graduates increased for clerical and technical jobs, but decreased for the managerial and scientific occupations. So graduates are substituting less skilled workers in mid-qualified jobs. More skilled workers are searched for less specialized works.

Another aspect that is interesting to observe is how employers' preferences are shaped with respect to the types of degree that are available in the post reform era: BA and MA degrees. Results are quite blur because employers could also tick the 'indifference' option, but it seems that the demand for BA graduates is still undefined and that big firms seem to be still undetermined about the difference between the two types. BA graduates are only required when a young and qualified worker is needed, which is not very frequent though.

This evidence showed here is suggestive of a small but significant increase in the share of graduates among the newly hired workers that is not attributable to changes in firms' characteristics. The growth in the demand for graduates grew at a slower rate if compared to the growth in the supply of graduates. From the Excelsior survey we know that the growth rate of the number of required graduates is equal to 40% (in the pre reform period firms asked for 72270 graduates while in the post reform period for 101163), while the growth rate in the supply of graduates is equal to 67%, according to CNVSU (in the three years before the reform there were 534318 new graduates, while after 893516). The reform was quite successful in creating more skilled human capital, but firms' demand for graduates reacts mildly to the increase in the number of well-educated workers.

3.11 Appendix

	2001-2003 and 2007-2009				2001-2003		2007-2009	
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Mean	Std. Dev.
<i>Legislators, senior officials and managers</i>	0,01	0,08	0	1	0,01	0,087	0,00	0,066
<i>Professionals</i>	0,05	0,23	0	1	0,06	0,235	0,05	0,219
<i>Technicians and associate professionals</i>	0,15	0,36	0	1	0,16	0,365	0,15	0,357
<i>Clerks</i>	0,09	0,28	0	1	0,08	0,270	0,10	0,297
<i>Sales workers and semi-qualified, non - qualified occupations</i>	0,70	0,46	0	1	0,70	0,460	0,70	0,459
<i>compulsory schooling: lower secondary</i>	0,35	0,48	0	1	0,39	0,487	0,32	0,468
<i>vocational training, regional</i>	0,09	0,29	0	1	0,08	0,272	0,11	0,309
<i>vocational training, state courses</i>	0,10	0,31	0	1	0,11	0,317	0,10	0,296
<i>secondary school</i>	0,32	0,47	0	1	0,30	0,458	0,33	0,471
<i>short degree/diploma</i>	0,02	0,15	0	1	0,02	0,144	0,02	0,153
<i>long degree/old regime</i>	0,11	0,31	0	1	0,10	0,298	0,12	0,321
<i>Permanent contract</i>	0,48	0,42	0	1	0,57	0,399	0,39	0,428
<i>age<25</i>	0,14	0,34	0	1	0,23	0,419	0,06	0,229
<i>age>25</i>	0,47	0,50	0	1	0,39	0,487	0,55	0,498
<i>indiff</i>	0,39	0,49	0	1	0,38	0,486	0,40	0,489
<i>Experience required</i>	0,61	0,40	0	1	0,54	0,424	0,68	0,383
<i>Experience not required</i>	0,39	0,49	0	1	0,46	0,498	0,32	0,466
<i>training required</i>	0,77	0,42	0	1	0,69	0,464	0,85	0,358
<i>training not required</i>	0,23	0,42	0	1	0,31	0,464	0,15	0,358
<i>it knowledge required</i>	0,34	0,47	0	1	0,34	0,473	0,33	0,472
<i>it knowledge not required</i>	0,66	0,47	0	1	0,66	0,473	0,67	0,472
<i>woman preferred</i>	0,11	0,31	0	1	0,07	0,256	0,14	0,346
<i>man preferred</i>	0,17	0,37	0	1	0,10	0,298	0,23	0,418
<i>sex indiff</i>	0,73	0,44	0	1	0,83	0,375	0,63	0,482
<i>foreign language required</i>	0,21	0,40	0	1	0,24	0,426	0,18	0,382
<i>foreign lang not required</i>	0,79	0,40	0	1	0,76	0,426	0,82	0,382
<i>no probl in finding</i>	0,70	0,46	0	1	0,64	0,479	0,76	0,426
<i>problems in finding,lack of people with the appropriate qualification</i>	0,08	0,27	0	1	0,10	0,296	0,07	0,248
<i>problems in finding,lack of appropriate educational facilities</i>	0,02	0,13	0	1	0,03	0,159	0,01	0,084
<i>problems in finding,high competition among firms for similar workers</i>	0,14	0,34	0	1	0,18	0,388	0,09	0,290
<i>problems in finding,other reasons</i>	0,06	0,24	0	1	0,05	0,215	0,07	0,261
<i>Exporting firm</i>	0,35	0,48	0	1	0,29	0,452	0,41	0,492
<i>Expected turnover, by firm</i>	0,02	0,16	-7.1	19	0,01	0,136	0,03	0,177
<i>employee<20</i>	0,05	0,22	0	1	0,04	0,205	0,06	0,228
<i>20<employee<50</i>	0,03	0,17	0	1	0,02	0,143	0,04	0,194
<i>50<employee<250</i>	0,22	0,41	0	1	0,21	0,410	0,23	0,419
<i>employee>250</i>	0,70	0,46	0	1	0,72	0,448	0,68	0,467
<i>centre</i>	0,18	0,39	0	1	0,17	0,377	0,19	0,392
<i>south</i>	0,11	0,31	0	1	0,09	0,291	0,12	0,330
<i>north-east</i>	0,31	0,46	0	1	0,30	0,460	0,31	0,463
<i>north-west</i>	0,40	0,49	0	1	0,43	0,495	0,37	0,484
<i>Obs</i>	1294990				620594		674396	

Table 8: Descriptive statistics for the characteristics of the required workers and of the firms. Each observation corresponds to the prospective worker.

<i>dependent variable:</i>	(weighted) number of graduates required			
	<i>period</i>	(1)	(2)	(3)
	2001-2003	2007-2009	01-03 vs 07-09	01-03 vs 07-09
2007-2009			0.00684***	0.0351***
Legislators, senior officials and managers	0.875***	0.869***	0.873***	
Professionals	0.877***	0.930***	0.904***	
Technicians and associate professionals	0.314***	0.538***	0.418***	
Clerks	0.0800***	0.339***	0.207***	
Sales workers and semi-qualified, non -qualified occupations	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>	
Permanent contract	0.00201	0.00109**	0.00251***	
age<25	-0.00356**	-0.00193***	-0.00437***	
age>25	0.00376**	0.0005	0.00172*	
indiff	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>	
experience required	-0.0003	-0.0002	-0.0002	
<i>experience not required</i>	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>	
training required	0.00525***	0.000377	0.00284***	
<i>training not required</i>	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>	
it knowledge required	0.000427	0.000342	0.00130	
<i>it knowledge not required</i>	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>	
woman preferred	-0.00488***	-0.00146**	-0.00375***	
man preferred	-0.00352***	-0.00208***	-0.00393***	
<i>indiff</i>	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>	
foreign language required	0.00771***	0.00379***	0.0067***	
<i>foreign lang not required</i>	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>	
problems in finding,lack of people with the appropriate qualification	0.00162	0.000701	0.00161*	0.0443***
problems in finding,lack of appropriate educational facilities	-0.000266	-0.000483	-0.0004	0.0246
problems in finding,high competition among firms for similar workers	0.00352**	0.00382**	0.0049***	0.0514***
problems in finding,other reasons	-0.0001	0.0003	-0.000376	-0.0398***
<i>no problems in finding</i>	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>
exporting firm	-0.000283	0.0002	0.0006	0.0330***
<i>not exporting firm</i>	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>
Expected turnover, by firm	-0.00595*	0.00127**	0.0003	0.0036
20<employee<50	0.00496*	0.001	0.00261**	0.0453***
50<employee<250	0.00698***	0.00270***	0.0057***	0.0712***
employee>250	0.00817***	0.00293***	0.0067***	0.0711***
<i>employee<20</i>	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>
south	0.0006	-0.0001	0.0002	-0.0178**
north-east	-0.00211	-0.000745	-0.00162	-0.0348***
north-west	0.0012	-0.0005	-0.0002	0.0103
<i>centre</i>	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>	<i>ref outcome</i>
Controls for industry	<i>yes</i>	<i>yes</i>	<i>yes</i>	<i>yes</i>
Observations	622159	674238	1299856	1299856
Pseudo R-squared	0.621	0.647	0.630	0.227

Table 9: column (1) and (2) marginal effect of the probit model for the probability of hiring a graduate, pre reform and post reform period respectively. Columns (3): the same probit model over the two periods. Column (4): the before and after comparison without workers' characteristics. *** p<0.01, ** p<0.05, * p<0.1.

Dependent variable	(1)	(2)	(3)	(4)	(5)
	(weighted) graduate required				
		only firms always respondent	only firms always respondent no char.	no 2009 only respondent	Only on 'control' group
2007-2009	0.0068***	0.0073***	0.0372***	0.0068***	4.34e-05**
Legislators, senior officials and managers	0.87***	0.883***		0.878***	0.140***
Professionals	0.9***	0.908***		0.908***	0.444***
Technicians and associate professionals	0.42***	0.433***		0.424***	0.0105***
Clerks	0.21***	0.214***		0.210***	0.00105**
<i>Sales workers and semi-qualified, non –qual occupations</i>	<i>base cat</i>	<i>base cat</i>		<i>base cat</i>	<i>base cat</i>
Permanent contract	0.0025***	0.003***		0.0028***	6.93E-07
age<25	-0.004***	-0.0046***		-0.0045***	-1.09E-05
age>25	0.00176*	0.00186*		0.00178*	8.04E-06
<i>indiff</i>	<i>base cat</i>	<i>base cat</i>		<i>base cat</i>	<i>base cat</i>
experience required	-0.0002	-0.00001		-0.0002	4.29E-06
<i>experience not required</i>	<i>base cat</i>	<i>base cat</i>		<i>base cat</i>	<i>base cat</i>
training required	0.0028***	0.0032***		0.0029***	3.47E-06
<i>training not required</i>	<i>base cat</i>	<i>base cat</i>		<i>base cat</i>	<i>base cat</i>
it knowledge required	0.0013	0.0016		0.0013	1.44E-05
<i>it knowledge not required</i>	<i>base cat</i>	<i>base cat</i>		<i>base cat</i>	<i>base cat</i>
woman preferred	-0.004***	-0.004***		-0.004***	-9.92e-06*
man preferred	-0.004***	-0.0043***		-0.004***	-1.24e-05*
<i>indiff</i>	<i>base cat</i>	<i>base cat</i>		<i>base cat</i>	<i>base cat</i>
foreign language required	0.0067***	0.0074***		0.007***	-2.15E-06
<i>foreign lang not required</i>	<i>base cat</i>	<i>base cat</i>		<i>base cat</i>	<i>base cat</i>
problems in finding,lack of people with appropriate qualification	0.00161*	0.00164	0.045***	0.00174*	2.42E-06
problems in finding,lack of appropriate educational facilities	-0.0004	-0.0004	0.025	-0.00027	-6.89E-06
problems in finding,high competition among firms for similar workers	0.005***	0.0052***	0.053***	0.005***	8.60E-06
problems in finding,other reasons	-0.0004	-0.0004	-0.042***	-0.0004	-1.25e-05*
<i>no problems in finding</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
exporting firm	0.0006	0.0007	0.0323***	0.0006	3.52E-07
<i>not exporting firm</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
Expected turnover, by firm	0.0003	0	0.002	0.0004	-2.16E-05
20<employee<50	0.0026**	0.0028*	0.05***	0.0028**	5.81E-06
50<employee<250	0.0057***	0.0055***	0.07***	0.0058***	1.05E-05
employee>250	0.0067***	0.0065***	0.07***	0.0068***	1.42e-05*
<i>employee<20</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
south	0.00019	0.000159	-0.02***	0.000272	-8.96e-06*
north-east	-0.0016	-0.0022*	-0.04***	-0.0016	-9.86E-06
north-west	-0.0002	-0.0006	0.01	-0.0002	-7.95E-06
<i>centre</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
Controls for industry	yes	yes	yes	yes	yes
Observations	1299856	1206828	1206828	1286458	1135210
Pseudo R-squared	0.629	0.63	0.227	0.63	0.684

Table 10: column (1) replicates column(3) of Table 9; column (2) includes only firms that were in the surveys in both periods; column (3) reports the result for the always respondent firms excluding the workers characteristics that could be determined jointly with educational level; column (4) excludes firms that were in the survey just in 2009; column (5) includes only the demand for graduates for those degrees whose length was not modified by the reform. * p<0.01, ** p<0.05, * p<0.1.**

Industries	Share of graduates		GROWTH RATE
	Pre Reform	Post Reform	
Real estate	0,24	0,10	-0,60
Food and beverages	0,08	0,04	-0,52
Retail	0,03	0,02	-0,37
Extraction (non oil)	0,04	0,03	-0,26
Automotive: trade and repair	0,12	0,09	-0,24
Energy: production and distribution	0,22	0,18	-0,17
Recreational	0,14	0,13	-0,10
Office machinery manufacturing	0,63	0,58	-0,07
Health and social assistance	0,27	0,26	-0,05
Chemical manufacturing	0,40	0,41	0,03
Hotels and restaurants	0,01	0,01	0,05
Textile	0,06	0,07	0,05
Family facilities	0,03	0,04	0,10
Press and publishing	0,18	0,21	0,15
IT	0,51	0,58	0,15
Wholesale trade	0,10	0,12	0,21
Construction	0,05	0,06	0,21
Research and development	0,62	0,75	0,22
Consultancy agencies	0,07	0,08	0,22
Land transport	0,02	0,03	0,25
Insurance and pension funds	0,34	0,44	0,29
Agriculture and hunting	0,03	0,04	0,32
Financial broker (auxiliary)	0,46	0,61	0,32
Financial broker (main)	0,41	0,54	0,32
Medical instruments manufacturing	0,16	0,22	0,35
Manufacturing	0,07	0,10	0,45
Rubber products manufacturing	0,07	0,11	0,45
Machinery manufacturing	0,15	0,22	0,47
Maritime transports	0,03	0,04	0,48
Education	0,37	0,58	0,57
Extraction (Oil)	0,39	0,61	0,57
Furniture manufacturing	0,04	0,06	0,57
Tv and radio manufacturing	0,25	0,40	0,58
Steel	0,05	0,08	0,58
Refinery	0,24	0,41	0,71
Water	0,11	0,19	0,72
Air transport	0,05	0,10	0,80
Leather manufacturing	0,04	0,07	0,81
Steel manufacturing	0,04	0,09	1,02
Automotive manufacturing	0,09	0,18	1,03
Mechanical machinery manufacturing	0,11	0,22	1,05
Travel agency	0,02	0,04	1,14
Waste disposal	0,03	0,06	1,17
Means of transportation (automotive excluded)	0,15	0,34	1,21
Clothing and furs	0,07	0,15	1,21
Paper	0,04	0,10	1,48
Wood (no furniture)	0,01	0,04	1,97
Means of transportation hiring	0,06	0,23	2,60
Telecommunications	0,07	0,26	2,76
Recycling	0,005	0,04	8,29
Forestry Coal and wood coal extraction, Oil-mining, Tobacco	0,00	0,00	0,00

Table 11: Share of graduates by sector in the pre reform and post reform period, and growth rate.

Occupations:	Pre reform			Post reform			Pre	Post	Pre	Post	Pre	Post		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)		(8)		(9)		(10)	(11)
	no grads	grads	Total	no grads	grads	Total	Tot _j /totocc		Grad _j / totgrad		Grad _j /occ _j		Fix sh gr + Var occ	Fix occ + Var sh gr
Legislators, senior officials and managers	1010	3673	4683	762	2371	3133	0,01	0,005	0,05	0,02	0,78	0,76	-0,002	0,000
Professionals	5610	30428	36038	5459	32133	37592	0,06	0,06	0,43	0,32	0,84	0,85	-0,003	0,001
Technicians and associate professionals	63997	31845	95842	49863	52727	102590	0,16	0,15	0,44	0,52	0,33	0,51	-0,002	0,028
Clerks	44483	4339	48822	53252	13664	66916	0,08	0,10	0,06	0,14	0,09	0,20	0,002	0,009
Sales workers and semi- qualified, non - qualified occupations	430715	1290	432005	470399	152	470551	0,70	0,69	0,02	0,00	0,00	0,00	0,000	-0,002
<i>Total</i>	545815	71575	617390	579735	101047	680782	1,00	1,00	1,00	1,00	0,12	0,15	0,0000	0,032

Table 12: Decomposition of the variation of graduates by occupation j. Columns (1) to (3) show the number of non graduates, graduates and their sum for each job type, in the pre reform period; columns (4) to (6) show the same variables, but are referred to the post reform period. Column (7) is the ratio between the number of new workers for each type of job and the new workers required in the whole economy, again for both the time periods considered. Column (8) is the share of graduates in each occupation on the total number of graduates in the economy, while column (9) is the share of graduates in job j on the number of workers in job j. Column (10) tells how would have changed the share of graduates if only the weight of job j would have changed in the economy. Column (11) instead reports how would have changed the share of graduates if the weight of job j in the economy would have stayed the same, but measures how each job j increased the skill intensity.

base category: no degree required	coefficients			marginal effects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	indiff	BA	MA	no degree	indiff	BA	MA
Legislators, senior officials and managers	9.073***	6.533***	8.951***	-0.913***	0.436***	0.0132**	0.464***
Professionals	10.08***	8.757***	9.791***	-0.946***	0.469***	0.0513***	0.426***
Technicians and associate professionals	8.316***	7.612***	7.593***	-0.572***	0.308***	0.0652***	0.199***
Clerks	6.986***	6.132***	6.461***	-0.391***	0.203***	0.0362***	0.151***
<i>Sales workers and semi-qualified, non-qualified occupations</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
Permanent contract	0.151	0.603***	0.436***	-0.000552**	9.16e-05	0.000142	0.000319**
age<25	-1.08***	0.451	-1.8***	0.000969**	-0.000426**	0.000131	-0.000674***
age>25	0.129	0.462**	0.0871	-0.000249	7.81e-05	0.000107	6.33e-05
<i>indiff</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
experience required	-0.0856	-0.180	0.0876	3.39e-05	-5.30e-05	-4.40e-05	6.31e-05
<i>experience not required</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
training required	0.287*	0.312*	-0.143	-0.0001	0.000159	6.61e-05	-0.000110
<i>training not required</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
it knowledge required	-0.248	0.127	0.321	-0.000134	-0.000145	3.05e-05	0.000248
<i>it knowledge not required</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
woman preferred	-0.278	-1.09***	-0.64***	0.000713**	-0.000154	-0.00018	-0.000378***
man preferred	-0.92***	-0.79***	-0.61***	0.001***	-0.00045**	-0.00015	-0.00038***
<i>indiff</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
foreign language required	0.633***	0.471***	0.806***	-0.00139***	0.00048*	0.00013	0.000783***
<i>foreign lang not required</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
problems in finding,lack of people with the appropriate qualification	-0.131	0.345	0.405***	-0.000375	-7.55e-05	9.49e-05	0.000355**
problems in finding,lack of appropriate educational facilities	-0.109	0.284	-0.92***	0.000427	-6.27e-05	7.74e-05	-0.000442***
problems in finding,high competition among firms for similar workers	0.526**	0.944***	0.721***	-0.00146**	0.0004	0.00034	0.00072**
problems in finding,other reasons	-0.234	0.755***	-0.266	5.27e-05	-0.00013	0.00025	-0.000174
<i>no problems in finding</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
exporting firm	0.0217	-0.0381	0.0337	-2.90e-05	1.32e-05	-8.96e-06	2.47e-05
<i>not exporting firm</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
Expected turnover, by firm	0.623**	-1.282	-0.345	0.00017	0.00038*	-0.0003	-0.00025
20<employee<50	0.280	0.145	0.313**	-0.0005*	0.00019	3.63e-05	0.0002
50<employee<250	0.999***	0.0851	0.448***	-0.00122***	0.0008**	2.02e-05	0.000372**
employee>250	1.551***	-0.116	0.988***	-0.00139***	0.0008**	-2.82e-05	0.0006***
<i>employee<20</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
south	-0.0972	-0.288	0.0333	9.35e-05	-5.71e-05	-6.11e-05	2.47e-05
north-east	-0.240	-0.90***	0.0309	0.0003	-0.00014	-0.00019	2.29e-05
north-west	0.004	-0.649**	-0.151	0.000249	2.59e-06	-0.000143	-0.0001
<i>centre</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
Controls for industry	yes	yes	yes	yes	yes	yes	yes
Observations	674315	674315	674315	674315	674315	674315	674315
Pseudo R-squared	0.530	0.530	0.530				

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

Table 13 Multinomial logit model for employers' choice among non graduates and the types of graduates.

base category: the employers is indifferent between the two types					
	coeff		marginal effects		
	BA	MA	indiff	BA	MA
Legislators, senior officials and managers	-3.614***	0.345	-0.0398	-0.0945***	0.134
Professionals	-2.257***	0.236	0.0148	-0.150***	0.135
Technicians and associate professionals	-1.664***	-0.168	0.102	-0.141***	0.0389
Clerks	-0.963	0.193	-0.0156	-0.0642***	0.0798
<i>Sales workers and semi-qualified, non-qualified occupations</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
Permanent contract	0.489***	0.407***	-0.102***	0.0221*	0.0802**
age<25	1.665***	-0.752*	-0.0403	0.324***	-0.284***
age>25	0.374*	-0.0355	-0.00568	0.0298**	-0.0241
indiff	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
experience required	-0.0197	0.250	-0.0505	-0.0126	0.0630
<i>experience not required</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
training required	-0.0423	-0.386**	0.0804**	0.0137	-0.0941**
<i>training not required</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
it knowledge required	0.468**	0.474*	-0.117**	0.0177	0.0994*
<i>it knowledge not required</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
woman preferred	-0.835*	-0.434	0.121	-0.0405**	-0.0804
man preferred	0.119	0.266**	-0.0586**	-0.00244	0.0610**
<i>indiff</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
foreign language required	-0.0302	0.129	-0.0254	-0.00808	0.0335
<i>foreign lang not required</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
problems in finding,lack of people with the appropriate qualification	0.445*	0.517***	-0.118***	0.0120	0.106***
problems in finding,lack of appropriate educational facilities	0.139	-0.856***	0.155**	0.0508*	-0.206***
problems in finding,high competition among firms for similar workers	0.232	0.201	-0.0496	0.0101	0.0395
problems in finding,other reasons	0.756*	0.0354	-0.0443	0.0797	-0.0354
<i>no problems in finding</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
exporting firm	-0.140	0.00832	0.00359	-0.0118	0.00821
<i>not exporting firm</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
Expected turnover, by firm	-2.591**	-1.799*	0.469**	-0.132*	-0.337
20<employee<50	-0.320	-0.0716	0.0261	-0.0206	-0.00556
50<employee<250	-1.151***	-0.732***	0.195***	-0.0537***	-0.141***
employee>250	-1.833***	-0.758***	0.220***	-0.152***	-0.0682
<i>employee<20</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
south	-0.343	0.0559	-0.000525	-0.0268*	0.0273
north-east	-0.689***	0.230	-0.0272	-0.0562***	0.0834
north-west	-0.716***	-0.166	0.0611	-0.0505***	-0.0106
<i>centre</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>	<i>base cat</i>
Controls for industry	yes	yes	yes	yes	yes
Observations	94635	94635	94635	94635	94635
Pseudo R-squared	0.202				

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

Table 14 Multinomial logit model for employers' choice among the length of degree.

	(1) (2) (3) type of degree vs all individuals			(4) (5) (6) type of degree vs other graduates		
	humanities	science	pol- sciences	humanities	science	pol- sciences
2007-2009	4.14e-05	0.0014***	0.00062**	0.0323***	0.0455**	-0.075***
Legislators, senior officials and managers	0.174***	0.309***	0.499***	0.286**	-0.413***	0.141
Professionals	0.175***	0.562***	0.236***	0.186***	-0.130	-0.155
Technicians and associate professionals	0.039***	0.142***	0.139***	0.100***	-0.320***	0.0448
Clerks	0.025***	0.0065**	0.065***	0.329***	-0.548***	0.106
Sales workers and semi-qualified, non - qualified occupations	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>
Permanent contract	-1.60e-05	0.00131***	-6.16e-06	-0.026***	0.0906***	-0.033
age<25	-1.77e-05	-0.001***	-0.0008**	-0.0083	0.051	0.0097
age>25	-8.98e-06	0.0003	0.0005	-0.0123**	-0.0016	0.0331
indiff	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>
experience required	1.31e-05	-0.0007**	0.00035	0.0197***	-0.048**	-0.016
<i>experience not required</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>
training required	-2.06e-06	0.0007***	0.000217	-0.0077	0.0695***	-0.064***
<i>training not required</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>
it knowledge required	-8.04e-06	0.0007**	0.0002	-0.028**	0.0506	0.0197
<i>it knowledge not required</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>
woman preferred	3.91e-05	-0.00135***	-0.000434*	0.0645***	-0.169***	-0.0121
man preferred	-1.65e-05	-0.0006***	-0.0007**	-0.017***	0.055**	-0.023
<i>indiff</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>
foreign language required	2.90e-06	0.000605**	0.0021**	-0.00097	-0.068***	0.0901***
<i>foreign lang not required</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>
problems in finding,lack of people with the appropriate qualification	5.52e-06	0.001***	-0.0003	0.0028	0.079***	-0.07***
problems in finding,lack of appropriate educational facilities	-1.35e-05	0.00106*	-0.0003	-0.024***	0.154***	-0.0521
problems in finding,high competition among firms for similar workers	-9.45e-06	0.003***	-0.0005**	-0.026***	0.217***	-0.114***
problems in finding,other reasons	9.22e-07	0.00059	-0.0006*	-0.00821	0.0687	-0.056*
<i>no problems in finding</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>
exporting firm	-4.22e-06	0.000780**	-0.000126	-0.0119*	0.116***	-0.0368
<i>not exporting firm</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>
Expected turnover, by firm	1.30e-05	-0.00244**	-0.000548	0.0120	-0.332*	-0.0649
20<employee<50	7.48e-06	6.88e-05	0.000357	0.00244	-0.0890**	0.0116
50<employee<250	8.22e-06	0.0012***	0.00106**	-0.00631	-0.0113	0.0159
employee>250	1.00e-05	0.00125***	0.00127***	-0.00731	-0.0331	0.0198
<i>employee<20</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>
south	-6.69e-06	0.0007*	-4.50e-05	-0.0118*	0.0849**	0.0148
north-east	1.49e-05	-0.001***	0.0003	0.031**	-0.144***	0.097***
north-west	2.95e-06	-0.0003	0.0003	0.0118	-0.0633**	0.061**
<i>centre</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>	<i>ref cat</i>
Controls for industry	<i>yes</i>	<i>yes</i>	<i>yes</i>	<i>yes</i>	<i>yes</i>	<i>yes</i>
Observations	1285545	1299806	1299856	169498	173149	173152
Pseudo R-squared	0.443	0.600	0.461	0.345	0.371	0.391

Table 15 Probit models for employers' preferences over workers' field of study *** p<0.01, ** p<0.05, * p<0.1

Conclusions

This work shed some light on the effects of various reforms in education on attainment, fertility and labor market.

The first chapter shows the effect of reforms in compulsory education that rose minimum leaving age on attainment. The work takes into account different countries that implemented the laws in the second half of the last century.

By mean of research discontinuity design method, we show that reforms are effective in increasing average years of schooling, even if the effect varies among countries. A quantile regression approach takes into account variations of the distribution of education and reforms are shown to be most effective at the first quartile. So the reforms increased the educational level of the marginal student that would have stayed out of school without the reform. Since endogeneity is a problem common to many empirical studies that make use of human capital as an explanatory variable, we here provide further evidence in support of the use of the reforms as an instrument for education. Not only we show how reforms are effective, but also we suggest strategies that are useful for tackling the issue of identifying correctly people that are exposed to reform. So the first chapter can be considered as a ‘guideline’ for correctly measuring the variation in educational attainment caused by reforms.

In the second chapter, focused on Ecuador, we analyze the relationship between education, teenage childbearing and labor supply. Since education is endogenous with respect to early motherhood, we instrument schooling with a reform that was implemented in 1977 and made compulsory the completion of lower secondary education.

The results show that completing lower secondary schooling affects women’s age at first birth delaying motherhood after adolescence. Women’s labor supply is also positively affected by education and negatively by teenage childbearing. In addition, data suggest that to more educated women and to older mothers are associated better children outcomes, in terms of schooling. However, further evidence is needed in order to estimate precisely the size and the duration of the intergenerational effects.

From a policy viewpoint, a reform on compulsory education not only can promote schooling attendance, but also increases women’s control over fertility choices and favors women’s labor supply. These results are relevant because in Ecuador gender differences on the labor market and on schooling attendance are evident, and investment in children’s education is still low.

The final chapter is about a reform in the Italian university system, implemented in 2001 to modify the length of university degree and simplify course contents.

A unitary-tier system that lasted for four to six years was substituted with a two-tier system, lasting 3 and 2 years respectively. Students that stop after the first three years get a BA degree, while those that continue for 2 more years specialize and get a MA equivalent degree. The reform’s purposes are to uniform Italy to the European higher educational, to lower age at degree and drop out rate and increase enrolment.

Since the reform is recent, scholars mostly focused on the effects on the enrollment decision, drop out and age at degree and showed that the reform has been successful in increasing access to university and decreasing drop out.

The present analysis investigates what happened when graduates from the new ‘regime’ entered the labor market and substituted the old ‘regime’ graduates. Using a data set on employers’ preferences over prospective workers, we identify the expected demand for graduates and we compare each worker’s required skills before and after the reform.

In the years after the reform the required share of graduates increased by 0.7%: the effect is small but significant and not attributable to a change in firms’ characteristics. The demand for graduates increased mostly for clerical and technical jobs, but decreased for the managerial and scientific occupations. So more skilled workers are searched for less specialized works.

Since the reform generates two types of graduate, we investigate how employers' preferences are shaped between these two options. The choice of BA graduates is not well characterized yet, while for some jobs is required a worker with the MA degree.

Overall the reform was quite successful in creating more skilled human capital, and firms' demand for graduates reacts, even if mildly, to the increase in the number of well-educated workers.

We take into account and discuss that other concurrent events might have increased/decreased the post reform expected demand for workers. Two reforms that liberalized the use of temporary contracts were implemented in the same years, potentially modifying labor cost. However, for our purposes this is only relevant if the labor market reforms altered the relative cost of graduates with respect to non graduates. Evidence suggests that the relative cost of highly skilled human capital is unaltered or at most increased. The international 2008 crisis might have also interfered with the reform's effect, but and we did our best to exclude 2009 from the analysis. More detailed data would probably help to solve at least a part of the potential biases.

This dissertation shows that reforms in education are good policies to pursue different goals. In fact, the increase in educational attainment triggered by reforms can have spillover effects on fertility choices, women's labor supply, children investment and on skill composition of the labor force.

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