

**RETURNS TO SCHOOLING IN ITALY:
OLS, IV and Gender Differences***

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The conventional wisdom in the literature is that OLS estimates of returns to schooling are biased and inconsistent due to endogeneity in the schooling variable (Griliches 1977). A recent solution to this endogeneity problem has been found in identifying exogenous sources of variation in schooling to build a new set of instrumental variables for years of education attained (Angrist and Krueger 1991; Card 1998).

The aim of this paper is to apply new instrumental variables estimation to Italian data using the Bank of Italy data set *Indagine sui bilanci della famiglia nell'anno 1991*. IV estimation results suggest a downward bias in OLS estimates. Moreover the extent of the bias is asymmetric in regard to gender: OLS estimates indicate the usual hierarchy - i.e. female returns higher than male returns (Lucifora 1994; Blau-Kahn 1992) - whereas IV estimates suggest an opposite hierarchy in the point estimates.

Key words: human capital, returns to education, earnings.

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1 - Introduction.

Education is a significant determinant of earnings as it is one of the most important components of individual human capital (Becker 1993). Therefore returns to education are a crucial issue in applied economics.

Since the first influential specification (Mincer 1974), wage functions estimates are the appropriate tools to evaluate private returns to schooling. These estimates are widespread and they are available for a great number of countries (Psacharopoulos 1994). In Italy the number of returns to schooling estimates¹ is lower than in other industrialized countries because of lack of micro data. But the real problem of previous works is an econometric one. As Griliches 1977 points out, OLS estimates of returns to schooling are biased and inconsistent due to omitted variables and measurement errors. Therefore it is necessary to find alternative estimators.

A recent development in this direction has been found in a renewed use of instrumental variables estimators, the so-called *natural experiment* approach². The aim of the paper is to apply this technique to Italian data in order to estimate correct returns to schooling by gender. As a baseline I also perform OLS and Heckman's correction estimates. The attention to gender is due to its importance in determining labor market performance (Cain 1996; Blau-Kahn 1992) and to the evidence about gender discrimination that these estimates could offer³.

The estimation sample is derived from the Bank of Italy *Indagine sui bilanci delle famiglie italiane nell'anno 1991* (henceforward, *Bank of Italy Survey*).

Section 2 focuses on the earning function specification and presents estimates calculated by standard techniques. Section 3 briefly discusses the econometric problems and explains the IV/natural experiment solution using the Card 1998 model. Section 4 applies this solution to obtain male and female returns to schooling in Italy. Section 5 draws some conclusions.

2 - Standard Estimates.

Specification and data set.

¹ Recent works on country representative samples are: Checchi 1999 (tab. 2.7, pg. 72); Colussi 1997; Cannari-D'Alessio 1995; Erickson-Ichino 1992; Blau-Kahn 1992; Lucifora-Reilly 1990.

² Angrist-Krueger 1991 and 1992 are among the first contributions. Card 1998 is a recent and complete survey. Kane-Rouse 1993, Card 1995a, Harmon-Walker 1995, Conneely-Uusitalo 1997, Ichino and Winter-Ebmer 1998a and 1998b are applications to different countries.

³ Previous works (Flabbi 1997 and Lucifora-Reilly 1990) have found some evidence of labor market discrimination in Italy.

In order to calculate returns to schooling, I estimate a typical wage function, that is a regression of individual wages on a vector of individual characteristics such as education, experience, family background and community background.⁴

The specification, as reported in table 2.1, consists of human capital attributes (schooling and experience⁵), and of controls for the occupational grade level, the sector and the labor market in which the individual is currently working, the composition of her/his family. Controls for occupational level, sector, and local labor market concern the demand side factors that are not fully explained by human capital variables, and they allow reducing the bias arising from an imperfectly competitive labor market.⁶ Controls for the family proxy the influence of housework, particularly important in the female labor supply (Heckman-Killingsworth 1986), and the possibility for assortative mating dynamic (Becker 1991 and Lam-Schoeni 1993).

The estimation sample is extracted from the *Bank of Italy Survey* for 1991. This is the most reliable (and available) collection of personal microdata representative of the Italian population. The aim of the Survey is to understand individual financial behavior, but it also contains fundamental individual characteristics, such as education, earnings, current family position, residence, and date of birth. From the total sample I obtained a subsample of 5,734 individuals comprehensive of almost all the full-year full-time employees.⁷

Results.

Table 2.2 presents returns to schooling estimated by Ordinary Least Square and by the Heckman's correction method.⁸ The last one is aimed at solving the potential sample selection bias on the female subsample⁹, under the hypotheses that labor supply is the result of the comparison of the reservation wage (w^*) with the supplied wage (w°). More formally (Greene 1993 and Heckman 1979), we could think both the wage and the differential between reservation wage and supplied wage as functions of personal characteristics (respectively represented by the vectors x and z) and of a white noise:

$$[1] \quad \log(w) = \beta'x + \varepsilon$$

$$\text{where: } (\mu, \varepsilon) \sim N(0, 0, \sigma_\mu, \sigma_\varepsilon, \rho)$$

⁴ For a similar definition, see Willis 1986 and Lucifora 1994.

⁵ Classical reference are Mincer 1974 and Becker 1993.

⁶ In Italy we observe significant sector and labor market differentials (see for example Bettio 1988 and 1991 and Lucifora-Reilly 1990).

⁷ All the steps from the total sample to the estimation sample are reported in table A.1.1 of Appendix 1.

⁸ Complete results are found in Appendix 2.

$$[2] \quad w^o - w^* = \gamma'z + \mu$$

In this case, sample selection bias means that $\log(w)$ is only observed when the difference $(w^o - w^*)$ is positive. This implies that¹⁰:

$$[3] \quad \begin{aligned} E[\log(w) \mid \log(w) \text{ observed}] &= E[\log w \mid \mu > -\gamma'z] \\ &= \beta'x + E[\varepsilon \mid \mu > -\gamma'z] \\ &= \beta'x + \beta_\lambda \lambda(-\gamma'z/\sigma_\mu), \end{aligned}$$

where λ is the inverse of the Mill's ratio, i.e. the ratio of the probability density function to the cumulative distribution function of the standard normal distribution calculated in $\gamma'z/\sigma_\mu$, and where $\beta_\lambda = \rho\sigma_\varepsilon$. Hence, the "true" regression will be:

$$[4] \quad \log(w) [(w^o - w^*) > 0] = \beta'x + \beta_\lambda \lambda(-\gamma'z/\sigma_\mu) + v$$

therefore, ordinary least square estimates on [1] will produce biased and inconsistent estimates because of omitted variables. An easy solution is to estimate λ and then to regress $\log(w)$ on x and λ . This procedure, developed by Heckman (1979) article, is the one performed here.

As table 2.2 shows, returns to schooling estimates are in the order of 2% meaning that one year of additional schooling is correlated with an average increase of 2 percentage points in post-tax earnings. Moreover there is a slight difference between male and female returns that is reduced but not eliminated using the Heckman's correction: point estimates of female returns are higher than male returns. This is the usual result of recent empirical works on Italian labor market, as reported in the low part of the table. The same result is found in similar estimates on other industrialized countries (Psacharopoulos 1994 and Blau-Kahn 1992) or on less developed countries (Appleton-Hoddinott-Krishnan-Max 1995).

Previous works on Italy generally show higher returns. The main reason is that my specification includes more controls than the other works reported in the table. Another reason is the difference in the sample selected. Where sample and specification are more similar to mine, coefficients are in the same order (Erickson-Ichino 1992).

⁹ For a survey of this and other problems on female labor supply, see Heckman-Killingsworth 1986. Examples of standard estimates of returns to schooling on gender separated samples are Callan 1991 and Wright and Ermish 1990.

¹⁰ Using expressions [1] and [2] and the *Moments of the Incidentally Truncated Bivariate Normal Distribution Theorem* (Greene 1993, p. 707).

Heckman's correction allows solving sample selection bias on the female sample. But this is only one of the possible sources of bias in returns to schooling estimates. The next section briefly reviews and discusses the others.

3 - The Problem: Biased Returns to Schooling Estimates.

As Griliches (1977) points out, OLS estimates of returns to schooling are biased and inconsistent. However, their calculation has not been abandoned in order to evaluate sign and amount of the bias. But results are ambiguous. Griliches suggested the possibility of both upward and downward bias. Upward bias remains the conventional wisdom (Ehrenberg-Smith 1991), even if some authors imply that this is true only if one analyses wages of mature workers (Blackburn-Neumark 1993). Calculation by instrumental variables and by fixed effects gives, instead, downward bias.¹¹ The same happens when the *natural experiment* approach is applied.¹²

These contrasting empirical results are due to econometric problems: omitted variables and measurement errors could involve opposite distortions without specifying which one prevails. A general framework to describe the problem could be found if we focus on the potential endogeneity of education in the wage function.

The classical model of human capital of Becker 1967 clarifies the general idea. In the following I will use the version developed by Card 1998: this version constitutes a very useful general framework to understand distortions in returns to schooling and to interpret the instrumental variables estimates that I will show in the next section.

The *Causal Model of Returns* proposed by Card begins considering the objective function of an individual choosing her optimal amount of schooling (S)¹³:

$$[5] \quad U(w, S) = \log(w) - f(S)$$

where $f(S)$ is an increasing convex function and wages are associated to schooling by $w = w(S)$.

The first order condition states that:

¹¹ Griliches-Hall-Hausman 1978 found an IV estimate double the OLS estimate using family background variables as instruments for schooling. Angrist-Newey 1991, in comparison to an OLS estimate equal to 0.036, found a fixed-effect estimate equal to 0.080.

¹² For example, the works surveyed by Card 1995a and 1998 report an increase in the estimates in a range of 10-100% in comparison to OLS estimations.

¹³ This form of the utility function generalizes the discounted present value of wages that depends on schooling, assuming that individuals earn nothing while in school and $w(S)$ per year thereafter.

$$[6] \quad \frac{w'(S)}{w(S)} = f'(S).$$

Now assume that the economic benefit of schooling is constant (equation [7]), but that there are differences in costs or tastes for schooling (equation [8]):

$$[7] \quad \frac{w'(S)}{w(S)} = \bar{b}$$

$$[8] \quad f'(S) = r_i + kS.$$

Equation [8] is justified by imperfect financial markets (leading to higher costs for schooling for people with poorer family background) or by different tastes for schooling (Becker 1993). Therefore, higher r_i are associated to people with higher difficulties to finance schooling or with lower preferences for studying.

Using equations [6], [7] and [8] to obtain the optimal amount of schooling:

$$[9] \quad S_i^* = \frac{\bar{b} - r_i}{k}.$$

and integrating equation [7] to obtain the wage function:

$$[10] \quad \log(w_i) = \alpha_i + \bar{b}S_i = a_0 + \bar{b}S_i + a_i,$$

it is possible to determine the *two-equation system for schooling and earnings* ([10] and [9]) suggested by Card 1998 as a general framework to evaluate returns to schooling estimates. In equation [10], consistent with equation [1], the constant of integration is decomposed in a constant component (a_0) and in a random component (a_i).

Using equation [10] it is immediately possible to understand the first potential source of bias in OLS estimates: distortion from **omitted variables**. The relevant variables typically omitted in wage functions estimates are those referring to talents and individual attributes roughly summarized by the idea of "ability".¹⁴ Let a_i , a person-specific unobservable variable, represents individual

¹⁴ "Ability" in these context means only *the capacity to earn higher wages at the same level of schooling*.

"ability". If $E(a_i | S_i)$ is linear¹⁵, the omission of variable a_i from an Ordinary Least Squares regression leads to the probability limit:

$$[11] \quad p \lim b_{OLS} = \bar{b} + \lambda_0 \quad \text{where:} \quad \lambda_0 = \frac{\text{Cov}(a_i, S_i)}{V(S_i)} = -k \frac{\sigma_{ar}}{\sigma_r^2},$$

and where σ_r^2 , σ_{ar} are respectively the variance of r_i and the covariance between a_i and r_i . If a_i and r_i are correlated, the OLS estimate is inconsistent with a bias equal to λ_0 .¹⁶ In particular if $\sigma_{ar} < 0$, the estimates would be upward bias. This negative correlation means that the higher the "ability" the lower the marginal cost of schooling. There are two possible explanations of this result. Interpreting r_i as tastes, it means that more "able" individuals have higher preference for schooling. Interpreting r_i as discount rate, it means that individuals facing lower discount rate are more "able". Assuming imperfect financial markets, poorer families are likely to face higher discount rate, therefore the previous implication means that individuals with higher family background are more "able".¹⁷ This correlation opens the important issue of the influence of family background on wages and returns. I will discuss this problem later, speaking about instrumental variables.¹⁸

Upward bias due to omitted variables was the conventional wisdom at the end of the 70s and many works tried to proxy ability, for example using IQ tests. Willis and Rosen 1979 is an example of a complete structural specification of this kind of wage function. But this solution is not satisfactory because these tests are a very incomplete description of the "ability" we are interested in.¹⁹ Moreover the lack of data makes it an impossible correction for the Italian labor market.

The second source of distortion in OLS estimates are *measurement errors* in the schooling variables. It means that observed years of schooling are equal to real years of schooling plus a stochastic error term:

¹⁵ For a complete analytical treatment, see Appendix A in Card 1998.

¹⁶ Following Card 1998 and considering that large samples are usually involved in these kinds of estimates, I define *bias* as "the difference between the probability limit of an estimator [...] and typically the average marginal return to schooling in the population" (Card 1998, footnote 21).

¹⁷ Again, pay attention that "ability" means only *the capacity to earn higher wages at the same level of schooling*.

¹⁸ Griliches 1977 is one of the first attempt to survey the issue. Willis and Rosen 1979 is an example of a structural model estimate considering multidimensional ability, family background and returns to schooling. Cannari and D'Alessio 1995 surveys possible recent explanations of the family background/ability/schooling linkage. Card 1998 summarizes the most recent development using the model presented in this paper.

¹⁹ See Griliches 1979, Chamberlain 1977 and Chamberlain and Griliches 1977. Willis and Rosen 1979 also raise some doubts where they say that there are "*questions about what is that this test supposedly measures*". Recent criticisms

$$[12] \quad S_i^{OBS} = S_i + \varepsilon_i \quad \text{with:} \quad E(\varepsilon_i) = 0; E(\varepsilon_i, S_i) = 0; E(\varepsilon_i^2) = \sigma_\varepsilon^2.$$

Assuming no omitted variables bias (i.e. $a_i = \bar{a} = 0$), equation [12] implies the following estimation function:

$$[10a] \quad \log(w_i) = a_0 + \bar{b}S_i^{OBS} - \bar{b}\varepsilon_i,$$

leading to:

$$[13] \quad p \lim b_{OLS} = \bar{b} + \xi \quad \text{where:} \quad \xi = \frac{Cov(-\bar{b}\varepsilon_i, S_i^0)}{V(S_i^0)} = -\bar{b} \frac{\sigma_\varepsilon^2}{\sigma_s^2 + \sigma_\varepsilon^2},$$

that is a downward bias estimates due to *attenuation*, i.e. if the variability in the measurement error tends to ∞ , the bias tends to \bar{b} and $p \lim b_{OLS}$ tends to 0. Empirical studies suggest that the amount of bias due to measurement errors could even offset the positive bias due to omitted variables.²⁰

To sum up, OLS estimates are inconsistent and it is not clear if they are downward or upward bias. Therefore researchers have decided to change estimation method, using instrumental variables estimation to solve the endogeneity in the schooling variable²¹.

Using the two equation system described by equations [9] and [10], instrumental variable estimation is appropriate under the existence of some variables Z correlated with the endogenous variable S , but not with the residual a_i . In this model, correlation between Z and S is only possible through r_i because \bar{b} and k are constant across the population.

A linear version of these conditions is:

$$[14] \quad S_i = \frac{\bar{b} - r_i}{k} = \frac{\bar{b} - (Z_i\pi + \eta_i)}{k} = \frac{\bar{b} - \eta_i}{k} - Z_i \frac{\pi}{k}$$

$$[15] \quad Cov(a_i, Z_i) = 0,$$

underline that the logical abilities measured by these tests are background dependent, so we cannot use them as an unbiased measure for ability.

²⁰ Empirical research has generally shown a reliability of self-reported schooling of about 90%. See for example Ashenfelter-Rouse 1998.

²¹ An old influential survey is Griliches 1979, an up-to-date survey is Card 1998. See in the following for more references.

leading to:

$$[16] \quad p \lim b_{IV} = \frac{Cov(\log y, Z)}{Cov(S, Z)} = \frac{Cov(a_0 + a_i + \bar{b}S, Z)}{Cov(S, Z)} = \bar{b},$$

that is a consistent estimate of the average return to schooling.

In this way the problem turns to be the search for valid instruments. There is a long tradition in using family background variables, typically the level of parent's schooling, as valid instruments²². This idea is based on the observation of persistence across generation about the level of schooling²³ and it is theoretically justified by involuntary transmission of human capital.

Criticisms against considering family backgrounds as appropriate instruments were developed both on the theoretical and the empirical ground. In particular in the intergenerational mobility literature it is a conventional wisdom the idea of a direct influence of background on wage. One of the first models in this direction is Becker and Tomes 1986, a human capital model that specifies a wage function comprehensive of family background variables as regressors. This arises from an intertemporal maximization of the children's utility made by the parents. Montgomery 1991 explains this connection by a *networking* mechanism, i.e. high level families have friends and connections which enable them to find a good job for their children. On the empirical side we find corroboration for these ideas: family background variables are highly significant when directly inserted in the wage function, even if years of schooling are introduced within the regressors.²⁴

In terms of Card's model, it is possible to represent these results setting a linear correlation between schooling (S) and family background (F):

$$[17a] \quad F_i = \alpha_{10} + \alpha_S S_i + e_{1i}$$

$$[17b] \quad S_i = \alpha_{20} + \alpha_F F_i + e_{2i},$$

where e_{1i} and e_{2i} are orthogonal respectively to S_i and F_i .

²² See Colussi 1997 and Cannari-D'Alessio 1995 for Italy; Griliches-Hall-Hausman 1978 and Griliches 1979 for United States; Card 1995a and 1998 for recent surveys.

²³ See for example box 3.1 in Checchi 1999.

²⁴ This idea is in Willis-Rosen 1979. A survey about the United States is Corcoran-Gordon-Laren-Solon 1990. An application considering gender differential is Neumark 1988. Recent applications on the Italian case are Checchi-Ichino-Rustichini 1994 and 1999.

Under these conditions, it is possible to obtain an unbiased estimate, as in equation [16], if F_i is uncorrelated with a_i . But, as previously shown, there is a strong suspicion of correlation between F_i and a_i and, again as previously shown about the ability problem, there is also the possibility of correlation between S_i and a_i .²⁵ Using the linear projection of a_i on S_i and F_i :

$$[18] \quad a_i = \lambda_1(F_i - \bar{F}) + \lambda_2(S_i - \bar{S}) + u_i$$

it is possible to obtain three estimators, as suggested in Card 1998, leading to the correspondent probability limit:

(i) the OLS estimate on S :

$$[19] \quad p \lim b_{OLS}^S = \bar{b} + \lambda_2 + \lambda_1 \alpha_S$$

(ii) the IV estimate using F as instrument of S :

$$[20] \quad p \lim b_{IV} = \bar{b} + \lambda_2 + \lambda_1 \frac{1}{\alpha_F}$$

(iii) the OLS estimate using both S and F as regressors:

$$[19] \quad p \lim b_{OLS}^{S,F} = \bar{b} + \lambda_2.$$

All the three estimators are upward biased, however using both S and F as regressors it is possible to obtain the lowest bias. Hence using family background variables as instruments does not seem a

²⁵ This happens in the model through r_i : a_i is correlated to r_i that is a linear function of S_i . If it does not happen, the linear projection of a_i su F_i is:

$$a_i = \lambda_1(F_i - \bar{F}) \quad \text{with:} \quad \lambda_1 = \frac{Cov(a_i, F_i)}{V(F_i)}$$

leading to:

$$p \lim b_{IV} = \bar{b} + \lambda_1 \frac{1}{\alpha_F}, \text{ that is an upward bias estimate.}$$

satisfactory solution²⁶ and it would be better to directly insert these variables between the regressors in the wage function.

The recent literature has proposed alternative kind of instruments that could be labeled as *natural experiment approach* instruments.

The basic idea of the approach is to approximate with real data some "ideal" experiments in which all the conditions are under control. In the wage functions context, a sufficient approximation could be the following. First, consider a population representative sample about which a sufficient amount of control characteristics are known. Second, try to find an exogenous event able to induce an education increase in a randomly selected subsample (*the treatment group*) without affecting the rest of the population (*the control group*). Third, evaluate the induced variation on the treatment group's wages as a causal effect of schooling on wages.

Angrist and Krueger 1992 is perhaps the closest example to this ideal experiment. The Vietnam-Era Draft Lottery is the event that "assigns" more schooling to people randomly selected.²⁷ Nevertheless it is difficult to replicate similar events. Without "pure" random assignment "*one needs to identify a causal determinant of schooling that can be legitimately excluded from earnings equation*" (Card 1995b).

In the recent literature a useful source of these "causal determinant" has been found in the institutional features of the school system. The kind of feature involved basically are: college proximity (Card 1995a; Kane and Rouse 1993; Conneely and Uusitalo 1997); cohort effect due to compulsory schooling legislation (Angrist and Krueger 1991; Harmon and Walker 1995), and cohort effect due to the consequences of the Second World War (Ichino and Winter-Ebmer 1998a and 1998b).

This methodology seems to be the more promising way to solve the endogeneity problem because the instruments involved seem to be an effective exogenous source of variation. Therefore I apply it to the Italian labor market using instruments directly inspired by these previous works.

4 - A Solution: Natural Experiments Estimates.

The Instruments.

²⁶ It is worth to note that, under the plausible hypothesis of $0 < \alpha_S < 1$ and $0 < \alpha_F < 1$, the IV estimate using family background as instruments reports the worse bias.

²⁷ The Vietnam-era draft lottery randomly assigned priority for military service to draft-age men. As a result, many men who were at risk of being drafted managed to avoid military service by enrolling in school and obtaining an educational deferment. (Angrist-Krueger 1991b)

The two instruments used in the estimates are: the indicator *province* that replicates the idea of college proximity, and the indicator *reforms* that exploits some cohort effects²⁸.

The idea of college proximity indicators is the following: people living far from a College face higher University costs because they cannot live together with their family or because they have to face high transportation costs. This higher cost could be considered the exogenous event able to influence the amount of schooling. Applying this indicator to Italy I choose the province as the appropriate geographical extent. Provinces are large enough to yield a significant cost increase and they allow for a precise connection between individual information about personal characteristics and aggregate information about the geographical distribution of Universities. To apply the instrument I had to extract a subsample of the original estimation sample because it is necessary to know place of birth and residence of the individuals²⁹.

Following Card 1995a, I preliminary verify the influence of geographical college proximity on schooling. First, I regress completed years of schooling on some personal characteristics variables.³⁰ Second, based on this estimate I predict completed years of schooling for people who lived in province with or without Universities. Third, I split the total sample in two subsamples: one containing people who lived in province with Universities, the other one containing people who lived in province without Universities. Fourth, I divide the two subsamples in four quintiles based on the predicted years of schooling and I calculated the average years of schooling effectively completed for every quintiles. The results are reported in figure 4.1 and 4.2 where the lines with circles refer to people without any University in their province of residence.

The two figures show that University proximity yields to equal or higher years of completed schooling both for the female and the male sample. The differential is significantly higher in the first quintile for both samples. This is not surprising since in the first quintile we find people with a low propensity to get higher education. These are individuals that could consider the cost of distance from the University one of the major determinant of the decision and for whom the years of schooling completed are more sensible to the province of residence.

²⁸ A previous version of these instruments is applied in Flabbi 1997 in order to calculate an index of gender discrimination for Italy.

²⁹ Sample for IV estimation is composed by 1055 women and 1929 men. I missed some observation because of missing variable on the place of birth variable but the stronger limitation is due to the cohort effects that I will explain in the following speaking about the instrument *reform*.

About the instrument *province* the information we would like to have is the province of residence when the individual chooses if going to the University or not. Unfortunately the Bank of Italy *Survey* reports only the current residence and the place of birth, so I assume that the place of birth is the better proxy to evaluate University proximity. It is worth to note that the use of the place of birth allows to reduce a potential endogeneity problem in the decision of the place of residence of the family.

³⁰ These are comprehensive of: age, dummies about family position (such as head of family, partner, child), civil status, dummies for macroregion of residence, dummy for the dimension of the town of residence.

The instrument *province* is a binary variable assuming value 1 if the province where the individual was potentially³¹ living at 19 years had a University and assuming value 0 elsewhere³².

The instrument *reform* considers cohort effects due to the Italian reforms of the educational system. The first relevant reform (the No. 1859 act, December 31, 1962) prescribes the unification of the previous *scuola media* and *scuola di avviamento professionale* in a single compulsory *scuola media*.³³ The second (the No. 910 act, December 11, 1969) makes University entrance independent from the high school degree (*diploma di scuole superiori*³⁴) obtained. Both reforms lead to a more liberalized access to higher education and could be seen as exogenous events able to increase the total amount of education in the population. The instrument *reform* is a binary variable assuming value 1 if the individual had the possibility to attend the correspondent school level after the relative reform. The problem with this indicator is the cohort effect³⁵: the estimation sample contains people that are currently working, but the individual school level influences the exit age from the working condition. On average, people with higher education stop working later leading to an overrepresentation of people with schooling above the average in the subsample of elderly workers. Using the instrument *reform* on the complete sample it would be impossible to explain the variability in schooling for this subsample. So I choose to reduce the sample by year of birth. Considering that the two reforms start to be effective on people born after the 1950, I choose people born between 1945 and 1962³⁶. I preliminary verify the influence of the reforms on schooling by the probit estimations reported in table 4.1. The table shows a positive influence of *reform* on the probability to reach a high school level and confirms the preliminary intuition.

Results.

Estimated returns to schooling appear in table 4.2. The first column reports the instruments used, and indicates usual statistics and tests. The second one indicates the estimation methods while the others show estimated coefficients and standard errors. Table 4.2 reports only returns to schooling, but complete results are found in the Appendix 2.

³¹ As noted above, this "potentially" means that I am deducing this information from the place of birth and date of birth of the individual.

³² In Italy Provinces were changing and new Universities were established during the period: I consider the appropriate Provinces and Universities for the year of reference.

³³ This is the secondary level of the compulsory education in Italy. It is attended after the first five years of *scuola elementare*, i.e. when students are about 10-14 years old.

³⁴ The *scuola superiore* is attended after the compulsory school when students are 14-19 years old. In the previous system only some kind of *scuola superiore* guaranteed a free access to University, e.g. the *licei*.

³⁵ People with the "possibility to attend the correspondent school level after the relative reform" means in this case "people born in 1951 or afterwards".

³⁶ The upper bound is introduced to allow individuals to complete their education.

Usual statistics indicate that IV estimates are more imprecise than OLS estimates, anyway they are comparable with other studies using instrumental variables. The Wu-Hausman test rejects exogeneity on the male sample, but not on the female sample. However (as it will shown later), this supposed exogeneity of schooling in the female wage function is likely to be the result of different sort of bias and not the signal of absence of endogeneity. Similar results are implied by the Sargan test using only the instrument *Reform*, while using *Reform* and *Province* leads to a Sargan test that does not reject the validity of the instruments.

Two effects should be stressed about the estimation results:

- the systematical increase in the coefficients when estimated by instrumental variables;
- the fact that female returns are higher than male returns in OLS estimates and lower in IV estimates.

The first one is the common result of empirical works using instrumental variables based on institutional features of the school system³⁷. The second one is a novel result because it suggests a different hierarchy from previous works on Italy, as shown in table 2.2.

Interpretation of results.

The downward OLS bias implied by IV estimates could arise, as outlined before, from the *attenuation* effect of a measurement error in the schooling variables. But also a distortion from omission of the variable "ability" could lead to a similar result. In terms of equation [11] it means that $\sigma_{ar} > 0$. Interpreting r_i as tastes, it means that more "able" individuals have lower preference for schooling. Considering that "able" means the capacity to earn higher wages, these preferences could be justified by the higher opportunity costs faced by the "able" individuals.

But there is another possible explanation of the higher IV estimates. It refers again to Card 1998 and it requires introducing *heterogeneity in the economic benefit* of schooling, as well as heterogeneity in costs or tastes for schooling.

In terms of the previous model, it means to remove the hypothesis of constant economic benefit in equation [7], leading to:

$$[7a] \quad \frac{w'(S)}{w(S)} = b_i - hS \quad \text{with: } h > 0$$

³⁷ All works previously quoted find this result. The survey reported in Card 1995a or in Card 1998 indicates IV estimates 10/100% higher than OLS estimates.

where b_i is the individual-specific component and where the hS indicates decreasing returns to schooling. It is assumed that b_i is known by the individual i and that it has mean \bar{b} and a joint distribution with r_i across the population.

The first order condition states that:

$$[9a] \quad S_i^* = \frac{b_i - r_i}{k + h},$$

leading to the following marginal return to schooling for each individual i at the optimal amount of schooling S^* :

$$[20] \quad \beta_i = b_i - hS^* = b_i - h \left(\frac{b_i - r_i}{k + h} \right) = \left(\frac{k}{k + h} \right) b_i + \left(\frac{h}{k + h} \right) r_i.$$

Equation [20] shows that the variation of returns across the population is due to two individual-specific components: a benefit/ability component (b_i) and a cost component (r_i).

Now assume the existence of an exogenous event that reduces the marginal cost of schooling only for a subsample of the population. Therefore it is possible to define an instrumental variable Z_i , assuming value 1 on this subsample (the *treatment* group) and value 0 on the rest of the population (the *control* group). Following again Card 1998, it could be described by:

$$[9a'] \quad S_i^* = \frac{b_i - r_i}{k + h}, \quad \text{if } Z_i = 0$$

$$[9a''] \quad S_i^* = \frac{b_i - \theta r_i}{k + h}, \theta \in (0, 1), \quad \text{if } Z_i = 1$$

Let $r_i = \bar{r} + \eta_i$ and assume that the random variables (b_i, r_i) have the same distribution in the treatment and in the control group. Rearranging equation [9a] to isolate random and constant components, it follows that:

$$[9b'] \quad S_i^* = \frac{(\bar{b} - \bar{r})}{k + h} + \frac{(b_i - \bar{b} - \eta_i)}{k + h}, \quad \text{if } Z_i = 0$$

$$[9b''] \quad S_i^* = \frac{(\bar{b} - \theta \bar{r})}{k + h} + \frac{(b_i - \bar{b} - \theta \eta_i)}{k + h}, \quad \text{if } Z_i = 1$$

and it is possible to obtain the probability limit:

$$\begin{aligned}
[21] \quad p \lim \beta_{IV} &= \\
&= \frac{E[\log(w_i)|Z_i = 1] - E[\log(w_i)|Z_i = 0]}{E[S_i|Z_i = 1] - E[S_i|Z_i = 0]} = \bar{\beta} + \frac{(1-\theta) \left[\sigma_\eta^2 \frac{h}{k+h} + \sigma_{b\eta} \left(1 - \frac{h}{k+h} \right) \right]}{(k+h) \left(\frac{\bar{b} - \theta \bar{r}}{k+h} - \frac{\bar{b} - \bar{r}}{k+h} \right)} = \\
&= \bar{\beta} + \frac{1}{\bar{r}} \left(\frac{\sigma_\eta^2 h + \sigma_{b\eta} k}{k+h} \right).
\end{aligned}$$

Equation [21] shows that the bias is zero only if $\eta_i = \bar{\eta}, \forall i$, i.e. there is no heterogeneity in r .³⁸ Otherwise the bias is different from zero because of imperfect financial market ($\sigma_\eta^2 > 0$) and heterogeneity in the returns ($\sigma_{b\eta} \neq 0$).

The reason of the bias is clarified considering that, under the previous assumptions, an instrument that reduces the cost of schooling is more effective on people with higher marginal cost of education. The intuition is straightforward: if the cost component plays a minor role in an individual school choice, then this individual would be virtually unaffected by an exogenous event that influences costs.³⁹ This means no random selection of the treatment group, violating the necessary assumption for IV and leading to an inconsistent estimation. The bias is positive if the average marginal return in the treatment group is higher than the average marginal return in the overall population ($\bar{\beta}$). The bias is negative otherwise. The same idea is formalized in the Local Average Treatment Effect literature (Imbens-Angrist 1994 and Angrist-Imbens-Rubin 1996): the IV estimate is the average return to schooling for people that acquire more education only because of this particular exogenous event and that would have not acquired additional education in the absence of this particular event.

My estimations represent a similar situation: the two indicators used as instruments affect the amount of schooling reducing its cost⁴⁰ and IV returns are higher than OLS estimated returns. In the light of equation [21], it is possible to interpret this result as an upward bias in IV estimates (instead

³⁸ Under this condition, it follows that $\sigma_\eta^2 = \sigma_{b\eta} = 0$ and $p \lim \beta_{IV} = \bar{\beta}$, i.e. a consistent estimate of the average marginal return to schooling, calculated at the average optimal amount of schooling.

³⁹ A formal proof is in Card 1998, pg. 25-28.

⁴⁰ About the instrument *province* the reason is straightforward. About the instrument *reforms*, it seems reasonably to assume the easier access to higher education similar to a reduction in costs of additional years of schooling.

of a downward bias in OLS estimates). It holds if the treatment group has marginal return above the average marginal return in the overall population. Under the plausible assumption that the higher the returns the lower the marginal disutility⁴¹, i.e. $\sigma_{b\eta} < 0$, this result seems counterintuitive but it becomes clear considering equation [20]. The marginal return is the difference between the individual component b_i and the weighted amount of schooling hS . Therefore the results are consistent with a treatment group composed by individuals with individual component b_i below the average, but with marginal returns β_i above the average induced by a sufficiently low amount of optimal schooling S^* . A low initial amount of schooling in the treatment group seems a plausible assumption in Italy where the amount of education at the tertiary level is traditionally low, in particular on the cohorts potentially affected by the instruments.⁴²

The other main result is the asymmetric increase in the male/female estimated returns. In contrast with the popular result of higher female returns to schooling, I find female returns lower than male's if the IV estimator is applied. The easiest explanation for the asymmetric effect could be an asymmetric amount of bias on the two subsamples.

Such a result could arise from a higher variance in the male error term. If the "real" coefficient is higher for males, the OLS estimates could suggest just the opposite when the downward bias on the male sample is sufficiently larger than the downward bias on the female sample. A similar argument is applicable to the "ability" bias if more able individuals terminate early their education. Moreover it is possible a mixture of both of them. As an example, consider the same amount of measurement error bias on both the male and the female sample. Then assume a higher ability bias for female workers: the net effect is a downward bias on both samples but with a larger decrease on the male samples. Some arguments could support the assumption of a higher omitted variable bias for the female sample. On the empirical side, it is interesting to note that female wage function estimates generally report a lower R^2 than males.⁴³ On the theoretical side it is important to consider the different labor participation pattern between men and women arising from an intermittent female labor supply.⁴⁴ My earning function estimates do not include the productive attribute *intermittence* within the regressors⁴⁵ and this effect could lead to an omitted variables bias on the female estimated return.

⁴¹ It means that higher returns are more frequently associated with higher tastes for schooling and /or lower discount rate.

⁴² See for example table 1.3 and table 1.4, pg. 19 in Checchi 1999.

⁴³ For Italy, see Erickson-Ichino 1992; for Great Britain, see Wright-Ermisch 1990; for U.S.A. and other countries, see Blau-Kahn 1992.

⁴⁴ The problem of the female intermittence is assessed in the human capital (e.g. Blau-Ferber 1991), in the efficiency wages and in the dual labor market (e.g. Bettio 1990) literature on gender discrimination.

⁴⁵ Wright-Ermisch 1990 is an empirical work where the introduction of intermittence variables within the regressors leads to an increase in the female R^2 , even if it remains lower than the male one.

But also this result could be interpreted in the light of Card 1998. As shown before, if we assume heterogeneity in the economic benefit of schooling, then the reverse hierarchy holds only on the two treatment groups. But why should the male treatment group have higher returns than the female treatment group?

A possible explanation is to assume *pre-labor market* discrimination (D'Amico 1987; Cain 1986). This idea refers to the possible gender discrimination arising in the acquirement of productive characteristics before the entrance in the labor market. The amount of schooling is just one of the typical productivity attribute on which this kind of discrimination could hold. In this context "pre-labor market discrimination" means that families choose to favor male children instead of female children in the acquirement of education. Therefore we could observe a lower average amount of schooling in the male treatment group because the lower cost induced by the exogenous event is sufficient to induce families to let the male child continue his studies, but not the female child. The exogenous event is effective on the female child only if the family is richer, so the female treatment group is composed by individuals coming from on average richer families. But if this is true they presumably have a higher amount of optimal schooling S^* in equation [20] and therefore lower marginal returns, leading to the results I have found.

5 – Conclusions.

My earning function estimates on the Bank of Italy Survey in 1991 suggest average returns to schooling of about 2%. This result is obtained using standard technique (OLS and Heckman's correction) and it is comparable with previous works with a similar specification on country representative samples⁴⁶.

But standard techniques do not solve the major problem of returns to schooling estimates: endogeneity in the variable years of education (Griliches 1977).

A recent development to solve this problem has been found in a renewed use of IV estimators (Angrist-Krueger 1991 and 1992; Card 1998). Applying this technique to Italy leads to the same result of previous works on other countries: OLS estimates could be downward biased⁴⁷. Moreover running regressions on male and female samples, I have found a novel result: the amount of bias is asymmetric by gender and it could hide the real hierarchy between the two coefficients (in point estimates).

⁴⁶ See table 2.2.

⁴⁷ IV estimates are in the order of 3/5%. See table 4.2 and Appendix 2 for complete results.

Using Card 1998 *Casual Model of Returns* it is possible to assess an alternative explanation. In the presence of heterogeneity both in the economic benefit of schooling and in costs or tastes for schooling, IV returns are representative only for a subsample of the population (the treatment group). If the subsample is not randomly selected the difference between OLS estimates and IV estimates could be only a difference between average returns in the entire population and average returns in the treatment group. The instruments I have used (the University proximity and the reforms of the educational system introduced in Italy during the 60s) are likely not to randomly select the treatment group because they are more effective on individuals with poor family background.

This conclusion reduces the generality of the results and it leaves the open issue of what is the "real" amount of returns to schooling in Italy. On the other hand, these estimates clarify that previous estimates of Italian returns have to be considered with great caution: OLS bias could be significant and it could even hide the real hierarchy between male and female returns.

A more constructive conclusion is that at least on the treatment group these techniques estimate unbiased returns. Therefore IV returns could be useful tools to draw policy considerations because they measure the effect of policy relevant exogenous events (a new university or a reform of the educational system) on people that these policies are likely to target (people that acquire more education only because of these particular exogenous events)⁴⁸.

⁴⁸ This interpretation of IV estimates is stressed by the Local Average Treatment Effect literature (Imbens-Angrist 1994; Angrist-Imbens-Rubin 1996; Ichino and Winter-Ebmer 1998b) and by Heckman 1997.

APPENDIX

APPENDIX 1: The estimation sample.

Estimates are performed on a sample of 5,734 individuals extracted by the Bank of Italy *Indagine sui bilanci delle famiglie italiane nell'anno 1991*. Table A.1.1 presents the sample was obtained. The first limitation is trivial. The second one comes from the low reliability of the self-employed earnings. As calculated by Brandolini-Cannari 1994 the *Bank of Italy Survey* seems to underestimate the self-employed earnings of about 50 percentage points. The third limitation is necessary because we have all the relevant information only on individuals with family position classified as head of family, partner or child. Then I had to limit my analysis on people working full-time throughout the year because the Survey only reports the annual wage. Finally some errors in the raw data force me to eliminate a 0.6% of the sample.

Finally I got an estimation sample of 5,734 individuals with 1,933 women and 3,801 men.

APPENDIX 2: Complete estimation results.

All the estimations are performed by the statistical package STATA.

Table A.2.1 reports complete results of standard estimations. OLS results suggest higher female returns on all human capital variables (schooling, experience and tenure), but lower female returns on grade levels. Proxies for houseworks show an interesting result: children significantly affect only male wages.⁴⁹ An explanation might be that they directly affect the female labor supply.⁵⁰ Partner's wage is significant in both regressions, but with opposite influence: it is positive in the female wage function and negative in the male wage function. Heckman's correction results do not confirm sample selection bias on the female sample because the Mill's ratio λ is not significant.

Table A.2.2 shows complete IV and OLS results. Samples include all the employees working full-time all the year, born between 1945 and 1962, and with no missing values on the place of birth variable.

⁴⁹ The children variables are jointly significant over the 99%.

⁵⁰ The probit estimation of the female labor supply (available upon request) confirms this intuition: children variables negatively affect the probability of labor supply.

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TAB. 2.1: Wage Function Specification: explanatory variables and economic theory.

Economic Theory:	Explanatory Variables:
General Human Capital.	<ul style="list-style-type: none">• Schooling: years.• Working experience: years in the labor market and dummy=1 if previous working experience.
Specific Human Capital.	<ul style="list-style-type: none">• Years with the current employer (tenure).
Job characteristic.	<ul style="list-style-type: none">• Grade level: dummies (4).• Sector of activity: dummies (5).
Labor Market characteristics.	<ul style="list-style-type: none">• Macroregion of residence: dummies (3) (North-Center-South).• Dummy=1 if resident in a big town.
Family characteristics.	<ul style="list-style-type: none">• Children: dummies for presence and age of children (3), two variables for number of children at various ages.• Partner: dummy=1 if partner cohabitant, its intersection with partner's earning.• Position in the family: dummy=1 if head of family.

Note: In parentheses the number of dummies.

TAB. 2.2: Returns to Schooling Estimated by OLS and Heckman's Correction: results and previous works - Italy.

Method		Women			Men		
		Coeff. (st. err.)	F	R ²	Coeff. (st. err.)	F	R ²
Results:		N=1933			N=3801		
	Year						
OLS ¹	1991	0.020 (0.002)	44.6	0.37	0.017 (0.002)	128.8	0.47
Heckman's correction ¹	1991	0.019 (0.002)	48.5	0.37			
Previous works ² :							
Authors	Year						
<i>Data-set</i> ³							
Cobalti-Schizzerotto 1995 ⁴ <i>Italian Social Mobility Survey</i>	1985	0.041			0.033		
Erickson-Ichino 1992 ⁴ <i>Bank of Italy Survey</i>	1987	0.028			0.017		
Blau-Kahn 1992 <i>Bank of Italy Survey</i>	1987	0.052 (0.003)			0.040 (0.002)		
Lucifora-Reilly 1990 <i>ENI-IRI Survey</i>	1985	0.040 (0.001)			0.036 (0.001)		
Colombino-Del Boca 1990 ⁵ <i>Survey in Turin</i>	1979	0.207 (0.020)			0.242 (0.017)		

Notes:

¹ Dependent variable = log of annual earning less tax plus non monetary integrations. Regressors include years of schooling completed and: working experience, linear and quadratic, tenure, a dummy=1 if previous working experience, 4 grade level dummies, 5 sector of activity dummies, a dummy=1 if resident in a big town, 3 dummies for the region of residence, 3 dummies for presence and age of children, two variables for the number of children at different ages, a dummy=1 if partner cohabitant and its intersection with partner's earning, a dummy=1 if head of family. Samples include all the employees working full-time all the year. Heteroskedasticity-consistent standard errors following White 1980. Complete estimation results are found in Appendix 2.

² I selected the previous works on the Italian labor market in which: (i) the sample is country representative (except Colombino-Del Boca 1990) (ii) the specification is at least comprehensive of schooling and working experience (iii) estimates are performed on male and female samples.

³ The *Italian Social Mobility Survey* is performed by a group of Italian sociologists (M. Barbagli, V. Capecchi, A. Cobalti, A. de Lillo, A. Schizzerotto): the dependent variable is an index of social prestige. The *Bank of Italy Survey* is the data-set I also used: the dependent variable is the annual earning less tax plus non monetary integrations. The *ENI-IRI Survey* is the ENI-IRI *Indagine sulle retribuzioni di fatto* performed on a sample of 100 Italian firms: the dependent variable is the annual gross wage. The *Survey in Turin* is a sample of 1,000 couples living in Turin, therefore, it is not a nationally representative sample. I choose to report this work because it is one of the few Italian works using the Heckman's correction.

⁴ Reported values are my calculations on dummies coefficients.

⁵ Female returns are estimated by Heckman's correction.

FIG. 4.1: Average completed years of schooling by quintiles of potential schooling – male.

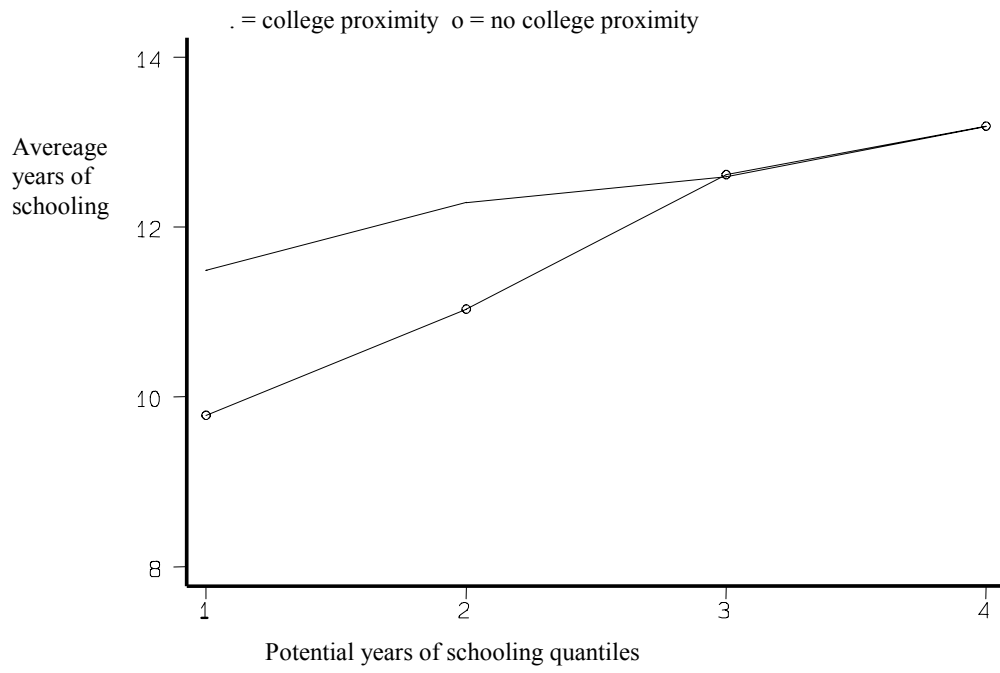
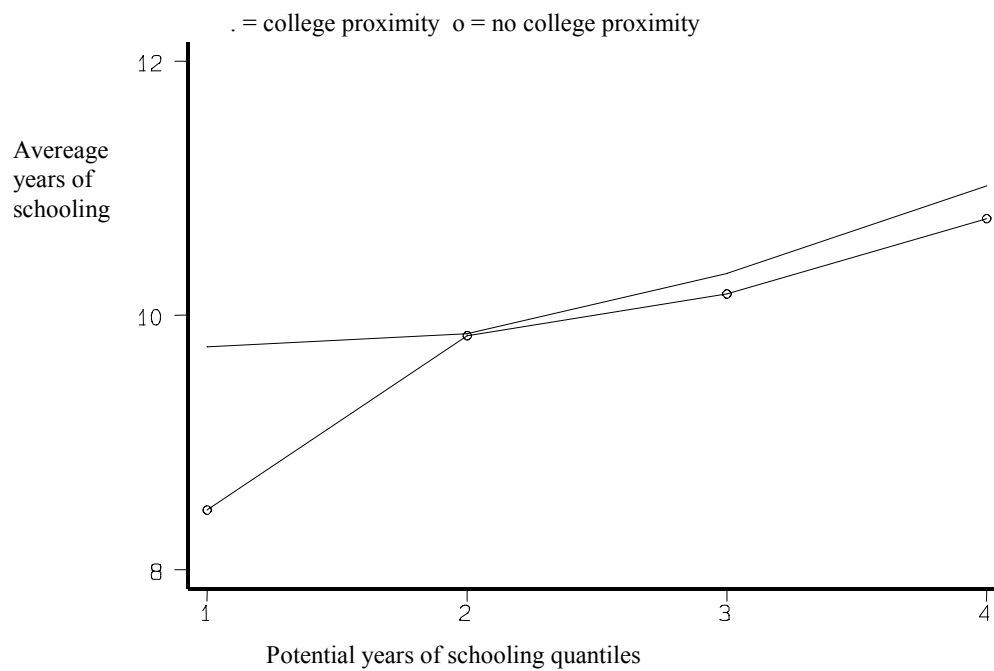


FIG. 4.2: Average completed years of schooling by quintiles of potential schooling – female.



TAB. 4.1: Influence of the instrument reform on the probability to reach an high education level. Probit estimates - Italy - 1991.

Dependent variable	<i>Women</i>			<i>Men</i>		
	N = 1055			N = 1929		
	Coeff. (<i>st. err.</i>)	chi2	PseudoR ²	Coeff. (<i>st. err.</i>)	chi2	PseudoR ²
<i>Over Average</i>	0.426 (0.084)	25.4	0.020	0.209 (0.059)	12.5	0.005
<i>Diploma</i>	0.379 (0.084)	20.3	0.016	0.117 (0.059)	3.9	0.002

Note: Dependent variable *Over Average* =1 if the individual has an education above the moving average over 3 cohorts; dependent variable *Diploma* =1 if the individual has an education corresponding to high school completed or more. A constant is inserted between the regressors. Samples include all the employees working full-time all the year, born between 1945 and 1962, and with no missing values on the place of birth variable.

TAB. 4.2: Returns to Schooling Estimated by OLS and Natural Experiments. - Italy - 1991.

		Women		Men	
		Coeff.	(st. err.)	Coeff.	(st. err.)
		N = 1055		N = 1929	
	OLS	0.023	(0.002)	0.017	(0.002)
F		23.3		51.4	
R ²		0.34		0.40	
Reform	IV	0.030	(0.012)	0.053	(0.012)
F		19.2		40.3	
R ²		0.33		0.29	
Hausman Test		0.32		10.89	
Sargan Test		0.00		0.00	
Reform and Province	GIVE	0.033	(0.012)	0.050	(0.011)
F		19.2		41.4	
R ²		0.32		0.31	
Hausman Test		0.65		10.4	
Sargan Test		2.2		0.53	

Note : Dependent variable = log of annual earning less tax plus non monetary integrations. Regressors include years of schooling completed and: working experience, linear and quadratic, tenure, a dummy=1 if previous working experience, 4 grade level dummies, 5 sector of activity dummies, a dummy=1 if resident in a big town, 3 dummies for the region of residence, 3 dummies for presence and age of children, two variables for the number of children at different ages, a dummy=1 if partner cohabitant and its intersection with partner's earning, a dummy=1 if head of family. Samples include all the employees working full-time all the year, born between 1945 and 1962, and with no missing values on the place of birth variable. Heteroskedasticity-consistent standard errors following White 1980. Complete results are reported in appendix 2.

TAB. A.1.1: How to get the estimation sample from the complete *Bank of Italy Survey* dataset.

Limitations	Number of individuals
Total raw sample	24,998
• Workers	- 16,456
• Employee	- 2,100
• Head of family, partner or child	- 127
Subtotal	6,315
Working:	
• All the year round	- 353
• Full-time	- 192
Subtotal	5,770
Deleted if:	
• More than one partner cohabitant	- 11
• Age not correspondent to working experience	- 4
• Missing values on relevant variables	- 21
Estimation sample	5,734
Women	1,933
Men	3,801

Note: The last column shows the number of observations deleted.

TAB. A.2.1: Returns to Schooling Estimated by OLS and Heckman's correction: complete results – Italy - 1991.

	Women		Men			
	OLS	Heckman	OLS			
N	1933	1933	3801			
F	44.57	48.46	128.75			
R ²	0.368	0.369	0.471			
	Coeff.	St. err.	Coeff.	St. err.	Coeff.	St. err.
Years of schooling	0.020	0.002	0.019	0.002	0.017	0.002
Working exp. (years/10)	0.122	0.023	0.108	0.024	0.103	0.017
Working exp.2 (years ² /1000)	-0.263	0.052	-0.232	0.053	-0.210	0.032
Tenure	0.005	0.001	0.005	0.001	0.003	0.001
Previous work. exp.	0.007	0.013	0.036	0.023	0.035	0.010
Manufacturing	0.045	0.032	0.046	0.031	0.053	0.013
Trade, transport	0.064	0.033	0.064	0.032	0.055	0.015
Banking, Insurance, Finance	0.071	0.037	0.071	0.035	0.135	0.024
Public Administration	0.075	0.031	0.076	0.030	0.033	0.014
White Coll. Low, Teachers	0.080	0.016	0.078	0.015	0.110	0.011
White Collars	0.212	0.024	0.209	0.022	0.258	0.016
Manager	0.383	0.053	0.381	0.049	0.569	0.031
Town>500mila inhab.	0.010	0.011	0.008	0.011	0.013	0.008
North	0.028	0.013	0.027	0.013	0.048	0.010
South	-0.007	0.016	0.006	0.017	-0.039	0.011
Partner cohabitant	0.011	0.019	0.013	0.020	0.054	0.021
(Partn. cohab.)*(partn. wage)	0.005	0.002	0.004	0.002	-0.005	0.001
Age of youngest child <14	-0.013	0.018	-0.011	0.018	0.019	0.015
Age of youngest child >14	0.018	0.018	0.020	0.018	0.039	0.016
Num. of children <14	0.022	0.015	0.023	0.015	0.022	0.009
Num. of children >14	0.004	0.011	0.005	0.011	0.017	0.007
Head of family	0.096	0.018	0.071	0.024	0.104	0.021
λ			-0.037	0.024		
Constant	9.212	0.037	9.255	0.045	9.357	0.023

Note : Dependent variable = log of annual earnings less tax plus no monetary integrations. Omitted categories are: blue collar; agriculture and building; center; no children. Samples include all the employees working full-time all the year. Heteroskedasticity-consistent standard errors following White 1980.

TAB. A.2.2: Returns to Schooling Estimated by OLS and IV: complete results – Italy – 1991.

	Women						Men					
	OLS		IV		GIVE		OLS		IV		GIVE	
	Coefficient	St. Err.	Coefficient	St. Err.	Coefficient	St. Err.	Coefficient	St. Err.	Coefficient	St. Err.	Coefficient	St. Err.
Years of schooling	0.023	0.002	0.030	0.012	0.033	0.012	0.017	0.002	0.053	0.012	0.050	0.011
Working exp. (years/10)	0.089	0.051	0.082	0.053	0.079	0.053	0.048	0.038	0.106	0.046	0.100	0.044
Working exp.2 (years ² /1000)	-0.227	0.138	-0.173	0.168	-0.151	0.165	-0.052	0.098	-0.053	0.105	-0.053	0.103
Tenure	0.007	0.001	0.007	0.001	0.007	0.001	0.004	0.001	0.004	0.001	0.004	0.001
Previous work. exp.	0.006	0.017	0.009	0.018	0.010	0.018	0.034	0.013	0.033	0.014	0.033	0.014
Manufacturing	0.129	0.049	0.125	0.050	0.124	0.050	0.071	0.018	0.039	0.023	0.042	0.023
Trade, transport	0.137	0.052	0.135	0.053	0.134	0.053	0.077	0.021	0.057	0.024	0.059	0.023
Banking, Insurance, Finance	0.170	0.057	0.160	0.060	0.157	0.059	0.132	0.031	0.061	0.040	0.068	0.039
Public Administration	0.151	0.048	0.139	0.054	0.134	0.053	0.043	0.019	-0.001	0.026	0.004	0.025
White Coll. Low, Teachers	0.045	0.023	0.013	0.060	0.000	0.058	0.108	0.015	-0.015	0.044	-0.003	0.042
White Collars	0.168	0.032	0.134	0.067	0.121	0.065	0.247	0.020	0.083	0.059	0.099	0.056
Manager	0.364	0.060	0.311	0.107	0.291	0.104	0.606	0.047	0.330	0.105	0.357	0.099
Town>500mila inhab.	0.008	0.014	0.006	0.014	0.005	0.014	0.020	0.011	0.024	0.012	0.023	0.012
North	0.055	0.018	0.053	0.018	0.053	0.018	0.051	0.014	0.055	0.016	0.055	0.015
South	0.018	0.022	0.020	0.022	0.020	0.023	-0.026	0.015	-0.013	0.017	-0.014	0.017
Partner cohabitant	-0.010	0.028	-0.011	0.028	-0.012	0.028	0.032	0.031	0.068	0.036	0.064	0.035
(Partn. cohab.)*(partn. wage)	0.006	0.002	0.006	0.002	0.006	0.002	-0.005	0.001	-0.009	0.002	-0.009	0.002
Age of youngest child <14	-0.016	0.025	-0.013	0.025	-0.012	0.025	0.021	0.019	0.026	0.021	0.025	0.021
Age of youngest child >14	0.006	0.025	0.007	0.025	0.008	0.025	0.050	0.026	0.049	0.028	0.049	0.028
Num. of children <14	0.018	0.017	0.019	0.017	0.019	0.017	0.021	0.010	0.029	0.011	0.029	0.011
Num. of children >14	0.011	0.016	0.011	0.016	0.011	0.016	0.005	0.011	0.004	0.012	0.004	0.012
Head of family	0.108	0.023	0.109	0.023	0.109	0.023	0.091	0.030	0.066	0.033	0.068	0.033
Constant	9.143	0.067	9.085	0.117	9.062	0.117	9.397	0.043	9.022	0.133	9.059	0.125
Instruments:			reform		reform and province				reform		reform and province	
N	1055		1055		1055		1929		1929		1929	
F	23.3		19.19		19.15		51.42		40.28		41.36	
R ²	0.335		0.329		0.323		0.403		0.289		0.310	
Hausman Test			0.32		0.65				10.89		10.4	
Sargan Test			0.00		2.2				0.00		0.53	

Note : Dep. Variable = log of annual earnings less tax plus no monetary integrations. Omitted categories are: Blue Collar; Agriculture and Building; Center; No children. Samples include all the employees working full-time all the year, born between 1945 and 1962, and with no missing values on the place of birth variable. Heteroskedasticity-consistent standard errors following White 1980.