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A ticket to ride. Education as a place-baseless policy?

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Abstract:
Worker mobility can dissipate the benefits of place-based policies aimed at increasing the accumulation of human capital in lagging areas. We focus on Italy, a country with a longstanding regional divide, and estimate the impact of education on the probability of migrating from a lagging area to a developed one. We deal with endogeneity by exploiting a mandatory increase in the minimum school-leaving age in an instrumental variable framework. Our results suggest that dissipation is non negligible: one additional year of education increases the probability to migrate by 1.7 percentage points (9% of the average migration rate). This effect is larger for males and proved robust to a number of robustness checks, which include a placebo test on the effect of education on mobility from more developed to backward regions. Counting and analyzing compliers is on the whole reassuring for the external validity of our estimate.

JEL classification: R23, R28
Keywords: Place-based policies, education, migration.

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1. Introduction

In the attempt to reduce divergence in economic performance across areas, place-based policies are often designed to support human capital. For example, the European Structural and Investment Funds (ESIF) 2014-2020 allocate more than 49 billion Euro (about 7.5% of the total budget) to Educational and Vocational Training activities. The view that human capital accumulation is at the heart of local development is taken for granted by policy-makers, who advocate more transfers. Localized human capital externalities (see, for instance, Moretti, 2011) represent the (efficiency) rationale for the provision of public subsidies for education in lagged areas. However, in the presence of worker mobility, it is not clear who ultimately benefits from such funding, as skilled workers could move away from the assisted area in search for better-paid jobs. Therefore, the subsidy could turn into a (one-way) ticket to ride for those who get more education; and the program - envisaged as enhancing the economic fortune of a poor area – could, in the end, favor other and richer places.

This paper aims to estimate the impact of education on the likelihood of migrating from a developing area to a developed one, thus (at least partly) offsetting the economic rationale of public intervention. Estimating such a haircut to investment in human capital is crucial for the proper design of place-based policies. Econometrically, it is not an easy task. Individual decision on education and migration are very likely to be affected by common unobservable factors, which could cause omitted variable bias. Moreover, reverse causation might be an issue, as people who want to migrate might decide to get more schooling, irrespectively of the subsidy (Vidal; 1998; Beine et al., 2008; Beine et al., 2011). Finally, there could be a measurement problem if our proxy for education corresponds poorly to the skills and competencies that matter when migrating. To overcome these difficulties, our identification strategy exploits an exogenous source of variations in local human capital. We focus on Italy, a country with a long-standing North-South divide and, consequently, a significant tradition of place-based policies. We consider the changes in the compulsory school laws that occurred in 1963, when the minimum school leaving age was increased from 11 to 14 years old (Brunello et al.; 2009 and 2017). By using data from the Bank of Italy's Survey on Households, Income and Wealth (SHIW), we are able to contrast the decision to migrate from the backward Southern regions to the more developed Central-Northern ones of people who were exposed to the compulsory increase in minimum school leaving age with their non-exposed counterparts. We find a non-negligible causal effect of education on migration from developing to developed regions. Our estimates suggest that an additional year of education increases the average migration rate by 0.17 percentage points (9% of the average outcome). Put differently, increasing education by one standard deviation implies an increase in the South-North migration rate equal to one-fifth of its standard deviation. The estimated impact effect is larger for males. This result proved robust to a number of robustness checks, including a placebo exercise on the effect of education on North-to-South mobility. Finally, we analyze the external validity of our findings and conclude that it is, on the whole, satisfying.

The idea that the mobility of skilled workers across areas can dissipate investments in human capital accumulation in poorer areas is not new. However, the evidence available so far refers only to the US and is not particularly concerned with identification issues. Bound et al. (2004) studies the relation between the flow of new college graduates and the stock of college-educated persons at the state level, finding a high degree of migration. However, Bartik (2009) finds there to be a lower mobility rate of skilled workers, therefore concluding that state investment in higher education is not completely dissipated by labor mobility. Sjoquist and Winters (2014) investigate the effect of state merit aid programs on post-college location, finding that the program increases the probability of a college attendee remaining in his/her birth-state. However, such studies do not exhibit a special focus on lagging regions and do not examine long term effects, those in which place-based policies are interested. Our main contribution is offering a new and reliable estimate of the haircut effect that limits the return of education investments in lagging areas. Our paper is also related, to a lesser extent, to two lines of research. First, some scholars study labor mobility as a key component of the functioning of the labor market and look at education as a useful greasing factor (Machin et al., 2012, study the Norwegian case; Weiss, 2015, looks at European regions).[2] While largely sharing the identification strategy, we depart from this literature because we are not interested in regional mobility per se, but only that which offsets place-based policies. We show below that the different research question translates into different results on the parameter of interest.[3] Second, a number of papers have analyzed the brain drain effect of international and internal migration (see Docquier and Rapoport, 2012, for a recent survey; Becker, Ichino, and Peri, 2004, for the Italian case). In principle, our case study might well be framed within this literature, as we study a national policy that generates different effects in more or less developed areas through the reallocation of human capital. However, our research question is very different: while that stream of literature has been mainly focused on the consequences of brain drain in the source country/region, we aim at precisely estimating the haircut effect of investing in education in lagging areas in order to help better design place-based policies.

The paper is structured as follows. The next section presents the data. The IV empirical approach is described in section 3, while the results are presented and discussed in section 4. Section 5 concludes.

2. Data

Our empirical analysis is focused on Italy, a country with a long standing regional divide. While economic development in the Northern and Central (NUTS 2-level) regions is largely comparable with the EU average, the Southern areas of Italy have historically lagged behind.[4] For example, the latter areas have been included in the Objective 1 EU program, with only a few areas being in recent years in the phasing out regime.

We rely on individual-level data taken from the SHIW, carried out every two years by the Bank of Italy on Italian households. Since the 1960s, the survey has been designed to collect data on the income and wealth of Italian households. For each household member, we also have information on educational attainment, as well as place and date of birth. The sample size comprises about 8,000 households for each wave. We use surveys from 1989 onward (before that date information on the place of birth is not available) and focus only on individuals born in a Southern region. A migrant is defined as an individual that currently lives in Northern or Central Italy, but was born in the South

[2] Malamud and Wosniak (2012) study the US case and use Vietnam war draft risk to instrument the probability of college graduation.

[3] We also share our identification strategy with a number of papers that analyze the effect of education on a number of different outcomes, such as lifetime earnings (Brunello et al.; 2017) or fertility (Fort et al.; 2016).

[4] Northern and Central regions are: Piemonte, Valle d'Aosta, Liguria, Lombardia, Trentino-Alto Adige, Veneto, Friuli-Venezia Giulia, Emilia-Romagna, Toscana, Umbria, Marche, Lazio. Southern regions are: Abruzzo, Molise, Campania, Puglia, Basilicata, Calabria, Sicilia, Sardegna.

(South-to-North migrant).[5] We only consider individuals older than 20 years and exclude students. In the former case, our approach is motivated by the circumstance that, for younger individuals, migration is in fact more likely to be their parents' choice. In the latter case, we exclude students because the focus of this paper is on individuals who move in order to search for a job. Furthermore, because in 1999 the minimum school leaving age was raised again to 15 year old, in order to avoid overlap between this reform and the one passed in 1963, we do not consider individuals born from 1985 onward, who represent a cohort potentially affected by the reform in the late 1990s. Table 1 provides the main descriptive statistics.

3. Empirical strategy

As stated in the Introduction, the OLS estimate of the impact of education on migration is likely biased because of (*i*) omitted variable bias. For example, if migrants' (omitted) ability is lower (Bartolucci et al., 2018), and ability is positively related to education, then the OLS estimate is downward biased; (*ii*) positive reverse causation (migrants deciding to attain more schooling), which might lead to an upward bias; (*iii*) and measurement error, which might deflate the OLS estimate. Overall, the sign of such bias is *ex ante* unpredictable. To overcome these difficulties and identify the causal effect of education on South-to-North migration, we leverage the exogenous variation in schooling induced by the mandatory schooling reform. Exogenous variation in school achievement was induced by the 1963 Mandatory Middle school reform, which increased the minimum school leaving age from 11 to 14 years old. Following Brunello et al. (2009), we consider the first cohort potentially affected by the reform to consist of persons who were born in 1949. We also assume that additional schooling was assigned only on the basis of the date of birth and independently of any future migration choices. The empirical model reads as:

$$Y_i = \beta_0 + \beta_1 S_i + \boldsymbol{X}_i' \boldsymbol{\beta}_2 + u_i \tag{1}$$

$$S_i = \gamma_0 + \gamma_1 Z_i + \boldsymbol{X}_i' \boldsymbol{\gamma}_2 + v_i \tag{2}$$

where equations (1) and (2) are the second- and first-stage equations, respectively. Y is a dummy variable for South-to-North migrants, S indicates years of education, $\boldsymbol{X}$ is a vector of controls, including a gender dummy, age, age squared and the fixed effects of province of birth (41 provinces); our instrumental variable, Z, is the number of mandatory schooling years given by law and equals 5 for those born up to 1948 and 8 after 1949; u and v are disturbance terms.

In Table 2 we estimate equation (2) and show that our instrument is a very strong predictor for the endogenous regressor: it turns out that 1 more compulsory year leads to more than half a year's increase in education (column 1). This result is stable when we restrict the sample to a ten year-window around the cut-off year (column 2). Figure 1 offers a graphical insight into the strength of our first stage: it depicts the non-parametric estimate of education Y (Panel A) and education Y net of the effect of controls $\boldsymbol{X}$ (Panel B) as a function of the distance of birth year from 1949. At the

[5] Unfortunately, we do not have data on migrants to abroad. However, they do not account for a large share of migration from the South of Italy (6% according to the Italian Statistical Institute in the 2002-2016 average; unfortunately, data for previous years are not available). In any event, on an *a priori* ground we do not see any strong reason to argue that migrants to abroad have a different reactivity of migration behavior to education compared to those migrating to the North of Italy.

threshold, there is a large jump in the average years of schooling, which ranges from half to one year.

4. Results

4.1 Regression estimates

Table 3 contains our main findings. Standard errors are clustered at the year of birth and household level. Column 1 reports the OLS estimate of the effect of education on the probability to migrate. The relationship is significant and positive: one additional year at school increases the probability to migrate by 0.004 percentage points. The IV estimate is shown in column 2. The F-statistic is very high, thus confirming that our instrument is very strong. Taking into account endogeneity entails a large upward revision of the point estimate (as in Machin et al., 2012; Weiss, 2015): one additional year of education increases the probability to migrate by 1.7 percentage points. Stated from the perspective of policy, the return to investment in education in lagging areas suffers from a 1.7 percentage point haircut. The size of such an estimated effect is non-negligible. It equals 9% of the average migration rate of the estimation sample. A one-standard deviation increase in the key regressor implies an increase in the dependent variable equal to 20% of its standard deviation. Columns 3-4 report the estimates by gender: males turn out to be much more responsive than females, whose migration choices are probably more likely to be shaped by those of their spouse.

Table 4 shows a number of robustness checks. In all cases, the first stage F-statistics are largely reassuring. In column 1, we restrict our sample to individuals born within a 10-year window centered in 1949. The results for this reduced sample do not significantly differ from those of our baseline estimate. In the next three columns, we test whether our results are driven by trends in migration that might be incorrectly attributed to the school reform (Cannari et al., 2000). When including a linear trend in birth cohorts, the results are confirmed even if the estimate is less precise and the corresponding p-value is just above the 10 percent threshold (column 2). When allowing for a more reliable quadratic trend in birth cohorts, the impact is even larger and, again, statistically significant (column 3). Results are also robust when the quadratic trend is allowed to be region-specific (8 regions). In the fifth column, we adopt a forward-looking policy perspective, excluding from the estimation sample those who were born in regions that were subsequently phased-out of the EU cohesion policy framework (Abruzzo, Molise and Sardegna). This gives us a more precise estimate of the expected effect of future EU place-based policies. The impact is even larger, thus further stressing the relevance of the migration channel as a haircut to education investments. In column 6 we exclude retired people, who may have moved after their retirement for reasons not related to the search for work: however, the point estimate is basically unaffected. Finally, we also run a placebo test by assessing the effect of education when migration takes place in the opposite direction, from North to South (column 7). Consistently, as with our priors, education has no significant effect on North-South migration, thus stressing the relevance of the estimated effect for lagging-behind regions only.

In Table 5, we compare our results with those obtained by Machin et al. (2012) and Weiss (2015), who exploit the mandatory minimum schooling reforms to estimate the causal effect of education on regional migration, *irrespective of the economic development of the region of birth*. Are our estimates, conditioned on being born in backward regions, different? We guess that the answer is affirmative. More developed regions usually provide better labor market opportunities than lagging areas; skill-biased amenities are more pervasive in richer areas. For such reasons, the impact of education on migration, when including more developed areas, might be very different from the effect as referred to lagging areas only. In the first column of Table 5, we mimic as much as possible the empirical setting of Machin et al. (2012) and Weiss (2015). The dependent variable is redefined as a dummy equal to 1 if the respondent currently lives in a region that is different from their region of birth, and 0 otherwise, while the sample now includes people born either in the South or the Centre-North. It turns out that the estimated effect of education on regional mobility is not statistically different from zero.[6] Interestingly, when we split our sample according to the area of birth, we find that the point estimate for those born in the South (column 2) largely overlaps with the haircut effect estimated in Table 3, column 2, thus implicitly suggesting that South-to-South migration is not very relevant. On the other hand, the Central-Northern sample shows the opposite behavior (column 3). For these regions, the increase in education reduces the likelihood of internal migration.

4.2 External validity

Our results estimate the local average treatment effect of education on South-to-North migration for those whose schooling attainment was affected by the 1963 reform (compliers). In order to better appreciate the external validity of our estimate, two questions arise: counting and characterizing compliers.

First, we start by noting that the exploitation of the compulsory schooling reform would naturally suggest that our estimates must apply to those at the bottom of the education distribution. The graph in figure 2, which shows the distribution of schooling in the pre- and post-reform samples, supports such an assumption: the vertical distance between the two cumulative density functions reaches its maximum for elementary school even if non-negligible differences are recorded for the next two levels. It follows that our estimates fully apply, say, to a compulsory school dropout prevention program, but not to a training program.

Although the compliers cannot be identified from the observed data, they can be easily counted and characterized according to some interesting pre-treatment variables, when both the endogenous variable and the instrument are binary (Angrist 2004, Angrist and Pinske, 2009). To this aim we discretize years of schooling S with a binary treatment equaling 1 if the individual's actual years of education are equal or more than 8 (the post-reform number of mandatory schooling years) and 0 otherwise; the instrument is a binary indicator, taking the value 1 for those born from 1949.

The percentage of compliers is rather large (47.3%). As far as their characterization is concerned, we analyze the sub-population of compliers according to the following set of pre-treatment variables: a dummy variable taking the value 1 if the respondent is male, and 0 otherwise, a dummy variable taking the value 1 if the age of the respondent is equal to or above the median, and 0 otherwise, and a dummy variable taking the value 1 if per capita value added in the birth province in

[6] Fully understanding the difference between this result and the positive effect detected in Machin et al. (2012) and Weiss (2015) is beyond our scope. However, we nevertheless argue that the differences in the samples under scrutiny may be a candidate explanation: Machin et al. (2012) deals with the Norwegian case, while Weiss's (2015) sample includes many European regions.

1963 is equal to or above the median, and 0 otherwise. We find that compliers are 12% less likely to be male, 16% less likely to be old and 8% less likely to be born in a more developed province. All these differences do not seem sizable.

All in all, even if our estimates undoubtedly point to those at the bottom of the educational distribution, the satisfying percentage of compliers in the sample and the limited characterization of compliers with respect to some pre-determined variable reassure about the external validity of our point estimate.

5. Conclusions

Place-based policies are widespread in many countries, often taking the form of subsidies for human capital accumulation. However, since labor is mobile, it is crucial to precisely estimate the effect of additional education on the probability of migrating from poorer to richer regions, so as to (at least partly) dissipate the investment. In this paper, we address this point and estimate the causal effect of education on migration from the Southern (poorer) to the Northern/Central (richer) regions in Italy. By exploiting the exogenous change in education related to the compulsory school reforms of 1963, we find that, in an instrumental variable sense, education has a significant and positive impact on South-to-North migration. We also discuss the external validity of our results: while the exploitation of the compulsory schooling reform would naturally suggest that our estimates must apply to those at the bottom of the education distribution, compliers are a relatively large portion of the sample who do not differ too much from the average. This is reassuring regarding the applicability of our results to different settings.

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Tables and figures

Table 1: Descriptive statistics

	N. obs	mean	median	S.D.	min	max
South-to-North migration (*Y*)	50,754	0.18462	0	0.38799	0	1
years of education (*S*)	50,754	8.01383	8	4.59040	0	21
treated (year of birth>=1949)	50,754	0.53621	1	0.49869	0	1
male	50,754	0.47872	0	0.49955	0	1
age	50,754	49.7826	49	17.0066	20	107

Table 2: the first stage

	(1) Controls	(2) 10 year window
mandatory schooling years (*Z*)	0.5720*** (0.0191)	0.3739*** (0.0226)
cohorts	All	1939-1959
sample size	50,754	20,395

Note: The dependent variable is the number of years of education (*S*). All regressions include age, age squared gender and the fixed effects of province of birth as regressors. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: the effect of education on South-to-North migration

VARIABLES	(1) OLS	(2) IV baseline	(3) Males	(4) Females
years of education (*S*)	0.0037*** (0.0006)	0.0170*** (0.0046)	0.0239*** (0.0066)	0.0132*** (0.0050)
F-test statistic	-	76.89	45.19	95.41
sample size	50,754	50,754	24,297	26,457

Note: The dependent variable is the South-to-North migration dummy variable (*Y*). All regressions include age, age squared gender and the fixed effects of province of birth as regressors. In columns 2-4 education (*S*) is instrumented with years of mandatory school (*Z*). Robust standard errors, clustered at the cohort and household level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Robustness checks

VARIABLES	(1) 10 year window	(2) Linear trend	(3) Quadratic trend	(4) Prov-sp. quadr. trend	(5) No phasing-out reg.	(6) No retired	(7) North-to-South migration
years of education (S)	0.0178*** (0.0059)	0.0192 (0.0119)	0.0211** (0.0085)	0.0222*** (0.0083)	0.0272*** (0.0050)	0.0174*** (0.0050)	-0.0008 (0.0008)
cohorts	1939-1959	All	All	All	All	All	All
F-test statistic	37.17	29.69	30.39	30.44	72.97	80.40	112.78
sample size	20,395	50,754	50,754	50,754	42,412	37,006	66,116

Note: The dependent variable is the South-to-North migration dummy variable (Y), except for column 7, where the dependent variable is the analogous North-to-South migration dummy. All regressions include age, age squared gender and the fixed effects of province of birth as regressors. In addition, a linear trend in birth cohorts, a quadratic trend in birth cohorts and a province-of-birth specific quadratic trend in birth cohorts are included in columns 2, 3 and 4, respectively. Education(S) is instrumented with years of mandatory school (Z). In column 5, we exclude Abruzzo, Molise and Sardegna, as they are phasing-out regions within the EU cohesion policy framework. In column 6, we exclude retired individuals. Robust standard errors, clustered at the cohort and household level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

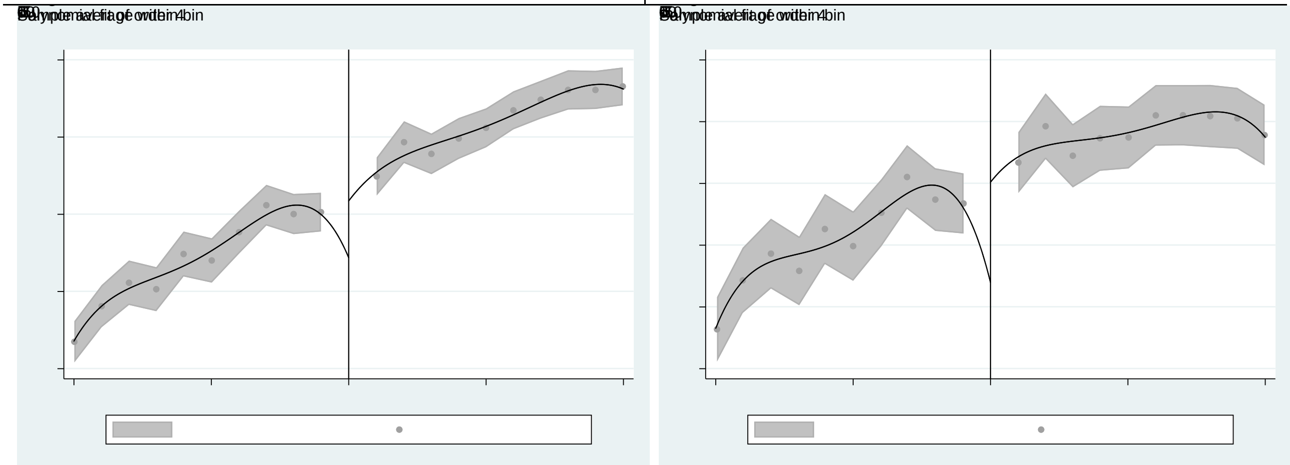
Table 5: comparison with the existing literature

	(1) Internal migration	(2) Internal migration – from South	(3) Internal migration – from North
years of education (S)	-0.0019 (0.0025)	0.0153*** (0.0048)	-0.0139*** (0.0022)
F-test statistic	99.84	76.89	112.78
sample size	116870	50754	66116

Note: The dependent variable is the migration dummy variable (Y) that equals 1 if the respondent was born in a region that differs from that of where she/he currently lives, and is 0 otherwise. All regressions include age, age squared, gender and the fixed effects of province of birth as regressors. Education (S) is instrumented with years of mandatory school (Z). Robust standard errors, clustered at the cohort and household level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

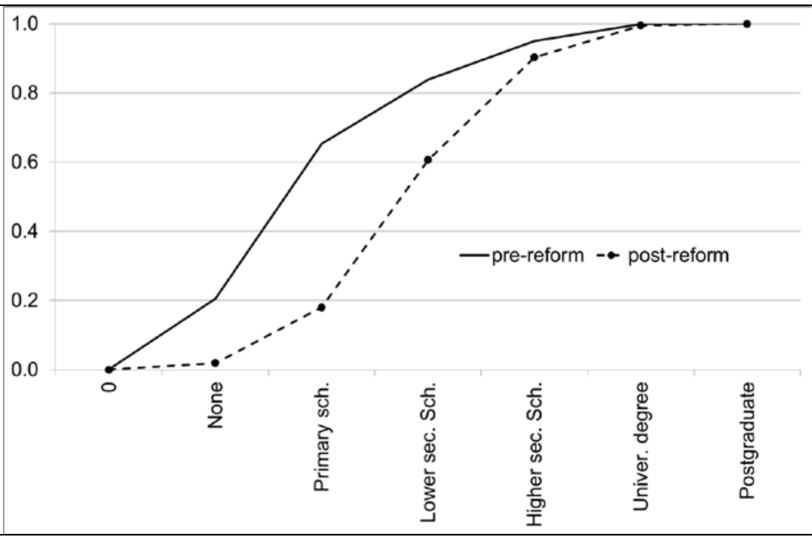
Figure 1: Graphical evidence (non-parametric estimation) on the first stage

Panel A	Panel B



Note: We report in the x-axis the distance from 1949 (the pivotal cohort) and in the y-axis the years of education (Panel A) and the years of education net of the effect of age, age squared, gender and the fixed effects of province of birth (Panel B). The lines are non-parametric estimates, based on a kernel function, used to construct the local-polynomial estimators. The shaded area is the 5-95 confidence interval.

Figure 2: Educational qualification distribution before and after the reform



Note: We report in the y-axis the cumulative density.