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FULL ARTICLE

# A ticket to ride: Education and migration from lagging areas

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## Abstract

National policies may have heterogeneous effects at the regional level. When coming to programmes aimed at increasing human capital, worker mobility from poorer to richer regions can reduce the benefits of the policy for the former areas. We focus on Italy and estimate the impact of education on the probability of migrating from a lagging area to a leading one. Endogeneity is addressed by exploiting an increase in the minimum school-leaving age in an instrumental variable framework. We find that one additional year of education increases the probability to migrate by 1.7 percentage points (9% of the average migration rate).

**KEYWORDS**

Lagging areas, education, migration

**JEL CLASSIFICATION**

R23; R28

## 1 | INTRODUCTION

Nation-wide policies may display different effects in different sub-national areas because of distinct local characteristics such as sectoral composition, economic development, human and social capital, local labour force, geographic conditions and so on. This phenomenon is pervasive and, in a regional divide perspective, it is particularly relevant since it may exacerbate spatial inequality even if the overall effect of the policy on the economy is positive. For example, D'Costa, Garcilazo, and Oliveira Martin (2019) find that reducing barriers to entrepreneurship, a well-celebrated structural reform, increases productivity growth only for regions closer to the frontier while the effect is null on regions that are the most lagging. Despite of the importance of this issue, existing research focusing on differential regional effects of national policies is rather limited.

This paper studies the impact of a national policy aimed at increasing education on the likelihood of migrating from a developing subnational area to a developed one. While in a general spatial equilibrium setting internal migration is usually desirable because it helps clearing local labour markets (Machin, Salvanes, & Pelkonen, 2012; Weiss, 2015), from the lagging area's viewpoint migration generates a haircut to the return to the human capital investment: estimating the haircut is thus very relevant for the proper design of the nation-wide policy. On the other hand, a credible estimate of the migration-related dissipation effect is also very important for all those place-based policies that are designed to support human capital, localized human capital externalities representing the (efficiency) rationale for the provision of public subsidies (Moretti, 2011).[1] In fact, because 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. Again, additional education could turn into a (one-way) ticket to ride and the programme could, in the end, favour richer places.

Econometrically, estimating the effect of education on migration 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 programme (Beine, Docquier, & Özden, 2011; Beine, Frédéric, & Rapoport, 2008; Vidal, 1998). 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. We consider the change in the compulsory school laws that occurred in 1963, when the mandatory years of schooling were increased from 5 to 8 years (Brunello, Fort, & Weber, 2009; Brunello, Weber, & Weiss, 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 from South to North by 1.7 percentage points (9% of the average outcome). Put differently, increasing education by one standard deviation implies an increase in the 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 analyse 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, Groen, Kezdi, and Turner (2004) study 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 labour mobility. Sjoquist and Winters (2014) investigate the effect of state merit aid programmes, created in some US states, on post-college location, finding that the programme 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. 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 labour mobility as a key component of the functioning of the labour market and look at education as a useful greasing factor (Machin

[1]In the European context, for instance, 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.

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 to the flows from poor to rich areas. We show below that the different research question translates into different results on the parameter of interest.[3] Second, a number of papers have analysed the brain drain effect of international and internal migration (see Docquier & Rapoport, 2012, for a recent survey; Becker, Ichino, & 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.

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 programme, with only a few regions 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 who currently lives in Northern or Central Italy, but was born in the South (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 years 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, Villosio, & Wagne, 2018), and ability is positively related to education, then the OLS estimate is downward biased; (ii) positive reverse causation

[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 analyse 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.

[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 behaviour to education compared to those migrating to the North of Italy.

**TABLE 1** Descriptive statistics

| | N. Obs | Mean | Median | SD | 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 |

(migrants deciding to attain more schooling), which might lead to an upward bias; (iii) 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 1963 mandatory schooling reform. We assume that, conditional on controls, additional schooling was assigned only on the basis of the date of birth and independently of any future migration choices.

Until 1962, it was mandatory to complete elementary school (5 years of school). Starting from 1963 a new law was in force: it increased compulsory school attendance until graduation from the new junior high school (*scuola media unica*). A minimum of 8 years of schooling was required, but drop out was allowed for 15-year old pupils who had been in school for at least 8 years. For a child starting elementary school at 6 years old and with no failure during the elementary school, the new regime implied an increase in the minimum school leaving age from 11 to 14 years old. There are three possible treatment statuses. In 1963, pupils belonging to the 1948 cohort (or earlier) were 15 years old (or older) so that they were not affected by the reform because drop out was allowed. People from the 1952 cohort were fully affected (they had just completed the elementary school) while the cohorts in between (1949, 1950, 1951) were partially treated: they were made of individuals who were not able to drop out and have not been in school for at least 8 years. To our aim, this discussion is relevant to choose the pivotal cohort. Since our instrumental variable basically distinguishes between treated and untreated cohorts, we consider the first cohort potentially affected by the reform to consist of persons who were born in 1949. This is also consistent with previous studies (Brunello et al., 2009). In the robustness section we also show that our results are confirmed if the pivotal cohort is 1952, if we exclude 1949 or exclude all partially treated cohorts (1949–1951).

The empirical model reads as:

$$Y_i = \beta_0 + \beta_1 S_i + X_i^{'}\beta_2 + u_i, \tag{1}$$

$$S_i = \gamma_0 + \gamma_1 Z_i + X_i^{'}\gamma_2 + v_{i,} \tag{2}$$

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, *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)), consistently with the design of the policy that generated partially treated units (those born in 1949, 1950, 1951. See above). This result is stable when we restrict the sample to a ten year-window around the cut-off year (column (2)).

**TABLE 2** The first stage

| Variables | (1) Controls | (2) 10 year window |
|---|---|---|
| Mandatory schooling years (*Z*) | 0.5720*** (0.0191) | 0.3739*** (0.0226) |
| Sample size | 50,754 | 20,395 |

*Notes*: 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. In the first column all cohorts are included. In column (2) only cohorts from 1939 to 1959 are included. Robust standard errors are in parentheses. ***$p$ < 0.01, **$p$ < 0.05, *$p$ < 0.1.

# 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.

In Table 4 we perform three robustness checks related to the treatment status (see Section 3). In all cases, the first stage F-statistics are largely reassuring. In column (1) we take into account the possibility that the pivotal cohort is 1952: our point estimate is a bit smaller but still significant and qualitatively unchanged. In column (2) we drop the 1949 cohort from the analysis (Battistin, Brugiavini, Rettore, & Weber, 2009; Fort, Schneeweis, & Winter-Ebmer, 2016): those born in 1949 may be only partially affected by the reform because of their birth month and/or difficulties in the implementation of the new regulatory regime in its early stage. In the same spirit, in column (3) we drop all partially affected cohorts (1949, 1950 and 1951). In both case, the results are fairly stable.

**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 |

*Notes*: 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 on the treated cohorts

| Variables | (1) First affected cohort 1952 | (2) No 1949 cohort | (3) No 1949–1951 cohorts |
|---|---|---|---|
| Years of education (*S*) | 0.0101** (0.0049) | 0.0159*** (0.0046) | 0.0135*** (0.0045) |
| F-test statistic | 68.04 | 85.14 | 85.80 |
| Sample size | 50,754 | 49,764 | 47,737 |

*Notes*: 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. Education (*S*) is instrumented with years of mandatory school (*Z*). In column (1), we assume that the first affected cohort is the 1952 one. In column (2) we exclude the 1949 cohort. In column (3) we exclude the 1949–1951 cohorts. Robust standard errors, clustered at the cohort and household level, are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 5 shows further robustness checks. Again, the instrumental variable is a very good predictor of education. In column (1), we restrict our sample to individuals born within a 10-year window centred 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 the omission of business cycle variables that may be correlated both to migration and to the school reform: Ballarino and Panichella (2015) and Cannari, Nucci, and Sestito (2000) show that South–North migration has roughly an inverted U shape in the 1955–2002 period; at the same time the years around the reform are broadly those of the so called Italian economic miracle. To control for such confounders, we include 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% 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; column (4)). In column (5) 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 (6)). Consistently 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 6, 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 labour 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 6, 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 behaviour (column 3). For these regions, the increase in education reduces the likelihood of internal migration.

[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.

**TABLE 5** Other robustness checks

| Variables | (1) 10 year window | (2) Linear trend | (3) Quadratic trend | (4) Reg.-sp. quadr. Trend | (5) No retired | (6) North-to-South migration |
|---|---|---|---|---|---|---|
| Years of education ($S$) | 0.0178*** (0.0059) | 0.0192 (0.0119) | 0.0211** (0.0085) | 0.0222*** (0.0083) | 0.0174*** (0.0050) | −0.0008 (0.0008) |
| Cohorts | 1939–59 | All | All | All | All | All |
| F-test statistic | 37.17 | 29.69 | 30.39 | 30.44 | 80.40 | 112.78 |
| Sample size | 20,395 | 50,754 | 50,754 | 50,754 | 37,006 | 66,116 |

*Notes*: The dependent variable is the South-to-North migration dummy variable ($Y$), except for column (6), 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 all columns all cohorts are included except for column (1) for which only cohorts from 1939 to 1959 are included. In column (5), we exclude retired individuals. In column (6) the sample is made of individuals born in the Centre-North and currently living in the South. Robust standard errors, clustered at the cohort and household level, are in parentheses.

***$p < 0.01$ **$p < 0.05$, *$p < 0.1$.

**TABLE 6** Comparison with the existing literature

| Variables | (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 | 116,870 | 50,754 | 66,116 |

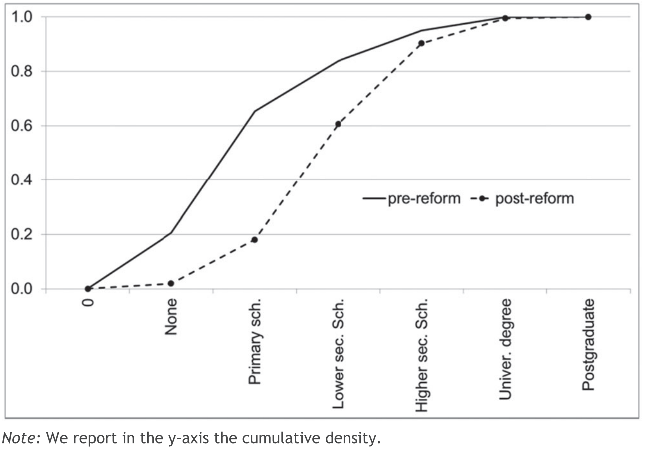
*Notes:* The dependent variable is the migration dummy variable that equals 1 if the respondent was born in a region that differs from that of where she/he currently lives, and is 0 otherwise. All regressions include age, age squared, gender and the fixed effects of province of birth as regressors. Education (*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.

## 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 1, 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 programme, but not to a training programme.

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 & Pischke, 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 to 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.




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

**FIGURE 1** Educational qualification distribution before and after the reform

The percentage of compliers is rather large (47.3%). As far as their characterization is concerned, we analyse 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 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

Nation-wide policies can have different regional effects that reflect local-area specific initial conditions. Spurring human capital accumulation is a widespread national policy; however, since labour is mobile, more educated individuals may migrate from poorer to richer areas so that the investment is at least partially dissipated in the former regions. 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 and do not differ too much from the average. This is reassuring regarding the applicability of our results to different settings.

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**Resumen.** Las políticas nacionales pueden tener efectos heterogéneos a nivel regional. Cuando se trata de programas destinados a aumentar el capital humano, la movilidad de los trabajadores de las regiones más pobres hacia las más ricas puede reducir los beneficios de tales políticas para las más pobres. El artículo se centra en Italia y estima el impacto de la educación en la probabilidad de migrar de una zona rezagada a una zona líder. La endogeneidad se aborda explotando el aumento de la edad mínima de finalización de la escolaridad en un marco de variables instrumentales. Se encontró que un año adicional de educación aumenta la probabilidad de migrar en un 1,7 puntos porcentuales (9% de la tasa promedio de migración).

抄録:　国家政策の効果は、地域レベルで異なる可能性がある。人的資本の増加を目的とした政策プログラムに関しては、労働者が貧困地域から裕福な地域に移住する確率が貧困地域に対する政策の利益を減少させることがありうる。本稿ではイタリアに注目し、遅れている地域から先進的な地域への移住の確率に対する教育の影響を推定する。操作変数法のフレームワークで、最低学卒年齢の上昇を利用して内生性に対処する。教育年数が一年増えると移住確率が1.7%ポイント(平均移住率の9%)上昇するという知見が得られた。