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Social Returns to Education: Evidence from Italian Local Labor Market Areas

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Social Returns to Education: Evidence from Italian Local Labor Market Areas

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Abstract

The paper provides a quantitative assessment of social returns to education in Italy. It shows that, after controlling for individual characteristics, local average human capital is positively correlated with individual wages, with estimated social returns between 2 and 3 percent. This result is robust to alternative estimation methods and does not seem to depend on endogenous sorting. The paper also shows that social returns are higher in the lagged areas of the south of Italy.

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I. INTRODUCTION

The idea that individuals do not entirely capture the benefits from their education is used to argue that governments should subsidize schooling. There are several explanations of why social returns to education may exceed private returns. For example, a high level of average human capital may favor the diffusion of knowledge among workers, as in Lucas (1988), or make it profitable to invest in new technologies, as in Acemoglu (1996,1997), or even generate effects that go even beyond the domain of economics.[2] Even though there are good theoretical reasons to believe in education externalities, empirical evidence is surprisingly mixed. By using cross-country data, Barro (1991); Mankiw, Romer, and Weil (1992); and others find schooling to be positively correlated with the percapita GDP growth rate. But Bils and Klenow (2000) argue that the impact of schooling on growth is likely to be modest and, possibly, that expected growth causes increases in school enrollment. A recent and fast-developing body of literature adopts a Mincerian wage-equation approach to detect human capital spillovers in U.S. local labor markets. But again, while Rauch (1993) and Moretti (2002) find evidence for substantial social returns to education, Acemoglu and Angrist (2000) and Ciccone and Peri (2002) claim that such returns are negligible.

Our paper follows the Mincerian approach to quantify social returns to education in Italian local labor markets.[3] We also ask whether social returns change across the Center-North and the South of Italy, since these areas are characterized by different levels of development.

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[2] According to Weisbrod (1962, p. 106): "[Education] benefits the student's future children, who will receive informal education at home; it benefits neighbors who may be affected favorably by the social values developed in children by the schools and even by the quietness of the neighborhood while the schools are in session. Schooling benefits employers seeking a trained labor force; and it benefits the society at large by developing the basis of an informed electorate."

[3] The Mincerian approach evaluates social returns to education by merely looking at wage differences across areas. This strategy has two main limits, which tend to bias the size of actual spillovers downwards. First, average human capital may have effects that go largely beyond the boundaries of the local labor market. For example, research at the Massachusetts Institute of Technology can have nationwide, or even worldwide, effects while affecting differential productivity in the Boston area only marginally. Second, Haveman and Wolfe (1984, 2002) have argued that wage differences capture only a portion (and possibly only a small portion) of the full "social" effects of education. For example, reductions in criminal activity owing to schooling may generate both higher average productivity and nonmarket effects, such as higher social cohesion. The Mincerian approach will only capture the productivity effect of less criminality on wages.

Rauch (1993) used 1980 U.S. Census data, treating average schooling as predetermined. He found that a one-year increase in Metropolitan Area (MA) average education raised wages by 3 to 5 percent. The existence of social returns to education has been confirmed by Moretti (2002), who exploits both 1979–94 National Longitudinal Survey of Youths and 1980–90 Census data and selects the share of college graduates in a MA as the measure of average education. He also accounts for endogeneity in average education (while treating individual schooling as exogenous) by using the lagged city demographic structure as an instrument, and finds social returns ranging from 8 to 13 percent. The findings in Rauch (1993) and Moretti (2002) are questioned by Acemoglu and Angrist (2000), who find no evidence for social returns in U.S. states. Their instrumental variable strategy for both average and individual schooling in 1960–90 U.S. Census data exploits differences in compulsory schooling and child-labor laws across states. Acemoglu and Angrist's results have been confirmed by Rudd (2000), who controls for a variety of state-level variables that may affect wages. Ciccone and Peri (2000) develop an approach that separates pure human capital spillovers from wage effects owing to changes in the labor-force composition. They also find no evidence for social returns to average education in U.S. MAs.

The conclusions on education externalities may depend on the definition of territorial unit adopted. Our analysis of Italian data employs a definition of local labor market consistent with the notion of "functional region." A functional region is defined as "a territorial unit resulting from the organization of social and economic relations in that its boundaries do not reflect geographical particularities or historical events" (OECD, 2002, p.11). In particular, a functional region is related to its local labor market, defined in terms of commuting conditions.[4] As Lucas (1988) pointed out, the effects of average skill on the productivity of each worker have to do with "the ways various groups of people interact, which may be affected by political boundaries but are certainly an entirely different matter conceptually" (p. 37). The studies we mentioned on U.S. data use two types of territorial units: (i) states, as in Acemoglu and Angrist (1999) and Rudd (2000), and (ii) MAs, as in Rauch (1993), Moretti (2002), and Ciccone and Peri (2002). U.S. states hardly fit the notion of local labor markets.[5] MAs are not defined by mere administrative boundaries, but their categorization is based on their urban character, rather than labor market features (OECD, 2002, pp. 122–26). Here we adopt the OECD definition of Local Labor Market Area (LLMA). LLMAs are built through the aggregation of two or more neighboring municipalities, characterized on the basis of daily travel flows from place of residence to

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[4] For the relevance of commuting in the definition of local labor markets, see also Manning (2002).

[5] As noted by Bils (2000, p.60), "particularly for models based on externalities in production, it is not clear if the state of residence is the relevant economy."

place of work. The 784 Italian LLMAs (140 in the northwest, 143 in the northeast, 136 in the center, and 365 in the south, respectively) span the entire national territory.[6]

Another relevant difference between the Italian and the U.S. cases is variance in school quality. The Italian education system is more centralized and egalitarian, with low variability of education quality across areas. By contrast, the United States exhibits high heterogeneity in school quality, since the public education system is mostly financed at the local level and private schools are more important.[7] Card and Krueger (1992a, 1992b, and 1996) show that the effects of school quality on private returns to education are substantial. Therefore, the omission of measures for school quality heterogeneity across U.S. areas may also bias the estimate of the social returns to human capital. This omitted variable problem, however, is likely to be much less severe for Italian data.

Our results show that LLMA average human capital in Italy is positively correlated with wages. In particular, we find that social returns range from 2 to 3 percent. These results are robust to an instrumental variable approach designed to deal with the bias that may arise from the correlation between average schooling and omitted LLMA characteristics. Moreover, by analyzing the subsample of those who have not moved away from their area of birth, we also conclude that our results are not likely to be driven by selective migration. We also find that the wage premium associated with higher-than-average human capital is largely absorbed by cost of living differentials across areas, as measured by housing prices. Again, as in Glaeser and Marè (2001), this suggests that local average human capital is unlikely to be correlated with the omitted ability of workers.

We extend the basic analysis in two main respects. First, we test the robustness of our result by restricting our sample to manufacturing workers. This also allows us to introduce additional controls into our basic specification. Second, we show that social returns tend to be higher for LLMAs located in the lagged areas of the south of Italy, which display lower levels of average human capital. This finding is robust to the inclusion of additional controls for LLMAs' characteristics. This result may be relevant for assessing the effectiveness of alternative allocations of public education funds. At the present time, Italian per-student schooling expenditures are roughly the same across LLMAs for each educational level; see OECD (2001).

The paper is structured as follows. Section II describes the dataset; Section III contains the empirical evidence; and Section IV concludes.

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[6] A detailed description of territorial units (MA and LLMA, respectively) is given in Appendix I.

[7] See OECD (2001) and Checchi, Ichino, and Rustichini (1999) for a throughout comparison. On the role of school quality see also Borjas (1995) and Benabou (1996).

II. Data

The analysis is mainly based on two datasets. Micro-data from the Bank of Italy's Survey of Household Income and Wealth (SHIW) are merged with the Cannari-Signorini dataset (CS), reporting several socio-economic characteristics of Italian LLMAs.

The SHIW is a biannual survey on the microeconomic behavior of Italian families. We use observations from four of the surveys (1993, 1995, 1998, 2000). The SHIW provides detailed information[8] on several characteristics of the worker, such as wage, educational attainment, job experience, sex, marital status, sector of employment, and size of the employer. Hourly wage is calculated as total annual earnings divided by the number of hours worked in a year. Thus, *Hourly Wage = Total Annual Earnings /(Average Hours Worked per Week × Months Worked × 4.3333)*, where the constant 4.3333 represents the average number of weeks in a month. Total annual earnings are net of taxes and social security contributions, and includes overtime, additional monthly salary, bonuses or special emoluments, and fringe benefits as evaluated by the interviewee. We restrict our sample only to employees with nonzero total annual income and nonzero weekly hours, or months, worked. We also exclude those who did not provide information on their family background, used here as an instrument for individual education. Our measure of work experience is calculated as the difference between worker's age at the survey date and the age when the first job was taken.[9]

The SHIW also provides information on industry and size of the current employer. The branches of activity of the company for which the individual works are recorded as follows: agriculture; manufacturing; building; trade; transportation; credit and insurance; real estate, IT and research; private services; government; extraterritorial organizations; others. Information on the employer size is divided into the following classes: up to 4 employees; from 5 to 19 employees; from 20 to 49; from 50 to 99; from 100 to 499; 500 or more employees; "not applicable" or public sector employee. Because of the sampling design of the SHIW, only a sub-sample of families is interviewed in more than one survey: for example, among the 8,001 households that constituted the sample in year 2000, 399 participated since 1993, 245 since 1995, 1,993 since 1998. Our sample thus has an unbalanced panel structure, and includes 17,251 workers, 12,224 of which are truly independent. In particular, there are respectively 649 workers who were observed in all the four surveys; 607 in three surveys; 1,866 in two; and 9,102 in only one survey. We use a

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[8] Full detail is provided in /locali/studi/misf/areaind/INDAGINI/ibf/pubblica/shiw00.pdfItalian Household Budgets, Supplements to the Statistical Bulletin, Banca d'Italia, *various years.*

[9] Workers who did not report their age when taking the first job are therefore dropped from the sample. Our measure of experience is more accurate than the most widely used measure of seniority (*Experience = Age - Years of Schooling – 6*), which attributes "waiting unemployment" after school to work experience.

confidential version of the SHIW, which includes information both on the place of birth and the place of residence. The place of residence is used as matching variable with the CS data-set.

The local cost of living is measured by exploiting the real estate section of the SHIW, where families provide information on their main residence, be it owned or rented (for details, see Appendix II).

The CS dataset contains an array of demographic and socio-economic variables for each of the LLMAs, and it is derived from a variety of sources (Census; Company Account Data Service; ISTAT's Surveys on Export, Value added, Labor Force, Capital Stock: see Cannari and Signorini (2000) for details). All the data refer to the beginning of the '90s. Our analysis mainly builds on the following measures for each LLMA: average human capital, unemployment rate, an index of infrastructures, as calculated by the ratio between kilometers of roads and LLMA's surface. As for the manufacturing sector, we employ a value-added based index of physical capital; manufacturing share, determined as the ratio between manufacturing employees and population or total employees; and average firm size. For a few additional variables, we use data from other sources[10].

The 17,251 workers of our sample are randomly distributed over 235 LLMAs. The variables used in the present analysis, together with their sample statistics, are reported in Table 1, describing the employee SHIW sample; Table 2, which reports the real-estate SHIW sample; and Table 3, which overviews CS data.

III. Empirical Results

We estimate the effect of average human capital at the LLMA level on individual log earnings (hourly wage rate). Estimation is based on the following Mincerian equation for individual i residing in LLMA j in period t:

$$\ln w_{ijt} = X_{it}\beta + \eta HC_j + z_j\delta + \varepsilon_{ijt} \tag{1}$$

where X is a vector of individual observable characteristics, which include individual education; HC denotes LLMA average human capital, as measured by average years of schooling of the population in the area; and z is a vector of LLMA characteristics, which may be correlated with average human capital. The variables referring to LLMAs do not vary over

[10] We use the 1981 Census to derive our demographic instruments; information from the 1993 Company Account Data Service for a measure of LLMA capital per worker; and data from the real estate private agency "Il Consulente Immobiliare" to check our index of housing costs.

time, since the CS dataset only contains cross-LLMA observations for the beginning of the 90s. ε denotes the regression error.

The goal of the paper is to estimate η, the impact of human capital on the average wage. As emphasized in Acemoglu and Angrist (2000), Moretti (2002), and Ciccone and Peri (2002), the parameter η captures all the external effects arising from higher human capital that are reflected in the wage rate. Whenever workers of different education are imperfect substitutes in production, external effects originate both from "composition effects", due to a higher proportion of educated workers in the labor supply, and "spillover effects", due to pure human capital externalities as in Lucas (1988). Competitive theory also predicts that, even when spillovers are absent, η must be positive[11]. Intuitively, an increase in the proportion of skilled labor tends to drive average productivity up. Although the Mincerian approach followed by Rauch (1993), Acemoglu and Angrist (2000), Moretti (2002), and here, does not allow us to disentangle pure human capital spillovers from composition effects, we will try to assess the relevance of the bias deriving from labor-force composition changes by using a simple test derived from Ciccone and Peri (2002).

A. Baseline Regressions

We start by estimating a baseline specification, which includes the Mincerian set of individual characteristics and controls for observable heterogeneity among individuals. Mincerian characteristics include labor market experience (EXP), its squared value (EXPSQR), the number of years of schooling (SCHOOL), and two dummies for sex and marital status.

We also control for inter-industry wage differentials, which appear to be quite relevant for the Italian case: see Mauro, Prasad, and Spilimbergo (1999). We add ten dummies to pin down the branch of activity of the company for which each individual works (with manufacturing being the left out dummy). Industry dummies can partly capture endogenous matching of better workers with high-wage firms: see Bartel and Sicherman (1999). Similarly, we control for firm-size wage differentials by including dummies that divide employment per firm in six classes (with size from 20 to 49 employees being the left-

[11] Acemoglu and Angrist (2000) and Ciccone and Peri (2002) point out that, unless the elasticity of substitution is infinite, the effect of the average level of local education on the average local wage-level is positive for any CES technology *even* in the absence of spillovers. Ciccone and Peri (2002) tackle this issue by adopting a "constant-composition approach" which is designed to measure pure human capital externalities.

out dummy)[12]. Finally, we add a set of geographic controls, one for each macro-area (North West, North East, Center, South, Islands).

Table 4 provides the results. Column (4.1) reports GLS estimates treating both average and private education as exogenous, for our sample of 17,251 observations. The R2 is close to 0.40, and all the Mincerian variables enter significantly with point estimates close to previous studies based on the SHIW: see Cannari and D'Alessio (1995), and Colussi (1997). We find that each individual year of schooling increases hourly wages by 4,3 percent[13]. Experience increases wages up to approximately 35 years of experience. Wages of women are 9.5 percent lower than men's wages. Married workers enjoy a 5.5 percent premium due to family allowances, a specificity of the Italian wage system. Also, wages are increasing in the size of the firm. Compared with wages in firms with 20 to 29 employee, wages in very small firms (up to four employees) are 14 percent lower, while wages in firms with more than 500 employees are 12 percent higher. We also find that inter-industry differentials are substantial. Compared with manufacturing, the wage premium is around 7 percent in transports, communications and in the public sector; the premium is above 23 percent in banking and insurance companies.

After controlling for private returns to education, local human capital enters the earning equation with a positive and statistically significant coefficient. A percentage point increase in LLMA average education is associated with a 2.9 percent increase in wages.

GLS estimates, however, may be biased for several reasons. In what follows, we tackle two main possible sources of bias: (i) local shocks, and (ii) unobserved workers' ability.

Local Shocks. As noted by Acemoglu and Angrist (2000) and Moretti (2002), productivity shocks on the local labor market may raise the wage of skilled workers and attract more skilled workers at the same time. Thus, shocks might drive our result by stimulating migration of skilled workers, or by making higher education more attractive to residents. Thus, we need an instrument which is related to average local human capital but uncorrelated with contemporary LLMA-specific productivity shocks. Similarly to Moretti

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[12] The inclusion of controls for the branch of activity and firm-size may be criticized on the ground that such variables are determined together with the labor market outcomes. However, the exclusion of these controls from equation (1) does not change our results.

[13] We also estimate a model in which private returns to education are non-linear in the years of schooling. For this purpose, we replace the categorical variable INDIVIDUAL EDUCATION with dummies for each year of schooling as suggested by Heckman, Layne-Farrar and Todd (1996). This has negligible effects on the estimates of average human capital returns.

(2002)[14], we take the lagged age-structure of the population in each LLMA as the instrument and estimate earnings by G2SLS. We use the 1981 share of individuals in each 5-year cohort between the age of 5 and 75, which generates 16 cohorts. The first-stage regression on the instrument, together with the exogenous right-hand-side variables as from Column (4.1), predicts over 70 percent of variation of average years of schooling across LLMAs in 1991.[15] We test for the validity of the instrument by using the Sargan test,[16] and find that exogeneity cannot be rejected at the 95-percent significance level. Results in Column (4.2) show that instrumenting average education by the lagged age structure of the population produces an increase in social returns from 2.9 to 3.2 percent.[17] Thus, the bias generated by shock-driven migration on GLS estimates appears to be rather unimportant.

This instrumental variable strategy, however, can deal only with "temporary" shocks. In fact, if our results were driven by historical, permanent local characteristics, our 1981 (or 1971) demographic instrument would be endogenous as well.

The treatment of average education in the labor market as an endogenous variable, while keeping the assumption of exogeneity for individual education has been criticized by Acemoglu and Angrist (2000, p.22). Private returns to education may in fact vary across individuals. Therefore, only an instrumental strategy that treats both individual and average education as endogenous can generate unbiased estimates of social returns. To address this issue we proceed by instrumenting individual education by both family background[18] and compulsory schooling laws. Instrumentation of individual education by family background variables has a long tradition in labor economics, and it has been applied by Cannari and D'Alessio (1995) and Colussi (1997) to SHIW data. However, family background-based instruments (in our case, mother's and father's years of schooling) might be criticized since a bias may still arise, unless all unobserved ability components are captured by family background: see Card (1999). To make our estimates more robust to this criticism, we complement family background with an instrument that captures the exogenous variation in school achievement that was induced by the 1962 Mandatory Middle-School Reform. The 1962 reform raised mandatory school attendance from 5 to 8 years of schooling. As explained by Brandolini and Cipollone (2002), the individuals exposed to the effects of the 1962 reform were those who were born between 1949 and 1956. In the first-stage regression

[14] Acemoglu and Angrist (2002, p.20) address the same problem by distinguishing between State-of-Birth and State-of-Residence across individuals.

[15] The F-test on the instruments displays a P-value equal to 0.000.

[16] See Sargan (1988).

[17] Instrumenting with the 1971 demographic structure delivers similar results.

[18] See, for example, Card (1999). For a discussion of the role of family background on schooling in Italy and the US, see Checchi, Ichino and Rustichini (1999).

of individual years of schooling on the set of instruments, we find that an increase in educational qualification of the father (mother), - recorded as none; elementary school; middle school; high school; university degree - leads to an increase in years of schooling of 0.46 (0.34). Moreover, exposure to the 1962 reform leads to a further increase of 0.23 (all the instruments enter with 1% significance). Roughly 40 percent of variation in individual education is explained by the set of instruments, together with the right-hand side variables in Column (4.2). The F-test for the set of instruments displays a P-value of 0.0000 and the Sargan test cannot reject instrument exogeneity at the 95-percent significance level[19]. Results reported in Column (4.3) show that the use of parental education and 1962 compulsory school reform as instruments leads to an increase in the estimate of individual education from 4.3 per cent to 6.0 percent. This is in line both with international evidence surveyed by Card (1999) and the results obtained by Cannari and D'Alessio (1995), Colussi (1997), and Brunello, Comi and Lucifora (2001) for Italy. The estimate of social returns decreases roughly by one third, from 3.2 per cent to 2.1 per cent, but remains highly significant.

Unobserved Ability. Mobility of talented workers across local markets may also complicate identification. In fact, average ability is likely to be correlated with average human capital across areas. Consequently, when high human capital in a LLMA is associated with high returns to unobserved ability, it is plausible that more able workers will migrate into areas characterized by a well-educated labor force.[20] In this case, the correlation between wages and local human capital may partially reflect unobserved ability, rather than true schooling externalities. In other words, omitted ability could affect G2SLS estimates as long as average unobserved ability is correlated with average schooling as predicted by the instruments: see Acemoglu and Angrist (2000). Our data allow us to provide an effective control for such endogenous sorting. Because our data include information on both the province of birth and the province of residence of each worker, we can identify stayers as those who never moved from their area of birth.[21]

[19] The first-stage F-test on the 1962 Mandatory Middle School Reform has a low predictive power in our sample when used as the only instrument. Similarly, Brunello, Comi, and Lucifora (2001) use compulsory schooling laws to augment family background variables when estimating private returns to schooling for the 1995 SHIW sample.

[20] Migration flows in Italy have limited size, compared with the U.S. Internal migration from the South of Italy to Northern regions, a salient feature of the Italian development process during the fifties and the sixties, died out in the first half of the seventies: see Faini, Galli, Gennari, Rossi (1997).

[21] In order to control for endogenous sorting, Moretti (2002) includes a set of *Individual*$\times$*City* dummies in the 1979-1994 NLSY panel, so that variation coming from migrants is lost and identification is based on stayers only. He concludes that unobserved ability is not a major source of bias. By contrast, Ciccone and Peri (2002) find some evidence that cities with higher average schooling do attract better workers. However, Ciccone and Peri (2002) restrict

Out of the 17,251 workers of our sample, 12,467 were resident in their original area of birth at the time of the survey.[22] Columns (4.4)-(4.6) show the results for the stayers sub-sample. The estimated coefficient of social returns is greater in all of the three specifications for the stayers sub-sample than for the entire sample. The specification in Column (4.6), where both individual and average education are instrumented, suggests that social returns to education are equal to 2.9 percent.[23]

Our results thus mitigate the concern that sorting may strongly bias our estimates. We will return to the issue of unobserved ability when considering cost-of-living in the next section.

Once we conclude that the effects of average schooling we find are unlikely to be spurious, there remains a central issue to be investigated. In particular, how much of our Mincerian estimate of η has to be attributed to human capital spillovers, and how much of it is merely due to differences in the composition of the labor force. Ciccone and Peri (2002) show that the magnitude of the bias arising from composition effects in the Mincerian wage equation largely depends on the interaction between individual return to schooling and the average level of human. To estimate the composition effect, we thus included the interaction term (*individual education* $\times$ *average education*) in specifications (4.1)-(4.9). The coefficient

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the definition of stayers to (i) those who lived in the same house over a 20-year period, and (ii) those who had been living in the same city five years before their wages were observed and that were born in the state where they reside.

[22] Since we can match the area of residence with the area of birth for each individual during the nineties, our identifying assumption fails to capture "comebacks", such as individuals who migrated in youth and returned to the place of birth later on. However, "comebacks" are a small percentage of the internal migration rates and are mostly confined to retired workers, who are not included in our sample: see Bonifazi (1999).

[23] Census variables (such as human capital in 1991 and population structure in 1981and 1971) may reflect the changes in the labor-composition structure induced by migration from Southern to Northern Italy, a phenomenon which largely died out after 1975. However, it must be noted that movers and stayers were broadly characterized by similar skills in Italy: see Cannari, Nucci, and Sestito (1997). Moreover, we also consider local human capital effects limited to *young* cohorts, which is, those born after 1959 (3.607 individuals, whose age were less than 15 in 1975), and after 1964 (1.592 individuals, who were less than 10 in 1975). Our Census variable tends to measure more accurately the human capital level to which the younger cohorts were actually exposed. The estimated social returns, obtained under G2SLS with both average and individual education instrumented, are respectively equal to 0.033 and 0.036.

associated with this term is statistically insignificant in all of the specifications.[24] This suggests that our estimates of η merely capture the effects of human capital spillovers.

B. Cost of Living Differentials

As emphasized in Acemoglu and Angrist (2000, p.19) and Moretti (2002), it is the difference in earnings unadjusted for the cost of living that is relevant for assessing the social returns to education. In equilibrium, the fact that firms in some areas pay higher wages must be justified by differences in local labor productivity[25].

Here, we assess whether differences in nominal earnings actually translate into differences in living standards. For instance, the advantage of living in a high-wage area may be largely offset by the price of inelastic factors such as "land". Similarly to Glaeser and Marè (2001), controlling for cost-of-living differentials allows us to test whether our results are driven by workers' unmeasured ability, or by externalities. Average human capital may in fact be positively correlated with unobserved workers' quality. Once individual schooling and other observable characteristics of the worker are controlled for, the omitted ability hypothesis predicts that areas rich in human capital have more able workers, who should earn higher real wages. Under this hypothesis thus, nominal wage differentials across areas will correspond to real wage differentials.

By contrast, when nominal wage differentials do not correspond to real wage differentials, it is unlikely that our results are merely explained by workers' unobserved ability. In this case, it is likely that area characteristics - such as a high average level of local human capital - generate positive spillovers on productivity. Such spillovers can explain why firms still find it convenient to locate in areas rich in human capital and pay higher (nominal) wages. On the other hand, scarce factors such as "land" tend to drive local prices up. As a consequence, nominal wage differentials will not correspond to real wage differentials.

We do not have information on price-level differentials across areas. However, we follow Cannari, Nucci, and Sestito (2000) and assume that cost of living differences can be summarized by the real estate market.[26] To adjust our earning functions for cost-of-living differences , we follow a three-step procedure (see the Appendix for additional details). First,

[24] We thank Antonio Ciccone for suggesting this procedure to us.

[25] This observation is relevant especially for firms that produce traded goods: see Acemoglu and Angrist (2000, p.19, Note 7). To this regard, our estimates for the manufacturing sector in the Section III.C are particularly useful.

[26] Glaeser and Marè (2001) also use housing prices to measure for cost-of-living differentials, a partial measure of local price levels. Another limit of the analysis is the inability to measure for local "amenities".

we use a bundle of characteristics of the dwellings reported in the SHIW to estimate hedonic house prices and rents. Second, we regress the residuals of these regressions – which can be interpreted as the variability of house prices not explained by dwellings attributes – on a full sets of territorial dummies relative to a "province", plus dummies for the size of the municipality where the house is located. Finally, we use the predicted value of this last regression – which can be interpreted as the variability of real estate prices due to territorial and municipality size differences – as a control in our wage equations.

Columns (4.7)-(4.9) show that the index for the cost-of-living always enters significantly at the 1-percent level. The correction for cost of living differentials reduces substantially the effect of social returns on living standards.[27] As can be seen from Column (4.7), the adjustment reduces the benefits from average human capital by roughly one-half. Moreover, when average and individual human capital are instrumented, the impact of social returns to human capital on the standard of living does not significantly differ from zero: see Column (4.9).[28]

Thus, since workers with high unobserved ability should get high real wages, the fact that average human capital has only effects on nominal wages suggests that average human capital is scarcely correlated with unobserved talent. For what it concerns observed ability, note instead that nominal wage differences generated by individual education do correspond to real wage differences. Therefore, we can conclude that unobserved ability is not a major source of bias in our data.

C. Manufacturing Sample

In this section, we restrict our attention to the sub-sample constituted by manufacturing workers. This exercise is motivated by three considerations. First, wages paid in the public sector may reflect an inter-regional redistributive motive, as suggested by Alesina, Danninger, and Rostagno (2001), which may bias cross-LLMA differentials. Second, wages in the service sector might reflect imperfections in the local markets for non-tradable goods and services. By contrast, for industries that produce tradable goods in national or international competitive markets, nominal wage differentials should reflect differences in the marginal productivity of labor. As noted by Rauch (1993) and Glaeser and Mare (2001), if nominal wage differentials did not reflect true productivity differences, firms would move to less expensive locations. Third, there are some LLMA characteristics, such as unemployment, endowment of public infrastructures, or intensity of industrial activity, that might be correlated with average human capital. Focusing on the manufacturing sub-sample

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[27] Results based on the index for rents are very similar.

[28] Restricting the sample to stayers leads to somewhat higher effects on living standards, but the main conclusion does not change.

enables us to control for these potential sources of spurious correlation, since we can use the specific information on manufacturing contained in the CS database.[29,30]

Benchmark estimations based on the manufacturing sample, which includes only 4,485 workers (3,390 of which are truly independent) are reported in the first line of Table 5.[31] We find that social returns in manufacturing range from 3.1 percent to 4.6 percent, about 50 percent higher than those based on the full sample reported in Table 4, Columns (4.1)-(4.3).

Table 5 shows the impact of each additional control on the estimates. We start by considering physical capital in the private sector. Due to capital-skill complementarities[32], local human capital might pick up the contribution of physical capital. The variable PHYSICAL CAPITAL denotes local capital intensity in manufacturing, calculated as the ratio between stock of capital (valued at the replacement price) and value added in each LLMA[33]. We also control for the local level of infrastructures (INFRASTRUCTURE). This variable is measured as the ratio between kilometers of roads and LLMA's surface in squared kilometers (see, e.g., Ciccone and Hall (1986) among many others). Our results show that PHYSICAL CAPITAL enters significantly but with a very low point estimate, while INFRASTRUCTURE does not enter significantly. More important, the coefficient for aggregate human capital is only very marginally affected.

The correlation between education and earnings might also be affected by the distribution of unemployment across LLMAs. If better-educated individuals are less likely to be unemployed, then average human capital might pick up the effect of the unemployment rate. When the LLMA-specific unemployment rate is considered, however, the average human capital coefficient remains essentially unchanged. As found by many others (see for example Casavola et al. (1995)), local unemployment is not a relevant determinant of wages in local labor markets.

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[29] Local unemployment and public infrastructure do not specifically refer to manufacturing. The inclusion of such controls in specifications (4.1)-(4.9) does not lead to any difference in our results.

[30] A limit of this extension, common to Rudd (2000) and Moretti (2002), is that additional controls are treated as exogenous. On this issue see also Duranton and Monastiriotis (2002).

[31] Estimates are unadjusted for cost-of-living differentials.

[32] See, for example, Goldin and Katz (1998).

[33] We have also used the 1993 Company Account Data Service index of capital per worker, which was calculated at the LLMA level by Fabiano Schivardi. The results do not differ from the those reported above.

The relation between LLMA's human capital and earnings could also reflect agglomeration effects, as suggested by Ciccone and Hall (1996).[34] If the density of economic activities attracts human capital by driving returns to education up, one should expect that controlling for agglomeration would reduce the impact of average human capital on earnings. We consider here the LLMA-level share of manufacturing workers over the population (SHAREMANUF).[35] Again, the average human capital coefficient is unaffected.

Finally, as in Glaeser, Kallal, Scheinkman and Shleifer (1992), we control for competition. The index COMPETITION, measured as the ratio between manufacturing average firm-size in the LLMA and the average size at national level, is not significant.

The last specification in Table 5 includes all the controls jointly. In conclusion, our findings from the manufacturing sub-sample support the existence of social returns to education.

In what follows we consider three additional tests which gather additional evidence on external effects from schooling. First, we estimate the impact of social returns when the sample is split according to educational attainments of workers, those who have high education and those who have low education. Second, we estimate the impact of social returns when the sample is split according to geographical areas, workers in the Center-North and workers in the South.

D. High- vs. Low-Education Workers

As emphasized above, when workers of different skills are imperfect substitutes in production, the average external effect of human capital on wages will depend both on the composition of labor supply, and on human capital spillovers. In Section III.A, we argued that composition effects seem to have a negligible impact on our results. However, as noted by Ciccone and Peri (2002), there is also the possibility that human capital externalities at the aggregate level are not Hicks-neutral. Aggregate human capital may have a different impact on the productivity of workers of different education.

In order to investigate the presence of differential effects of aggregate human capital, we estimate separately social returns to education for two skill groups. The first group, the

[34] In a search model of skill acquisitions, Jovanovic and Robb (1989) suggest that productivity depends on both the overall level and spatial concentration of human capital in a local market.

[35] Cingano and Schivardi (2001) find that this measure of agglomeration affects total factor productivity in Italian manufacturing. To account for differences in labor market participation rates, we also replaced this measure with the LLMA share of manufacturing workers over total employment. Results did not change.

unskilled, are those with 8 years of schooling, corresponding to a middle-school Italian diploma, or less. The second group, the skilled, are those with more than 8 years of schooling (high school, college and post-graduate).[36] Results are reported in Table 6. Consistently with the simple theoretical example presented in Moretti (2002), there is some evidence that average education has a larger effect on the wage of the less educated, both for the full sample and the manufacturing sub-sample.

E. Center-North vs. South

In this section we concentrate on regional asymmetries, to assess whether human capital effects depend on the level of local economic development. As is well known, Italy exhibits a pronounced gap between the center-north and the south. In particular, in 1991 per-capita income in the south amounted only to 57 percent of the corresponding figure for the center-north, and this gap has persisted over the last eleven years. Census data also indicate that in 1991 average education in the south was 6.5 years of schooling, against 7.5 years of schooling in the center-north.[37]

A key question is to evaluate whether social returns are similar between center-northern and southern Italy. Schultz (1994) suggests that social returns to schooling are higher in lagged areas. In Table 7 we estimate social returns separately for the center-north and the south. Even though we find no substantial difference for the sample including all sectors, there is large and significant evidence of social returns for southern workers in manufacturing, ranging from 8.0 to 9.0 percent.

IV. Concluding Remarks

The role of social returns to education has been extensively debated during the last decade, after Lucas (1988) showed that human capital externalities may generate sustained growth over the long run. However, cross-country evidence on the effects of human capital on aggregate productivity remains quite controversial – see Barro (1991); Mankiw, Romer, and Weil (1992); Bils and Klenow (2000); and De la Fuente and Domenech (2000).

Recent applied work has shifted towards a Mincerian wage-regression approach to investigate the role of average human capital on individual wages across local labor markets. This approach, which has focused on U.S. data, still casts doubts on the relevance of social

[36] This two-group separation is quite natural in the Italian case, given that mandatory school covers up to 8 years of schooling.

[37] Our sample confirms this 1 percentage point difference between North and Center-South.

returns to education. This paper adds new evidence to the debate by exploiting a sample of workers in Italian local labor market areas.

Our results rely on a definition of local labor market based on the concept of "functional region" (OECD, 2002). We find that social returns to education range between 2 and 3 percent. In addition, our results underscore a feature of the relation between local average human capital and individual productivity that has received limited attention so far. We show that social returns are higher for local markets located in the lagged areas of the south of Italy, which characterized by lower levels of average human capital.

Our conclusions also raise some questions about Italian schooling financing policy, which provides the same amount of per-student funding across areas. Although social benefits of education go beyond the productivity effect measured in this paper, our preliminary results suggest that funding should be primarily directed to the southern lagged areas.

Table 1. Descriptive Statistics for Workers. SHIW Dataset

	1993	1995	1998	2000
Log of HOURLY WAGE RATE	2.49	2.53	2.62	2.66
	(0.46)	*(0.43)*	*(0.44)*	*(0.42)*
INDIVIDUAL EDUCATION	10.36	10.75	11.28	11.29
	(4.34)	*(4.26)*	*(4.11)*	*(4.09)*
EXP	22.72	23.42	23.31	23.57
	(10.31)	*(10.36)*	*(9.93)*	*(10.06)*
DFEMALE	0.37	0.39	0.41	0.41
DMARRIED	0.89	0.88	0.88	0.85
Branch of Activity:				
Agricolture	151	126	129	149
Manufacturing	1,124	1,163	1,086	1,112
Building and construction	248	214	167	201
Wholesale and retail trade	347	363	339	356
Transport and communication	160	142	163	178
Credit and insurance	152	163	157	158
Real estate	103	88	135	125
Domestic services	200	200	139	171
Government	1,969	1,888	1,721	1,569
Extraterritorial organizations	5	7	14	9
Others	83	118	96	63
Firm size:				
Up to 4 employed	321	365	324	330
From 5 to 19	596	672	653	651
From 20 to 49	395	338	479	425
From 50 to 99	267	192	293	344
From 100 to 499	425	389	417	456
500 or more	687	600	542	544
not applicable, public sector employee	1,851	1,916	1,438	1,341
GEO controls:				
Northwest	1,111	1,102	1,047	1,047
Northeast	864	974	791	958
Center	1,047	931	926	914
South	1,121	1,049	974	789
Islands	399	416	408	383
HOUSE PRICES	2.62	1.87	2.14	2.31
	(26.17)	*(25.68)*	*(24.72)*	*(24.51)*
FATHER'S BACKGROUND	2.16	2.19	2.31	2.37
	(0.98)	*(0.97)*	*(0.98)*	*(1.13)*
MOTHER'S BACKGROUND	1.98	2.02	2.13	2.16
	(0.85)	*(0.84)*	*(0.86)*	*(1.00)*
DREFORM62	0.30	0.30	0.32	0.31
	(0.46)	*(0.46)*	*(0.47)*	*(0.46)*
N. Obs.	*4,542*	*4,472*	*4,146*	*4,091*

Notes: Standard deviations of continuous variables in parentheses.

Table 2. Descriptive Statistics for Dwellings. SHIW Dataset

	1993	1995	1998	2000
Log of HOUSE PRICE (in thousand lira)	11.96	12.06	12.13	12.24
	(0.70)	*(0.68)*	*(0.67)*	*(0.66)*
Log of the SURFACE AREA (in m^2)	4.55	4.56	4.59	4.60
	(0.40)	*(0.40)*	*(0.40)*	*(0.40)*
Location:				
Isolated area, countryside	228	267	231	250
Town outskirts	1,602	1,454	1,244	1,359
Between outskirts and town center	1,476	1,474	1,284	1,483
Town center	1,068	1,148	998	950
Other	5	14	34	16
Hamlet	138	161	248	325
Rating of the neighborhood:				
Highly desirable	1,081	1,179	1,054	980
Rundown	222	244	183	173
Neither highly desirable nor rundown	3,213	3,078	2,802	3,216
Other	1	17	0	14
Rating of the dwelling:				
Luxury	27	25	57	49
Upscale	354	443	498	625
Mid-range	2,808	2,847	2,569	2,782
Modest	882	828	594	582
Low-income	393	331	274	281
Very-low-income	53	44	47	64
Dummy if two or more bathrooms available	0.37	0.37	0.41	0.40
Dummy if heating system available	0.84	0.83	0.85	0.86
Dummy if renovation in the last 5 years	0.29	0.32	0.28	0.30
Year the building was constructed	1950.16	1951.06	1950.27	1953.77
	(61.47)	*(62.17)*	*(87.12)*	*(75.07)*
Population density of the dwelling's municipality:				
Up to 20,000 inhabitants	922	1,143	1,014	1,080
From 20,001 to 40,000	864	1,028	859	843
From 40,001 to 500,000	2,220	1,877	1,656	2,083
More than 500,000	511	470	510	377
N. Obs.	*4,517*	*4,518*	*4,039*	*4,383*

Notes: Standard deviations of continuous variables in parentheses.

Table 3. Descriptive Statistics for LLMAs. CS Dataset and 1981 Census Data

CS dataset

HC	7.01	UNEMPLOYMENT	10.35
	(0.64)		*(6.03)*
PHYSICAL CAPITAL	44.70	SHAREMANUF	9.26
	(17.43)		*(6.19)*
INFRASTRUCTURE	43.86	COMPETITION	8.08
	(22.48)		*(3.14)*

1981 Census: Shares of the Population in the indicated Cohort

AGE<5	5.93	40<AGE<44	6.60
	(1.52)		*(0.68)*
5<AGE<9	7.34	45<AGE<49	6.26
	(1.38)		*(0.57)*
10<AGE<14	7.92	50<AGE<54	6.38
	(1.27)		*(0.73)*
15<AGE<19	8.17	55<AGE<59	6.21
	(1.11)		*(0.91)*
20<AGE<24	7.26	60<AGE<64	4.29
	(0.72)		*(0.73)*
25<AGE<29	6.68	65<AGE<69	4.78
	(0.50)		*(0.95)*
30<AGE<34	6.98	70<AGE<74	4.07
	(0.48)		*(0.95)*
35<AGE<39	6.09	AGE<75	5.04
	(0.62)		*(1.44)*

N. Obs. = 235

Notes: Standard deviations in parentheses.

Table 4. Estimates of Social and Private Returns to Education for Full Sample

	(4.1)	(4.2)	(4.3)	(4.4)	(4.5)	(4.6)	(4.7)	(4.8)	(4.9)
					Only Stayers			*CoL Included*	
	GLS	G2SLS	G2SLS	GLS	G2SLS	G2SLS	GLS	G2SLS	G2SLS
SOCIAL	0.029***	0.032***	0.021***	0.031***	0.037***	0.029***	0.015**	0.013	0.007
RETURNS	*(0.006)*	*(0.008)*	*(0.008)*	*(0.007)*	*(0.009)*	*(0.010)*	*(0.006)*	*(0.009)*	*(0.009)*
PRIVATE	0.043***	0.043***	0.060***	0.042***	0.042***	0.057***	0.043***	0.043***	0.059***
RETURNS	*(0.001)*	*(0.001)*	*(0.002)*	*(0.001)*	*(0.001)*	*(0.003)*	*(0.001)*	*(0.001)*	*(0.002)*
Average education instrumented?	NO	YES	YES	NO	YES	YES	NO	YES	YES
Individual education instrumented?	NO	NO	YES	NO	NO	YES	NO	NO	YES
EXP	0.014***	0.014***	0.015***	0.014***	0.014***	0.015***	0.014***	0.014***	0.015***
	(0.001)	*(0.001)*	*(0.001)*	*(0.001)*	*(0.001)*	*(0.001)*	*(0.001)*	*(0.001)*	*(0.001)*
EXPSQR	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	*(0.000)*	*(0.000)*	*(0.000)*	*(0.000)*	*(0.000)*	*(0.000)*	*(0.000)*	*(0.000)*
DFEMALE	-0.095***	-0.095***	-0.097***	-0.086***	-0.086***	-0.089***	-0.095***	-0.095***	-0.097***
	(0.007)	*(0.007)*	*(0.007)*	*(0.008)*	*(0.008)*	*(0.008)*	*(0.007)*	*(0.007)*	*(0.007)*
DMARRIED	0.055***	0.055***	0.057***	0.047***	0.047***	0.050***	0.056***	0.056***	0.057***
	(0.009)	*(0.009)*	*(0.009)*	*(0.011)*	*(0.011)*	*(0.011)*	*(0.009)*	*(0.009)*	*(0.009)*
HOUSE	-	-	-	-	-	-	0.082***	0.085***	0.072***
PRICES	-	-	-	-	-	-	*(0.015)*	*(0.016)*	*(0.016)*
N. Obs.	17,251	17,251	17,251	12,467	12,467	12,467	17,251	17,251	17,251
N. Groups	12,224	12,224	12,224	8,796	8,796	8,796	12,224	12,224	12,224

Notes: Random-effects regressions. Dependent variable: Log of Hourly Wage Rate. Standard errors in parentheses. (**) [***] denotes statistical significance at 10 (5) [1] percent level. The additional controls included in the regressions are 4 geo-controls, 6 firm size dummies, 10 branch of activity dummies, and 3 year dummies.

Table 5. Estimates of Social Returns for Manufacturing Sample

	GLS	G2SLS	G2SLS
BENCHMARK	0.045***	0.046***	0.031***
	(0.009)	*(0.011)*	*(0.011)*
1) PHISICAL CAPITAL	0.045***	0.048***	0.033***
	(0.009)	*(0.011)*	*(0.011)*
2) INFRASTRUCTURE	0.045***	0.046***	0.031***
	(0.009)	*(0.011)*	*(0.011)*
3) UNEMPLOYMENT	0.045***	0.045***	0.029***
	(0.009)	*(0.011)*	*(0.011)*
4) SHARE MANUF	0.050***	0.047***	0.034***
	(0.010)	*(0.011)*	*(0.011)*
5) COMPETITION	0.045***	0.046***	0.030***
	(0.009)	*(0.011)*	*(0.011)*
1) + 2)	0.044***	0.049***	0.034***
	(0.009)	(0.011)	(0.011)
4) + 5)	0.051***	0.047***	0.033***
	(0.010)	(0.012)	(0.011)
1) + 2) + 3) + 4) + 5)	0.054***	0.052***	0.038***
	(0.010)	(0.012)	(0.012)
Average education instrumented?	NO	YES	YES
Individual education instrumented?	NO	NO	YES
N. Obs.	4,485	4,485	4,485
N. Groups	3,390	3,390	3,390

Notes: Random-effects regressions. Dependent variable: Log of Hourly Wage Rate. Standard errors in parentheses. (**) [***] denotes statistical significance at 10 (5) [1] percent level. The additional controls included in the regressions are INDIVIDUAL EDUCATION, EXP, EXPSQR, DFEMALE, DMARRIED, 4 geo-controls, 6 firm size dummies, and 3 year dummies.

Table 6. Estimates of Social Returns: Skilled vs. Unskilled Workers

	UNSKILLED			SKILLED		
	GLS	G2SLS	G2SLS	GLS	G2SLS	G2SLS
Full Sample	0.032***	0.035***	0.030***	0.022***	0.021**	0.005
	(0.009)	*(0.011)*	*(0.010)*	*(0.008)*	*(0.011)*	*(0.011)*
N. Obs.	7,856	7,856	7,856	9,395	9,395	9,395
Manufacturing Sample	0.044***	0.032**	0.034***	0.040**	0.054**	0.023
	(0.010)	*(0.013)*	*(0.013)*	*(0.017)*	*(0.021)*	*(0.023)*
N. Obs.	2,731	2,731	2,731	1,754	1,754	1,754
Average education instrumented?	NO	YES	YES	NO	YES	YES
Individual education instrumented?	NO	NO	YES	NO	NO	YES

Notes: Random-effects regressions. Dependent variable: Log of Hourly Wage Rate. Standard errors in parentheses. * (**) [***] denotes statistical significance at 10 (5) [1] percent level. The additional controls included in the regressions are INDIVIDUAL EDUCATION, EXP, EXPSQR, DFEMALE, DMARRIED, 4 geo-controls, 6 firm size dummies, and 3 year dummies; 10 branch of activity dummies, only for the Full Sample.

Table 7. Estimates of Social Returns: Center-North vs. South

	SOUTH			CENTRE NORTH		
	GLS	G2SLS	G2SLS	GLS	G2SLS	G2SLS
Full Sample	0.038***	0.026**	0.021*	0.026***	0.029***	0.019***
	(0.013)	(0.013)	(0.012)	(0.007)	(0.008)	(0.007)
N. Obs.	5,539	5,539	5,539	11,712	11,712	11,712
Manufacturing Sample	0.080***	0.090***	0.089**	0.035***	0.037***	0.018
	(0.028)	(0.035)	(0.033)	(0.009)	(0.011)	(0.011)
N. Obs.	793	793	793	3,692	3,692	3,692
Average education instrumented?	NO	YES	YES	NO	YES	YES
Individual education instrumented?	NO	NO	YES	NO	NO	YES

Notes: Random-effects regressions. Dependent variable: Log of Hourly Wage Rate. Standard errors in parentheses. * (**) [***] denotes statistical significance at 10 (5) [1] percent level. The additional controls included in the regressions are INDIVIDUAL EDUCATION, EXP, EXPSQR, DFEMALE, DMARRIED, 4 geo-controls, 6 firm size dummies, and 3 year dummies; 10 branch of activity dummies, only for the Full Sample.

APPENDIXES

I. DEFINITIONS OF TERRITORIAL UNITS

Local Labor Market Area (LLMA). LLMAs are functional regions that correspond to local labor markets. The concept of local labor market is strictly related to the concept of self-containment, which describes the ability of an area to concentrate the highest possible amount of human relations taking place between the places where production activities are performed (place of work) and the places related to social reproduction (place of residence). The areas so identified form a local system, because inside them there is a concentration of residential activities (such as most individual and family consumption), of work activities (such as expenses for production and distribution) as well as those social relations that are created between those two poles. The reference to daily travels contributes to the definition of local system in terms of space and time. LLMAs are the aggregation of two or more neighboring municipalities defined on the basis of daily travel flows from place of residence to place of residence to place of work. The procedure is based on the 1991 census intra-municipality daily commuting flows matrix: see ISTAT (1997). Self-containment is defined in terms of both labor demand side (number of employed persons living and working in a LLMA as compared to total number of employed persons in that LLMA) and the supply side (number of employed persons living and working in a LLMA as compared to total number of residents in that LLMA), with a threshold level set at 75% (which is fully stringent on the demand side, while on the supply side it does not apply in 270 LLMAs). As emphasized in OECD (2002), LLMAs provide an attractive concept of local labor markets: by construction, labor mobility within LLMAs is very high while mobility from and to other LLMAs is little.

Metropolitan Area (MA). MAs are based on county units. To be considered as a MA, a county needs a city or "urbanized areas" (residents in contiguous area with a population density of at least 1,000 residents per square mile) of at least 50,000 residents. Adjacent counties are part of the metropolitan area if at least half of their population is in the urbanized area surrounding the largest city, while additional outlying counties are included in the MA if they meet specified requirements of commuting to the central counties and other selected requirements of metropolitan character (such as population density and percent urban). Approximately 20 percent of US population and 80 percent of its territory are outside of MAs.

II. Local Cost of Living

Calculation of the index of the local cost of living is based on the real estate section of the SHIW, where families provide information on their main residence, be it owned or rented. In the former case, the owner provides her best estimate both for the rent she could charge and the price that could be set for the dwelling. In the second case, the tenant reports the actual rent, as well as her best estimate of the dwelling's price. In addition to rents and house prices, the SHIW reports several other characteristics. Our estimates of hedonic prices and rents exploit the following: surface area, geographic location (isolated area, countryside; town outskirts; between outskirts and town center , town center, other; hamlet); rating of the neighborhood (highly desirable; neither highly desirable nor run down; run-down; other); rating of the dwelling (luxury; highly desirable; mid-range; modest; low-income; very low-income); year of construction; dummies for (i) renovations carried on recently;(ii) two or more bathrooms available; (iii) presence of heating system. House prices and rents are estimated using respectively 17,457 and 15,574 observations at the family level (all the available families are considered, no matter the employment status of family's components). We use a logarithmic specification, as suggested by standard RESET tests. Houses' characteristics explain roughly 50 percent of the variability of house prices and 42 percent of the rent variability. In the second step, the 103 "province" dummies and the 3 dummies for municipality population explain 30 percent (20 percent) of the house price (and rent) residual variability (these results are available by the authors). Both rents and prices are estimated because rents might still reflect both laws (the "equo canone") that artificially compressed rents below market values, and specific rent policies applied to public property. The index for rents, however, gives results similar to those obtained by considering house prices.

Our price index mimics well other indexes for cost of living differentials. As for the North – South differential, our index points to a 29 percent difference in housing costs over the period 1993-2000. This is in line with the 30 percent North–South differential calculated by Cannari, Nucci, and Sestito (2000) on another source ("Il Consulente Immobiliare", 1995). This is much larger than the 14.3 per cent differential estimated by Alesina, Danninger, and Rostagno (2001). In order to calculate cost of living adjustments relative to 1995, they cumulate city price deflators over 50 years. City price deflators are available only for 13 major Italian cities, and the authors themselves warn that their measure "probably underestimates the extent of higher cost of living in the North" (p. 469). As additional check, we have compared the ranking for the regional cost of housing obtained from our index with the index provided by "Il Consulente Immobiliare" (this index is available only for newly built houses over the period 1991-2001), and we have found that the two rankings are remarkably similar.

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