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Intergenerational earnings elasticity of actual father-son pairs in Italy accounting for lifecycle and attenuation bias

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Abstract

Using a rich longitudinal dataset built by merging administrative and survey data, this article contributes to the literature on intergenerational inequality by providing the first reliable estimate of the intergenerational earnings elasticity (IGE) in Italy based on actual father-son pairs and by comparing the size of the lifecycle bias when sons are selected by age or by potential experience (i.e., the distance from the year they ended their studies). Our findings confirm that Italy is a low-mobility country because in our baseline estimates, the IGE at sons' potential experience level 6 is approximately 0.40 and is robust to various measures of fathers' lifetime earnings. However, our results might be downward biased by the young age of sons. To measure the lifecycle bias and correct the IGE estimates, we run the "forward regression" of yearly earnings on lifetime earnings on a sample of workers followed for 30 years. We find that selecting sons by potential experience rather than by age reduces the lifecycle bias at young ages and that the "corrected" IGE is 0.47.

Keywords: intergenerational earnings elasticity; lifecycle bias; attenuation bias; intergenerational inequality; Italy.

JEL Classification: J62, D31, D63.

1. Introduction

Cross-country comparisons of intergenerational inequality – usually measured through intergenerational earnings elasticity (IGE), which is estimated regressing log children's earnings (when adult) on log parental earnings (Björklund and Jäntti, 2009) – agree on the rankings among developed countries (e.g., Blanden, 2013; Corak, 2013): Nordic European countries are the most mobile, while the US, the UK and Italy are among the most unequal countries, with IGEs above 0.40.

To correctly estimate the IGE, panel datasets covering subsequent generations are needed. However, when proper longitudinal datasets following the two generations for many years are not available, IGE estimates are downward biased because of attenuation and lifecycle biases deriving from the fact that point-in-time earnings are far from a good proxy of lifetime earnings (Haider and Solon, 2006; Mazumder, 2005). Furthermore, where, as in most countries, longitudinal datasets observing parents in middle age and their children when adult are not available, it is not possible to directly link parents' and children's earnings, and the intergenerational association can only be estimated applying the two-sample two-stage least squares (TSTSLS) method, i.e., by imputing parents' earnings exploiting repeated cross-sectional datasets where retrospective information on parents' characteristics is available.[1] The literature has often noted that the TSTSLS method might produce biased coefficients not perfectly comparable to those obtained from the baseline OLS estimator, thus limiting the cross-country comparability of IGEs estimated through different methods (Blanden, 2013). The direction of the bias is undetermined (Olivetti and Paserman, 2015) even if, according to most authors, the TSTSLS estimate of the IGE is likely upward biased (Björklund and Jäntti, 1997).

Regarding Italy, the focus of this article, previous estimates of the IGE – which draw the picture of an immobile country with an IGE between 0.45 and 0.50 (Mocetti, 2007; Piraino, 2007; Barbieri et al., 2019) – were based on the TSTSLS method due to the lack of longitudinal data tracking subsequent generations for at least a portion of their working careers. Moreover, these studies observed the earnings of the two generations in at most a few years. Therefore, because of the possible biases that might have affected the previous estimates, a crucial question concerns the reliability and cross-country comparability of IGE estimates for Italy.

In this article, exploiting a recently developed longitudinal dataset, we are able to answer this question and provide the first reliable estimate of the IGE in Italy since we can consider actual father-son pairs instead of relying on the TSTSLS method and can carefully take into account both the attenuation and the lifecycle biases.

This dataset was recently built merging the 2004-2008 waves of the Italian component of the European Union Statistics on Income and Living Conditions (EU-SILC; the Italian component is called the IT-SILC) with longitudinal social security records managed by the Italian Social Security Institute (INPS). For all the individuals interviewed in the IT-SILC, the dataset enriches information available in the survey's waves with administrative records about employment and earnings histories from the year the individuals entered the labour market until 2014. The characteristics of the dataset allow us, on the one hand, to couple the son and his co-residing father by using IT-SILC variables that link individuals living in the same household at the moment of the interview and, on

[1] The TSTSLS method was introduced in the empirical literature on intergenerational mobility by Björklund and Jäntti (1997) and has then been used in many countries where panel datasets covering subsequent generations are not available (see the review by Jerrim et al., 2016).

the other hand, to track over time sons' and fathers' gross annual earnings (from employment and self-employment) through the longitudinal information collected in administrative archives.

Specifically, using IT-SILC variables regarding the current educational status and the year when the highest education was attained, we select a main sample of 763 sons who achieved the highest degree and ended their studies at most 2 years before the calendar year of the interview (e.g., in 2002-2004 for those belonging to the IT-SILC 2004 wave, in 2006-2008 for those belonging to the 2008 wave), i.e., we select sons who had just become active at the moment of the interview. Since Italy is characterized by an extremely high share of youngsters co-residing with their parents before and right after leaving education, this selection rule allows us to couple the son and his actual parents for more than 90% of the sample of respondents who finished their studies at most 2 years before the interview, thus reducing a possible co-residence bias (Nicoletti and Francesconi, 2006). Once we have selected father-son pairs according to this rule, we use social security records observing sons' earnings until 2014, while, in the baseline estimates, average fathers' earnings when the sons were aged 0-14 years are considered.

Apart from the advantage from having actual father-son pairs at our disposal, our analysis has further advantages with respect to previous studies about Italy. First, as in Barbieri et al. (2019), we measure the IGE by observing earnings of two subsequent generations from high-quality longitudinal administrative data that track over time all individuals working in Italy and, by definition, are not affected by measurement errors and attrition. Second, unlike other ongoing research on intergenerational inequality in Italy on actual parent-child pairs (Acciari et al., 2019), we observe fathers' earnings for several years, thus reducing the attenuation bias related to transitory shocks and variations (Solon, 1992; Mazumder, 2005). Third, exploiting the "prospective" longitudinal dimension of our dataset, we can observe sons' earnings along the early stages of their working careers, thus showing the trend of the IGE in the early phase of the careers and verifying the possible existence of a lifecycle bias.

Our findings clearly confirm that Italy is a low-mobility country since the IGE is 0.404 at the 6th year after sons stopped studying – i.e., at potential experience level 6 – and it becomes 0.450 when we follow up to potential experience level 11 the small sub-sample of those who ended their studies in 2002-2003 (207 individuals).[2] However, these results might still be downward biased because of the young age of the sons, especially in the main sample (on average, the sons are aged approximately 26 at potential experience level 6).

In fact, the empirical literature on intergenerational inequality has noted that the association between children's current and lifetime income varies over the lifecycle and that this creates a downward bias in the IGE estimate if children are observed when they are too young since earnings profiles are steeper for those with higher long-run earnings (Böhlmark and Lindquist, 2006; Haider and Solon, 2006). However, the bias is likely increased when we consider children at the same age – as is usually done in the literature on intergenerational inequality – since those who study more (who, on average, come from more advantaged backgrounds; Hertz et al. 2008) enter the labour market later and, at a given young age, have less work experience. In other words, at a certain young age, differently educated individuals are in different parts of their career patterns. Instead, selecting individuals by potential experience rather than by age, as we do in this article, allows us to estimate the IGE by observing sons at the same point in their careers,

[2] Unless differently specified, in this article we use "potential experience" and "experience" as synonyms.

regardless of their age, and might reduce the lifecycle bias.

As a further novelty of this article, our very rich dataset allows us to directly compute the size of the lifecycle bias by the sons' ages or potential experience, applying the error-in-variables model proposed by Haider and Solon (2006) and, therefore, to correct our IGE estimate by the size of the estimated bias. To this aim, we run the "forward regression" of log yearly earnings on a very good proxy of log lifetime earnings since we can observe 30-year working histories of a representative sample of Italian males who stopped studying between 1978 and 1984.

Notably, we find that the lifecycle bias is largely reduced when sons are observed at low experience rather than at young ages: It is 13% at potential experience level 6 and becomes zero between potential experience levels 12 and 14, while, conversely, at age 26 (the mean age of sons observed at potential experience level 6), the lifecycle bias is approximately 20%. This finding confirms that a good portion of the bias is related to the fact that at the same age young individuals have different work experience and that more educated workers – who more frequently come from better backgrounds – have less experience than less educated workers as the latter left education earlier.

Finally, correcting estimated IGEs at potential experience levels 6 and 11 by the size of the estimated lifecycle bias, we find an IGE of approximately 0.47, which confirms that Italy is among the least mobile developed countries.

Therefore, this article contributes to the literature on intergenerational inequality in two ways: i) providing the first reliable estimate of actual father-son pairs in a low-mobility country such as Italy, carefully taking into account both attenuation and lifecycle biases, and ii) showing that selecting sons by potential experience rather than by age reduces the lifecycle bias in the early phase of the career, thus allowing researchers to improve estimates when samples of relatively young sons who differ by age but have the same experience are available.

The article is structured as follows. Section 2 briefly reviews empirical issues related to the IGE estimate (Section 2.1) and the previous evidence for Italy in cross–country comparison (Section 2.2). Section 3 explains the main features of our dataset. Section 4 presents the baseline IGE estimates when sons are followed in the period after they left education (Section 4.1), taking into account the issue of attenuation bias related to the measure of fathers' permanent earnings (Section 4.2). Section 5 presents the results of the "forward regression" of log yearly earnings on log lifetime earnings for a representative sample of Italian workers, run to compute and compare the size of the lifecycle bias by age or potential experience. Then, Section 6 shows "corrected IGEs" when the size of the estimated lifecycle bias is considered, and Section 7 concludes.

2. Related literature

2.1 Empirical issues in the IGE estimate

In the last few decades, many empirical studies carried out by economists and social scientists have analysed to what extent economic advantages are transmitted from one generation to the next.[3] In this literature, different indicators have been used to summarize the degree of intergenerational mobility. The most common index used by economists to measure the degree of

[3] For more details about the approaches used in empirical research to estimate mobility across generations, see, among the others, Black and Devereux (2011); Björklund and Jäntti (2009); Blanden (2013); Chetty et al. (2014).

intergenerational earnings persistence (i.e., of intergenerational inequality) is the IGE, which is estimated by regressing the log of children's earnings on the log of parents' (usually the father's) earnings as in the following equation:

$$y_i^c = \alpha + \beta y_i^p + \varepsilon_i \qquad (1)$$

where y^c and y^p are log children's and parents' earnings, respectively, and ε_i is a disturbance. According to this measure, a country is completely mobile when the estimated β is 0, while the higher the estimated β is, the higher the degree of intergenerational inequality is.

Since background-related earnings premia may develop over the entire working career, IGE estimates should be run considering lifetime rather than point-in-time (usually yearly) earnings of the two generations. However, lifetime earnings of children and their parents are usually not available because of the lack of panel data following two subsequent generations during their entire working lives. This is why researchers face several measurement issues when estimating the IGE without having lifetime earnings for both generations at their disposal, and these issues may cause both right-hand and left-hand side measurement errors in equation (1).

The first type of error is related to the number of years of observation of parents' earnings and might cause the so-called attenuation bias, i.e., an underestimation of the IGE when parents' earnings are observed for few years. For instance, early studies estimating the intergenerational earnings persistence in the US by using point-in-time earnings found an IGE of approximately 0.2 that confirmed the ideal of the US as a very mobile society, consistent with the "American Dream" (Becker and Tomes, 1986; Behrman and Taubman, 1986). However, subsequent works demonstrated that these pioneering estimates were substantially downward biased due to the "attenuation bias":[4] Solon (1992) and Zimmerman (1992), averaging fathers' earnings over four or five years, found for the US an IGE of approximately 0.4/0.5, while, later, Mazumder (2005) and Chen et al. (2017), in two studies on the US and Canada, respectively, showed that even using 5-year averaged earnings may lead to underestimation of the IGE since transitory shocks that cause right-hand side measurement errors are likely to be extremely persistent.

Left-hand side measurement errors may instead cause a lifecycle bias lowering the estimated IGE if children's earnings are observed when they are too young. Estimated IGEs are indeed influenced by the amount of earnings dispersion, and earnings dispersion at a given young age is very likely to be lower than the dispersion of lifetime earnings. Earnings dispersion tends to rise as children grow older since age-earnings profiles are heterogeneous and are steeper for those with higher permanent earnings (Böhlmark and Lindquist, 2006; Haider and Solon, 2006). Heterogeneity in earnings growth across individuals is mostly related to the heterogeneity in human capital investment (Rubinstein and Weiss, 2006) and – with earnings profiles being steeper for more educated children and education being associated with parental background (Hertz et al., 2008; Holmlund et al., 2011) – the IGE estimate is clearly biased if too-young children are observed. Moreover, especially at young ages, individuals have different years of work experience according to their education since high-skilled workers usually enter the labour market several years after low-skilled workers belonging to the same birth cohort.

Nevertheless, two empirical estimates of lifecycle earnings variation made by Haider and Solon (2006) and Böhlmark and Lindquist (2006), for the US and Sweden, respectively, suggest that, for

[4] These studies were also plagued by a serious lifecycle bias due to the young age of observed sons.

males, the lifecycle bias can be greatly reduced by observing children when they are aged approximately 35 since the difference between yearly and lifetime earnings is minimized when individuals are observed at mid-career. In contrast, a simple rule for minimizing the lifecycle bias does not emerge for women, as they display more variety in their lifecycle income profiles. However, Nybom and Stuhler (2016), while confirming that the best way to minimize lifecycle bias is to take male individuals at median ages, warn that it can be very difficult to identify the specific age at which males' lifetime earnings are perfectly approximated by annual earnings since age-earnings profiles may be worker-, country- or cohort-specific.

2.2 IGE in Italy in cross-country comparison

Previous empirical studies for Italy carried out by Mocetti (2007), Piraino (2007), and Barbieri et al. (2019) estimated the IGE through the TSTSLS approach due to the impossibility of directly linking adult children with their parents' past earnings in the longitudinal datasets available to them. Therefore, these scholars exploited repeated cross-sectional datasets where retrospective information on parents' characteristics (e.g., education, occupation, region of residence) recalled by the offspring was available and used these characteristics to predict parents' earnings through a sample of "pseudo-parents" observed when the children were young and then regressed the children's on the pseudo-parents' earnings to estimate the IGE.[5]

Using the TSTSLS method, these studies estimated an IGE between 0.45 and 0.50 depending on the number of predictors used to impute fathers' earnings, the number of years the two generations were observed, the data used, and the earnings definition adopted (e.g., net or gross of taxes). According to these estimates, Italy is considered an immobile country within the cluster of the most developed countries, with IGE values similar to those found in the most reliable estimates for the US and the UK (Table 1). However, recently, Acciari et al. (2019), using administrative data on tax returns for two generations of Italians, have linked for the first time in Italy children to their actual parents and have found an IGE equal to 0.25, considerably lower than the values estimated through the TSTSLS method. The value estimated by Acciari et al. (2019) would thus rank Italy among the countries with the highest intergenerational income mobility.

These very divergent estimates for Italy are puzzling and demand careful consideration of the authors' methodological choices.

Actually, on the one hand, the TSTSLS method may produce coefficients not perfectly comparable to those obtained from an OLS regression of the log of sons' earnings on the log of their actual fathers' earnings for two main reasons (Olivetti and Paserman, 2015). First, TSTSLS estimates are upward biased as the standard deviation of imputed fathers' earnings is by construction lower than the standard deviation of actual fathers' earnings, unless the R^2 in the first-stage regression (where parents' incomes are imputed) equals one. Second, a downward bias might also emerge due to the unpredicted share of fathers' income positively correlated with sons' income. The direction of the bias is theoretically undetermined, even if, comparing IGE estimates obtained in the same US dataset using OLS and TSTSLS, Björklund and Jäntti (1997) argue that TSTSLS estimates are likely upward biased.[6]

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[5] As in most empirical studies estimating the IGE, they focus on father-son pairs to avoid the selection bias related to the lower labour force participation of women.

[6] Following the results of Björklund and Jäntti (1997), Blanden (2013) suggests to re-scale estimated IGEs obtained

On the other hand, however, while the TSTSLS estimates might be plagued by an upward bias, the study by Acciari et al. (2019), due to data limitations, is likely to have been heavily affected by both downward lifecycle bias – they observed children aged 28-42 – and, mostly, attenuation bias since the authors were forced to proxy lifetime parents' earnings by considering only three yearly observations measured when fathers were aged, on average, approximately 51.[7]

Table 1: Intergenerational earnings elasticity in developed countries

Country	Source	Empirical approach	IGE
US	Zimmerman (1992)	OLS	0.54
Italy	Barbieri et al. (2019)	TSTSLS	0.50
Italy	Mocetti (2007)	TSTSLS	0.50
Italy	Piraino (2007)	TSTSLS	0.44
UK	Gregg et al. (2017)	OLS	0.43
Spain	Cervini-Plá (2015)	TSTSLS	0.42
France	Lefranc and Trannoy (2005)	TSTSLS	0.40
Germany	Schnitzlein (2016)	OLS	0.39
Norway	Nilsen et al. (2012)	OLS	0.34
Canada	Chen et al. (2017)	OLS	0.32
Sweden	Heidrich (2017)	OLS	0.25
Italy	Acciari et al. (2019)	OLS	0.25

Notes: To maximize comparability and avoid the inclusion of downward biased estimates of the IGE, almost all IGEs which use the OLS estimator reported in this Table have been estimated after averaging fathers' earnings using at least 4/5 yearly observations. The only two exceptions are the studies by Acciari et al. (2019) and Gregg et al. (2017), which take three and two parental income observations, respectively.

Therefore, in this article we aim to shed light on the true level of intergenerational inequality in Italy, presenting new OLS estimates on actual father-son pairs (i.e., without relying on the TSTSLS approach) and less plagued by the severe measurement issues that might have downwardly biased the estimates by Acciari et al. (2019).

through the TSTSLS method by a factor of 0.75, even if she acknowledges that results found for the US cannot be generalized to other countries.

[7] However, note that, following Chetty et al. (2014), Acciari et al. (2019) are mainly interested at inquiring within country geographical differences in the degree of mobility by using the rank-rank slope instead than the IGE as a measure of intergenerational persistence.

3. Data and sample selection

We use a longitudinal dataset, called the AD-SILC (where the "AD" stands for "administrative"), built merging (using fiscal codes as a matching key) the 2005–2008 waves of the Italian component of the EU-SILC survey (IT-SILC) with high-quality administrative data collected by the Italian Social Security Institute (INPS). INPS data record employment and earnings histories of all individuals working in Italy from the moment they entered the labour market until the end of 2014.[8] Therefore, the AD-SILC dataset enriches IT-SILC cross-sectional waves with the entire longitudinal working history of the individuals sampled in the IT-SILC.

INPS archives track over time the employment histories of all individuals working in Italy, i.e., employees in the public and private sectors, para-subordinate workers (individuals who are formally self-employed but are usually dependent on a single employer) and all self-employed categories (craftsmen, dealers and professionals). However, since no variables linking parents and children are available in these archives, INPS data do not allow researchers to couple parents and children and investigate intergenerational inequality. This limitation is overcome through matching with IT-SILC waves where family linkages for individuals co-residing in the same household are recorded. Hence, our dataset allows us, on the one hand, to couple the child and co-residing parents at the moment of the IT-SILC interview and, on the other hand, to track over time children's and parents' gross annual earnings (from employment and self-employment, including personal income taxes and social contributions paid by the worker) through the longitudinal information collected in administrative archives. Consistently with most IGE estimates, in this article we focus on father-son pairs to get rid of participation constraints, which are particularly cumbersome to address for females due to the low labour force participation of women in Italy.

However, a crucial issue concerns the possible existence of a co-residence selection bias when sons are coupled with their parents (Nicoletti and Francesconi, 2006; Dunn, 2007). Indeed, if too-old sons were selected – e.g., considering adult sons who have been working for a long time – the sample of those still residing with their parents would be clearly biased. To mitigate this possible type of selection bias, we exploit a peculiar feature of Italy where almost all youngsters co-reside with their parents before and right after leaving education.[9] Making use of the IT-SILC variables regarding current educational status and the year when the highest education was attained, we select in each IT-SILC wave sons who achieved their highest degree and ended their studies at an age no lower than 15 and no higher than 30 at most 2 years before the calendar year of the interview (e.g., in 2002-2004 for those belonging to the 2004 wave, in 2006-2008 for those belonging to the 2008 wave).[10] In other words, we select sons who had just become active at the moment of the IT-SILC interview. Thanks to this selection rule, we are able to couple an extremely high share of parents-son pairs, thus strongly mitigating the risk of a possible co-residence bias; indeed, we link to their actual parents 90.43% of all respondents who left education at most 2

[8] The IT-SILC survey differs from the standard EU-SILC only for the inclusion of additional country-specific variables.

[9] To have an idea, ISTAT (2013) shows that – independently on their education and occupational status – 93.8% of males aged 18-24 resided with their parents in Italy in 2012. Note also that young individuals studying in a city different from the city where their parents reside are included in the original household in IT-SILC if they do not change their legal residence (a very rare event in Italy).

[10] Individuals belonging to the panel component of the IT-SILC are considered only once in our sample selection. We exclude from the analysis non-Italian sons as their fathers' earnings might be badly measured due to possible periods spent working in the origin country (not recorded in Italian administrative archives).

years before the calendar year of the interview.[11]

As explained below, we exclude from the main sample the few father-son couples (28 pairs) whose fathers have in the administrative archives fewer than 4 yearly observations with positive earnings in the period when the sons were aged 0-14. According to our selection rules, our main sample is then composed of 763 father-son pairs. In additional estimates carried out in Section 6, we also use a small subsample – composed of 207 father-son pairs – selected considering sons who ended their studies in 2002-2003 and are followed in our dataset until the 11th year of potential experience.

Once we have selected the father-son pairs, we use longitudinal social security records to estimate the IGE according to equation (1). Specifically, similarly to the approach proposed by Raitano and Vona (2018) to analyse the association between fathers' education and sons' earnings over their entire working careers, we distinguish sons according to the distance from the year when they became active – i.e., by potential experience – and regress through OLS at various sons' experience levels log of sons' annual earnings on log of fathers' earnings, where various good proxies of fathers' lifetime earnings are considered to eliminate a possible attenuation bias.

Since our dataset records individual earnings until the end of 2014 and we select sons who ended their studies from 2002 to 2008, we may observe all the selected sons until the sixth year of their potential careers – i.e., at potential experience level 6 – independently of their age (i.e., the first cohort of those who ended their studies in 2002 is followed in the 2003-2008 period, while the last cohort of those who ended their studies in 2008 is followed in the 2009-2014 period). At each value of potential experience, age differences within the sample of sons thus depend on the length of the educational path since more educated sons leave education at higher ages.

Although we are able to directly link to their actual fathers the vast majority of sons who left education no later than 2 years before the interview, it is important to compare summary statistics on the characteristics of sons in our final sample to the same characteristics observed in the full sample of individuals who left education no later than 2 years before the interview (i.e., also including those sons not linked to their actual fathers and those whose fathers have fewer than 4 positive annual earnings observations).

Reassuringly, individuals in the two samples are highly comparable in terms of the mean and the standard deviation of gross earnings at various experience levels, as shown in Figure 1.[12] Specifically, in both the selected and full samples, on average, sons' earnings steadily increase in the 6-year period after they left education, while the earnings dispersion increases at higher experience levels.

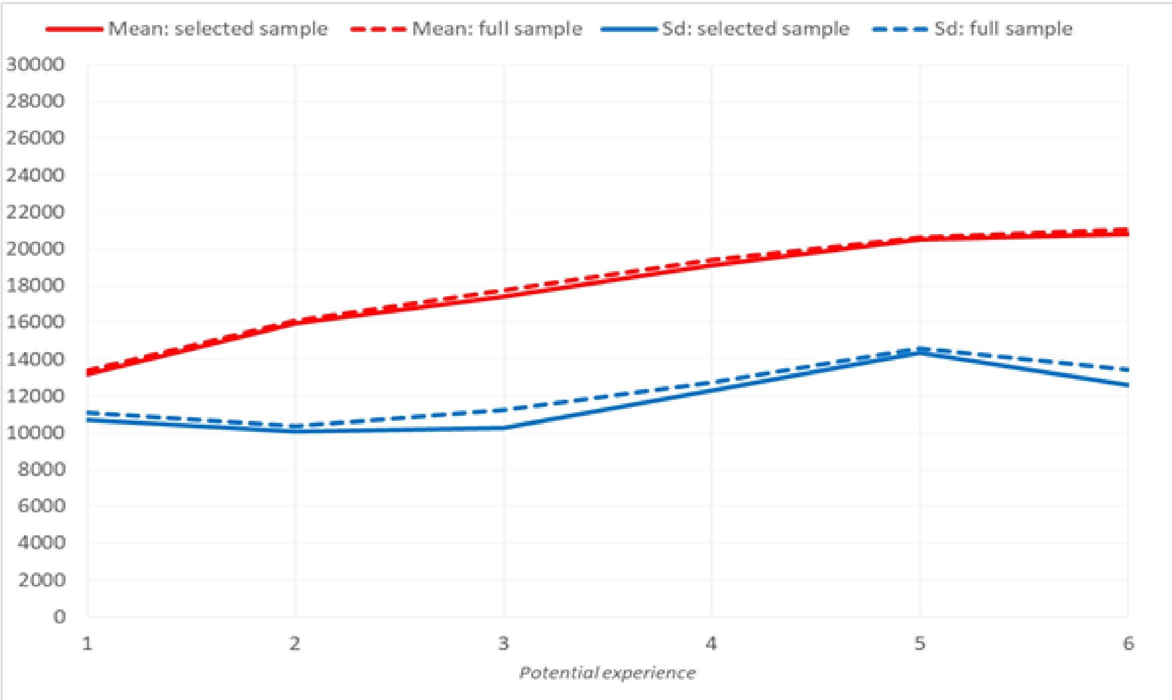
Table 2 summarizes other socio-economic characteristics of sons comparing the selected sample to the full sample of males interviewed no later than 2 years after they left education. As is clear from Table 2, the observed distribution of characteristics is similar in the two samples since

[11] According to computations on a representative sample of Italian sons aged 1 to 14 who were born between 1980 and 1994 recorded in the Bank of Italy's Survey on Household Income and Wealth (SHIW), we find that around 3% of Italian sons belonging to the same birth cohort of our selected sons did not grow up with their parents. Hence, taking into account also this type of individuals who, by definition, cannot be linked to their co-resident parents, the share of sons coupled with their parents according to our selection rule becomes still higher.

[12] Note that, throughout this article, individuals with zero earnings at a certain potential experience are not considered in the analyses concerning that experience.

selected sons are highly comparable to the full sample of sons in terms of age when leaving education (20.31 vs. 20.49), possible weeks of work experience before leaving education (26.16 vs. 29.39) and years of education (13.98 vs. 14.11).

Figure 1: Sons' earnings by potential experience: selected sample versus full sample



Notes: Only individuals with positive earnings are considered. Source: Authors' elaborations on the AD-SILC dataset.

Table 2: Summary statistics of sons: selected sample versus full sample

	Selected sample		Full sample	
Age when stopped studying	20.31	(3.47)	20.49	(3.57)
Experience (weeks) when stopped studying	26.16	(65.36)	29.39	(72.57)
Years of education	13.98	(2.72)	14.11	(2.76)

Notes: Only sons with positive earnings are considered. Mean values without the parentheses, standard deviations in parentheses. Source: Authors' elaborations on the AD-SILC dataset

While, as clarified, we consider sons' annual earnings at various experience levels, a crucial issue concerns the measure of fathers' lifetime earnings. In our baseline estimates, we observe fathers when their sons were aged 0-14 and average their earnings over that 15-year period (without including in the computation possible years with zero earnings). As mentioned, to reduce the incidence of right-hand measurement errors that may cause a downward bias in the IGE estimate, we include in our baseline estimate only fathers with at least 4 positive annual earnings observations.

Confirming the high quality of our longitudinal dataset, nearly all the fathers have positive earnings observations in most of the 15-year period considered: 59.1% have positive earnings in the entire 15-year period when their sons were aged 0-14, and, on average, the number of fathers' observations with positive earnings in the 15-year period is 13.24. Hence, our measure of fathers' lifetime earnings should not be highly affected by measurement errors related to transitory shocks and variations affecting yearly earnings, and, thus, our IGE estimates should not be plagued by a serious attenuation bias. Furthermore, as shown in Section 4, our results are also robust to additional estimates run by using proxies of fathers' lifetime earnings computed by considering a different number of observations or by focusing on a different age class of fathers.

Finally, Table 3 shows mean and standard deviation of fathers' and sons' age and earnings (computed at constant prices by using the consumer price index - CPI), where sons' values are observed at potential experience level 6, while fathers' values are averaged over the 15-year period when their sons were aged 0-14. At potential experience level 6, sons are aged on average 26.11 years. Therefore, we are not able to follow the selection rule suggested by Haider and Solon (2006) and Böhlmark and Lindquist (2006) to minimize the lifecycle bias when lifetime earnings of the sons' generation are not available.

Table 3: Distribution of age and earnings in the main sample of sons and their actual fathers

	Earnings		Age	
	Sons	Fathers	Sons	Fathers
Mean	21,059.8	24026.7	26.1	38.0
Sd	12,747.8	11826.3	3.3	5.2
Obs.	763	763	763	763

Notes: Earnings in Euros at 2012 constant prices (deflated by using CPIs). Sons observed at potential experience level 6. Only those with positive earnings and their fathers with at least 4 positive earnings observations recorded in the 15-year period when the son was aged 0-14 are considered. Source: Authors' elaborations on the AD-SILC dataset.

Nevertheless, as discussed in Section 2 and proven in Section 5, the specific selection rule adopted in this article allows us to reduce one of the two factors that may cause the lifecycle bias. Indeed, our estimated IGEs are obtained by observing sons at the same point of their career, independently of their age. This means that we are not estimating the intergenerational earnings persistence by generically selecting young sons within a given age range and, thus, at different points of their working career. The only potential source of lifecycle bias in our estimated coefficients is related to existing differences in the earnings growth rate, which are likely to persist even after potential experience level 6.

4. IGE estimates controlling for the attenuation bias

4.1 Baseline estimates

In this section, we present the OLS estimates of the IGE run according to equation (1). As noted, we consider as dependent variable the log of gross annual earnings of sons at various potential experience levels to observe the trend of the estimated IGE in the early phase of their careers, while as proxy of fathers' lifetime earnings we average earnings obtained in the 15-year period when their sons were aged 0-14 (excluding from the analysis fathers with fewer than 4 positive observations over the 15-year period).

As control variables in our baseline model (model 1 henceforth), we include year dummies to capture earnings variability related to the different calendar years when sons' earnings are observed at the various experience levels (e.g., at potential experience level 6, the 2002 cohort is observed in 2008, the 2008 cohort in 2014). An additional model is estimated by also controlling for the number of the sons' possible weeks of work experience before they left education (model 2 henceforth). However, we do not expect large differences in the results obtained from the 2 models – especially at higher potential experience levels – since we know from Table 2 that sons had on average only approximately 26 weeks of work experience when leaving education.[13]

Table 4 shows estimated IGEs at potential experience levels 2, 4 and 6 in the two specifications. In our baseline estimate at experience level 6, we find an IGE equal to 0.404, which slightly increases to 0.413 when we also control for work experience during the educational path. As expected, consistent with the existence of a serious lifecycle bias when sons are observed too early in their working lives, IGEs steeply increase along the early career pattern: In model 1, the IGE is indeed 0.216 and 0.306 at potential experience levels 2 and 4, respectively.[14] When we also control for the sons' weeks of work experience gained before leaving education, the estimated IGEs are slightly higher than those obtained from the baseline model 1, but the gap between the two models shrinks at higher experience levels. This is why, in the following, we use model 1 as the benchmark and focus on IGE values estimated at potential experience level 6 only.

Table 4: Estimated IGE of sons' yearly earnings with respect to fathers' lifetime earnings

Potential experience	Model 1	Model 2	Obs.
6 (*baseline*)	0.404 (0.064)	0.413 (0.062)	763
4	0.306 (0.063)	0.321 (0.060)	747
2	0.216 (0.074)	0.244 (0.071)	651

Notes: Sons with positive earnings and their fathers with at least 4 positive earnings observations are considered. Additional controls: year dummies in model 1; year dummies and sons' weeks of work experience before leaving education in model 2. Robust standard errors in parentheses. Source: Authors' elaborations on the AD-SILC dataset.

[13] We do not control for a polynomial of sons' age, as is usually done in the empirical literature where sons are selected by age, because, at each potential experience level, sons' age is related to their education, a crucial channel through which fathers' economic status is transmitted to their sons.

[14] The number of observations increases from experience levels 2 to 6 since not all individuals had positive earnings in the years following the end of studies due to unemployment and long job searching time.

An IGE of approximately 0.40 – even if observed at an early stage of the career – is rather high in an international comparison and clearly clashes with the findings by Acciari et al. (2019), who instead rank Italy as a mobile country (Table 1). However, our estimated IGE is lower than IGEs estimated for Italy by the previous studies, which relied on the TSTSLS approach (Mocetti, 2007; Piraino, 2007; Barbieri et al., 2019). At this stage we cannot argue whether the gap between our OLS and previous TSTSLS estimates is due to a lifecycle bias affecting our estimate (due to a left-hand side measurement error since young sons are considered) or to an upward bias affecting TSTSLS estimates. In the next sections, we will try to shed light on the gap between the two types of estimates directly computing the size of the lifecycle bias by workers' experience and correcting our OLS IGE by the size of the bias.

4.2 Robustness checks for the attenuation bias

Before carrying out the empirical exercise about the lifecycle bias, it is important to test the sensitivity of our estimated IGEs to right-hand side measurement errors, i.e., to the risk of an attenuation bias due to the used proxy of fathers' lifetime earnings. To this aim, we compare IGEs estimated at sons' potential experience level 6 by varying the number of years used to average fathers' earnings (Table 5).

Table 5: Sensitivity tests to right-hand side transitory shocks and measurement errors

Measure of father's earnings	Estimated IGE	Obs.
1-yr obs. when sons were 14	0.240 (0.046)	710
3-yrs-avg when sons were 12-14	0.291 (0.052)	734
At least 1 positive obs. over 15 years	0.310 (0.057)	791
At least 2 positive obs. over 15 years	0.337 (0.069)	784
At least 3 positive obs. over 15 years	0.391 (0.060)	771
At least 4 positive obs. over 15 years	0.404 (0.064)	763
At least 5 obs. over 15 years	0.406 (0.067)	755

Notes: Robust standard errors in parentheses. Source: Authors' elaborations on the AD-SILC dataset

Specifically, our baseline estimate at potential experience level 6 obtained by excluding fathers with fewer than 4 positive earnings observations in the 15-year period considered is compared with alternative estimates produced: i) observing fathers in a unique year when their sons were aged 14; ii) observing fathers over three years when their sons were aged 12 to 14; iii) excluding from the analysis fathers with fewer than 1, 2, 3 or 5 positive earnings observations in the 15-year period when the sons were aged 0-14.

Table 5 shows that our baseline IGE obtained at potential experience level 6 is almost identical to the IGE estimated considering fathers with at least 5 positive observations. This result suggests that our measure of fathers' earnings is basically robust to right-hand side measurement errors, probably because, as noted in Section 3, the selected fathers had positive earnings in most of the 15-year period. In contrast, IGEs estimated by measuring fathers' earnings in a unique year when their sons were aged 14 or by observing fathers for three years only (when their sons were aged 12-14) are considerably lower than our baseline estimate.

Table 6: Sensitivity tests to different measures of father's lifetime earnings

Measure of father's earnings	Estimated IGE	Fathers' age	Obs.
Baseline	0.404 (0.064)	38.02	763
At least positive 4 obs. in the age range 30-49	0.390 (0.057)	40.37	742
At least 10 positive obs. in the age range 30-49	0.396 (0.066)	40.29	681
At least 4 positive obs. in the age range 30-54	0.401 (0.055)	42.42	752
At least 10 positive obs. in the age range 30-54	0.413 (0.060)	42.39	718

Notes: Robust standard errors in parentheses. Source: Authors' elaborations on the AD-SILC dataset.

Hence, confirming previous evidence for the US (Solon, 1992; Zimmerman, 1992; Mazumder, 2005), it is extremely important to take into account right-hand measurement errors to avoid overestimating the degree of intergenerational mobility. Therefore, the likely existence of a serious attenuation bias is the main reason why Acciari et al. (2019) find a very low intergenerational elasticity (0.249) by averaging fathers' income using only three yearly observations. Note, for comparison, that our estimated IGE falls to 0.291 when we follow their approach and compute the proxy of fathers' earnings using three annual observations only.[15] Note also that, unlike our baseline analysis where we average fathers' earnings according to the sons' ages, Acciari et al. (2019), due to data limitations, were forced to compare parents' and offspring's earnings at only 14 years of distance. Furthermore, they considered relatively old fathers (aged around 51), whereas Nilsen et al. (2012) demonstrate that taking fathers aged around 50 is likely to cause a high downward bias in the estimated elasticity, even if children are selected following the lifecycle bias-minimizing age suggested by Haider and Solon (2006).

Finally, Table 6 shows additional sensitivity tests to right-hand side measurement errors carried out by estimating the IGE using 4 alternative measures of fathers' lifetime earnings when fathers are observed according to their age (i.e., in the age groups 30-49 and 30-54), instead of according

[15] For the US case, a similar issue, is noted by Mazumder et al. (2016) who demonstrate that the IGE estimated by Chetty et al. (2014) using tax data is largely downward biased due to lifecycle and, mostly, attenuation biases related to the small longitudinal dimension available in their study (they are indeed able to average fathers' earnings over 5 years only).

to the sons' ages (0-14) as in our baseline estimate. Reassuringly, our estimates change only slightly when different measures of fathers' lifetime earnings are used.

5. Measuring the lifecycle bias

To assess whether, and how much, IGE estimates might be downward biased due to a lifecycle bias at the sons' various potential experience levels, in this section we adapt the textbook error-in-variables model to assess the career-earnings profile in Italy, following the approach first proposed by Haider and Solon (2006) in their study about the US and subsequently applied by Böhlmark and Lindquist (2006) and Chen et al. (2017) to Swedish and Canadian data, respectively. Given that we are only interested in the bias arising from left-hand side measurement errors, we will from now on use the simplifying assumption of no right-hand side measurement errors, although we are aware that, even if fathers' earnings are averaged using many yearly observations, we are not able to observe the "true" lifetime earnings of fathers.

The method introduced by Haider and Solon (2006) consists of evaluating the bias associated with the use of yearly instead of lifetime earnings by regressing the former on the latter having at our disposal a proper dataset that follows individuals more or less over their entire working career. Ideally, we would estimate the IGE by means of OLS according to the following equation:

$$y_i^s = \alpha + \beta \, y_i^f + \varphi_i \qquad (2)$$

where y^s and y^f are log of sons' and fathers' lifetime earnings, respectively, φ_i is a disturbance, and the estimated coefficient β is the IGE. As noted in Section 2.1, since we cannot directly observe the lifetime earnings of sons, we are likely to obtain biased estimates of the IGE due to left-hand side measurement errors.

Nevertheless, exploiting the large longitudinal dimension of the AD-SILC dataset where individuals are followed since their entry in the labour market up to 2014, we can compute a good proxy of lifetime earnings for a representative sample of Italian workers, thus measuring the gap between annual and lifetime earnings at the various workers' ages or potential experience levels.

Specifically, we can regress log yearly earnings on log lifetime earnings of individuals *i* belonging to a certain cohort *g* according to the so-called "forward regression" of y_{it} on y_i :

$$y_{it}^g = \theta_t \, y_i^g + \omega_{it} \qquad (3)$$

where y_{it}^g is the log of yearly earnings that can be observed in our dataset at either a given age or potential experience level (i.e., the distance from the year when the worker ended their studies), ω_{it} is a disturbance and θ_t is the coefficient capturing the relative gap between yearly and lifetime earnings at each age (or experience level). Therefore, by assuming that the estimated relation between yearly and lifetime earnings is approximately the same in the representative sample of individuals who we can track over time and in the sample of our selected sons – hence assuming that age and experience earnings profiles are not cohort specific, i.e., $g = s$ – we can replace equation (3) with equation (2) and obtain:

$$y_{it}^s = \lambda_t + \theta_t \beta \, y_i^f + (\omega_{it} + \theta_t \vartheta_i) \qquad (4)$$

where $\lambda_t = \alpha\theta_t$ is the intercept.

Equation (4) is very useful since it informs us of the amount of the lifecycle bias affecting the estimated IGE when lifetime earnings of sons are not available. If we assume that $Cov(y_i^f, \omega_{it}) = 0$, the lifecycle bias equals θ_t, and it disappears only when $\theta_t = 1$. In contrast, the estimated β is likely to be downward biased when sons are observed when they are too young – or have low experience – ($\theta_t < 1$) or upward biased after a given median age or experience level ($\theta_t > 1$). Therefore, according to this empirical approach, it is theoretically possible to correct a biased estimated β by simply using the estimated θ_t corresponding to the specific age/experience at which sons are observed.

Haider and Solon (2006) suggest that it is not correct to use θ_t estimated for a specific country to correct the estimated IGE in a different country as the age-income profile is likely to differ by country according to workers' and firms' behaviours and to institutional differences. Likewise, as noted, the age-income profile might change across cohorts within the same country, or the estimated θ_t might be biased if $Cov(y_i^f, \omega_{it}) \neq 0$ (Nybom and Stuhler, 2016 and 2017; Chen et al., 2017).

Nevertheless, all studies acknowledge that the approach proposed by Haider and Solon (2006) is the best one available to identify the age at which sons should be taken when it is not possible to measure their lifetime earnings.

In this article, we estimate the "forward regression" by selecting a representative sample of Italian male workers who left education from 1978 to 1984 and can be tracked in our dataset for the subsequent 30 years (from 1979 to 2008 for those who left education in 1978; from 1980 to 2009 for those who left education in 1979, and so on). Specifically, we use a sample of 4,551 male workers for whom we are able to obtain a good proxy of their lifetime earnings.

Following Haider and Solon (2006) and Chen et al. (2017), we compute the proxy of lifetime earnings by averaging earnings of workers with at least 10 years of positive observations in the 20-year period comprising the 6th from the 25th year of distance from the year when the male workers ended their studies.[16] Although we are not able to measure the "true" lifetime earnings since in our dataset we do not observe potential earnings obtained in the last years before retirement by individuals working for more than 30 years, our measure of lifetime earnings can be considered a very good proxy of lifetime earnings.[17] Indeed, on average, our sample of Italian workers is followed for 17.8 years, and 55.7% of them are followed for the entire 20-year period from potential experience levels 6 to 25. As noted, we run the forward regression and measure the lifecycle bias by both worker age (θ_a) and potential experience ($\theta_{p.e.}$).

Consistent with the usual analysis in the intergenerational inequality literature, we first show the results of the estimated "forward regression" for each worker's age between 20 and 55 (Panel A of Figure 2). Consistent with previous studies, we find that the estimated θ_a is lower than 1 when individuals are very young and increases to slightly above 1 later during the lifecycle. According to

[16] We control for year dummies in the regressions. Note that our results do not change if we run the "forward regression" considering individuals with at least either 8 or 12 positive earnings observations in that period (see Figure A.1 in the online Appendix).

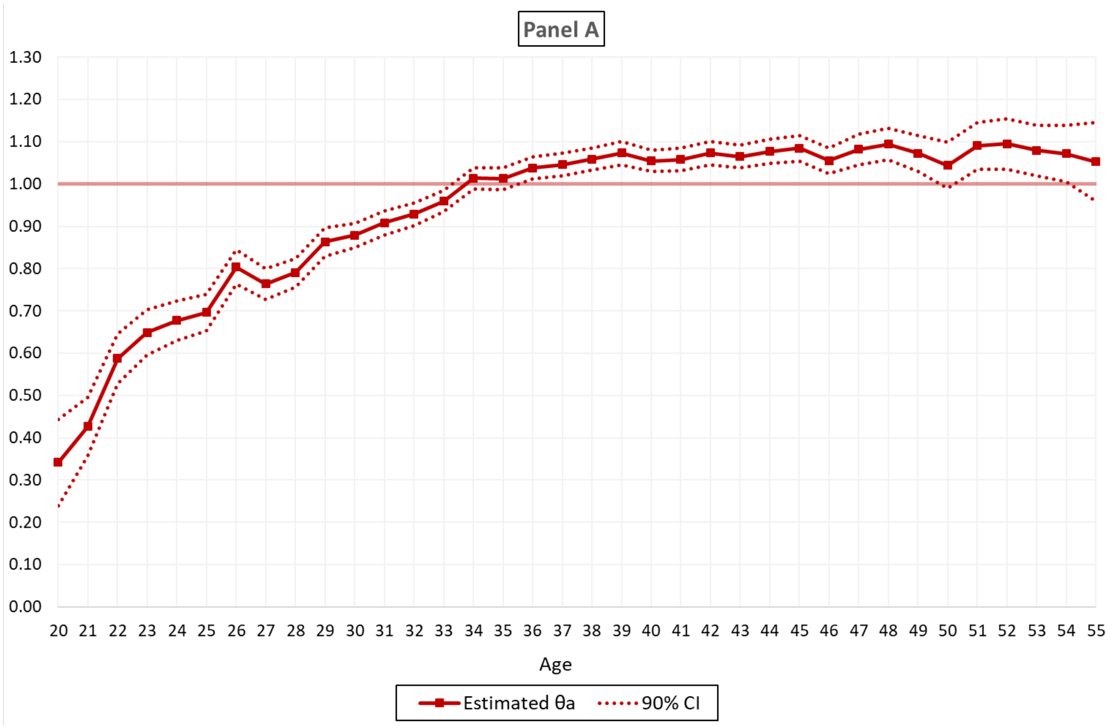
[17] We do not consider the first and the last 5 years in the 30-year period to compute the proxy of lifetime earnings since – being the duration of the working career heterogenous – our choice allows us to consider long-term earnings in the main phase of the working career. Note also that a not negligible share of males stopped working with less than 30 working years due to generous early-retirement options. Our results are, however, robust to a different definition of the observation period used to compute lifetime earnings.

our estimates, the age at which the estimated θ_a is closer to one is between 34 and 35 years old.[18]

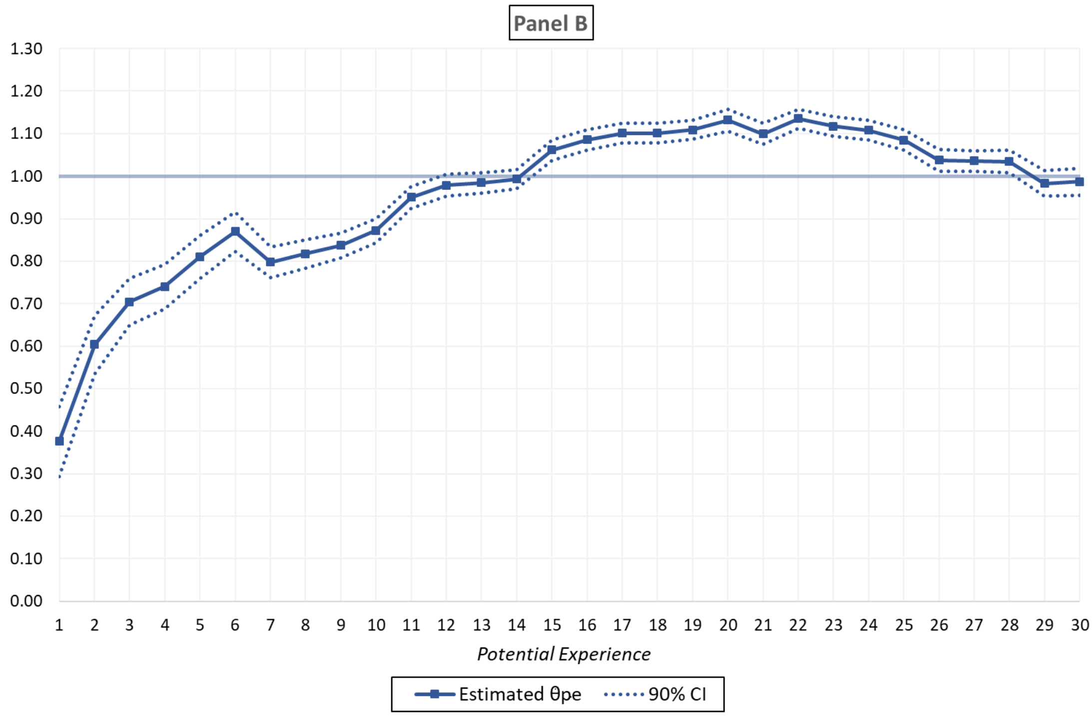
Then, we re-estimate the "forward regression" by observing log yearly earnings of male individuals by potential experience rather than by age, obtaining for each potential experience level between 1 and 30 the estimated $\theta_{p.e.}$ plotted in Panel B of Figure 2. This second graph shows that left-hand side measurement errors are minimized between potential experience levels 12 and 14. Moreover, we find that at potential experience level 6 – the benchmark in our baseline estimates – the estimated downward bias $\theta_{p.e.}$ is 13%, while θ_a is 20% at age 26, which is the mean age of workers at their 6th year of potential experience (Table 3).

According to this result, we can argue that selecting young sons by potential experience instead of age – thus observing individuals with different educations at the same stage of the career – contributes to mitigating the attenuation bias in IGE estimates of individuals observed at young ages. Our findings thus confirm that a non-negligible portion of the lifecycle bias is related to the fact that at the same age young individuals have different experience levels and more educated workers (who come more frequently from more advantaged family backgrounds) have less experience than less educated workers as the latter left education earlier.

Figure 2: Forward regressions of log annual earnings on log lifetime earnings by age (Panel A) or potential experience (Panel B)



[18] This estimated optimal age range is slightly higher than the one suggested in the study by Haider and Solon (2006) for the US (i.e., approximately 32 years old).



Notes: Male workers who ended their studies between 1978 and 1984 are followed for the 30 subsequent years. The proxy of their lifetime earnings is calculated by averaging at least 10 positive earnings observations from the 6th to the 25th year after they left education. The "forward regression" is estimated by age in Panel A and by potential experience level in Panel B. In all estimates we control for the year dummies. Source: Authors' elaborations on the AD-SILC dataset.

6. IGE corrected by the lifecycle bias

The results of the estimation of the "forward regression" suggest that our estimated IGE is moderately downward biased when sons' earnings are observed at potential experience level 6. In this section, we use the estimated coefficients of equation (3) to correct the estimated IGE and, thus, obtain values comparable with OLS estimates computed for other countries when workers are observed at ages when the lifecycle bias should be minimized. Using the estimated parameter from the "forward regression" of log yearly earnings on log lifetime earnings obtained at potential experience level 6 – whose value is 0.130 – we thus obtain a corrected IGE of 0.465 (Table 7, Panel A).

However, we again note as a caveat that, as suggested by Haider and Solon (2006), Nybom and Stuhler (2016) and Nybom and Stuhler (2017), the estimated coefficients in the "forward regression" can be exploited to correct an IGE affected by a lifecycle bias only if the potential experience-earnings profile of individuals observed during their entire careers is comparable to that of selected sons, i.e., if potential experience-earnings profiles do not vary across workers' cohorts. Actually, a sort of bias might emerge in our exercise since we correct the IGE regarding sons belonging to entry cohorts 2002-2008 estimating the "forward regression" for male workers who left education from 1978 to 1984, thus approximately 20/25 years before the period when sons' earnings are observed.

Table 7: Comparable estimates of the IGE

Panel A: Full sample of sons at potential experience 6			
	Estimated IGE	Estimated (1-$\theta_{p.e.}$)	Corrected IGE
	0.404	0.130	0.465
	(0.064)	(0.028)	
Obs.	763	3172	
Panel B: Full sample of sons at potential experience 11			
	Estimated IGE	Estimated (1-$\theta_{p.e.}$)	Corrected IGE
	0.450	0.050	0.474
	(0.070)	(0.016)	
Obs.	207	3879	

Notes: The estimated IGE in panel A is obtained by considering the sample of sons who ended their studies from 2002 up to 2008. The estimated IGE in panel B is obtained by considering the subsample of sons who ended their studies between 2002 and 2003. All estimated θ_s are obtained following for 30 years a sample of male workers who ended their studies between 1978 and 1984. In all estimates we control for the year dummies. Robust standard errors in parentheses. Source: Authors' elaborations on the AD-SILC dataset.

To verify whether this potential source of bias affects our corrected IGE, we test the goodness of our estimated $\theta_{p.e.}$ by correcting the IGE estimated at potential experience level 11 using the small subsample of sons who left education in 2002 and 2003 (207 father-son pairs).[19] Descriptive statistics for this sub-sample are presented in Table A.1 in the online Appendix, where we note that the mean worker age at potential experience level 11 is 31.5.

According to these new estimates, presented in Table 7 (Panel B), we find that the estimated IGE increases to 0.451 when sons are observed at potential experience level 11 (Panel B). At that experience level, the estimated lifecycle bias amounts to 5%; hence, after the correction, we obtain an IGE of 0.474.

Therefore, reassuringly, correcting the estimated IGEs obtained at potential experience levels 6 and 11 using the corresponding estimated $\theta_{p.e.}$, we obtain very similar corrected IGEs, i.e., 0.465 and 0.474, respectively. This means that the $\theta_{p.e.}$ coefficients estimated through the "forward regressions" are applicable to our representative sample of sons, even if they were estimated on experience-earnings profiles of previous workers' cohorts.

[19] Note that many empirical studies that estimated the IGE were forced to use very small samples due to data limitations. See, for instance, the studies by Björklund and Jäntti (1997), Mazumder (2005) and Schnitzlein (2016).

7. Concluding remarks

In this article, we presented the first comparable OLS estimates of intergenerational earnings elasticity in Italy. Making use of a longitudinal dataset built by merging administrative and survey data, we were able to observe actual father-son pairs, while due to data limitations, previous studies were forced to impute parents' earnings (Mocetti, 2007; Piraino, 2007; Barbieri et al., 2019) or to use a dataset with characteristics that led to a significant underestimation of the intergenerational elasticity (Acciari et al., 2019).

Specifically, in our baseline estimate, we observed sons at the 6th year of their potential working careers, while due to the high quality of our dataset, we averaged fathers' earnings over the 15-year period when their sons were aged 0-14, thus strongly mitigating measurement errors related to an attenuation bias (Mazumder, 2005). Our baseline estimate – even if run at a relatively early stage of the son's working history – confirms that Italy is a low-mobility country since we find an IGE of 0.404. However, this estimate might be downward biased by the early phase of the career when sons are observed.

To measure the extent of the lifecycle bias, exploiting the richness of our longitudinal dataset, we adapted the methodology proposed by Haider and Solon (2006) to estimate, at each worker age or potential experience level, the gap between annual earnings and a good proxy of lifetime earnings. We find that the bias is largely reduced when sons are observed at low experience rather than at young ages, thus confirming that a non-negligible portion of the lifecycle bias is related to the fact that at the same age young individuals have different work experience. Hence, by correcting estimated IGEs by the size of the estimated bias, we find an IGE of approximately 0.47, which, unfortunately, confirms Italy as among the least mobile developed countries.

Our results are then crucial to answering an important methodological question that has been further underlined by the puzzling result recently found by Acciari et al. (2019) that the IGE in Italy is relatively low: "Is the degree of intergenerational earnings persistence highly overestimated in Italy due to the TSTSLS method used in previous studies?" Given that the value of our corrected IGE is approximately 0.47 and is extremely close to the values obtained in previous studies using the TSTSLS method by Piraino (2007), Mocetti (2007) and Barbieri et al. (2019), we can conclude that previous TSTSLS estimates did not overestimate the level of intergenerational inequality in Italy.

Furthermore, our findings clearly indicate the importance of comparing IGE across countries by considering only those estimates that try to carefully address all the possible measurement errors due to the presence of attenuation and lifecycle bias. Moreover, given that it is still not possible to directly follow two subsequent generations over their entire careers, our corrected IGE estimates might still be a lower bound of the "true" IGE due to a potential residual lifecycle bias.

By exploiting the data sources that we used in this article, future studies might allow researchers to estimate the true ideal IGE between lifetime sons' and fathers' earnings if the dataset we used is constantly updated over time, thus also tracking sons along their entire working careers. Furthermore, if a dataset with a larger sample size becomes available (we use in the baseline estimate a dataset composed of 763 father-son pairs), the possible heterogeneity behind the intergenerational transmission process – e.g., by geographical area of residence or by parents' and sons' education and occupation – might be investigated.

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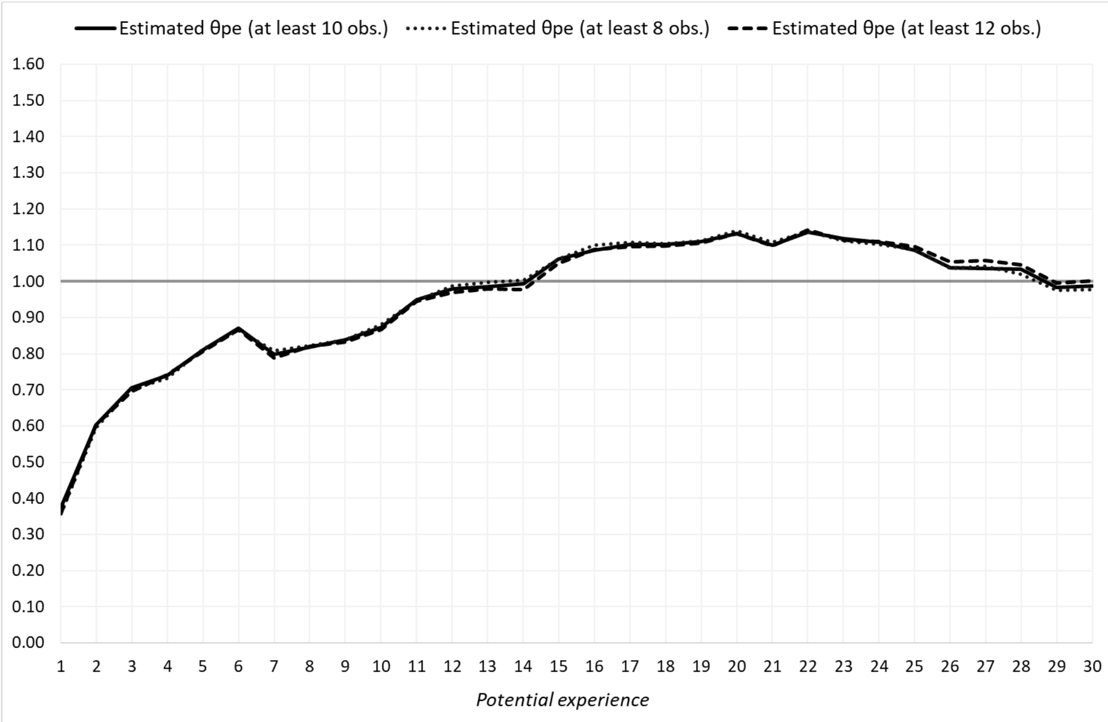
Appendix

Figure A.1: Empirical test of lifecycle bias by sons' potential experience levels: sensitivity tests to the number of positive earnings observations used to proxy fathers' lifetime earnings



Notes: Male workers who ended their studies between 1978 and 1984 are followed for the 30 subsequent years. The proxy of their lifetime earnings is calculated by averaging at least 10 positive earnings observations from the 6th to the 25th year after they left education (black line). Alternative measures of lifetime earnings are calculated by averaging at least 8 (dotted line) or 12 (dashed line) positive earnings observations in the 20-year period considered. In all estimates we control for the year dummies. Source: Authors' elaborations on the AD-SILC dataset.

Table A.1: Summary statistics of the subsamples of sons observed at potential experience level 11

	Earnings		Age	
	Sons	Fathers	Sons	Fathers
Mean	23,887.1	25,266.8	31.5	38.5
Sd	(14,623.2)	(12,512.6)	(3.7)	(5.1)
Obs.	207	207	207	207

Notes: The sample is obtained by considering individuals who ended their studies between 2002 and 2003. Monetary values are CPI adjusted at 2012 prices. Source: Authors' elaborations on the AD-SILC dataset