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Economic approach to intergenerational mobility

Measures, methods, and challenges in developing countries

M. Shahe Emran[1] and Forhad Shilpi[2]

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Abstract: This paper provides a critical survey and synthesis of the recent economic literature on intergenerational mobility in developing countries, with a focus on data and methodological challenges. The attenuation due to measurement error is compounded by sample truncation resulting from co-residency and causes substantial downward bias in intergenerational regression coefficient. In contrast, intergenerational correlation and intergenerational rank correlation are much more robust to such data limitations. To understand heterogeneity across groups, cohorts, and spatial units, reliable estimates of both the intercept and the slope are necessary. The ordinary least squares estimate of the intercept is biased upward, but less so in the rank–rank regression. Sibling correlation is a broader measure of mobility, particularly useful with limited data. Estimating causal effects is challenging because one needs credible exogenous variation in the parental generation and then to track down and collect information on all the children when they grow up. A less demanding but policy-relevant approach is to focus on the causal effects of policies on intergenerational regression coefficient, intergenerational correlation, and intergenerational rank correlation, without trying to disentangle the role of genetic inheritance.

[1] Columbia University, New York City, USA; [2] World Bank, Washington DC, USA. Corresponding author: shahe.emran.econ@gmail.com

1   Introduction

Intergenerational mobility is an under-researched area in development economics. Although there is a large literature on poverty and inequality in developing countries, the effects of family background on the economic outcomes of children remain a largely neglected topic.[1] The focus of much of the development literature has been on trade and development, poverty, inequality, and factor market imperfections, and more recently credit, education, and health interventions using randomized controlled trials. However, the long-term intergenerational effects are in general not studied.[2] There has been a recent upsurge in the interest in intergenerational mobility, which can be seen as a natural progression from the rich and mature literature on poverty and inequality in the era of liberalization and globalization that resulted in impressive poverty reduction but higher inequality in many countries. To what extent does the observed increase in inequality reflect the deep roots of family background rather than rewards for hard work and innovation? Has market liberalization increased the economic opportunities of children with poor socioeconomic backgrounds? Are the children born with favourable family backgrounds reaping the greatest benefits from globalization?[3] These and other related questions have provided a fresh impetus for the study of intergenerational persistence in economic outcomes in developing countries.

The literature on intergenerational economic mobility in developed countries is rich, with many fundamental theoretical and empirical innovations (for excellent surveys, see Bjorklund and Salvanes 2011; Black and Devereux 2011; Solon 1999). Almost all of the work on developing countries in the last few decades closely followed the literature on developed countries and adopted the measures and methods widely used there. As a result, the implications of data constraints and differences in economic structure in developing countries have not always been adequately appreciated.[4] For example, because of the predominance of informal and household-based economic activities, it is difficult to obtain reliable data on individual and household income in most developing countries. This makes it difficult to rely on income as a measure of economic status, as is used in most of the literature for developed countries. Another example of a common data limitation in developing countries is that most of the surveys use co-residency to define household membership at the time of the survey (Deaton 1987). Using data that only includes the co-resident members results in sample truncation and the standard estimates of intergenerational persistence, such as the intergenerational regression coefficient, can be severely biased downward. An important question in the context of developing countries thus is whether different measures are affected differently by the truncation bias arising from co-residency requirements in the surveys. Are there measures more robust to such data limitations that can be fruitfully used with the available household surveys? The goal of this chapter is to provide a critical survey and synthesis of the recent economic literature on intergenerational mobility in developing countries, with a focus on such data and methodological challenges.

The rest of the chapter is organized as follows. Section 2 discusses the difficulties in using income as a measure of economic status in a developing country and how information on education and occupation commonly available in surveys can be leveraged to better understand the role of family background. Section 3 is devoted to the standard measures of intergenerational mobility used in the current litera-

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[1] The literature on intergenerational mobility in the context of Latin American countries is relatively more developed (see, for example, Behrman et al. 2001; Dahan and Gaviria 2001; Neidhofer et al. 2018), while sub-Saharan Africa remains the least researched (recent contributions include Alesina et al. 2019; Bossuroy and Cogneau 2013; Sepahband and Shahbazian 2017).

[2] The leading textbooks on development economics, such as those of Ray (1998), Todaro and Smith (2015), and Schaffner (2014) all contain full chapters on inequality, but do not devote even a page to the discussion of intergenerational mobility.

[3] The evidence presented by Fan et al. (2019) suggests that, in China, liberalization has not only increased income inequality, it has also led to lower intergenerational income mobility.

[4] For an excellent discussion on this point, see Iversen et al. (2019).

ture, with a discussion on the choice of 'control variables' in the regression model. Section 4 provides a discussion on the effects of truncation due to co-residency restrictions and measurement error on the measures of relative mobility, such as the intergenerational regression coefficient (IGRC), intergenerational correlation (IGC), and intergenerational rank correlation (IRC).[5] Section 5 discusses the issues relevant for understanding heterogeneity in intergenerational mobility across social groups, over time, or across spatial units. Section 6 contains an exposition of and discussion on sibling correlation (SC) as a broader measure of intergenerational mobility, which is especially suitable in the context of developing countries with limited available data. The challenges in causal interpretation of the estimates from the standard measures are the focus of Section 7, which emphasizes the importance of understanding the effects of policies on intergenerational persistence in economic status. The paper concludes with a summary of the main insights and a catalogue of suggestions for research on intergenerational mobility in developing countries.

2   Measuring economic status/family background in developing countries

2.1   Limits of the income-based approach

A fundamental question is how to measure economic status for the analysis of intergenerational mobility. Not surprisingly, income is the measure of choice for economists; many existing analyses, especially in developed countries, treat permanent income as the most informative measure of family background relevant for the opportunities children are offered. The focus on income in economics is in sharp contrast to the focus on occupational prestige and social class in sociology. It is, however, important to note that the original contribution by Becker and Tomes (1979, 1986) and the more recent contributions by Becker et al. (2015, 2018) also highlight the direct (non-financial) role played by parental characteristics; for example, more educated parents may be more effective with homework help, and motivate their children as role models.

The literature on intergenerational income mobility in developed countries has a long and distinguished pedigree. Early estimates suggested high income mobility in the USA (intergenerational income elasticity (IGE) estimates of 0.20–0.30), but this optimistic picture was largely driven by attenuation bias from measurement error in cross-section or short-panel income data. A substantial research programme was devoted in the decades following Becker's presidential address to the American Economic Association in 1988 to understand the extent of bias in the intergenerational income elasticity estimates in developed countries due to life cycle effect and measurement error in income data. A number of important contributions in the 1990s and 2000s showed that cross-sectional and short-panel data can substantially underestimate intergenerational income elasticity, thus giving a false impression of high intergenerational mobility (see, among others, Mazumder 2005; Solon 1992; Zimmerman 1992).

The accumulated evidence in the context of developed countries noted above suggests that to address the measurement error and life cycle biases, one needs good-quality data on income for more than a decade over the appropriate phase of the life cycle. Thus, the data requirements for understanding intergenerational persistence in (permanent) income may be too demanding in most developing countries. In fact, an income-based approach may not be feasible in the context of most developing countries for two reasons. First, it is difficult to get a reliable estimate of household income when a substantial portion of economic activity is home-based and market interactions are limited—a salient structural feature of developing countries. This is compounded by the fact that multiple families (extended family) may

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[5] For a complementary analysis of the properties of alternative measures of intergenerational mobility with a particular focus on the implications of involuntary descent into destitution common in the high-risk low-income environment of developing countries, see Iversen (forthcoming).

live in one household and work on the same land, making it impossible to separate out individual and family income.[6] Second, there are only a few panel data sets with good-quality income data spanning a decade or more that would allow credible estimates of permanent income to be derived for both the parents and the children. There are some panel data sets now being collected that may address the time dimension problem in 5–10 years, but the issues related to informal economic activities are likely to remain important for many years to come, particularly in rural areas, which is where most poor people live.

Given the data constraints, there are not many papers that estimate IGE in developing countries. Among recent papers, see Mohammed (2019) on rural India and Fan et al. (2019) on China. Mohammed (2019) relies on single-year income data for both generations, and Fan et al. (2019) use four rounds of panel data in a cohort-based analysis. The IGE estimate for India reported by Mohammed (2019) is 0.30, which is identical to the IGE estimate for the USA using single-year income data for fathers and sons in Solon (1992). Fan et al. (2019) provide an IGE estimate of 0.390 for the 1970–80 birth cohorts, which increased to 0.442 for the 1981–88 birth cohorts in China. Again it is interesting and informative to compare with similar estimates from the USA; in fact, the IGE estimate using five-year average income for the USA reported by Solon (1992) is 0.41, very close to the estimates of Fan et al. (2019) for China using four-year average income. When 16-year average income of fathers is used for the USA, the estimated IGE is much higher at 0.61, as reported by Mazumder (2005). This suggests that the availability of longer panels in India and China is likely to yield much higher estimates of IGE for (permanent) income.[7]

An important question in this context is whether there are econometric approaches that can be fruitfully used to address (at least partially) the data constraints for an income-based analysis of intergenerational mobility. When there are different sources of income data on parents and children that cannot be linked across generations, the two-sample two-stage least squares (TS2SLS) has been widely used in both developed and developing countries (see, for example, Bjorklund and Jantti (1997) on Sweden and the USA, Gong et al. (2012) on China, Piraino (2015) on South Africa, and Jerrim et al. (2016) for a list of 30 papers that use TS2SLS for estimating IGE). The basic idea is to use an auxiliary data source with richer information on parental income to get a better estimate of a parent's permanent income than what is available in the main data set.[8] Unfortunately, in general such auxiliary data sources are not available in developing countries.[9] Moreover, the available analysis and evidence suggest that the IGE estimate from the TS2SLS approach is often biased upward.[10] In a recent analysis, Jerrim et al. (2016) show that the bias in IGE is sensitive to the choice of the set of instruments, and the bias (inconsistency) may be as high as 50 per cent upward or 40 per cent downward. Their empirical analysis, however, finds that the correlation estimate is downward biased in most cases.[11] Perhaps more importantly, the magnitude of

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[6] An imperfect option to address this issue is to re-scale household income by some measure of household size, such as adult equivalent, following Deaton and Zaidi (2002)

[7] It is, however, important to note that the central conclusion of Fan et al. (2019), that income mobility has declined for the younger generation in China, is likely to be robust. Although the estimates for both the older and younger generations are likely to be biased downward, the statistically significant lower estimate for the younger generation cannot be explained by data limitations. This is because the data come from the same panel survey and thus are likely to contain comparable measurement error. Moreover, the data used (China Family Panel Studies) do not suffer from truncation due to co-residency.

[8] The auxiliary data set must refer to the same population as the main data set.

[9] Most of the existing studies in the context of developing countries rely on a single year (or only a few years) of income data for parents in the auxiliary data set.

[10] The most widely used set of instruments to predict parental income in the auxiliary data set are parent's education and occupation. Bjorklund and Jantti (1997) report an estimate of 30 per cent upward bias (inconsistency).

[11] They also point out that the direction of bias in the estimated intergenerational income correlation (Pearson) in contrast is not unambiguous on a-priori grounds.

bias in income correlation (Pearson) is substantially smaller, and the estimated correlation is much more robust with respect to the choice of instruments. Thus, Pearson correlation seems preferable to IGE as a measure of relative mobility when using the TS2SLS approach.[12]

2.2 Measuring economic status with limited data

Although reliable estimates of permanent income are not available, most of the household surveys in developing countries collect data on education and occupation at the individual level.[13] One can make progress in understanding the pattern and evolution of intergenerational transmission of economic status by focusing on these two indicators.[14] A survey of the recently published papers on developing countries shows that most of the studies use a parent's education (usually, the father's education) as the measure of family background, and the analysis concentrates on the intergenerational educational linkage between the father and sons. In comparison, studies that rely on parental occupation as the relevant measure of family background focus on the influence of the father's occupation on children's occupation choices.[15] As a result, there are two sub-strands of the literature with little or no cross-over: 'intergenerational educational mobility' and 'intergenerational occupational mobility'.[16] While intergenerational persistence in education and occupation are useful and of interest in their own right, it is important to remember that they are partial measures, and one can use multiple indicators of parental economic status to understand the effects of family background on a given outcome for children (please see Section 4.2 for elaboration on this point).

The emphasis on education in the current literature is partly driven by the widely shared belief that better education is the key to opening up opportunities for children from disadvantaged socioeconomic background in the age of skill-biased technological change (Rajan 2010; Stiglitz 2012). However, note that the returns to education can vary across social groups, gender, and over time. Given the same level (and quality) of education, children from different socioeconomic backgrounds may enjoy very different life-time income because of network and referral effects in the labour market.

The recent evidence from India and China suggests that returns to education beyond the secondary level are higher for women (see Hannum et al. (2012) for urban China and Kingdon (2007) for India). The cross-country evidence reported by Psacharopoulos and Patrinos (2018) suggests that higher returns to education for girls is also found in many other countries. This implies that, in the absence of any gender differences in intergenerational persistence in education, being the child of better-educated parents might give more advantage to a girl than to a boy in terms of income. In so far as one uses education as a

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[12] There is a growing body of evidence that Pearson correlation (IGC) estimates are much more robust to data limitations when compared to IGE or IGRC. We discuss the implications of truncation bias arising from co-residency restrictions in a household survey for IGRC and IGC in a later section of this paper. See also Iversen et al. (forthcoming) on robustness of Pearson correlation in the context of developing countries.

[13] More precisely, most of the data sets include measures of inputs to education, such as schooling. For a discussion on the distinction between schooling and education, see the chapter by Behrman (2019).

[14] Another option is to focus on consumption expenditure, which is less affected by the transitory shocks because of intertemporal smoothing. We thank Vito Peragine for suggesting this. However, there are some limitations to this approach. For example, intergenerational persistence in consumption reflects not only intergenerational persistence in permanent income, but also the correlation in savings propensity across generations. Savings propensity, however, may change across generations with development of insurance markets and social welfare systems, among other things. Another potential issue is that household expenditure would depend on the life cycle, and the resulting bias when using a single year of expenditure data may not be small.

[15] There are some recent contributions that go beyond education and occupation. For example, Sepahband and Shahbazian (2017) report estimates of intergenerational persistence in risk attitudes in Burkina Faso.

[16] For a recent attempt to understand the role of parental occupation in intergenerational educational persistence, see Emran et al. (2019) on rural India and rural China. For an analysis that looks at both educational and occupational persistence without taking into account any cross-effects, see Emran and Sun (2015) on rural China.

proxy for permanent income, it is important to look at both the effects of a parent's education on their children's education, and the estimates of the returns to education in those children's generation. Most of the studies on intergenerational educational mobility in developing countries that we are aware of do not provide evidence on children's returns from education, which partly reflects the difficulties in getting good-quality income and earnings data.

When the income information is limited in a survey, but there are multiple indicators of family background, including parental education, occupation, grandparent's education and occupation, ethnicity, political affiliation, etc., two methodological issues arise. First, how to combine these different indicators to provide a meaningful estimate of the effects of family background? Second, are the effects heterogeneous with respect to the 'other' dimensions of family background. For example, does intergenerational educational persistence depend on the occupation of the father? In the context of developing countries, where agriculture dominates the economic structure, does intergenerational educational persistence depend on whether the parents are in farm or non-farm occupations? We discuss the issues related to heterogeneity and inter-group comparison of mobility in Section 5.

With regards to the aggregation of different indicators of economic status, a simple approach that comes immediately to mind is to use principal components analysis to create an index. However, it is widely appreciated that it is difficult, if not impossible, to provide economic interpretations to the weights used in the principal components analysis. In an interesting contribution, Lubotsky and Wittenberg (2006) develop an approach to the problem of aggregation of a set of mis-measured indicators of economic status and show how it can be used in intergenerational mobility analysis. Instead of creating a summary measure of economic status (permanent income) from the different parental indicators, their approach includes all of the indicators such as education, occupation, ethnicity, etc. as separate regressors in the regression specification, and then derives a summary measure of their effects by a weighted sum of the estimated coefficients. The weights are estimated from an auxiliary regression using a variant of the instrumental variables procedure. This approach has, however, not yet been widely adopted in the intergenerational mobility literature; we are aware of only three papers, one of which is devoted to developing countries (Neidhofer et al. 2018; Vosters 2018; Vosters and Nybom 2017)). We provide a more detailed treatment of the Lubotsky and Wittenberg (2006) approach in Section 4.2.

3   Measures of intergenerational mobility

Given a suitable measure of economic status, the next step is to decide appropriate measures of mobility. Economists, in general, prefer regression-based methods, while sociologists are traditionally more reliant on transition matrices, although regression-based approaches are being increasingly adopted. In this section, we discuss some of the most widely used measures of intergenerational mobility in the economics literature, with a focus on the challenges in the context of developing countries.

There are two different approaches to measurement of intergenerational mobility in the economics literature. The first approach is based on an underlying economic model (Becker–Tomes and its extensions), and the second is the axiomatic approach following the literature on inequality and income mobility over time for the same generation (Cowell and Flachaire 2018; Fields and Ok 1996; Shorrocks 1978). There is an important trade-off: the measures based on an economic model can be readily used to analyse the underlying economic mechanisms and to identify policy interventions, but they may not satisfy commonly accepted axioms (see Iversen et al., forthcoming). The axiomatic measures, on the other hand, may not be easily amenable to interpretation in terms of policy-relevant economic mechanisms. Most of the recent works on intergenerational mobility in developing countries that we are aware of are based on the economic approach. Thus, the focus of this paper is the measures based implicitly or explicitly on an economic model in the tradition of Becker and Tomes. However, it is also important to appreciate

that these alternative approaches are not orthogonal to each other; for a discussion on the link between standard economic measures such as IGRC and IGC and the axiomatic approach, please see Iversen et al. (forthcoming).

Many existing studies rely almost exclusively on measures of *relative mobility*, usually estimated as the slope parameter in an AR(1) regression. The standard measures of relative mobility are IGRC and IGC. Following the influential paper by Chetty et al. (2014), IRC is becoming increasingly popular. However, there is an appreciation in the recent literature that measures of relative mobility provide only a partial picture; measures of absolute mobility that combine the estimated slope and intercepts are important and offer complementary evidence (for an excellent discussion, see Chetty et al. (2014)).[17] Absolute mobility is especially important for cross-country, inter-group, and cohort-based analysis.

3.1 Parents and children: three measures of relative mobility

Most of the economic literature focuses on the effects of parents on children, using the following regression specification:

$$E_i^c = \beta_0 + \beta_1 E_i^p + \Pi X_i + \varepsilon_i \tag{1}$$

where E_i is the indicator of economic status, such as permanent income, education, or occupation, and superscripts c and p refer to children and parents, respectively. The parameter of interest is the slope β_1, a measure of intergenerational persistence. Higher persistence implies lower mobility, as children's outcomes are more closely tied to parental characteristics in this case, and $(1 - \beta_1)$ is usually taken as the measure of mobility. To get a sense of the magnitudes involved, it is instructive to consider the model in Equation 1 as a description of dynastic evolution of economic status across generations, and look at the long-term variance. Ignoring the controls X for simplicity, we have the following expression for long-term variance of children's economic status:

$$\sigma_{Ec}^2 = \frac{\sigma_\varepsilon^2}{1 - (\beta_1)^2}$$

We call $\dfrac{1}{1 - (\beta_1)^2}$ the 'family background multiplier', which amplifies the effects of the variance due to exogenous shocks ('market luck' *à la* Becker and Tomes (1979)) as captured by σ_ε^2. One way to understand the 'family background multiplier' is that in a perfectly mobile society the multiplier equals 1, implying that the only source of inequality in the long-run cross-sectional distribution is exogenous shocks, and family background does not play any role. Now consider the estimates of (β_1) for China and Indonesia reported by Hertz et al. (2008) for intergenerational educational persistence: China (0.34) and Indonesia (0.78) (Hertz et al. 2008: table 4). The estimated family background multiplier for long-term educational variance is 1.13 in China, but 2.55 in Indonesia. Given the same variance of the exogenous shocks, the long-term variance in education in Indonesia is 155 per cent higher due to family background factors, while it is only 13 per cent higher in China! In the current literature, it is uncommon to report estimates of the family background multiplier, but we believe this would be useful for many readers and policy makers.

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[17] There is a somewhat different definition of absolute mobility used by many authors where children experience upward (downward) mobility if they are better-off (worse-off) than their parents; for example, if they have more (less) schooling than their parents. We discuss the advantages and disadvantages of the alternative approaches in a later section.

The analysis of income mobility uses a double-log functional form so that the slope parameter is interpreted as intergenerational income elasticity (IGE).[18] In contrast, all of the estimates of intergenerational educational mobility we are aware of rely on a level–level specification, which is partly motivated by the concern that a substantial proportion of parents may have zero years of schooling. This is an especially important issue in the context of developing countries, where 20–40 per cent of parents have zero schooling in many data sets (especially in rural samples). The estimated slope parameter from a level–level specification is called the intergenerational regression coefficient (here, IGRC) in the literature.

The vector X_i is a set of controls, the elements of which depend on the context and goal of the analysis. In the context of income mobility, seminal papers such as that of Solon (1992) include quadratic age controls for both child and parents to control for life cycle bias.[19] But many recent papers on income mobility do not control for any variables, following the influential analysis of Chetty et al. (2014). For the analysis of educational persistence, the most commonly used interpretation of E_i is years of schooling, but in some cases binary indicators such as dummy variables for completion of primary, secondary, and college education have also been used. There is even less conformity in the set of controls used for the analysis of intergenerational educational mobility. One consequence of a lack of a commonly accepted specification of X_i is that it is difficult to compare estimates from different papers even when all of them refer to the same age cohorts in the same country. It is important to recognize that when the interest of the analysis is in measuring intergenerational persistence, controlling for other covariates in general would provide us a biased estimate of the effects of family background, as the controls capture part of the effects we are interested in. For example, many studies employ geographic fixed effects, which wipes off the effects of the part of family background related to neighbourhood characteristics. It is thus important to report estimates without any additional controls as a benchmark. In contrast, if the goal is to understand causal effects, it is important to include a set of controls for unobserved ability and preference to minimize the omitted variables bias.[20] For a discussion on the issues related to causal interpretation, see Section 7.

A second widely used measure is IGC, which is estimated using the following specification of the regression:

$$\frac{E_i^c}{\sigma_{Ec}} = \rho_0 + \rho_1 \frac{E_i^p}{\sigma_{Ep}} + \Pi X_i + \varepsilon_i \tag{2}$$

where σ_{Ec} and σ_{Ep} are the standard deviations of children's and parent's outcomes, respectively, and the parameter of interest is ρ_1. Compared to IGRC, IGC is a normalized measure that adjusts for the changing variance across generations. It is well understood in the literature that as measures of relative mobility IGRC and IGC are closely related, but they often give substantially different results. Denoting the OLS estimate of a parameter by a hat,

$$\hat{\rho}_1 = \hat{\beta}_1 \left(\frac{\sigma_{Ep}}{\sigma_{Ec}} \right) \tag{3}$$

To get the IGC estimate, we need to adjust the IGRC estimate by the ratio of standard deviation of parental education to that of children's education.

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[18] For evidence that the standard log-linear specification is subject to instability in the USA, see Chetty et al. (2014).

[19] Life cycle effects are, however, not an important concern in educational mobility assuming that the age cut-off is chosen to ensure that the children have completed their schooling.

[20] Some data sets contain information on preference such as estimates of risk aversion which would clearly be useful in tackling omitted variables bias.

There is substantial evidence that the conclusions depend on whether one uses IGRC or IGC as the relevant measure of intergenerational mobility. For example, in a widely cited cross-country study of educational mobility, Hertz et al. (2008) find that the estimated IGRC has declined over time across different age cohorts, suggesting that intergenerational educational mobility has increased for the younger generation. However, when the measure of choice is IGC, there is no evidence of such improvements. Similar conclusions are reached by other more recent country-specific studies; Emran and Shilpi (2015) present evidence that the IGC estimates for the youth (16–27 years old at the time of the survey) in India did not change substantially between 1993 and 2006, suggesting that even after impressive growth and poverty reduction following economic liberalization in 1991, intergenerational educational mobility has not improved for a large proportion of people in India. This evidence based on the IGC estimates is in sharp contrast to other studies of India that rely on IGRC as the measure of choice; all of these report that intergenerational educational mobility has improved substantially for the younger generation in the post-reform period (e.g. Jalan and Murgai 2008; Maitra and Sharma 2010.[21] While part of the differences between the IGRC and IGC estimates in India may be due to data limitations arising from co-residency restrictions, the observation that IGRC and IGC may yield different conclusions remain valid even when one uses data without any serious sample truncation resulting from co-residency (see Azam and Bhatt (2015) on father–sons persistence in education in India).

A third measure is based on ranks; the regression function is:

$$R_i^c = \delta_0 + \delta_1 R_i^p + \Pi X_i + \upsilon_i \tag{4}$$

Taking educational mobility as an example, R_i^c is the rank of child i in the schooling distribution of all children, and R_i^p is the rank of the parents of child i in the schooling distribution in the parental generation. The parameter of interest is δ_1, which provides an estimate of rank correlation (IRC) which is a measure of relative (im)mobility. It is important to note that rank correlation is different from IGRC and IGC in that it is a copula, and thus captures the fundamental dependence in economic status of parents and children, not affected by certain changes in the marginal distribution.[22] When conducting comparative analysis, the rank needs to be calculated from the distribution of an appropriate reference group. This can be highlighted by considering the fact that a move from the lowest decile to the median of the income distribution would mean very different things in Bangladesh versus the USA in terms of gains in income, if the income rank is calculated using the national distribution in the respective countries. For cross-country analysis, the ranking thus needs to be calculated in terms of the world income distribution.[23]

4 Measures of relative mobility: robustness to data limitations

With the three alternative measures of relative mobility—IGRC, IGC, and IRC—the question immediately arises whether some measures are better than others, especially when there are important data limitations as is usually the case in the context of developing countries. As we noted above, data limi-

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[21] Azam (2016) also finds similar evidence for girls in India using data from the second round (2012) of the India Human Development Survey (IHDS).

[22] Given the marginal distributions of parent's and children's schooling, a copula can be thought of as a function that joins them together to form a bivariate distribution. A copula represents the fundamental dependence between the marginal distributions in the sense that it is not affected by strictly increasing transformations of the marginal distributions. For a discussion, see Nelsen (2006).

[23] We thank Patrizio Piraino for alerting us to the importance of the 'scale' or 'absolute distance' in cross-country analysis. See also the discussion in Chetty et al. (2014) on cross-county differences in the USA.

tations in the form of measurement error have been a central focus of the literature on intergenerational income mobility in developed countries. In addition to measurement error, the recent literature on developing countries has also highlighted the issues that arise when data limitation takes the form of sample truncation because of co-residency restrictions used to define household membership in a household survey.

4.1 Measurement error

While income data in developing countries is subject to substantial measurement error, it is well recognized that data on consumption, and other parental characteristics such as education and occupation are less subject to such measurement errors. As a rule of thumb, one would thus expect the intergenerational persistence estimates based on income to be the most biased downward, especially when the income estimate comes from a single year or a few cross-sections. However, the current literature on developing countries is in general silent on the possible attenuation bias in the estimates of educational and occupational persistence. Since in many surveys, the information on parental education and occupation is based on recall data from the children, it is highly likely that they suffer from substantial measurement error.[24] The evidence from the literature on returns to education even in the context of developed countries suggests that measurement error in education is not ignorable.[25] Emran and Shilpi (2015) provide evidence that the IGC estimates for education in India go up by about 30–50 per cent (with the OLS estimate as the base) when the average of the peers' education (the same cohort and same neighbourhood) is used as an instrument for parental education to correct for attenuation bias, following Borjas (1995).[26]

In the context of intergenerational income persistence in developed countries, there is a small but growing literature that provides evidence on the reliability of different measures of mobility in the presence of measurement error and model mis-specifications. The evidence suggests that the rank-based measures such as IRC are more stable (Chetty et al. 2014; Mazumder 2014), and less affected by mis-specification when compared to IGE and IGC. Taking advantage of exceptionally rich panel data on income from Sweden, Nybom and Stuhler (2017) show that, among the three measures (IGE, IGC, and IRC), IRC suffers the least from attenuation bias arising from measurement error when short panel data on parental income are used. Although we are not aware of similar evidence using data on education or occupation, it seems reasonable to expect the rank-based measures to be more reliable in the presence of measurement error in education and occupation data also. Credible evidence on the relative magnitudes of attenuation bias in IGRC, IGC, and IRC in education and occupation data from developing countries would be a valuable contribution to the literature.

4.2 Multiple indicators of economic status and measurement error

When there are multiple indicators of economic status with measurement error such as education, occupation, annual income, etc., the approach due to Lubotsky and Wittenberg (2006) can be useful to aggregate them in a way to minimize the attenuation bias in the estimated effects of latent economic status. We provide a brief discussion of the approach here (henceforth called the LW approach); for details, see Lubotsky and Wittenberg (2006) and Vosters and Nybom (2017).

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[24] There is evidence that self-reported education measures also suffer from non-negligible measurement error (Blundell et al. 2005; Bound et al. 2001).

[25] For evidence on measurement error in education in the context of the USA, see Kane et al. (1999) and Black et al. (2003); and in the context of the UK, see Battistin et al. (2014). Ranasinghe and Hertz (2008) provide evidence of substantial measurement error in education data in Sri Lanka.

[26] The specification includes neighbourhood fixed effects.

Consider the example in which a researcher is interested in estimating the effects of family background on educational attainment of children (E^c). The data set includes average household income for three years (Y_3), which is not adequate for a reliable estimation of household permanent income Y^*; but it also contains information on the father's education $\left(E^f\right)$.[27] A common approach is to use the average income over three years as a measure of permanent income (Y_3), but this ignores the information on economic status contained in the father's education. Alternatively, one can choose parental education as the relevant indicator and estimate the intergenerational persistence in educational attainment, ignoring the information in the income data, as is routinely done in the 'intergenerational educational mobility' strand of the literature. The LW approach utilizes all of the available information in an optimal way in the sense of minimizing the attenuation bias.

LW begins with the following measurement error model linking the observed proxies of permanent income to latent permanent income (Y^*) and an additive error term:

$$E_i^c = \beta Y_i^* + \varepsilon_i \tag{5}$$

$$\begin{aligned} Y_{3i} &= \delta_1 Y_i^* + \nu_{iy} \\ E_i^f &= \delta_2 Y_i^* + \nu_{ie} \end{aligned} \tag{6}$$

It is assumed that Y_i^* is uncorrelated with ε_i, ν_{iy}, and ν_{ie}. These are the standard assumptions in a classical measurement error model. One of the indicators of permanent income is chosen as the numeraire—for example, the average income and set $\delta_1 = 1$. The normalization fixes the scale of unobserved permanent income in terms of the three-year average income. More importantly, this implies that we can estimate δ_2 as follows:

$$\hat{\delta}_2 = \frac{\mathrm{Cov}\left(E_i^c, E_i^f\right)}{\mathrm{Cov}\left(E_i^c, Y_{3i}\right)} \tag{7}$$

Thus we can estimate $\hat{\delta}_2$ by an IV regression with E_i^f as the dependent variable and Y_{3i} as the independent variable, using E_i^c as the instrument for Y_{3i}. The next step in the LW approach is to estimate the following auxiliary regression:

$$E_i^c = \varphi_1 Y_{3i} + \varphi_2 E_i^f + \varsigma_i \tag{8}$$

We denote the OLS estimates of the parameters of Equation 8 as $\hat{\varphi}_1$ and $\hat{\varphi}_2$. The LW estimate of the effects of permanent income on children's education is given by:

$$\hat{\beta}_{LW} = \hat{\varphi}_1 + \hat{\delta}_2 \hat{\varphi}_2 \tag{9}$$

To our knowledge, the only study that implements the LW approach in the context of developing countries is that of Neidhofer et al. (2018), which reports estimates of the effects of family background on children's educational attainment for 18 Latin American countries. There is scope for broader adoption

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[27] Vosters and Nybom (2017) use parents' income, education, and occupation to estimate intergenerational persistence in 'latent economic status' in Sweden.

of the LW approach to better understand the role of family background in shaping the opportunities faced by children in developing countries. One should, however, keep in mind the caveat that while the LW approach minimizes the measurement error in a set of family background variables, it does not correct fully for the attenuation bias, and we do not know whether it helps at all in dealing with the truncation bias due to co-residency.[28] The LW estimates may be biased downward, especially in co-resident samples.

The focus of this paper is intergenerational persistence in economic status, but there is an important strand of related literature on inequality of opportunity (IOP) that offers an alternative approach to combine multiple indicators of family background to understand the role played by the 'circumstances' into which one is born in shaping opportunities later in life. The challenge in this approach, however, is how to isolate the role played by 'effort', which is usually estimated as the residual from a multivariate regression.[29] Inequalities due to effort are considered fair and thus acceptable, which is called the 'reward principle'. For excellent discussions of this approach, see Roemer and Trannoy (2016) and Brunori et al. (2013).

4.3   Co-residency and sample truncation

Most of the existing household surveys in developing countries such as Living Standards Measurement Surveys (LSMS) done by the World Bank, and Demographic and Health Surveys (DHS) rely on a set of co-residency criteria to define household membership at the time of the survey. As Deaton (1987) notes in the context of the World Bank LSMS surveys, the standard co-residency criteria refer to 'living together', 'eating together', and sometimes 'pooling of funds'. Defining household membership on the basis of co-residency results in sample truncation, as some children of the household head (or the parents of the household head) are not counted as members of the household. Consider, for example, the case of LSMS surveys which, in most cases, count a person as part of the household only if he or she lived at least three months in the house during the last year. This would result in the exclusion of most children attending colleges in a large city for the households located in villages or small towns.[30] The resulting truncation of the sample is likely to affect the estimates of intergenerational educational and occupational and income persistence. Are the different measures of persistence affected differently by such sample truncation arising from co-residency restrictions in the surveys?

In a recent paper, Emran et al. (2018) provided evidence on the effects of sample truncation on the two widely used measures of relative mobility in the literature (IGRC and IGC), using education as the indicator of economic status. They use data from two rich surveys from India and Bangladesh that contain the education information on all the children, irrespective of their residency status, and conclude that the most commonly used measure, IGRC, suffers from substantial downward bias, while the bias in IGC is much smaller. For example, in the sample of 13–60 years of age in Bangladesh, the average downward bias in IGRC estimates is 29.7 per cent, while the corresponding bias in IGC estimates is only 8.7 per cent. The intuition for this evidence can be seen from the relation between IGRC and IGC in Equation 3. While truncation causes downward bias in the OLS regression of the slope parameter (IGRC) in a regression of children's schooling on parent's schooling, it also affects the ratio of variance of schooling across generations. Truncation reduces the variance of children's education as the surveys tend to miss observations from the tails of children's schooling distribution. In contrast, the variance of parental schooling is not substantially affected, as the survey captures a random sample of parents. They

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[28] Evidence on the effects of sample truncation on LW estimates would be valuable in this regard.

[29] When there is heteroscedasticity in the residual, the conditional variance partly reflects the effects of circumstances. To get a cleaner measure of effort one thus needs to take out the effects of circumstances. For a discussion, see Bjorklund et al. (2012).

[30] Some notable exceptions include CFPS survey and the 2013 round of CHIP survey for China, REDS and IHDS for India, IFLS for Indonesia, and MxFLS for Mexico.

also find that the magnitude of truncation bias is less sensitive to the degree of truncation in the case of IGC.[31]

In a follow-up paper, Emran and Shilpi (2018) extended the analysis to rank-based measures of mobility, using the same Bangladeshi and Indian data, and found that the co-residency bias in rank correlation (IRC) is smaller than that in IGRC, but similar to that in IGC. The evidence thus suggests that, for researchers working with the surveys readily available in developing countries, it is better to rely on IGC or IRC as the measure of relative mobility, and the current reliance on IGRC as the preferred measure seems ill-advised.

The evidence presented by Emran et al. (2018) and Emran and Shilpi (2018) relates to intergenerational educational persistence. Should we expect that the conclusions are likely to be applicable to other measures of economic status, such as income and occupation? A recent analysis of income persistence in rural India by Mohammed (2019) suggested that the effects might be different; he found that the IGE estimate is *higher* in the co-resident sample, in sharp contrast to the downward bias found for educational persistence by Emran et al. (2018) in rural India. As Mohammed (2019) noted, the estimated higher persistence may reflect the fact that, in a rural economy, it is more likely for a son to live with his father when both are farmers and tied to the land owned by the family. The difference between the evidence from Emran et al. (2018) and Mohammed (2019) may, however, primarily be due to the differences in data. Emran et al. (2018) use the 1999 round of the Rural Economic and Demographic Survey (REDS) done by NCAER (the National Council of Applied Economic Research), and Mohammed's analysis is based on the IHDS. More importantly, these two papers focus on two different aspects of co-residency; while Emran et al. (2018) are primarily concerned with the truncation due to the fact that the sample does not contain all the children of the household head when some children leave the household for higher study or marriage, Mohammed's analysis focuses on the fact that surveys have information on the parents of the household head (and the spouse) only when the parents co-reside for old-age support.[32] Additional evidence on the effects of truncation due to these two types of co-resident samples on different measures of economic status, and measures of mobility for rural and urban samples separately, would be valuable.

5 Understanding heterogeneity: social groups, cohorts and countries (and regions)

An important goal in many mobility studies is to estimate and understand possible heterogeneity in intergenerational persistence across different social groups (for example, gender, caste, ethnicity), and across cohorts, countries, and regions. It is, however, important to recognize that, to understand heterogeneity across groups, cohorts, or countries, the three measures of relative mobility discussed above are not adequate, we also need to look at the intercept estimates of the intergenerational regression equations (Equations 1, 2, and 4). The fact that the measures of relative mobility can be misleading for inter-group comparison of mobility has been emphasized by Mazumder (2014) and Hertz (2005) in their analysis of black–white differences in intergenerational mobility in the USA.[33]

Most of the cross-country analysis of educational mobility in developing countries are based on the estimates of IGRC and IGC; they do not report the estimates of the intercepts. Some prominent examples

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[31] As noted by Hertz et al. (2008), IGC is also less sensitive to the details of the empirical model and implementation. For example, the magnitude of IGRC in education can change substantially depending on how the parent's education is measured (the average of parent's education, the maximum of parent's education, etc.), but the magnitude of IGC is less sensitive.

[32] Emran et al. (2018) also provide evidence using a sample that contains both the non-resident parents and non-resident children of the household head and spouse. The conclusions do not change.

[33] Torche (2015) discusses similar issues in the sociological literature on intergenerational mobility.

are the works by Hertz et al. (2008) and Narayan et al. (2018).[34] To see the pitfalls in relying on the measures of relative mobility in a cross-country analysis, consider the estimates of IGRC for China (0.34) and Indonesia (0.78) from Hertz et al. (2008), discussed earlier. The evidence clearly implies that intergenerational educational persistence is much lower in China (relative mobility is higher), but does it necessarily mean that the children in China enjoy educational advantage? We cannot answer this question without the estimates of the intercept terms. If the intercept term is lower in China, then the expected educational attainment for children in Indonesia is uniformly higher, conditional on parental education for the entire span of the parent's education distribution; clearly, it makes little sense to say that children in China have educational advantage in this case (see Figure 1).

Figure 1: Relative mobility and educational advantage in China versus Indonesia: case A
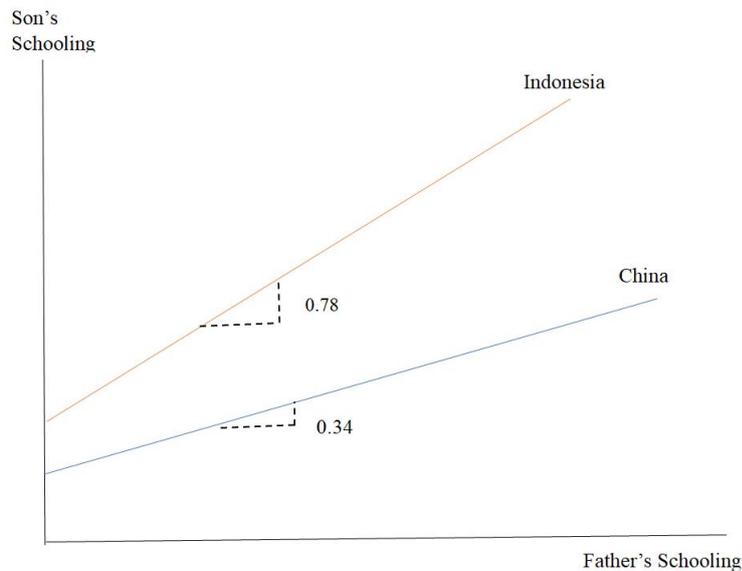
Source: authors' compilation.

When the intercept term is higher in China, we can have two cases. First, the conditional expectation function (CEF) for the Chinese children may intersect the CEF for Indonesian children from above,[35] implying that while the children from less educated households fare better in China, the advantage flips in favour of the Indonesian children after a threshold of parental education. Second, when the difference between the intercepts is large enough, the CEF for China can lie above that for Indonesia for the entire distribution of parental education. The ranking in terms of relative mobility provides meaningful conclusions about educational advantage only in this last case.[36] Figure 2 depicts these two different cases. From this perspective, two groups (or countries) can be said to enjoy the same educational opportunities when there are no differences in both the long-term variance (determined solely by the slope (IGRC)) and the steady-state mean (determined by both the slope and the intercept).

In a widely cited paper on intergenerational income mobility in the USA, Chetty et al. (2014) combine the intercept and the slope estimates from the rank–rank regression equation (Equation 4) and construct a measure of absolute mobility called P_{25}, which shows the expected income rank for the children of the parents with income in the 25th percentile of parental income distribution. With linear CEF, this can be

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[34] Although many authors report estimates of both IGRC and IGC in cross-country analyses (e.g. Hertz et al. 2008), the recent extensive study by Narayan et al. on cross-country educational mobility does not use the IGC estimates for their analysis. In the light of the evidence that IGC estimates are more robust to sample truncation and details of empirical implementation, an analysis based on the IGC estimates would be valuable for cross-country comparisons.

[35] This is expected to be the case when the difference in the intercept terms is small enough.

[36] For a discussion of these issues and evidence on intergenerational educational mobility in farm versus non-farm households in rural China and rural India, please see Emran et al. (2019).

Figure 2: Relative mobility and educational advantage in China versus Indonesia: case B
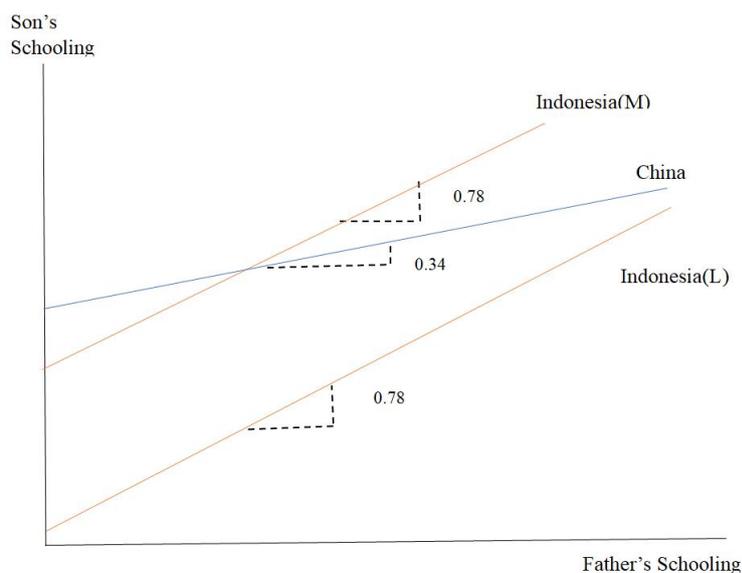
Source: authors' compilation.

interpreted as a measure of absolute upward mobility, as it captures the expected income of the children born into the lower half of the parental income distribution. Following the influential contribution of Chetty et al. (2014), the P_{25} measure has been adopted increasingly in research on intergenerational educational (and income) mobility in developing countries.

There is a related but somewhat different interpretation of *absolute mobility* adopted by many authors which relates to the question of whether a child is doing better than his/her parents (more income, higher schooling, etc.). This is especially informative for an analysis of income and occupation, but may be less so for understanding educational mobility in developing countries. For example, about 40 per cent of fathers in rural India had zero schooling in the 1999 REDS survey.[37] In this case, the only direction of educational mobility possible for their children is upward, and with almost universal primary school enrolment, (almost) everyone is doing better than their parents for the 40 per cent of households at the lower tail of the distribution.[38] This is a weak criterion for analysing educational mobility in developing countries. In contrast, the Chetty et al. (2014) approach is based on the rank of a child in the education distribution of his or her generation, and if the children of the fathers with zero schooling now have more schooling, but their rank in the distribution of their peers remains unchanged, this would not be considered upward mobility.

5.1 Implications of data limitations for the analysis of heterogeneity

Since the intercept estimates are important for understanding heterogeneity across groups, cohorts and countries, the question immediately arises as to how the intercepts in the regression functions of different measures of mobility (Equations 1, 2, and 4) are affected by data limitations. Since classical measurement error causes downward bias in the slope estimate, it is likely that the estimate of the intercept term will be biased upward, as the regression line rotates clockwise. To see this clearly, consider a slightly different version of the measurement error model used in Section 4.2:

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[37] In 2012 IHDS data, it is about 25 per cent.

[38] An implication of this observation is that evidence of no significant effect of policies on absolute educational mobility needs to be interpreted with appropriate caution. Because 40 per cent of the households enjoyed upward mobility irrespective of the incidence of the policy, one might find a weak statistical relationship between absolute mobility and the incidence of the policy, even when a policy actually affects relative mobility (IGRC).

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$$E_i^c = \beta_0 + \beta_1 Y_{3i} + \eta_i \tag{10}$$

$$Y_{3i} = Y_i^* + \nu_i \tag{11}$$

We denote the OLS estimate of the intercept term in Equation 11 by $\hat{\beta}_0$, then we have:

$$\text{Plim}\left(\hat{\beta}_0\right) = \beta_0 + \beta_1 \mu_y^* \left\{ \frac{\sigma_\nu^2}{\sigma_{y*}^2 + \sigma_\nu^2} \right\} > \beta_0$$

where $\mu_y^* = E\left(Y_i^*\right)$, $\sigma_{y*}^2 = \text{Var}\left(Y_i^*\right)$ and $\sigma_\nu^2 = \text{Var}\left(\nu\right)$. This suggests that the estimates of absolute mobility may be less biased compared to the estimates of relative mobility, an upward-biased intercept estimate partly offsetting the downward-biased estimate of the slope. We are not aware of any systematic evidence on the magnitude of biases in the intercept estimates of the different intergenerational regression functions caused by measurement error.

Emran and Shilpi (2018), using data on schooling of parents and children from Bangladesh and India, provided evidence on the effects of sample truncation due to co-residency on the estimates of the intercept terms. They found that the estimated intercept terms are biased upward, but the extent of the bias varies substantially across different measures of mobility. The evidence suggests that among the three regression functions discussed above, the intercept of the rank–rank regression is the least affected by sample truncation. The bias in the intercept term of the IGC regression (Equation 2) is, however, larger than the bias in the intercept term of the IGRC regression (Equation 1), in sharp contrast to the evidence on the slope parameters (IGRC and IGC) presented by Emran et al. (2018) and discussed in Section 4.3. The intuition behind this reversal in ranking can be seen from the fact that the intercepts from these two regression functions are related as follows: $\rho_0 = \dfrac{\beta_0}{\sigma_{Ec}}$. Since truncation results in a downward-biased estimate of the variance of children's education, the upward bias in the estimate of β_0 is magnified by a downward-biased estimate of the standard deviation of children's schooling σ_{Ec}. When considering the truncation bias in both the slope and intercept terms, the rank-based measures of mobility *à la* Chetty et al. (2014) thus seem preferable for a researcher with data only on co-resident members. As measures of relative mobility, rank correlation performs equally well as IGC, but the intercept from the rank regression has the lowest bias, while the intercept from the IGC regression shows the highest bias. The evidence shows that the absolute mobility measure P_{25} suffers the least from co-residency bias compared to the other similar measures based on the IGRC regression (Equation 1).

Measurement error and a co-resident sample make comparison across groups, cohorts, and countries difficult even when the focus is on relative mobility. For example, Emran et al. (2018) find that the cross-country ranking in educational persistence can be reversed when using co-resident samples compared to the ranking in the full samples. Sample truncation due to co-residency restrictions is likely to affect different genders and cohorts differently. In many developing countries, daughters leave the natal house after marriage, but sons continue to live with the parents after marriage. The extent of sample truncation is thus likely to be more severe for the daughters. In rural India, the co-residency rate for sons is 79 per cent, but only 39 per cent for daughters, according to the estimates reported by Emran et al. (2018).[39] For cohort-based analysis, it is important to recognize that the co-residency rates are likely to decline for the younger cohorts as the extended family living arrangements become less prevalent and geographic mobility increases, thus making the estimates of relative mobility spuriously lower for the younger cohorts. In contrast, we would in general expect measurement error in education, income, and occupation data to be lower for the younger cohorts in developing countries because of higher education

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[39] These estimates are based on the 1999 round of REDS data.

level, better record keeping, and decline in informal economic activities.[40] Since the the direction and magnitude of the net bias is unknown, the finding in many cohort-based studies that the younger cohorts enjoy higher relative mobility (see Azam and Bhatt (2015) and Jalan and Murgai (2008) on India) should be interpreted with some degree of caution. Similar (perhaps stronger) caveats are warranted for cross-country analysis using data for different cohorts, and unknown cross-country variations in the extent of co-residency.

6 Sibling (and kin) correlation: capturing the unobservable common family and neighbourhood background

The discussion so far has dealt with different ways to estimate the effects of permanent income as captured by a vector of parental characteristics observed in the data (in the absence of the required income data). However, even when there are sufficient data to estimate permanent income, the exclusive focus on permanent income may be restrictive. It has long been recognized that income and wealth are not sufficient statistics for the family background that shapes the life chances of the children. As noted earlier, Becker and Tomes (1979, 1986) provide a substantial discussion on the non-financial channels through which parents can affect economic opportunities of children. Moreover, the children in a family, when growing up together, share a lot more than the parents: they go to the same school, have social interactions with the same neighbourhood kids, and look up to the same role models in the community. The correlation in economic outcomes among siblings thus can be interpreted as an omnibus measure of the effects of family and neighbourhood on the economic opportunities of children.

Motivated by this insight, a substantial literature in the context of developed countries use SCs as a broader measure of intergenerational mobility that captures both observable and unobservable family and neighbourhood influences shared by siblings which shape their economic opportunities later in the life. The early contributors include, among others, Solon et al. (1988, 1991), with more recent contributions from Mazumder (2008) and Bjorklund et al. (2010b). For excellent treatment of the issues relevant for estimating SC and the advantages of SC in understanding the effects of family background, see Solon (1999) and Bjorklund and Jantti (2012). As we discuss in this section, an important advantage of SC in the context of developing countries is that the data requirements are not as demanding—for example, one can fruitfully utilize readily available surveys that contain only the co-resident children.

The estimating equation for SC can be written as a mixed effects model (for concreteness, we couch the discussion in terms of education):

$$E_{ij}^c = \mu + a_j + b_{ij} \tag{12}$$

where E_{ij}^c is the education level of sibling i in family j, μ is the population mean, a_j is the common family effect shared by siblings, and b_{ij} is the idiosyncratic component of child i measuring the deviations from the common family effect and assumed to be uncorrelated with the family component. Intuitively, SC is a measure of the variance in the household-specific component a_j across different households relative to the variance in children's educational attainment E_{ij}^c. It thus nets out the population mean, which captures the factors common to all households determining the average educational attainment.

Since the family component is uncorrelated with the individual idiosyncratic component, we can write the variance of children's schooling as follows:

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[40] However, there is substantial evidence that informal activities in developing countries have increased in the last few decades (Kanbur 2015).

$$\sigma_E^2 = \sigma_a^2 + \sigma_b^2 \tag{13}$$

Now, SC can be written as:

$$SC = \frac{\sigma_a^2}{\sigma_a^2 + \sigma_b^2} \tag{14}$$

Equation 14 makes it clear that SC is the share of variance in children's education that can be attributed to the common family background factors. It provides an estimate of the correlation in educational attainment among the siblings in a randomly selected household from the population. As noted by Emran and Shilpi (2015: 364), '[S]ibling correlation can be thought of as a summary statistic of the importance of common family and community effects which include anything and everything shared by the siblings. This is a measure of immobility because all these factors affecting the educational attainment of the siblings are not chosen by the children themselves, but they "are born into it".'

Since Becker and Tomes (1986), economic analysis of intergenerational mobility has identified market imperfections, especially credit constraints, as major sources of intergenerational persistence (see the recent survey by Lochner and Monge-Naranjo (2012) on the role of credit constraints in human capital accumulation). A focus on the credit market imperfections is also helpful in understanding SC as a measure of intergenerational immobility.[41] Consider the case of perfect markets: every household faces the same interest rate in the credit market (and wages in the labour market), and the parents optimally invest in education of a child irrespective of the family a child is born into; the accident of birth plays very little role in educational attainment, making the variance of the family component very low.[42] Now, consider the case in which there are two social groups—the rich and the poor—and the poor pay higher interest rates because of credit market imperfections.[43] Facing higher interest rates, the poor parents invest less in education of a child, keeping cognitive ability constant. Compared to the case of perfect credit markets, now the *average education level* of children born into a randomly drawn poor household is lower, which makes the variance of the family component larger, and the SC higher.[44]

6.1 The relation between SC, IGC, and IGRC

As measures of intergenerational mobility, SC is closely related to IGC, but it is a broader measure. To see this, note that the family component in Equation 13 can be written as:

$$a_j = \beta E_j^p + Z_j \tag{15}$$

such that the correlation of Z_j and E_j^p is zero. In other words, we decompose the family component into two orthogonal parts; one captures parental education and the other is uncorrelated with parental education. This allows us to rewrite SC as follows:

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[41] The following discussion is based on Emran and Shilpi (2015).

[42] The only source of variance in the family component in this case is genetic variations across households. However, we believe that this is unlikely to be the primary driving force, especially given the wealth of evidence that cognitive and non-cognitive abilities are largely determined by nutrition and the early-life family environment.

[43] Becker et al. (2015) adopt a similar specification of credit constraints.

[44] An important and relatively unexplored issue here is the implications of parental optimal investment when they compensate the relatively disadvantaged child in the family, making the variance of the idiosyncratic component lower compared to the standard 'efficient' investment.

$$SC = \frac{\sigma_a^2}{\sigma_{Ec}^2} = \frac{\beta^2 \sigma_{Ep}^2 + \sigma_Z^2}{\sigma_{Ec}^2} = (IGC)^2 + \text{orthogonal family factors} \tag{16}$$

Equation 16 shows clearly that SC is a broader measure because it captures both the effects of parental education and also other common family and neighbourhood factors shared by the children, but not correlated with parental education.[45] The available evidence on India shows that the proportion of SC in educational attainment explained by IGC is around 50–55 per cent (Emran and Shilpi 2015).

However, as noted recently by Bingley and Cappellari (2019), the above decomposition assumes that β is the same for all the households, and relaxing this assumption leads to the following decomposition:

$$SC = (IGC)^2 + \sigma_\beta^2 + \text{orthogonal family factors}$$

where σ_β^2 (the variance of IGRC) captures the degree of heterogeneity in intergenerational persistence β. They find that, allowing for heterogeneous β, in fact makes the share of SC explained by the intergenerational persistence much higher compared to the earlier studies on SC in developed countries (Solon 1999).

6.2 Discussion

As a broad measure of intergenerational (im)mobility, SC seems an attractive option for researchers in developing countries. It is important to appreciate that a sample of co-resident children is more likely to provide us with a credible estimate of SC because they would capture the common family, school, and neighbourhood influences shared by them. In contrast, non-resident children who left for college, say 10 years ago, may not share many of the common factors as the neighbourhood and schools change with time. Thus, missing such non-resident children from the sample is likely to be less damaging for the estimation of SC relative to the case of IGRC. In fact, unpublished estimates for India, Bangladesh, and China by the current authors (with Hanchen Jiang) suggest that the bias due to sample truncation because of co-residency is significantly lower in SC estimates compared to the IGRC estimates. The use of SC may be especially fruitful to understand the evolution of mobility across cohorts when the researcher has access to multiple surveys with co-resident children over the years; each survey can provide a reliable estimate of the relevant school-aged cohort with the co-resident children capturing most of the common family, school, and neighbourhood effects.

It is curious that there are only a few papers in the existing literature that adopted SC as a measure in the context of developing countries.[46] In an important contribution, Dahan and Gaviria (2001) present estimates of SC in schooling for 16 Latin American countries. Emran and Shilpi (2015) report estimates of sibling correlation in schooling for India in 1993 and 2006, and find that the spectacular economic growth and substantial poverty reduction have not translated into any substantial improvements in educational mobility from 1993 to 2006. Their findings show that the conclusions regarding cross-country comparisons of mobility (India versus Latin America) vary depending on whether we rely on IGRC or SC. When measured in terms of SC (0.64 in 1993 and 0.62 in 2006), the impact of family background in India is *higher* than the Latin American countries, including Brazil and El Salvador. In contrast, the

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[45] This relation is well known in the literature. See, for example, Solon (1999).

[46] This seems especially surprising in light of the fact that the authoritative survey of the field by Solon (1999) contains a substantial discussion on SC and highlights its advantages as a broader measure of the effects of family background compared to the standard measures of intergenerational persistence.

estimated IGRC in schooling for India is *lower* than that in Brazil and El Salvador. This implies that while the direct parents-to-children transmission of education is not as strong in India, the overall family background, in fact, plays a more important role.

7 Challenges for causal interpretation

The available work on intergenerational mobility in developing countries is primarily descriptive; only a small part of the literature is devoted to estimating causal effects of family background on children's income, education, and occupation. The central challenge in estimating causal effects is how to address possible upward bias in the estimates because of genetic transmission of ability and preference (called 'ability bias' for short), assuming that the attenuation bias has been taken care of with appropriate data. However, for some analyses, ignoring the ability bias may not be a bad approximation. For example, in a comparative analysis of different castes and religion in India, the differences observed in intergenerational persistence are unlikely to be driven by differences in genetic correlations in ability (there is no scientific basis for such differences across caste or religious groups). The recent finding by Asher et al. (2018) that the Muslims in India are among the most disadvantaged groups in terms of educational opportunities in the post-reform period thus is unlikely to be because of different ability correlations across different groups. Similarly, the differences in absolute mobility across different counties in the USA highlighted in the recent work of Chetty and Hendren (2018) are unlikely to be primarily driven by spatial variation in ability correlation across generations. Even though geographic sorting is based partly on unobserved ability and preference, there is no reason to expect that the strength of intergenerational ability correlation would vary across different counties in a systematic way.

Most of the existing studies assume that the unobserved ability is captured by the error term in the estimating equations (Equations 1, 2, and 4). For concreteness, consider a researcher interested in estimating IGRC for schooling using Equation 1. A common strategy is to set up a triangular model and use some source of exogenous variations in parental education. The triangular model is:

$$E_i^c = \beta_0 + \beta_1 E_i^p + \Pi X_i + \varepsilon_i$$
$$E_i^p = \gamma_0 + \Pi_1 X_i + \zeta_i \tag{17}$$

The correlation between the error terms is expected to be positive because of genetic transmission of academic ability and preference from parents to children—that is $\mathrm{Corr}\,(\varepsilon_i, \zeta_i) > 0$—resulting in an upward bias in the OLS estimate of the parameter β_1. The most common approach, both in developed and developing countries, is to develop an instrumental variables strategy based on a policy experiment or other natural experiments.[47] A recent example in the context of developing countries is the study on Indonesia by Mazumder et al. (2019), where a large-scale school construction in the 1970s is used as a source of identifying variation, following the influential work of Duflo (2004). They find an important causal role for the mother's education for the educational performance of children.[48] Taking advantage of the education reform in 1980 in Zimbabwe that eliminated the apartheid-style policies against blacks, Aguero and Ramachandran (2019) develop a fuzzy regression discontinuity design, and find that both

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[47] For a survey of the literature on developed countries, please see Black and Devereux (2011).

[48] In a related paper, Akresh et al. (2018) also study the long-term effects of Indonesian school construction, and find significant intergenerational transmission of education, especially through the mother's education. However, as noted by Mazumder et al. (2019), their data suffer from sample truncation due to co-residency. According to the estimates reported by Mazumder et al. (2019), about 55 per cent of children are non-resident. The large-scale school construction in Indonesia was first used by Hertz and Jayasundera (2007) in the literature on intergenerational mobility to study the effects of school access on the intergenerational persistence in education. Their focus is on estimating the causal effects of school construction on intergenerational correlation, not on tackling the ability bias. For a detailed discussion, see Section 7.2.

father's and mother's education positively affect their children's educational attainment. The advantages of a credible IV strategy are well-known: it corrects for both the ability bias and the measurement error. However, the estimates are relevant for only a subset of the households (i.e. the compliers). This implies that while the estimates are very useful when the focus is on understanding the effects of certain policies such as school construction (increased supply of schooling), they may not be appropriate for other policies or for the broader population.[49]

When credible sources of exogenous variation are not available (which is most of the time), one can implement the approach developed by Altonji et al. (2005) and extended by Oster (2019), which relies on selection on observables as a guide to selection on unobservables to understand the role played by ability correlations. Emran and Shilpi (2011) use the Altonji et al. (2005) approach to estimate the lower bound on the causal effects of parental occupation on children's occupation in rural Nepal and rural Vietnam under the restriction that selection on observables is equal to selection on unobservables. They find that the lower bound is significant for the mother–daughter occupational link, especially in Nepal, and interpret it as role model effects in a traditional society (given the set of controls). This approach, however, requires that we include a rich set of observables to ensure the assumption of equality between selection on observables and on unobservables, and thus may be of limited use for estimating the causal effects (i.e. the total derivative) of, for example, parental education and/or occupation. The rich set of controls would capture part of the causal effects of education and occupation. When the vector of control variables is parsimonious,[50] one can use the sensitivity analysis for various values of the correlation $\text{Corr}(\varepsilon_i, \zeta_i)$. Such sensitivity analysis looks more promising when one takes advantage of the recent estimates of the correlation in cognitive ability between parents and children from the economics and behavioural genetics literature to restrict the interval to which the correlation parameter belongs. The available evidence suggests that a plausible interval would be $\text{Corr}(\varepsilon_i, \zeta_i) \in [0.20 - 0.40]$ (for the economic literature, see, for example, Black et al. (2009) and Bjorklund et al. (2010a); for the behavioural genetics literature, see Plomin and Spinath (2004)). Emran et al. (2019) use this approach to analyse the role played by parental farm and non-farm occupations in intergenerational educational persistence in China and India. They find that when the value of genetic correlation is set at the maximum of 0.40, the educational persistence becomes small and insignificant in the case of rural China, for both the farm and non-farm households. In contrast, in India, the persistence estimates remain substantial and significant, with the strongest effects found for the non-farm households. The evidence thus suggests that the observed intergenerational educational persistence in rural China could be driven by genetic correlations alone, while genetic correlations cannot explain the observed persistence in rural India. An important advantage of this approach not adequately appreciated in the current literature is that the bias-corrected estimates refer to the broader population rather than a subset of 'compliers' as is the case with an IV approach.

In the triangular model above, ability is assumed to affect the educational attainment in an additively separable way. The recent contributions in economics, however, suggest that this is a too simplistic description of the role played by ability in human capital formation. The effects of ability may be multiplicative, rather than additively separable. In an interesting theoretical analysis, Becker et al. (2015, 2018) develop a model of human capital approach to intergenerational income persistence where parental financial investment is complementary to the ability of a child, which makes IGRC a function of ability in the Equation 1. When ability enters multiplicatively, the estimating equation becomes a random coefficient model, as discussed in an interesting paper by Murtazashvili (2012). She shows that the 2SLS estimator is not appropriate in this case, and develops a control function approach for estimating

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[49] Aguero and Ramachandran (2019) argue that their estimates are relevant for a large proportion of the population in Zimbabwe, as 86 per cent of the eligible students changed their behaviour in response to the policy change.

[50] For example, it is common to control for only quadratic age variables for the father and son in the analysis of intergenerational income mobility, following Solon (1992)

the causal effect (when there is a credible IV). Her analysis finds that the estimate of income persistence from the control function approach is much higher when compared to the IGE estimate using the 2SLS estimator.

Another important issue highlighted by the work of Heckman and his co-authors is that the implicit assumption of ability as innate is likely to be misleading. A large literature has developed in the last few decades that shows that ability is largely determined by the early-life environment, including the mother's health when a child is in utero, and early-life nutrition (Heckman and Corbin 2016; Heckman and Mosso 2014).[51] An implication of this evidence is that most of the estimates of correlations in cognitive and non-cognitive ability of parents and children available in the current literature are likely to be biased upward, as they are measured not at birth, but later in life. This also implies that when we set $\text{Corr}(\varepsilon_i, \zeta_i) = 0.40$, the resulting estimate of IGRC should be considered as a lower bound, both because the ability correlation is likely to be overestimated, and the IGRC estimate will be biased downward because of measurement error.

7.1 The implications of sample truncation due to co-residency

As noted in Section 4.3, there are two types of co-residency we need to think about; some papers focus on the non-resident children, while others focus on the non-resident parents of the household head and spouse. It is important to appreciate that it is difficult to estimate the causal effects even when we have a data set containing information on the non-resident parents of the household head and spouse. In this case, we have a random sample of children with data on their parents irrespective of the residency status of the parents at the time of the survey. However, for estimating causal effects, what we need is a random sample of parents with the necessary information on all of the children, irrespective of the residency status at the time of the survey. This can be better understood if we think of a mental experiment in which we could do a randomized controlled trial to estimate the causal effects of the father's education on the children's schooling. Clearly, we need to randomize the father's education (the 'treatment'), not the children's education (the outcome variable), and then track all the children irrespective of the residency at the time of the follow-up survey after the children complete their education.

7.2 Causal effects: estimating effects of policy on intergenerational persistence

A number of recent contributions focus on estimating the effects of policy (or economic environment) on intergenerational persistence in developing countries; thus, the focus is not on the biases caused by unobserved genetic transmissions, but on the possible endogeneity in policy placement and implementation. Most of the current literature on intergenerational persistence and the causal effects of a parent's education ask different research questions, and they have evolved somewhat independently. It is not clear how to connect the two sets of estimates to get an overall picture of economic mobility across generations for answering policy-relevant questions. The focus on the causal effects of policy on the measures of persistence such as IGRC, IGC, and IRC provides a bridge: one can ask policy-relevant questions without trying to disentangle the role of genetics in the estimated intergenerational persistence. For example, whether investment in roads and schools improve mobility by weakening the intergenerational persistence in education and occupation are clearly important for both the policy makers and the researchers.[52]

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[51] For evidence on the effects of family background on brain development of children, see the widely cited work by Noble et al. (2015). They conclude that 'These data imply that income relates most strongly to brain structure among the most disadvantaged children' (abstract).

[52] In the context of developed countries, Pekkarinen et al. (2009) estimate the effects of comprehensive school reform in Finland during the period 1972–77 on intergenerational income elasticity. They find that the school reform that eliminated the two-track system reduced IGE from 0.30 to 0.23.

An early example of this approach in the context of developing countries is presented by Hertz and Jayasundera (2007), who study the effects of school construction in Indonesia. They find that school construction lowered the IGRC for education of sons, but had no appreciable effect on that of daughters. Using the 1991 liberalization in India as a quasi-experiment, Ahsan and Chatterjee (2017) estimate the causal effects of trade liberalization on occupational persistence in urban India, and find that while the higher demand for skills following trade liberalization increases cross-sectional inequality, it also promoted occupational mobility. Zou (2018) estimates the effects of the one-child policy on intergenerational educational mobility in China, with a focus on the channel of fertility decline and increased demand for higher-quality children. They find that the one-child policy improved educational mobility in China. Assaad and Saleh (2018) estimate the effects of increased supply of public primary schools in Jordan on the father–sons, mother–sons, father–daughters, and mother–daughters schooling correlations. Their findings suggest that the availability of public primary schools weakens the intergenerational linkages, especially for daughters. In a recent paper, Ahsan et al. (2019) analyse the effects of better market access on intergenerational educational persistence, both theoretically and empirically. Their theoretical analysis identifies a set of conditions under which better market access increases (complementarity) or reduces (substitutability) the intergenerational persistence in education. The empirical analysis uses data from the IHDS and National Sample Survey (NSS), and relies on the location of historical railroads in 1880 *à la* Donaldson (2018) and the arc distance to the Golden Quadrilateral (GQ) highway network as sources of identifying information. Their evidence shows that, in rural India, better access to markets reduces the influence of family background on children's educational attainment; better markets thus act as substitutes for better-educated parents.

8   Conclusions

This paper provides a critical discussion of the economic literature on intergenerational mobility in developing countries with a special focus on the data and methodological challenges. While many existing works on developing countries rely on the standard measures of relative mobility, such as IGRC, the recent evidence shows that IGRC suffers from severe biases due to data limitations such as measurement error and sample truncation due to co-residency. As measures of relative mobility, intergenerational correlation and rank correlation perform better with limited data.

The analysis of heterogeneity across social groups, countries, and cohorts raise additional methodological issues, as we need reliable estimates of both the slope and the intercept of the intergenerational regression function. The rank-based measures of absolute mobility, such as P_{25} due to Chetty et al. (2014), seem to be much less affected by data limitations, and thus deserve more attention in the research on intergenerational mobility in developing countries. Although cohort-based analysis is popular, some additional caveats and cautions are warranted. The pattern of co-residency changes across cohorts, and measurement error is likely to be more pronounced in the case of older cohorts. Thus, it may be difficult to interpret the observed changes in persistence across cohorts. It is also important to appreciate that parental characteristics such as education and occupation are partial measures of family background, and complementary analysis using measures such as SC and the 'LW approach' to latent economic status can provide valuable insights. Although largely neglected in the recent literature, SC is an attractive measure for understanding the broader role of family background, including the neighbourhood and peer effects shared by siblings but not captured by parental observed characteristics. Compared to IGRC, SC is less affected by sample truncation due to co-residency, and can provide useful evidence on the evolution of mobility across cohorts even when we have only repeated cross-section data on co-resident children. Following the lead of Chetty et al. (2014), a lot of recent effort has been devoted to understand spatial heterogeneity, but research on possible gender differences remains seriously neglected, both in developed and developing countries.

The literature on the causal effects of family background in developing countries is relatively small, but has attracted increasing attention in recent times. While the standard approach of relying on a policy experiment to break the genetic correlation between parent and children is valuable, it is important to appreciate that the estimates in most cases refer only to a subset (sometimes a rather small subset) of the relevant population. We discuss alternative approaches that can help make progress on the role played by genetic transmissions in the persistence of economic status in the broader population. We also highlight some recent contributions where the goal is to estimate the effects of policies such as school and road construction on intergenerational persistence in economic status. This approach can provide policy-relevant evidence without confronting the difficult, if not intractable, issue of disentangling the role of genetic transmissions at birth.

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