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Introduction

Citation for published version:
Guell, M, Rodríguez Mora , JV & Solon, G 2018, 'New directions in measuring intergenerational mobility: Introduction', *The Economic Journal*, vol. 128, no. 612, ecoj.12607, pp. F335-F339. https://doi.org/10.1111/ecoj.12607

Digital Object Identifier (DOI):
10.1111/ecoj.12607

Link:
Link to publication record in Edinburgh Research Explorer

Document Version:
Peer reviewed version

Published In:
The Economic Journal

Publisher Rights Statement:
This is the pre-peer reviewed version of the following article: Güell, M. , Rodríguez Mora, J. V. and Solon, G. (2018), New Directions in Measuring Intergenerational Mobility: Introduction. Econ J, which has been published in final form at https://onlinelibrary.wiley.com/doi/10.1111/ecoj.12607. This article may be used for non-commercial purposes in accordance with Wiley Terms and Conditions for Self-Archiving

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Download date: 13. Aug. 2024

New Directions in Measuring Intergenerational Mobility: Introduction

(Short title: Mobility Introduction)

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This Feature contains seven new articles that exemplify several of the cutting edges in research on intergenerational transmission of socioeconomic status. Section 1 of this introduction briefly summarises the articles and highlights some cross-cutting themes. In Section 2, we offer a few suggestions for future research.

1. The Articles

One of the growth areas in mobility research is the study of "multigenerational mobility," that is, status transmission across three or more generations. The article by Solon (2018) summarises both the theoretical and empirical literatures. One of the greatest challenges in advancing the empirical literature is simply to assemble good data that span at least three generations. The article by Long and Ferrie (2018) develops and analyses three generations of occupational data from Great Britain and the United States over the 1850-1910 period. An example of the basis for the title's message that "grandfathers matter(ed)" is that, when the authors apply least squares to the regression of the log of average earnings in the occupation of British sons on the same variables for both their fathers and grandfathers, the father coefficient estimate is 0.285 (with standard error 0.018), and the grandfather coefficient estimate is 0.051 (0.016). (We will say a bit more about such results in section 2.) The article by Adermon, Lindahl, and

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Waldenström (2018) reports similar results for intergenerational wealth transmission across three generations from Malmö, Sweden. Continuing on the subject of wealth inequality, the article by Boserup, Kopczuk, and Kreiner (2018) studies wealth inequality among children in Denmark. The authors find that wealth at age 18 is a significant predictor of adult wealth. They suggest two main reasons: inter vivos transfers at young ages foreshadow additional transfers later in life, and children may inherit saving/investment propensities from their parents.

Long and Ferrie's 1850-1910 period exemplifies another growth area in mobility research – historical analysis of earlier eras. This also is the focus of the article by Feigenbaum (2018), which estimates intergenerational associations in multiple indicators of socioeconomic status between U.S. sons and fathers in the state of Iowa during the first half of the 20th century. The two generations are linked through data from the 1915 Iowa census and the 1940 U.S. census. Across all the various indicators, Feigenbaum estimates intergenerational associations smaller than those typically estimated in more recent years. Accordingly, Feigenbaum suggests that the United States used to be a more mobile society than it is today.

Another active area of mobility research has been cross-country comparison of intergenerational mobility (e.g., Solon 2002, Corak 2006). Such comparisons typically have found substantial cross-country differences and have noted that countries with greater cross-sectional inequality tend to display lesser intergenerational mobility. The influential work of Chetty, Hendren, Kline, and Saez (2014) documented a similar sub-national pattern among cities in the United States. The article by Güell, Pellizzari, Pica, and Rodríguez Mora (2018) reports similar results for provinces in Italy. The authors are especially struck that such cross-area variation appears in Italy even though government policy is much more uniform in Italy than in the United States.

To measure intergenerational mobility at the province level in Italy, Güell et al. use the surnames-based method they developed in Güell, Rodríguez Mora, and Telmer (2007, 2014). Much-publicised later work by Clark (2014) used surnames in another way, estimating intergenerational regressions with group-average data for rare surnames. Clark's controversial conclusion was that the true intergenerational correlation in social status is in the 0.7-0.8 range and has been the same in all societies and eras. Clark pointed out that one of the testable hypotheses implied by his interpretation is that combining multiple indicators of socioeconomic status in a micro-level analysis of intergenerational mobility would result in much higher estimates of intergenerational persistence. The article by Vosters (2018) checks that hypothesis with U.S. data from the Panel Study of Income Dynamics. Contrary to Clark's conjecture, she finds only a marginal increase in estimated intergenerational persistence. In a follow-up study (Vosters and Nybom 2017), she and her co-author add Swedish data and find again that combining multiple indicators has only a marginal impact. They also reaffirm the finding of earlier research (such as Björklund and Jäntti 1997) that Sweden is much more mobile than the United States.

A section in the article by Solon (2018) notes several other ways that the evidence refutes Clark's conclusion, most strikingly that the U.S. group-average surname regressions reported in an appendix to Chetty et al. (2014) showed results dramatically different from Clark's. An appendix to the article by Güell et al. (2018) reports a similar finding for Italy. They note that the only way they can obtain as large an intergenerational correlation estimate as Clark does for rare surnames is by restricting to especially *common* surnames, and they show evidence that this result is due to province effects. This exemplifies the potential importance of group effects in mobility research, a topic we will discuss further in the next section.

2. A Few Thoughts about Future Research

The articles in this Feature demonstrate that intergenerational mobility is a vibrant research area, and we are confident that the years to come will bring many more efforts to advance the literature. In this section, we offer a couple of suggestions for future research.

First, we recommend that future analyses of multigenerational mobility develop clearer metrics for interpreting the substantive importance of estimated effects from grandparents (or more distant ancestors). As an example, we return to the finding in Long and Ferrie's (2018) table 3 of a 0.285 coefficient estimate for fathers and a 0.051 coefficient estimate for grandfathers in the British data on occupational earnings. Based on the statistical significance of the estimated grandfather coefficient, Long and Ferrie conclude that "assessments of mobility based on two-generation estimates significantly overstate the true amount of long run social mobility." It is not immediately obvious, though, whether that conclusion is warranted. For example, table 3 shows that adding the grandfather regressor to the model increases the adjusted R^2 from 0.110 to only 0.116. Similarly, the wealth regressions in table 3 of the article by Adermon et al. (2018) show that adding the grandparental variable moves the unadjusted R^2 from 0.174 to only 0.181. By the R^2 metric, extending to a multigenerational perspective does not importantly alter our impression of the importance of family origins.

Long and Ferrie's reference to the long run suggests another relevant metric – the correlogram for how intergenerational autocorrelations evolve with longer lags between generations. For simplicity, suppose that the intergenerational mobility process is stationary. Then Long and Ferrie's reported 0.302 coefficient estimate when father's status is the only regressor

implies a second-order autocorrelation (that is, a correlation between sons and grandfathers) of 0.091 (the square of the coefficient estimate), a third-order autocorrelation of 0.028, and a fourth-order autocorrelation of 0.008. The autocorrelations implied by the second-order autoregression including grandfather's status decline at a slower-than-geometric rate: 0.300 at the first order, 0.137 at the second order, 0.054 at the third order, and 0.022 at the fourth order. Thus, at the fourth order for example, considering grandfathers more than doubles the implied autocorrelation. On the other hand, the fourth-order autocorrelation is close to 0 either way. Our point is simply that making these implications explicit will give readers a better opportunity to judge for themselves how much the multigenerational perspective matters.

Second, we wish to emphasise the potential importance of group effects for interpreting intergenerational mobility results. This point has been made by numerous authors (including all three of the present authors in their articles in this Feature), but we are surprised that it has not received even greater attention. One reason we are surprised is that Borjas (1992) already provided a highly accessible econometric formulation of the issue in a quite visibly published article.

Reiterating part of the Borjas analysis, suppose that the intergenerational mobility process is

$$y_{igt} = \delta_1 y_{ig,t-1} + \delta_2 \bar{y}_{g,t-1} + u_{igt} \tag{1}$$

where the intercept is suppressed by expressing all variables as deviations from their population means, y_{igt} is a measure of the socioeconomic status of individual *i* in group *g* and generation *t*, $y_{ig,t-1}$ is the socioeconomic status of that individual's parents, $\bar{y}_{g,t-1}$ is the average status of group *g* in the parents' generation, and $Cov(u_{igt}, y_{jgs}) = 0$ for all *i* and *j* (including $i = j$) and $s < t$. We expect $0 < \delta_1 < 1$ and $0 \le \delta_2 < 1$. If no group effects are present, $\delta_2 = 0$. If group effects are

present, δ_2 presumably is positive, i.e., offspring from the more advantaged group have higher expected status. For example, if racial discrimination in the United States causes African-Americans to be at a disadvantage relative to whites even when their parents have the same income, this would be reflected in a positive δ_2.

This simple model has strong implications for the measurement of intergenerational mobility. Let us begin with the group-average intergenerational regressions estimated by many authors. Averaging equation (1) across all members of group *g* yields

$$\bar{y}_{gt} = (\delta_1 + \delta_2)\bar{y}_{g,t-1} + \bar{u}_{gt}. \quad (2)$$

How does this compare to the individual-level intergenerational regressions estimated by many other authors? That is, what is the coefficient in the regression of y_{igt} on $y_{ig,t-1}$ alone (with $\bar{y}_{g,t-1}$ excluded from the regression)? Denoting that coefficient as β, the Borjas analysis shows that

$$\beta \cong \delta_1 + \delta_2\rho \quad (3)$$

where $\rho \equiv Var(\bar{y}_{g,t-1})/Var(y_{ig,t-1})$ is the between-group proportion of the population variance in $y_{ig,t-1}$. Borjas reasonably conjectures that ρ typically is small, that is, most of the population variance is within-group.

Subtracting the result in equation (3) from the group-level coefficient in equation (2) shows that the difference between the group-average intergenerational coefficient and the individual-level one is approximately $\delta_2(1-\rho)$. If $\delta_2 = 0$, this is no difference at all. Then the group-level and individual-level coefficients are the same, as assumed by Clark (2014) and Aaronson and Mazumder (2008). But if ρ is small and δ_2 is substantial, the difference can be quantitatively important.

This finding sheds light on a puzzle in the existing literature: Why do some researchers (such as Clark 2014) estimate group-level coefficients much larger than the usual individual-level coefficients while others (such as Aaronson and Mazumder 2008; Card, DiNardo, and Estes 1998; Chetty et al. 2014) do not? The answer may be that δ_2 varies across different contexts and sampling schemes. Torche and Corvalan (forthcoming) have illustrated this point with a series of NLSY-1979 analyses that generate very different group-level intergenerational coefficient estimates by switching among group samples with and without powerful group effects. And, like several other authors, they conjecture that Clark's high estimates stem from his choice of rare-surname groups that represent the extremes of the socioeconomic status distribution.

The model in equation (1) also sheds light on the multigenerational mobility literature, which typically estimates the regression of y_{igt} on both parental status $y_{ig,t-1}$ and grandparental status $y_{ig,t-2}$. But what if equation (1), with a group effect but no grandparental effect, is the true data-generating process? What would be estimated by the regression on both parental and grandparental status with the group effect omitted? It seems reasonable to guess that the grandparental coefficient estimate would come out positive because grandparental status would operate as a proxy for the omitted group effect. Indeed, doing the math (available from the corresponding author) shows that, if we assume the variances and covariances are nearly stationary across periods *t*, *t-1*, and *t-2*, then the grandparental coefficient comes out approximately to $\delta_2\rho/(1+\beta)$. Thus, if δ_2 and ρ are sufficiently positive, omitted group effects could cause a noticeably positive grandparental coefficient estimate even when no grandparental effect really exists. And one reason that different multigenerational studies get different results could be that δ_2 and ρ vary across settings.

The articles in this Feature leave us more enthusiastic than ever about the prospects for intergenerational mobility research. We have learned a great deal already. For example, research published in the early 1990s and since has taught us that intergenerational persistence is stronger than we previously had realised. It varies considerably across nations, across areas within nations, and across time, and this variation is at least loosely correlated with cross-sectional inequality. One of the main remaining research challenges is to understand better the sources of such variation. We look forward to further advances coming from both theoretical and empirical inquiries that complement and inform each other.

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