What Do We Know So Far about Multigenerational Mobility?

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Abstract

"Multigenerational mobility" refers to the associations in socioeconomic status across three or more generations. This article begins by summarizing the longstanding but recently growing empirical literature on multigenerational mobility. It then discusses multiple theoretical interpretations of the empirical patterns, including the one recently proposed in Gregory Clark's book *The Son Also Rises*.

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"… practically all the advantages or disadvantages of ancestors tend to disappear in only three generations: 'from shirtsleeves to shirtsleeves in three generations.' Parents in such 'open' societies have little effect on the earnings of grandchildren and later descendants."

– Gary S. Becker and Nigel Tomes (1986, p. S28)

"… all social mobility is governed by a simple underlying law, independent of social structure and government policy:

$$x_{t+1} = bx_t + e_t$$

where x_t is the underlying social status of a family in generation t, e_t is a random component, and b is in the region 0.7-0.8…. [T]his law of mobility implies that on average, the status of the descendants will move toward the mean for the society generation by generation. When the persistence rate, b, is as high as 0.8, this is a slow process, taking many hundreds of years for families who are initially far above or below the mean."

– Gregory Clark (2014, p. 212)

Most analysis of mobility across generations (as exemplified by most of the research in this issue) focuses on the association in socioeconomic status between adjacent generations. Both of the above quotations, however, pertain to what has come to be called "multigenerational mobility" – the pattern of associations across three or more generations. Strikingly, the two quotations reach wildly different conclusions about the rapidity of regression to the mean across multiple generations. Just as strikingly, the two reflect a shared belief in the importance of multigenerational mobility. All the same concerns about "openness" and "equal opportunity" that motivate interest in intergenerational mobility apply with at least as much force to multigenerational mobility.

The purpose of the present article is to provide a status report on what we know so far about multigenerational mobility. Section I gives an overview of the empirical literature. Section II presents several alternative theoretical interpretations of the evidence. Section III focuses on a particular interpretation due to Gregory Clark. Section IV summarizes and discusses the findings.

I. The Empirical Literature

Mobility between adjacent generations often is measured by estimating a first-order autoregression [AR(1)] between the two generations – for example, a regression of offspring's log income on parental log income. Whatever the true data-generating process that connects socioeconomic status between the two generations, estimating an AR(1) regression is a reasonable way of producing a simple summary statistic (such as the intergenerational income elasticity) to describe the strength of the intergenerational association.

But what if we also want to know about higher-order associations, such as the association between the offspring and grandparents or great-grandparents? Occasionally, writers implicitly or explicitly assume a stationary AR(1) data-generating process by assuming that intergenerational autocorrelations die out at a geometric rate – for example, extrapolating a first-order autocorrelation of 0.4 (between offspring and parents) to impute that the second-order autocorrelation (between offspring and grandparents) is 0.16 and the third-order autocorrelation (between offspring and great-grandparents) is 0.064.[1] As we will see in section II, however, there is no theoretical basis for presuming an AR(1) data-generating process. Therefore, in the present section, we will treat the evolution of economic status across multiple generations as an empirical question.

Over the last quarter-century or so, the empirical literature on intergenerational mobility has advanced tremendously, thanks in large part to the acquisition of new and better data linking adjacent generations. The multigenerational literature has advanced more slowly because it is much more difficult to obtain data linking three or more generations. Nevertheless, there exists a substantial multigenerational literature, which originated many decades ago.

One of the pioneering contributions was the classic 1966 study by sociologist Robert Hodge (1966).[2] Hodge used three-generation U.S. data on mobility across occupational categories to test the categorical counterpart to an AR(1) regression specification – that the transition probabilities follow a first-order Markov process, in which grandfather's occupation has no predictive power for son's occupation once father's occupation has been controlled for. Hodge rejected the first-order Markov process, but also concluded that the observed departure from a first-order process was not quantitatively important. Quoting directly (p. 25), "Although the discrepancies between the actual and expected values shown in Table 1 clearly indicate that grandfather's occupation bears some relation to grandson's occupation, which is not fully explained by father's occupation, we must emphasize the discrepancies are not large…. [G]randfather's occupation does not have any appreciable *direct* effect upon a person's occupation beyond the *indirect* effect induced by its influence upon father's occupation." As we

[1] See, for example, the passage on intergenerational mobility in the undergraduate labor economics textbook by Borjas (2013, section 7.6).

[2] Even earlier occupational mobility studies with data on three generations include Mukherjee (1954) and Svalastoga (1959).

will see in the remainder of this section, this finding is not such a bad characterization of the central tendency of the entire existing literature.

Although many sociologists since Hodge have continued to analyze mobility across occupational categories, I will focus instead on the part of the subsequent literature that has studied the income and education outcomes more commonly considered by economists.[3] An early example is the three-generation part of Behrman and Taubman's (1985) mobility study based on the U.S. NAS-NRC Twins data. Behrman and Taubman estimated regressions of offspring's years of education on the years of education of both parents and grandparents. Their estimated coefficients for grandparental education were very small and statistically insignificant.

This finding of no apparent departure from a first-order process is fairly common in the empirical literature. Other examples are the studies by Peters (1992), who used U.S. National Longitudinal Surveys data to estimate regressions of offspring's log income or earnings on parental log income or earnings and grandparental education; Warren and Hauser (1997), who used the U.S. Wisconsin Longitudinal Study to estimate regressions of offspring's occupational prestige or education on the earnings, occupational prestige, and education of both parents and grandparents;[4] Ridge (1973), who similarly used British data on education and occupational prestige in three generations; and Lucas and Kerr (2013), who used Finnish data to estimate regressions of offspring's log earnings on parental and grandparental log income.

On the other hand, some other studies estimating multigenerational regressions have gotten non-trivially positive coefficient estimates for grandparental status. A prominent recent example is the study of multiple generations from Malmö, Sweden, by Lindahl et al. (forthcoming). When Lindahl et al. estimated a regression of son's log earnings on both father's

[3] A recent three-generation study by economists that does analyze occupational categories is Long and Ferrie's (2012) study of the United States and Great Britain during the 1850-1910 period.
[4] More recently, Jaeger (2012) reported some similar results from the Wisconsin Longitudinal Study.

and grandfather's log earnings, the parental coefficient estimate was 0.281 (with standard error 0.045), and the grandparental coefficient estimate was 0.084 (0.044). Lindahl et al. obtained similar results in three-generation regressions for years of education.[5]

As we have seen, some studies have not found evidence of a grandparental "effect," and some others have. An instructive study with a combination of the two findings is Zeng and Xie (2014). Using data from rural China, Zeng and Xie estimated regressions of offspring education on parental and grandparental education.[6] Their regressions included the interaction of grandparental education with whether the grandparents were co-resident with the offspring and parents. Their intriguing finding (pp. 610-611) is that, "although the education of noncoresident and deceased grandparents has little or no effect on grandchildren's dropout rate, the effect of coresident grandparents' education is quite large.... These results suggest that grandparents can play an important role in their grandchildren's schooling if they all live under the same roof." Zeng and Xie concluded (p. 614), "This suggests that causal processes of intergenerational influences occur primarily inside households through daily interactions. Our research thus reaffirms the primary importance of the socioemotional pathway for intergenerational effects."

Zeng and Xie's study serves to illustrate two important general points. First, there is no reason to expect a universal pattern across all times and places with respect to whether the mobility process is or is not first-order. It makes sense, for example, that the role of grandparents would vary with the circumstances. Second, the *ways* in which it varies with circumstances might provide leverage for identifying underlying causal processes. This leads

[5] In ongoing dissertation research at Michigan State University, Kelly Vosters is using the Panel Study of Income Dynamics to estimate three-generation U.S. regressions for log income and earnings. Her preliminary results are qualitatively similar to those of Lindahl et al. In addition, Vosters is developing an instrumental variables approach for testing an errors-in-variables interpretation of these results, which will be described below in section II.

[6] Zeng and Xie estimated non-linear regressions specified to account for right-censorship of the offspring's measured education.

directly to the topic of the next section: What are some of the plausible theoretical explanations of the empirical patterns we have reviewed?

II. Theory

To develop a theoretical framework for interpreting the empirical multigenerational patterns, I will start with the initial model in Solon (2014), which adapts the classic model of Becker and Tomes (1979) to rationalize the double-log functional form of the regression equations typically estimated in empirical studies of intergenerational income mobility.[7] This baseline model will turn out to be inadequate for accounting for some of the empirical patterns in section I, so I later will proceed to extending it in several ways.

As spelled out more fully in Solon (2014), the assumptions include these:

- A single parent divides her income between her own consumption and investment in a single child's human capital so as to maximize a Cobb-Douglas utility function in which the two goods are the parent's consumption and the child's adult income.
- The specifications of the human capital production function and the earnings function are such that the elasticity of the child's adult income with respect to parental investment in the child's human capital is a positive constant γ.
- The human capital production function includes an additively separable term e that denotes the human capital endowment the child receives regardless of the family's conscious investment choices. This endowment is intergenerationally correlated because of both inheritance of genetic traits and cultural inheritance, such as the effects of

[7] The model in my 2014 article is a simplification of the model in Solon (2004), which considered the intergenerational mobility implications of public investment in children's human capital by including such government investment and taxation in the model. In the present paper, including a government role would clutter the model without affecting the model's implications for the structure of multigenerational mobility.

parental role-modeling. The initial model follows Becker and Tomes in assuming that inheritance of the endowment follows the AR(1) process

(1) $$e_{it} = \delta + \lambda e_{i,t-1} + v_{it}$$

where e_{it} is the endowment of the child in family i, $e_{i,t-1}$ is the parent's endowment, v_{it} is a white-noise innovation, and the heritability coefficient λ lies between 0 and 1.

As demonstrated in Solon (2014), maximization of the Cobb-Douglas utility function leads to a steady-state intergenerational income elasticity of

(2) $$\beta = (\gamma + \lambda)/(1 + \gamma\lambda).$$

This equation shows that the intergenerational income elasticity is positive for both of two reasons – because γ is positive (i.e., richer parents' greater investment in their children's human capital makes their children richer) and because λ is positive (i.e., richer parents tend to have more favorable endowments, which tend to be passed on to their children through genetic and cultural inheritance). So, for example, if $\gamma = 0.3$ and $\lambda = 0.2$ (or vice versa), then the intergenerational income elasticity is $\beta = (0.3 + 0.2)/[1 + (0.3)(0.2)]$, which is about 0.47.

For present purposes, though, the key question is what the model implies for multigenerational mobility. Solon (2014) shows that multigenerational mobility in this model follows the AR(2) process

(3) $$\log y_{it} = \text{intercept} + (\gamma + \lambda)\log y_{i,t-1} - \gamma\lambda \log y_{i,t-2} + \text{white-noise error term}$$

where y_{it} is the income of the child from family i, $y_{i,t-1}$ is parental income, and $y_{i,t-2}$ is grandparental income. In this regression of the child's log income on both parental and grandparental log income, the coefficient of parental log income is positive, but the coefficient of grandparental log income is a small *negative* quantity! For example, with $\gamma = 0.3$ and $\lambda = 0.2$, the coefficient of parental log income is 0.50, and the coefficient of grandparental log income is -0.06. This implication of a negative coefficient for grandparental income, first noted by Becker and Tomes (1979), is initially surprising, but it does not really mean that an exogenous increase in grandparental income harms the child's income. Rather, it reflects a subtle implication of higher grandparental income *conditional on the amount of parental income*. If the parent did not earn more despite the advantages of higher grandparental income, this signals that the parent got a poor draw on her genetic/cultural endowment, and that poor draw tends to be passed on to some extent to the child.

Note that, if the multigenerational mobility process is really AR(2) with a negative coefficient for grandparental status, then multigenerational autocorrelations decline *more* rapidly than geometrically. For example with $\gamma = 0.3$, $\lambda = 0.2$, and hence about a 0.47 correlation between parent and child log incomes, the correlation between the grandparent's and child's log income is about 0.18, somewhat less than the square of 0.47. And the correlation between the great-grandparent's and child's log incomes is only about 0.06. This implication of Becker and Tomes's theory, combined with their belief that even the first-order autocorrelation is small, accounts for their pronouncement, quoted at the beginning of this paper, that "practically all the advantages or disadvantages of ancestors tend to disappear in only three generations."

Thanks to the accumulation of new and better evidence over the last quarter-century, we now understand that, in many countries, the first-order autocorrelation is higher than Becker and Tomes thought. And, as discussed in section I, there is very little evidence to support their theory's prediction of a negative coefficient for grandparental status. Rather, some studies suggest that, in some times and places, the grandparental coefficient is positive, in which case the multigenerational correlations decay more slowly than geometrically.

This does not mean anything is wrong with Becker and Tomes's analysis as far as it goes. Instead, it suggests that their model is incomplete, as models always are. In this instance, the model appears to be leaving out additional ways in which grandparental status may foretell children's outcomes. The remainder of this section highlights three straightforward extensions of the theory that could explain why the tendency for a negative grandparental coefficient noted by Becker and Tomes is offset or even dominated by other factors.

First, as in Zeng and Xie's interpretation of their evidence on multigenerational education mobility in rural China, grandparents may contribute to cultural inheritance. Indeed, this possibility was recognized in chapter 6 of Becker's *A Treatise on the Family* (1981), which explicitly entertained generalizing the endowment transmission model beyond the AR(1) specification in equation (1) to more complex specifications incorporating influences from other relatives besides parents. An extension that incorporates grandparental influence is the AR(2) specification

$$(4) \qquad e_{it} = \delta + \lambda_1 e_{i,t-1} + \lambda_2 e_{i,t-2} + v_{it}$$

where $0 \le \lambda_2 < \lambda_1 < 1$.

As shown in Solon (2014), redoing the analysis with equation (4) in place of equation (1) leads to an AR(3) process for multigenerational income mobility:

$$(5) \qquad \log y_{it} = \text{intercept} + (\gamma + \lambda_1)\log y_{i,t-1} + (\lambda_2 - \gamma\lambda_1)\log y_{i,t-2} - \gamma\lambda_2 y_{i,t-3}$$

$$+ \text{ white-noise error term.}$$

The AR(2) model in equation (3) is the special case in which $\lambda_2 = 0$. When instead $\lambda_2 > 0$, the model in equation (5) shows two differences from the one in equation (3). First, great-grandparental log income now appears (with a small negative coefficient). Second, the grandparental coefficient could be positive now because of the incorporation of a grandparental contribution to cultural inheritance.[8] In particular, the grandparental coefficient is positive if $\lambda_2 / \lambda_1 > \gamma$. As suggested by Zeng and Xie, whether this condition holds presumably would vary with circumstances. For example, it might be likelier to hold in a society where children typically live with or near their grandparents.

A second extension is to incorporate group effects.[9] Suppose, for example, that racial discrimination in the United States causes African-Americans to have a lower earnings function intercept than whites. As shown in Solon (2014), this also would translate into a lower intercept for African-Americans in a multigenerational mobility equation such as equation (3) or (5). Indeed, empirical support for race-specific intercepts in intergenerational mobility equations has appeared in a long history of studies such as Duncan (1968), Corcoran et al. (1992), and Hertz (2005). If such inter-group differences in intercepts exist, a failure to model them amounts to omission of group fixed effects. Applying the usual omitted-variables-bias analysis to equation (5) shows that, because parental log income, grandparental log income, and great-grandparental log income all have positive partial correlations with the omitted group effects, all the ancestral coefficient estimates are pushed in a positive direction. This in turn would be a force towards slower-than-geometric decay in observed multigenerational autocorrelations. I will return to this subject in the next section.

[8] Solon (2014, p. 16) also noted a parallel potential role for genetic inheritance: "genetic transmission is really more complicated than the simple first-order autoregression…. Recognizing the reasons that manifestations of family genetic traits can 'skip a generation' is another way of opening up the possibility of a positive coefficient for grandparents' status." That paper also discussed the possible role of direct grandparental investment in children's human capital.

[9] This extension was introduced in section VI of Becker and Tomes (1979) and later explored in Borjas (1992).

A third extension is to consider the effects of measurement error. Suppose, for example, that the true multigenerational process is AR(1) with a 0.5 parental coefficient and zero coefficients for grandparents, great-grandparents, etc. Then the true multigenerational autocorrelations would decline geometrically: a 0.5 correlation between child and parents, 0.25 between child and grandparents, 0.125 between child and great-grandparents, etc. But now suppose that each generation's status is measured with classical (i.e., purely random) measurement error. And suppose that the measured variation in each generation consists 80 percent of true variation and 20 percent of measurement noise. Then each of the measured autocorrelations would be attenuated by a factor of 0.8. That is, the measured autocorrelations would tend towards 0.4 between child and parents, 0.2 between child and grandparents, 0.1 between child and great-grandparents, and so forth. And because these *measured* autocorrelations decline more slowly than geometrically, fitting an autoregression of child's status on both parental and grandparental status would result in a spuriously positive coefficient estimate for grandparental status.

These three extensions of the initial analysis do not exhaust the possibilities for explaining why some evidence shows positive grandparental coefficient estimates and slower-than-geometric decay in measured autocorrelations. For example, footnote 8 mentions a couple of other possibilities, and the next section will discuss a different version of the errors-in-variables story due to Gregory Clark. The three extensions discussed in this section, however, suffice to demonstrate that a multitude of different processes can generate positive higher-order coefficient estimates and autocorrelation estimates that decay more slowly than geometrically. An important part of the agenda for future mobility research is to devise empirical approaches for ascertaining which underlying processes are quantitatively important under which circumstances.

III. Clark's Interpretation

In the book *The Son Also Rises: Surnames and the History of Social Mobility* (2014) and related papers, economic historian Gregory Clark and his co-authors offer a provocative and fascinating new account of mobility across generations. As indicated in the quotation at the beginning of this paper, Clark maintains that, in all societies in all eras, mobility across generations follows an AR(1) process with a high autoregressive coefficient between 0.7 and 0.8. His evidence for this "law of social mobility" is based on data that Clark and his collaborators have gathered, from many countries over many centuries, on various socioeconomic outcomes for individuals with rare surnames. Most of these data do not contain direct intergenerational links between offspring and parents in the same families. Instead, Clark aggregates individuals within a generation by surname and then examines the association between generations in group-average outcomes. For adjacent generations, the correlation tends to be about 0.75, and the higher-order autocorrelations decline approximately geometrically. Clark concludes (p. 125), "Surname evidence shows that all social mobility can essentially be reduced to one simple law,

$$x_{t+1} = bx_t + e_t,$$

where x is the underlying social competence of families. The persistence rate, b, is always high relative to conventional estimates, generally 0.7-0.8. It seems to be little affected by social institutions." And, as indicated in the quotation from Clark at the beginning of this paper, with an autoregressive coefficient of 0.7-0.8, multigenerational regression to the mean takes hundreds of years to play out.

Clark is well aware that his hypothesis appears to fly in the face of at least two seemingly well-established stylized facts. First, the large empirical literature on intergenerational associations shows a great deal of variation across countries, with most of the estimated intergenerational correlations far below Clark's 0.7-0.8 range. Second, as discussed in section I, some multigenerational studies have found non-trivial departures from an AR(1) process.

With respect to the first stylized fact, Clark argues that existing estimates are biased substantially downward by a sort of errors-in-variables problem. Of course, many existing intergenerational studies, including my own (e.g., Solon 1992), have emphasized and treated measurement error in reports of income or other variables. But Clark is making a different point: that variables like income, education, and occupational prestige – no matter how precisely measured – are noisy indicators of underlying "social status." Assuming that the classical errors-in-variables analysis applies, he suggests that the intergenerational association in any single indicator substantially understates the intergenerational association in social status, and that his surnames-based approach mostly eliminates this errors-in-variables bias by averaging over many individuals in each surname group. With respect to the second stylized fact, Clark uses the same errors-in-variables argument presented here in section II: that even if the true multigenerational mobility process is AR(1), classical measurement error (in Clark's version, the error in a single indicator as a measure of deeper social status) causes the *measured* autocorrelations to decline at a slower-than-geometric rate.

Clark's radically different account of social mobility has given some welcome food for thought to long-time intergenerational mobility scholars such as myself. In my case, Clark's work has amplified my interest in multigenerational mobility, which I am acting on by writing the present paper. Ultimately, though, the question is whether Clark's hypothesis is supported by the evidence. Fortunately, his hypothesis generates numerous testable predictions, some of which Clark helpfully has pointed out himself.

To begin with, Clark's hypothesis implies that any group-average estimation – grouping not only by rare surnames, but also by "race, religion, national origin, or even common surnames" (Clark 2014, p. 110) – should deliver intergenerational correlation estimates in the 0.7-0.8 range. Fortuitously, several intergenerational studies over the years have worked with group averages. In a study of intergenerational assimilation of immigrant groups, Card, DiNardo, and Estes (1998) used U.S. decennial censuses to estimate intergenerational regressions of years of education or log weekly earnings for immigrants, grouping by country of origin. Their typical coefficient estimates were about 0.45, considerably less than the 0.7-0.8 range predicted by Clark. Results similar to those of Card, DiNardo, and Estes also appeared in the intergenerational study of immigrant groups by Borjas (1993).

A different type of group-average intergenerational study is the one by Aaronson and Mazumder (2008). Also using U.S. decennial censuses, Aaronson and Mazumder estimated intergenerational regressions of men's log annual earnings on the log of the average income for their parents' generation in the men's state of birth. According to Clark's hypothesis, the group nature of the explanatory variable should lead to coefficient estimates in the 0.7-0.8 range. Instead, the estimated coefficients again averaged at about 0.45.

Most striking of all is the surnames-based portion of the intergenerational study by Chetty et al. (2014). Reacting to Clark's work, Chetty et al. wrote an on-line appendix that used their massive data base drawn from U.S. income tax records to estimate group-average regressions based on surnames. Using all surnames, they estimated an intergenerational income elasticity of 0.42. Their estimates using only rare surnames were even smaller at around 0.35.

Evidently, the results reported by Clark do not reflect a universal law of social mobility. Quite to the contrary, other studies based on group-average data, even surnames data, frequently produce intergenerational coefficient estimates much smaller than Clark's.

A second testable prediction of Clark's hypothesis, noted by Lindahl et al. (forthcoming),[10] is that instrumental variables (IV) estimation of the regression of son's log earnings on father's log earnings should yield a coefficient estimate in the 0.7-0.8 range if father's log earnings are instrumented with grandfather's log earnings. When Lindahl et al. estimated that regression with their data from Malmö, Sweden, the IV coefficient estimate was 0.515, considerably higher than their ordinary least squares (OLS) estimate of 0.303. They obtained a remarkably similar comparison of IV and OLS estimates when they used years of education instead of log earnings as the status measure. The pattern of IV estimates exceeding OLS estimates is consistent with Clark's general story about measurement error in particular indicators as proxies for social status. It is equally consistent with all the alternative stories listed in section II for why grandparental status may not be "excludable" from a multigenerational regression.[11] What the results are *not* consistent with is a universal law of social mobility in which the intergenerational coefficient is always 0.7 or more.

Nearly three decades before Lindahl et al., Behrman and Taubman (1985) used their NAS-NRC Twins data to perform IV estimation of the intergenerational education regression using the education of the father's twin (the son's uncle) as the instrument for the father's

[10] Lindahl et al. credited the idea to Hoyt Bleakley and an anonymous referee.

[11] In particular, if grandparental status enters the AR(2) multigenerational regression with a positive coefficient, then using grandparental status as the instrument in IV estimation of the regression of offspring status on parental status is upward-inconsistent for the slope coefficient in the population linear projection of offspring status on parental status. This follows directly from the analysis of IV estimation in the appendix of Solon (1992). Incidentally, it is of some interest that this IV estimator is the ratio between the estimated coefficients in the regressions of offspring status and parental status on grandparental status. With stationarity, it also would be the ratio of correlations. This is of some interest because empirical multigenerational studies sometimes focus on exactly those ratios, apparently without noticing the connection to IV estimates.

education. In this instance, the intergenerational coefficient estimate rose from an OLS estimate of 0.17 to an IV estimate of 0.21, still way below the 0.7-0.8 range.

A third testable prediction, explicitly stated by Clark and Cummins (forthcoming), is that using an omnibus index that combines multiple indicators of social status should make the intergenerational coefficient estimate "much closer to that of the underlying latent variable." Vosters (2014) has used the U.S. Panel Study of Income Dynamics to test that prediction. Applying Lubotsky and Wittenberg's (2006) multiple-proxies method to construct an aggregate parental index that supplemented log income with education and occupation measures, she found that using the additional measures increased the intergenerational elasticity estimate only a little, from 0.44 to 0.47. The resulting estimate was not "much closer" to the 0.7-0.8 range.

In sum, when Clark's hypothesis is subjected to empirical tests, it does not fare so well. But then why *do* Clark's group-level autocorrelation estimates for rare surnames (though not some other researchers' group-level estimates) come out so high? I see that mainly as a matter for future research, but here I will briefly sketch one alternative explanation.

Returning to section II's group-effects explanation of positive grandparental coefficients, suppose that the status of family i in group g (surname or otherwise) in generation t can be decomposed as

(6) $$y_{igt} = a_{gt} + b_{igt}$$

where the a term is a group-level average and the b term is an orthogonal family-specific deviation from the group average.

Following a suggestion in footnote 13 in Becker and Tomes (1979), suppose that the intergenerational process for the group average a is a stationary AR(1), with a coefficient of 0.8 (thus according with Clark's group-level evidence). Also suppose that b separately follows a stationary AR(1) with a coefficient of 0.3.[12] Finally, suppose that the cross-sectional variance of *y* is 60 percent within-group and 40 percent between-group.

Then it is easy to calculate that, at the family level, the first-order intergenerational correlation is 0.5 (the weighted average of 0.8 and 0.3). The higher-order autocorrelations are 0.31 for two generations apart, 0.22 for three generations apart, 0.17 for four apart, and 0.13 for five apart.

Note the following points about this illustrative example:

- By construction, it accords with Clark's group-level evidence. If the within-group sample sizes are large, it involves a 0.8 first-order autocorrelation at the group level, which declines geometrically at higher orders.
- At the individual level, the first-order autocorrelation of 0.5 is much smaller. Unlike in Clark's interpretation, in this story the smaller individual autocorrelations are not spuriously attenuated by errors-in-variables bias, but reflect the true individual-level social mobility.[13]
- In accordance with some of the multigenerational evidence discussed in section I, the individual-level autocorrelations decline more slowly than at a geometric rate and therefore would generate a positive coefficient estimate for grandparental status.

[12] That a large gap in autocorrelations between the *a* and *b* terms can be realistic is vividly illustrated by results reported by Hertz (2008). Using PSID data, he estimated an intergenerational elasticity of 0.32 within his sample of African-Americans, an elasticity of 0.39 within his sample of whites, but a between-group elasticity of 1.18. He also presented the algebra to explain how his 0.53 estimate from the sample pooling the two races was a weighted average of the two within-group estimates and the between-group estimate.

[13] Another perspective on this example is that group-average estimation of the intergenerational regression is equivalent to IV estimation of the micro-level regression of offspring's status on parental status with group dummies as the instruments (Solon 1999, footnote 15). But, as already discussed in footnote 11, such an IV approach is inconsistent for the micro-level regression unless the instruments are "excludable," which they are not in this instance if group effects are operative.

Of course, this model should not be taken too seriously. It is simple to a fault, and I made up all the parameter values out of thin air. But it does serve as an example that Clark's theory need not be the only possible explanation of his rare surnames evidence and other empirical patterns such as positive grandparental coefficients.

IV. Summary and Discussion

As summarized in section I, the empirical literature on multigenerational mobility contains some studies indicating that multigenerational mobility is well approximated as an AR(1) process. Some other studies have suggested that the coefficient of grandparental status is positive, so that multigenerational autocorrelations decay more slowly than at a geometric rate.

Where positive grandparental coefficients are estimated, there are many possible sources. Section II highlighted three examples: direct causal effects from grandparents, such as cultural inheritance effects when grandparents are present in the children's lives; group effects, such as effects associated with race or ethnicity; and errors-in-variables bias. Section III focused on another example, the variant of the errors-in-variables story that Gregory Clark has advocated based on his work with surnames data. While there undoubtedly is something to Clark's point that any single indicator of socioeconomic status is an imperfect status measure, a variety of empirical tests have rejected his claim of a universal law of social mobility in which the intergenerational correlation is in the 0.7-0.8 range in all societies in all eras. For example, contrary to Clark's prediction, most group-average studies other than his own – including the surnames-based work by Chetty et al. – have estimated much smaller intergenerational associations.

The broader lesson of the empirical assessment of Clark's hypothesis is that much can be learned from relevant evidence. Future research should explore which underlying processes – those discussed in section II as well as others – are quantitatively important under which circumstances. My conjecture is that, as we learn more about multigenerational mobility, we will find that the reality is more complex than suggested by either of the quotations at the beginning of this article. I doubt that all multigenerational mobility is as simple as either Clark's "law of social mobility" or "shirtsleeves to shirtsleeves in three generations." Just as recent research has found that the intergenerational income elasticity varies considerably across countries, we may find that multigenerational mobility behaves differently in different times and places. For instance, it seems quite plausible that grandparental cultural influence varies across societies that differ in how present grandparents are in children's lives. It also seems plausible that group effects, such as those associated with race and ethnicity, loom larger in some societies than others.

Thanks to both better data and better analysis, we now know much more about intergenerational mobility than we did a quarter-century ago. I hope and expect that ongoing research on multigenerational mobility also will advance our understanding in the years to come.

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