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ECONOMIC MOBILITY

A STATE-OF-THE-ART PRIMER

Contemporary Levels of Mobility | Scott Winship

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Economic Mobility in America

A State-of-the-Art Primer

Contemporary Levels of Mobility[1] | Scott Winship

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[1] This paper benefitted from comments from Gonzalo Schwarz and Erin Currier, for which I am grateful. Any errors are solely the author's.

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Executive Summary

Economic mobility has become a leading policy concern across the political spectrum in America. But "opportunity" and "mobility" are elusive concepts. Without clearheaded thinking about what they mean and how to measure a particular way of viewing opportunity, it is easy to misinterpret the evidence on economic mobility. Further, getting a handle on the evidence itself is no easy task. The present report is the first in a series that, together, will constitute a state-of-the-art primer on intergenerational economic mobility in the United States. The need for such a primer is not only evident from the explosion of mobility research in recent years that has scrambled our understanding of the topic. Less understood is the need to clear up rampant confusion over what different mobility measures actually measure.

The report provides an overview of the different ways of measuring both relative and absolute mobility (i.e., movement in ranks and movement in dollars). It distinguishes between mobility indicators that assess movement in different parts of the parental and child income distributions, as well as summary measures that describe how mobility does or does not reduce childhood inequalities.

Using a survey that has recorded information on thousands of families for nearly fifty years—following children as they leave home and establish their own households—the report presents new state-of-the-art estimates of an unprecedented range of economic mobility measures. The estimates constitute the most comprehensive suite of mobility measures that anyone has produced. The report also discusses the strengths and weaknesses of summary measures in assessing the extent of equal opportunity. An up-to-the-minute literature review on levels of American economic mobility is included in an appendix.

Among the highlights of this report:

1. Consistent with past research, the report documents the strong odds that poor children will fare no better relative to their peers than their parents did. Nearly half of children with parents in the bottom fifth of family income end up in the bottom fifth as well. Children who grow up with the richest parents are only somewhat less immobile.

2. These estimates are based on averaging family incomes over 9 to 15 years within a window of up to 31 years. If they could be averaged over entire careers, immobility would look even stronger.

3. Nevertheless, roughly three in four adults—and the overwhelming majority of poor children—live better off than their parents after taking the rising cost of living into account. This rate is higher than in the headline findings of a recent well-publicized paper by Stanford University economist Raj Chetty and his colleagues. This report shows that Chetty et al.'s results can be replicated with survey data but illustrates why the headline finding paints too dour a picture of mobility.
4. The report shows, for the first time, tables that illustrate the likelihood poor and well-to-do adults will have a same-sex sibling who is also poor or well-to-do. The family incomes of siblings do not strongly resemble each other, except at the bottom and top. Among adults in the bottom fifth of income who have a same-sex sibling, over 40 percent of the time that sibling is also in the bottom fifth. The family incomes of siblings at the top are nearly as similar.
5. Sibling similarity is especially strong for brothers' earnings.
6. The report includes innovative illustrations of how sensitive summary measures of economic persistence are to the number of years of income averaged and to restrictions on how many years are required in order to be included. Estimates for over 200 samples were produced for each summary indicator in the report, with preferred ranges and point estimates reported for each.
7. Relative mobility reduces percentile gaps between children by about 35 to 55 percent for the earnings of men, by 55 to 70 percent for the earnings of women, and by 45 to 50 percent for family income. The "income rank association," on which these estimates are based, reflects the high degree of mobility within the broad middle of the income distribution but masks the "stickiness at the ends" found earlier in the report.
8. The income rank associations estimated here—on the order of 0.45 to 0.65 for men's earnings, 0.30 to 0.45 for women's earnings, and 0.50 to 0.55 for family income—are higher than in almost all previous studies. They improve on those earlier studies by averaging up to 15 years of income within a window of up to 31 years, centered on age 40, when incomes most closely resemble lifetime income.
9. The report explains why the most popular mobility estimate—the "intergenerational elasticity," or IGE—summarizes absolute mobility rather than relative mobility, contrary to the conventional wisdom among mobility researchers.

10. IGE estimates have become increasingly large as research methods have improved, indicating a smaller reduction of childhood income gaps by adulthood than previously believed. Nevertheless, this report concludes that nearly all previous estimates are too low, overstating the extent to which childhood income gaps are diminished in adulthood. The report estimates IGEs of between 0.70 and 0.80 for male earnings, 0.35-0.55 for female earnings, and 0.65-0.75 for family income. It also speculates that they could be higher. Roughly, at an IGE of 0.75, the future grandchild of an adolescent growing up with twice the income of his classmate will still have an income 34 percent higher than his classmate's grandchild.

11. The report summarizes very recent critiques of Gregory Clark's *The Son Also Rises*, which claimed that mobility is remarkably low and consistently so across nations and eras.

12. It includes sibling rank association estimates—only the second time such estimates have been presented, to my knowledge. The relative earnings gap between the brother of a higher-earning man and the brother of a lower-earning man will tend to be 40 percent as large as the gap between those two men. The gap between the sister of a higher-earning woman and the sister of a lower-earning woman will tend to be 30 percent as large as the gap between those two women. This narrowing of relative gaps obscures the greater similarity between poor siblings and rich siblings, however.

13. Conventional sibling correlations are also estimated, and the report demonstrates that averaged income data can yield correlations as large as those produced from complicated modeling of the evolution of "permanent" and "transitory" income.

14. The sibling correlations indicate that nearly half of female earnings inequality occurs between sisters within the same family, while roughly 30 to 35 percent of male earnings inequality and of family income inequality occurs within families.

15. The report explains why the intergenerational rank association is a better indicator of equality of opportunity than the intergenerational elasticity or correlation. The sibling rank association may be the best indicator of all, among summary measures.

16. The literature review covers studies completed as recently as December 2016 and several forthcoming journal articles.

The primer will include two more installments—one on cross-national differences in economic mobility and another on trends in mobility in the United States.

1. Introduction

Economic mobility has become a leading policy concern across the political spectrum in America. You see it reflected in the "Better Way" antipoverty plan of House Republicans[1], the report of the AEI/Brookings Working Group on Poverty and Opportunity[2], and the Center for American Progress "Progressive Agenda to Cut Poverty and Expand Opportunity."[3] It shines through in Third Way's focus on the "mobility mentality"[4] and the "Room to Grow" agenda of the Conservative Reform Network.[5] It motivates the work of established foundations like the Pew Charitable Trusts, the Annie E. Casey Foundation, the MacArthur Foundation, and the Peter G. Peterson Foundation, and it is central to the mission of brand new think tanks such as the Foundation for Research on Equal Opportunity and the Archbridge Institute.[6] Policymakers and researchers left, right, and center increasingly agree that raising upward mobility rates and expanding opportunity should be among the nation's primary goals.

But "opportunity" and "mobility" are elusive concepts. Without clearheaded thinking about what they mean and how to measure a particular way of viewing opportunity, it is easy to misinterpret the evidence on economic mobility. Further, getting a handle on the evidence itself is no easy task. The past few years have seen a number of major mobility studies that have overturned what was only recently the conventional wisdom about mobility. The risk is that an out-of-date read of the research literature or confusion about what different mobility indicators tell us will lead to the pursuit of misguided policies.

The present report is the first in a series that, together, will constitute a state-of-the-art primer on intergenerational economic mobility in the United States. The need for such a primer is not only evident from the explosion of mobility research in recent years that has scrambled our understanding of the topic. Less understood is the need to clear up rampant confusion over what different mobility measures actually measure.

The primer will present original estimates for the United States of an unprecedented variety of economic mobility measures, using consistent methods informed by the research advances of recent years. It will summarize the evolution of research on economic mobility measurement in the United States. It will also dive into the questions of how American mobility levels today compare with those of the past and in other developed nations. Finally, the primer will consider in detail how to think about what different mobility measures tell us, clarifying what we can and cannot say about mobility with one indicator or another.

The focus of this initial installment is on contemporary levels of intergenerational economic mobility in America; future installments will address cross-national comparisons and changes in mobility over time, both focused on the United States.

Inevitable constraints of time and space necessitate that a variety of topics be neglected. In particular, this primer will have little to say about the factors that promote or discourage mobility. A strong case could be made that we actually know shockingly little about the relative importance of different factors. It is certainly the case that no one can credibly rank factors (even roughly) in order of importance. A number of methodological advances will be mentioned only in passing, the emphasis being on understanding the most widely used mobility indicators. Evidence from other nations will be discussed only in relation to American mobility rates. Differences in mobility between groups of people in the United States—such as the sizable mobility gaps between blacks and non-Hispanic whites—and between different parts of the country—such as the low-mobility Southeast and the high-mobility Upper Midwest—are also beyond the scope of this primer.[7]

Before reviewing the ways that economic mobility has been measured and presenting new estimates (Section 2), the rest of this section defines the scope of the paper and explains how the estimates were produced. Subsequently, Section 3 considers the strengths and weaknesses of different measures for assessing equality of opportunity. Following the conclusion, appendices provide more methodological detail and an up-to-the-minute review of the literature on American economic mobility levels.

The Many Facets of Mobility

Even when it is clear that the subject at hand is not *residential* mobility, the term "mobility" is far too imprecise to be of much use on its own. First, assessing the extent to which people are moving up or down may depend on what outcome is under consideration. We may be interested in the amount of mobility in terms of hourly wages, earnings, income, wealth, occupational status, or educational attainment, to name only the most often examined outcomes.

Second, there is the distinction between intergenerational and intra-generational mobility. Intergenerational mobility is concerned with the extent to which people do or do not differ from their parents on a given outcome. In contrast, intra-generational mobility focuses on whether one's own outcomes differ over time.[8] For instance, we can ask whether rich parents tend to also have rich children (intergenerational mobility), but it may also be of interest whether poor 25-year-olds are also poor 55-year-olds (intra-generational mobility).

Finally, movement up or down may involve absolute or relative mobility. In assessing the extent of individual movement, we might be interested in whether people end up better or worse off in absolute terms. Do they tend to have higher education levels than their parents did at the same age? After adjusting for increases in the cost of living, do people have higher family incomes than they did earlier in their careers? Absolute mobility at the individual level takes no account of how well or poorly peers have done. With strong economic growth, the vast majority of people may experience upward absolute income mobility. One person's income may increase by less than the average person's, leaving her behind more people than used to be the case. But she will still have experienced upward absolute mobility.

In contrast, relative mobility is about where people *rank* compared with their peers and whether that position improves or worsens over time. Someone whose income rises but by less than his peers' income will experience upward absolute mobility but downward relative mobility. A person who was raised in the top half of the income distribution and is richer in adulthood than his parents were may nevertheless have fallen to the bottom half. If economic growth makes everyone richer by the same amount, everyone will experience upward absolute mobility but no one will see any relative mobility.

Both absolute and relative mobility are appropriate policy concerns. We want the sons of fast-food workers to have a better standard of living than their fathers enjoyed. But they should also have the opportunity to become engineers it they want, rather than better-paid fast-food workers.

Narrowing the Scope

This primer focuses on intergenerational earnings and income mobility, examining both absolute and relative measures. The great advantage of economic mobility measures—as opposed to occupational and educational ones—is that hourly wages, earnings, income, and wealth are measured on a broad continuum that facilitates analyses of both absolute and relative movement. Educational attainment falls within a narrow range of years, effectively with a floor of around 10 years of schooling and a ceiling at roughly 20 years. Educational mobility also makes for imprecise comparisons, not only because people tend to clump at a few points in the educational attainment distribution (12 years, 16 years) but because two people who have been in school for the same amount of time can experience very different educational quality.

Occupational status measures involve a degree of subjectivity that the other measures do not. Rank ordering in terms of “status” is always arbitrary. It is also hard to justify treating occupational status measures as continuous; the difference in occupational status between two people with status scores of 60 and 80 cannot be assumed to be the same as the difference between two people with scores of 40 and 60. This is partly because “occupational status” is a social construct and partly because there is no such thing as a job with no occupational status (a score of zero). If occupations are not ranked, then it becomes difficult to assess whether a given amount of mobility between occupations is “good” or “bad.”

Hourly wages and wealth share many of the analytic advantages of earnings and income in mobility analyses. In fact, wealth may be a better indicator of the resources available to people to pursue their aims. But good intergenerational data on wealth is hard to find. Furthermore, measuring wealth is more complicated than measuring earnings and income, even with good data. Wealth is typically measured as “net worth”—assets less debt. But while student loans are generally included on the debt side of the ledger, the asset financed by that debt (human capital) is almost never included on the other side.

There is also ambiguity about how to value future retirement benefits as wealth. Both private defined benefit pensions and public programs like Social Security and Medicare take the form of promises from employers or the government to support people in retirement. But because these “assets” are not in the control of workers or taxpayers and because promises may be broken, they are typically not included in wealth. At the same time, defined contribution pensions, like 401(k) plans, over which workers have control are included in wealth. Since people will save less money inside accounts they control the more they think they will receive promised retirement benefits outside their control, the distinction between the two in wealth measurement is especially problematic.

Hourly wages only partly reflect the opportunities that employment brings workers. Less-skilled workers may face not only low hourly pay but fewer hours of work or more unemployment. In addition, hourly wages are often measured with considerable error if they are computed by dividing annual earnings by annual hours (as must be done for salaried workers). Finally, non-wage compensation may comprise a different share of pay among different kinds of workers, and may grow or shrink in importance between generations.

Earnings and income are not perfect measures for every policy question about mobility either. The children of the very rich may not need to work much or at all. Or they may be more able to pursue vocations that are often low-paying, such as being an artist or running a bookstore. More generally, people with the most opportunities—graduates of Ivy League colleges, for instance—often choose careers that do not maximize their incomes (nonprofit and government work come to mind). Some unappealing jobs such as sanitation work are better compensated to attract enough people to them, but the higher income as compared with lower-paid workers in more desirable jobs does not necessarily indicate a better outcome.

There are also complications around income measurement, such as which income concept to examine (before or after government redistribution? pre- or post-tax?), how to adjust for increases in the cost of living, whose incomes within a household to combine (those of family members? cohabiters? roommates?), how to account for noncash benefits like employer- or government-sponsored health insurance, and how or whether to account for the size of families and households.

Nevertheless, earnings and income convey meaningful information about what kinds of opportunities people enjoy and have enjoyed. Money does indeed buy happiness, though it is not, of course, the only or most important determinant of life satisfaction.

A New Comprehensive Suite of Mobility Estimates

This installment of the primer presents a fuller set of similarly measured American mobility estimates than has ever been assembled before. The original estimates highlighted in this installment come from the Panel Study of Income Dynamics (PSID), the longest-running survey in the world following a nationally representative population.[9] The PSID began in 1968. It has followed the original participants since then, and it has also followed children as they have left home to start their own households. The most recent year of data available is for 2013, which collected data on 2012 incomes. See Appendix 1 for more methodological detail.

The complexity of measuring economic mobility is a source of great uncertainty in our understanding of opportunity. Mobility estimates depend on the ages at which incomes are measured. If the income of grown children is measured at a relatively young age, then mobility will be higher than it would if measured at an older age (assuming that parental income is measured at an older age). That is because at, say, age 25, many less-skilled children of less-skilled parents have accumulated years of work experience or are working jobs that will pay little more over time, while many higher-skilled children of higher-skilled parents are just out of school or pursuing postgraduate studies. Inequality between the children of lesser- and higher-skilled parents will look smaller than it will in middle age.

Another issue is that in any one year, parent or child incomes may be sensitive to either measurement error or idiosyncratic circumstances. They may be noisy measures of income, in that income in the previous or next year might look different simply due to these chance distortions.

What we really have in mind when we think about economic mobility is *lifetime* income. That is, ideally, we would add up or average the income parents and children earned across their entire careers and assess mobility on that basis. Another way of thinking about this issue is that intergenerational mobility estimates, when proxied by income measured over only part of workers' lifetimes, are affected by intra-generational mobility. And because single-year measures of income are noisy, intergenerational mobility estimates are affected by income volatility.

Researchers typically address these problems in two ways. First, they average multiple years of income, which amplifies the steadier portion of annual incomes while canceling out the chance elements, thereby better approximating lifetime income. Second, they measure income around age 40, the age (roughly) at which research has determined that annual income best proxies lifetime income.[10]

Alternatively, particularly in analyses that compare siblings, some researchers attempt to model the "permanent" (steadier) and "transitory" (idiosyncratic) components of income. Through equations, they specify how related this year's new chance "error" component is to last year's, how persistent last year's chance error is in affecting measured income over subsequent years, and how permanent income evolves over time. Then they estimate the permanent income levels and conduct mobility analyses using those. This strategy obviously depends on how well the specified model actually reflects the way incomes evolve.

My approach to producing original mobility estimates is related to and inspired by that taken in a recent paper by Bhashkar Mazumder (2015).[11] I start with parents and children who reported income in the PSID when they were between the ages of 38 and 42. Taking the income reported at the age closest to 40, I then work forward and backward over their lives, averaging income received between the ages of 25 and 55. For technical reasons explained in Appendix 1, I only use every other year of income, which means that a maximum of 15 years of income are averaged over a period of up to 31 years.[12]

I do this for every person in the PSID between the survey years of 1968 and 2013. People turn 40 in different survey years, but roughly half of the parents in these analyses were 40 by the mid-1970s (born no later than the mid-1930s), and roughly two-thirds were by 1980 (born no later than 1940). About half of children were 40 by 2003 (born no later than 1963), and two-thirds were by 2007 (born no later than 1967). There are parents in the analyses (with 40-year-old children in the survey) who turned 40 as early as 1966 and as late as 1998, the latter being teenagers when their child was born. Children turned 40 as early as 1987 and as late as 2013.

The analyses conducted include numerous robustness checks to assess the importance of missing data and of including or excluding years when people report they received no income. The result is a kind of reference suite of estimates for understanding how much intergenerational income mobility we have in America today. The next sections describe the ways that researchers typically measure relative and absolute economic mobility and present the new estimates.

2. Distributional Measures of Economic Mobility

Rather than starting with the most commonly used mobility measures, which summarize the association between parent and child incomes in a single number, it makes sense to begin with more disaggregated measures.[13] A single-number summary provides a convenient overall picture of mobility, but at a cost. Namely, it is often of interest whether mobility is high or low for children depending on the level of their parental income, and we often care specifically about upward or downward mobility. In other words, we care about the *distribution* of mobility—how much there is from some income levels to other income levels. Table 1 summarizes the results of the new mobility analyses in this section.

Table 1. Summary of Key Distributional Measures of Economic Mobility

Measure	Men's Earnings	Women's Earnings	Family Income
Relative Mobility			
Of those with parents in the bottom fifth, % in bottom fifth as adults	44	30	46
Of those with parents in the top fifth, % in top fifth as adults	50	33	41
Of those with parents in the middle fifth, % below the middle fifth as adults	37	37	34
Of those with parents in the middle fifth, % above the middle fifth as adults	31	42	43
Absolute Mobility			
% with real income higher than their parents' at the same age	60	76	73
Sibling Similarity			
Of those in the bottom fifth with a same-sex sibling, % of siblings in the bottom fifth	35	34	43
Of those in the top fifth with a same-sex sibling, % of siblings in the top fifth	48	32	40

Earnings estimates compare grown children to their same-sex parent or sibling. Family incomes are adjusted for family size. Incomes are adjusted for inflation, which matters only in the absolute mobility analyses.

Relative Mobility—The Transition Matrix

Of course, the distribution of mobility is far too complex to understand without some simplification. Traditionally, researchers examining relative mobility divide parents and children into groups based on their incomes. Then they estimate the share of children from each parental income group who end up in each child income group. The results are displayed in a "transition matrix"—a table with parent income groups arrayed across the columns, child income groups across the rows, and the table cells showing the percentage of children within a parental income group who end up in each child income group.

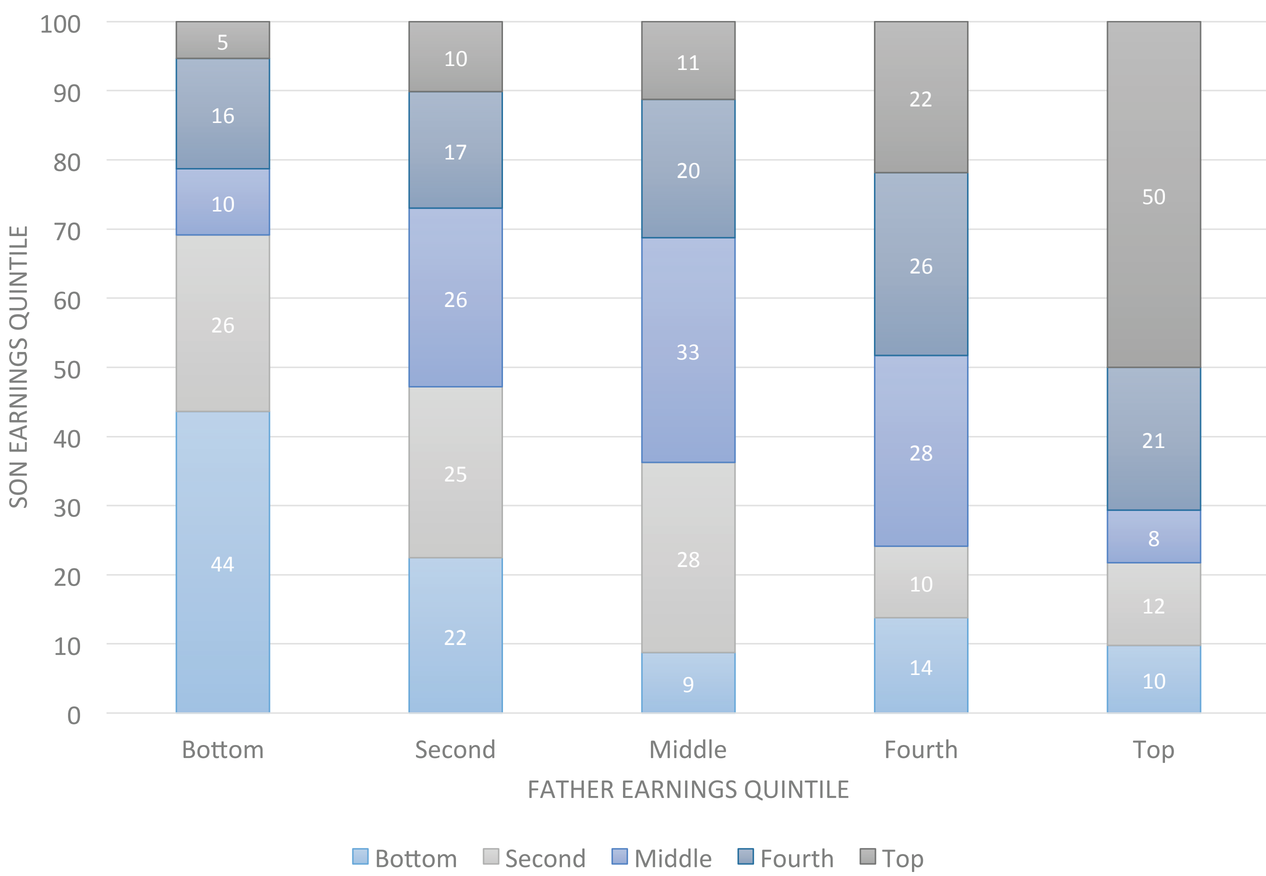
While parents and children can be organized into groups in any number of ways, typically, they are allocated into the same number of groups in both generations, equally sized within each generation. Then the transition matrix indicates, for instance, the share of children raised in the bottom fifth of parental income who make it to the top fifth of income as adults, or the share of children starting in the middle fifth who remain in the middle fifth in adulthood. Because such transition matrices are based on rank ordering parents and children before grouping them, they show how relative mobility is distributed across people who start out in different relative positions.

Transition matrices most commonly show mobility across income fifths, or "quintiles," but matrices based on quartiles are also popular. Quartiles and quintiles keep the level of detail manageable without losing the information obscured in, say, a three-by-three or two-by-two matrix. The amount of data available also limits how large transition matrices can be. Even with data from fairly large surveys, some transitions—from bottom to top, for instance—may be rare enough that few survey respondents end up in some cells. That renders the estimates imprecise. A different survey with different people drawn from the same geography might produce a very different number of people in thinly populated cells of the matrix. The larger the sample, the less of a problem rare transitions are. Chetty et al. (2014), for instance, accessed a massive number of tax returns and were able to produce a 100 by 100 transition matrix.

Figure 1 displays the first original PSID mobility estimates in this primer. It presents a quintile-based transition matrix graphically. Each bar represents a different quintile of male earnings, with the leftmost bar representing the poorest fifth of fathers and the rightmost bar the richest fifth. Within each bar, the segments show the percentage of children raised in a given fifth of father earnings who ended up in each fifth of grown-son's earnings. The percentages displayed within each bar add to 100.

The label in the lower-left corner of Figure 1 reveals that 40 percent of sons raised in the bottom fifth of father earnings (centered on age 40) remained in the bottom fifth of male earnings (centered on age 40) in adulthood. Note that the bottom fifth of sons' earnings was better-off on average in absolute terms than the bottom fifth of father earnings; the rank ordering is conducted within each generation.

Figure 1. Percent of Grown Sons in Each Fifth of Male Earnings by Each Fifth of Father Earnings



Source: Author's analysis of the Panel Study of Income Dynamics (PSID). The sample includes the 442 father–son pairs where fathers had at least 8 years of non-missing earnings (out of a maximum of 15) and sons had at least 9 years. See Appendix 1 for methodological details.

The sample used in Figure 1 requires fathers to have at least 8 years of income out of a possible 15 years, and it requires grown sons to have at least 9 years. There are 442 father–son pairs in the sample. It was selected from over 200 samples examined in Section 3, below, because it produced the lowest estimate of relative mobility among the samples that included at least 400 father–son pairs. Figure 1 indicates that many boys who grow up with low-earning fathers do not transcend those origins. In a world where family background did not matter, 20 percent of sons starting in the bottom fifth would end up in the bottom fifth in adulthood (because they would have an equal chance of ending up in any of the five quintiles). The share stuck at the bottom in Figure 1 is more than double that.

Some observers might look at that 44 percent figure and interpret it positively—56 percent of the sons of the lowest-earning fathers escaped the bottom fifth as adults, after all. But they did not move far. Seven in ten ended up in the bottom two-fifths of male earnings. If we define the middle class as being rich enough to be in the middle fifth of earnings or the top two fifths, then only three in ten sons raised by the lowest-earning fathers had middle-class earnings in adulthood. Only one in 20 made it to the top fifth. No middle-class parent would accept such long odds for their children.

There is a similar "stickiness" at the top of the paternal earnings distribution. As shown in the upper-right corner of Figure 1, 50 percent of sons starting out in the top fifth remained there as adults, and seven in ten ended up in the top two quintiles. Only one in ten of these sons ended up in the bottom fifth as adults. Whatever the cause, there appears to be something of a "glass floor" supporting many upper-income children, to use the evocative phrase of Brookings Institution scholar Richard Reeves.[14]

Between the top and bottom, there is a notable amount of churn—of upward and downward mobility between childhood and adulthood. If family background were unimportant, all of the segments in all of the bars in Figure 1 would be labeled "20". For sons raised in the middle three quintiles of father earnings, the distribution of adulthood earnings comes closer to approximating this scenario than for sons raised in the top or bottom fifth. A pattern of similarity between fathers and sons is apparent, however.[15]

While mobility analyses comparing fathers and sons dominate the research literature, one problem with these studies is that they exclude sons whose fathers were not living with them growing up. In my analyses, if a biological father is not present in the data between the ages of 38 and 42, then his son will be excluded. One way of checking to see whether this exclusion affects the estimates is to see how they change when we replace biological father earnings with those of the biological mother's male partner (whether husband or cohabiter) when a father is absent and a partner is present. Doing so produces a transition matrix indicating a bit more upward mobility from the bottom fifth, and perhaps slightly more downward mobility from the top. However, men's earnings might be expected to be less closely tied to their father-substitute's earnings than to their biological father's earnings.

Figure 2 provides a transition matrix comparing mothers' and daughters' earnings. Once again, I use the sample that produced the lowest estimates of relative mobility in Section 3 below. Women appear to have significantly more earnings mobility than men do. About one-third of daughters with mothers in the bottom or top fifth of maternal earnings ended up in the same place. While only 31 percent of sons with the lowest-earning fathers made it to the middle class, 44 percent of daughters with the lowest-earning mothers did.

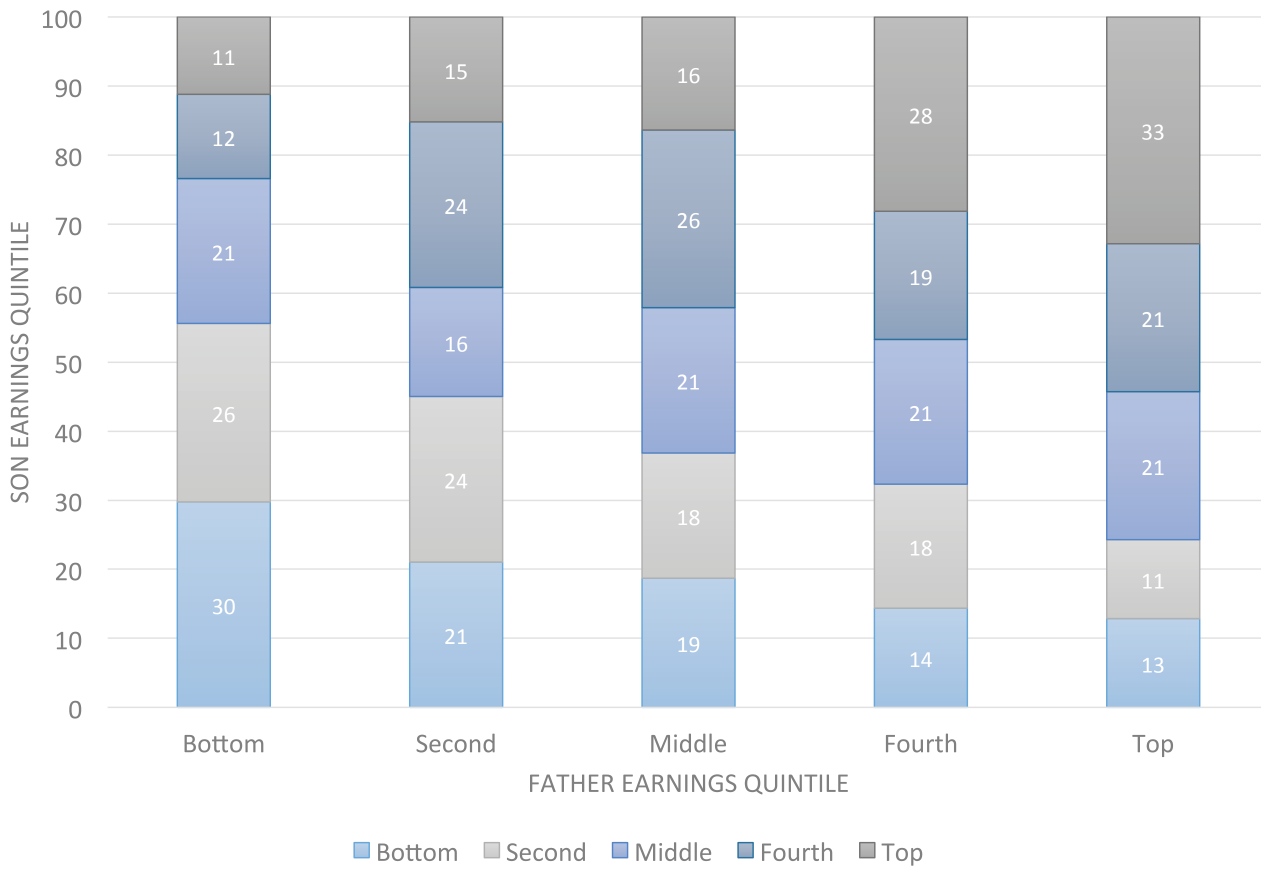
These estimates include years without earnings in the averaging of permanent earnings (though they exclude pairs in which the mother or daughter had no earnings at any age). The estimates, however, are similar if years without earnings are excluded from averaging. They are also similar if daughter earnings are compared with father earnings.

Estimating a transition matrix using family income allows for a larger sample (and, thus, more reliable results) by pooling men and women, whereas doing so for earnings analyses would produce difficult-to-interpret estimates. Men and women often work in very different occupations, and many women take time off from work to raise children.[16] This was especially true in earlier generations from which parent earnings are drawn in the PSID.

Figure 3 reveals that 46 percent of children raised in the bottom fifth of permanent parental income remained in the bottom fifth of permanent income in adulthood. Three in four ended up in the bottom two-fifths of family income, meaning that only one in four poor children made it to the middle class in adulthood. Only one in 33 made it to the top fifth.

Figure 3 also shows that 41 percent of children starting out in the top fifth remained there as adults, and two-thirds ended up in the top two quintiles. Barely any of these children ended up in the bottom fifth as adults. Within the middle three quintiles, mobility is once again more common than at the ends of the parental income distribution.[17]

Figure 2. Percent of Grown Daughters in Each Fifth of Female Earnings by Each Fifth of Mother Earnings

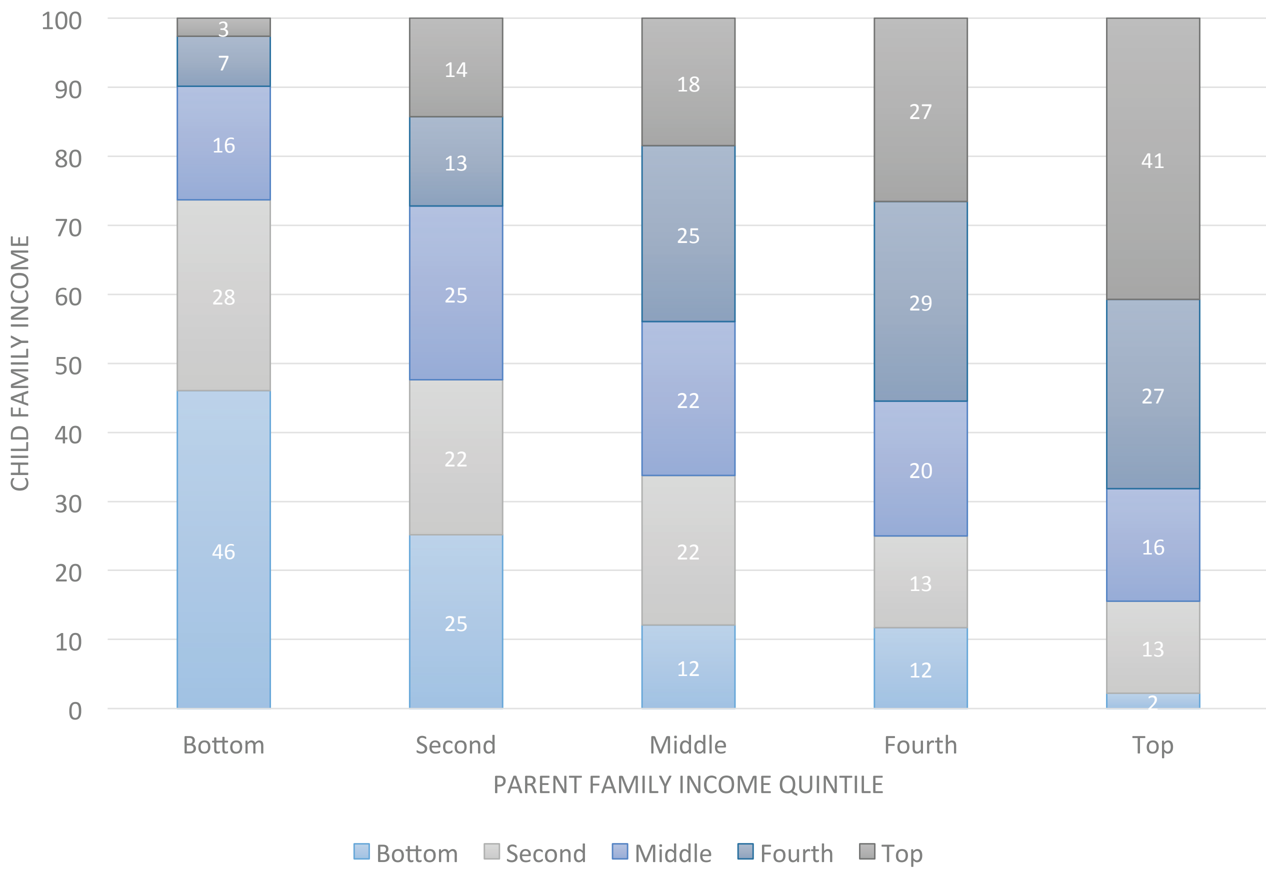


Source: Author's analysis of the Panel Study of Income Dynamics (PSID). The sample includes the 854 mother–daughter pairs where mothers had at least 5 years of non-missing earnings (out of a maximum of 15) and daughters had at least 7 years. See Appendix 1 for methodological details.

As discussed above, averaging more years of income tends to lower mobility estimates, so it is likely that Figures 1 through 3 actually overstate mobility. Ideally, they would be based on samples in which all parents and children have 15 years of income data within a 31-year window (or 31 years of data, or more). However, there are no such parent–child pairs available. The more years of income we require, the smaller the sample gets, and the less reliable the estimates in thinly-populated cells.

In Appendix 2, I review the previous literature on economic mobility levels. Several of the studies with transition matrices based them on quintiles. Three studies—one using the PSID, the others using administrative data—found that between 29 and 32 percent of men with fathers in the bottom fifth of earnings ended up in the bottom themselves,

Figure 3. Percent of Grown Children in Each Fifth of Family Income by Each Fifth of Parental Income



Source: Author's analysis of the Panel Study of Income Dynamics (PSID). The sample is restricted to the 719 parent–child pairs where parents had at least 10 years of non-missing income and children had at least 9 years. Incomes are adjusted for family size. See Appendix 1 for methodological details.

and the estimates of stickiness at the top ranged from 38 to 43 percent.[18] Fertig (2003), using the PSID, found higher levels of stickiness—52 percent at the bottom and 46 percent at the top. My estimates indicate less mobility than these studies (except for Fertig's estimate of upward mobility from the bottom).

Fertig is the only researcher of whom I am aware who estimated a mother–daughter earnings transition matrix, but her results show implausibly high mobility. Dahl and DeLeire (2008) estimate a father-daughter transition matrix using administrative data, finding results very similar to mine whether I compare daughters to fathers or mothers.

My family income transition matrix estimates are comparable to those estimated in earlier PSID studies, particularly if Eberharter (2014) is excluded (which found more upward mobility from the bottom and less from the top).[19] They indicate less mobility than past research using Internal Revenue Service data or either of two other surveys. One of those surveys, dating to the mid-1960s, is known to produce relatively high estimates of mobility and the other study appears to have been superseded by the author's subsequent analyses.[20] The IRS data excludes non-taxable transfer payments and so may not be comparable to the survey-based estimates.[21] Relatedly, Eberharter uses a post-tax measure of family income, which may account for the higher upward mobility she finds. These are subjects ripe for additional research.

The fact that my family income estimates are comparable to past ones using the PSID and pre-tax income is surprising given that mine average more years of income than past studies. One possibility is that quintile-based transition matrices do not provide enough detail and obscure differential mobility patterns *within* income fifths (perhaps at the very top or bottom). However, it is also likely that my transition matrices understate how much movement occurs between quintiles. For one, research suggests that the greater likelihood of poor children with poor parents to drop out of the PSID makes transition matrix estimates of upward mobility look too high.[22] Research also suggests that transition matrices overstate mobility to the extent that child earnings have classical measurement error and to the extent that parent and child earnings have correlated measurement errors.[23] In addition, with a bigger survey, I could examine samples with more years of income averaged, which would be expected to produce lower mobility estimates.

To sum up, it is likely that no one has yet estimated a transition matrix that fully depicts the degree of relative immobility in the US. The summary measures in Section 3 will hint at the extent to which we have understated this immobility.[24]

Relative Mobility—Other Measures

With adequate data, one can estimate transition matrices based on deciles or even centiles, which may uncover the sorts of dynamics that quartiles and quintiles might miss. However, as the number of cells in a transition matrix grows, the more complex it is to analyze.

One alternative would be to estimate the percentage of children whose income rank in adulthood exceeds their parents' income rank when they were growing up. Because someone must move down in ranks for every person who moves up, this percentage will generally be close to 50 in sufficiently large samples, making it uninformative as an aggregate indicator of relative mobility. However, it may be useful at times to consider the percentage of people in different parts of the parental income distribution who exceed their parental rank or who exceed it by some threshold (and likewise for downward mobility). Alternatively, groups may be compared—such as blacks and whites—using quintiles or quartiles based on the combined groups.

Bhashkar Mazumder, of the Federal Reserve Bank of Chicago, has pursued this approach with colleagues over several papers. He has found, for instance, that black–white differences in upward relative mobility look worse using transition matrices than when the probabilities of moving up by a given number of percentiles is used.[25] (To understand percentiles, imagine ordering parents or children from poorest to richest. A person's percentile is the share of people with income lower than or equal to her own. Someone at the 30^{th} percentile is richer than or as rich as 30 percent of the people in her generation. It is a way of ranking people where the ranks range, for technical reasons, from just above 0 to just below 100.)

In another paper, Mazumder and his coauthors show that while 68 percent of men whose fathers were in the bottom fifth of earnings make it out of the bottom fifth as adults, 85 percent attain a rank that is higher than their father's was.[26] The two estimates differ because some sons move up in ranks without crossing into the second quintile.

Another measure of relative mobility is the average increase in ranks for a given part of the parental income distribution or for some population subgroup. This has been examined directly only by Davis and Mazumder (in progress) and in a paper by Bratberg et al. (2017) on which Mazumder and Davis are coauthors. These papers show the average increase at each percentile of parental income. However, statistical models have been used to estimate an equivalent indicator—the average child rank at each percentile of parental income.

These models usually impose a linear relationship between parental and child ranks. The slope of this "regression line," when multiplied by a given parental income percentile and then added to the intercept of the line, provides an estimate of the average percentile in adulthood among children whose parents were at a particular percentile. Typically, researchers are interested not so much in this estimated average, but in the slope specifically, which may be interpreted as a summary measure of inequality reduction. We will return to this interpretation in Section 3. Mazumder's research also uses *nonlinear* curves to relate parental and child ranks across the distribution of parental ranks.[27]

Absolute Mobility—Surpassing Parental Income

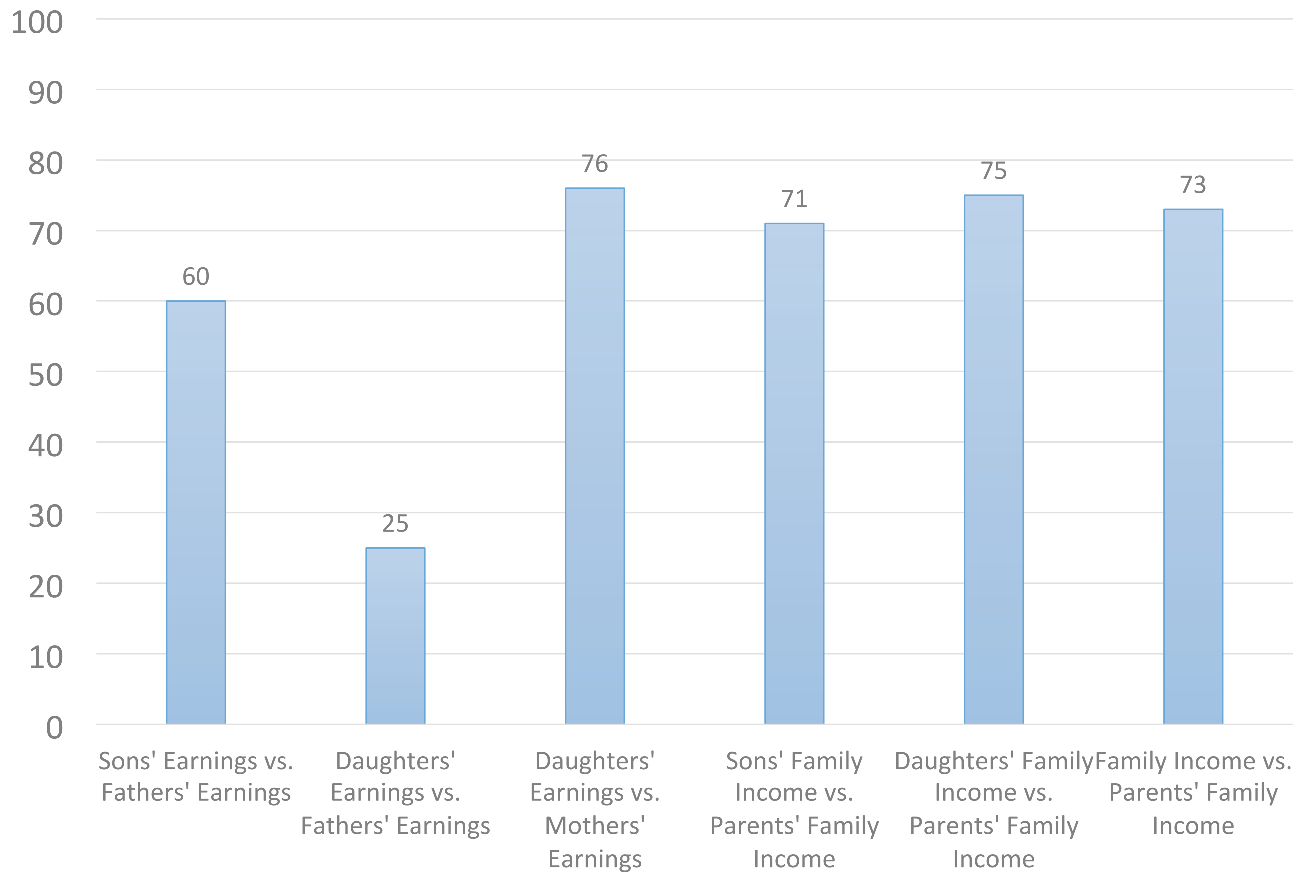
For all of these relative mobility measures, one can imagine analogues that focus on absolute mobility. For instance, rather than assessing how many people exceed their parents' rank, the analogous measure of absolute mobility would consider how many exceed their parents' inflation-adjusted income. This indicator, in fact, has come to define what researchers mean by "absolute mobility."

Figure 4 displays the share of adults in the PSID who, around age 40, exceed their parents' earnings or income around the same age.[28] The bar on the left side of the chart indicates that 60 percent of men exceed their father's earnings. In contrast, only one in four women exceed paternal earnings. When compared against their mother's earnings, however, women do much better—three in four exceed their maternal earnings.[29] The greater upward absolute mobility of women when compared with their same-sex parent is unsurprising. Labor force participation among women has increased and occupational segregation has declined over recent generations.

Toward the right side of Figure 4, absolute mobility results when looking at family income are similar for men and women, separately or pooled. Around 75 percent of grown children exceed their parents' family income. Family income combines the earnings of children and their spouses or partners, so we might expect absolute mobility rates to be similar for sons and daughters. At the same time, given the increase in single motherhood over recent generations, it is something of a surprise to see daughters doing as well as sons, though it may be that many single-mother daughters also were raised by single mothers. Many unmarried mothers also live with a male partner, and the income of these partners is included in family income in the PSID.

Previous research indicates that upward absolute mobility is more common among adults whose parents had low earnings or income. It is natural that adults with poor parents are more likely to surpass them; it will tend to be easier to exceed, say, $10,000 in earnings than $200,000. In my own analyses, this pattern recurred, but because the sample sizes for each quintile of parental earnings or income were so small, I do not show the results.

Figure 4. Percent of Grown Children Surpassing the Income of Parents



Source: Author's analysis of the Panel Study of Income Dynamics (PSID). The sample begins with all parent–child pairs with income measured at either age 38, 39, 40, 41, or 42, and that single year of income is used (starting with age 40 and moving outward if unavailable). It then is restricted to pairs in which the parent turned 40 after 1974 and the child before 2006. Up to seven years of income are then averaged, using every other year, within a 13-year window. Family incomes are size-adjusted and all earnings and income measures are adjusted for inflation. Sample sizes are 129 for sons, 175 for daughters, and 308 for pooled family income. See Appendix 1 for methodological details.

Compared with past research using the PSID, I find the same rate of upward absolute earnings mobility for sons as the Pew Charitable Trusts did in a 2012 report (60 percent versus 59 percent).[30] I find very similar rates of upward absolute mobility in terms of family income compared with the four previous PSID studies.[31] The exception is that my family income estimates are lower than in the 2012 Pew report. For instance, Pew reports that 84 percent of grown children are better off than their parents, while I estimate it at just 73 percent. It is possible this relates to the fact that I incorporate more years of grown-child income from the Great Recession and its immediate aftermath.

Since unemployment was relatively high during these years, the incomes of grown children were relatively low, pushing absolute mobility downward. Finally, I find more absolute mobility in terms of family income than Davis and Mazumder (2016) report for daughters. This difference is partly due to their using an inflation adjustment that overstates the rise in the cost of living.

The Chetty et al. Study

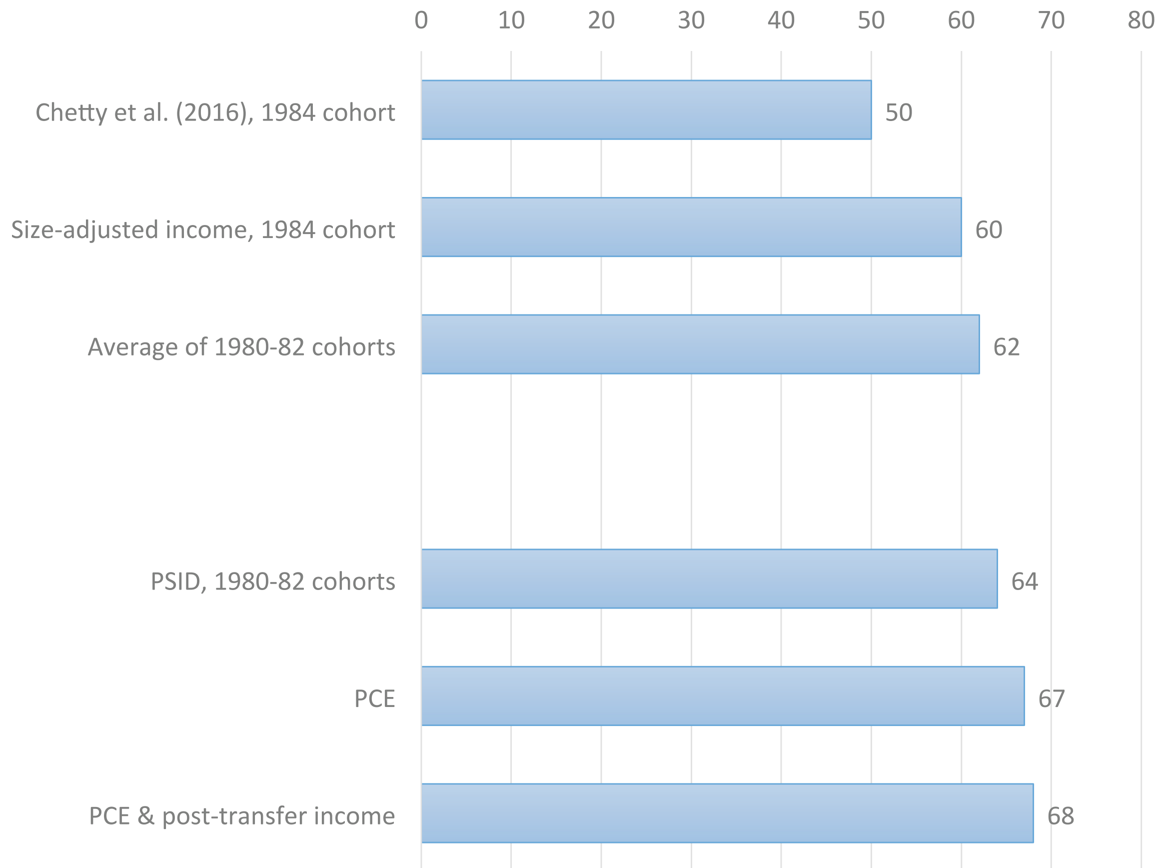
The most important study on absolute mobility does not rely on PSID data. Stanford economist Raj Chetty and his colleagues recently released a working paper showing that absolute mobility rates in the U.S. have fallen over the past 75 years (Chetty et al., 2016). Future installments of this primer will address the trend evidence presented in that paper; here, the focus is on the estimated contemporary level of absolute mobility. As widely reported, the paper found that in 2014, just 50 percent of 30-year-olds born in 1984 exceeded their parental family income measured at the same age. What accounts for the higher estimates in the PSID?

Two important differences turn out largely to explain the discrepancy. First, the Chetty team's 50 percent absolute mobility rate is for the 1984 birth cohort. In my PSID sample, the cohorts were born no later than 1966, and one grown child in the sample was born as early as 1952. Second, the headline Chetty results do not adjust incomes for family size. With smaller contemporary families, the same amount of money goes further than in the past, so absolute mobility is understated if incomes are not adjusted for the smaller number of mouths to feed.

Three other differences also matter. My analyses use a superior cost-of-living adjustment to account for inflation—the "PCE deflator" rather than the "CPI-U-RS" that Chetty and his colleagues use.[32] Furthermore, the PSID family income measure includes income from cash transfers, including safety net benefits like those from Temporary Assistance for Needy Families and social insurance benefits such as unemployment compensation. Finally, I center incomes around age 40 rather than measuring them near age 30.

In Figure 5, I provide a reconciliation between the Chetty and PSID results. After adjusting incomes for family size, Chetty and his team report that the absolute mobility rate for the 1984 birth cohort is 60 percent rather than 50 percent (shown in the second

Figure 5. Percent of Grown Children Surpassing the Income of Parents, Chetty vs PSID



Source: Author's analysis of the Panel Study of Income Dynamics (PSID) and Chetty et al. (2016), Online Data Tables 1 and 4. See the text and Appendix 1 for methodological details. All income measures are adjusted for inflation, using the indicated price deflator (CPI-U-RS or PCE) or the CPI-U-RS if not otherwise indicated.

bar in Figure 5).[33] To assess the importance of the other differences, I re-ran my PSID analyses to produce estimates more comparable to those of Chetty and his team. Specifically, I used single-year measures of pre-transfer family income, taken at age 30 (or 28, 29, 31, or 32), and I adjusted incomes for inflation in the same way that they did.[34] To ensure a sufficient sample size in my data I pool children born from 1980 to 1982. They turned 30 between 2010 and 2012, the last year for which income data is available in the PSID.

As displayed by the third bar of Figure 5, the average absolute mobility rate across the 1970–1982 birth cohorts (using size-adjusted income) is 62 percent in the data Chetty and his team have published online. That compares with a rate of 64 percent in my sample (fourth bar). Measured consistently and for the same birth cohorts, the PSID and the Chetty estimates are very similar.

The remaining bars incorporate other modifications into my estimates. The fifth bar uses the best available cost-of-living adjustment for incomes and raises the absolute mobility rate to 67 percent. The sixth bar switches to post-transfer income, which makes little difference, raising the rate to 68 percent.

This 68 percent is lower than it otherwise would be because of the Great Recession. The upward mobility of 30-year-olds between 2005 and 2007 was 77 percent. Interestingly, in the PSID, 40-year-olds have less absolute mobility than 30-year-olds do. Those who were 40 between 2010 and 2012 had an upward mobility rate of just 63 percent. That was lower than the rate of 30-year-olds between 2010 and 2012 (68 percent) as well as the rate of the same birth cohorts when they were 30 ten years earlier (75 percent). The same was true in 2005–07: 40-year-olds had less absolute mobility compared with 30-year-olds in those years and compared with their 30-year-old selves ten years earlier. This is a topic that merits further exploration in future research.

Comparing the estimates in Figure 4 to those in Figure 5 reveals that the former overstate contemporary absolute mobility rates by including earlier birth cohorts that experienced higher mobility than recent ones.[35] Unfortunately, I cannot produce comparable estimates for, say, the 1984 birth cohort in Figure 4. This is the cost of centering income on age 40, which precludes the 1984 birth cohort from being analyzed.

At the same time, the headline Chetty results understate absolute mobility. They are based on incomes that are not adjusted for the fact that families are smaller today, that are adjusted for inflation in such a way that real income growth over time is understated, and that ignore government benefits. The analyses in this section suggest that improving the income measures in Chetty paper would show that roughly two-thirds of today's adults are better off than their parents were at the same age. Prior to the Great Recession, around three in four 30-year-olds were better off.

Even this estimate is likely to understate the absolute mobility experienced by today's 30-year-olds. These figures do not include employer-provided health insurance or other benefits as income, and they exclude noncash government transfers as income, such as Medicaid, food stamps, and housing assistance. Nor do they deduct taxes from or add refundable tax credits to income. Fringe benefits have become a larger share of employee compensation, noncash government transfers have grown more rapidly than cash transfers, tax rates have declined, and refundable tax credits have expanded.[36] Further, it is likely that even the PCE deflator used in these analyses overstates inflation.[37] Finally, these estimates miss Americans whose parents lived in another country at age 30, who are surely more likely than average to experience upward absolute mobility.

It is also worth noting that while absolute mobility is important, it is not the only metric by which to judge the strength of the American dream. As noted, research has found that the children of rich parents are less likely to experience absolute mobility than those of poor parents. Should we conclude that it is better to be poor? Of course not. Similarly, as we will see in a future installment of this primer, absolute mobility has declined over time. Should we conclude it was better to be a child of the Great Depression? We should not. The American dream is alive and well.

Absolute Mobility—Other Measures

The other relative mobility measures described earlier in this section also have absolute mobility analogues. For instance, one could create a transition matrix that sorts parental and child incomes into groups of $10,000 ($0–9,999; $10,000–19,999; etc.) rather than into quintiles or quartiles. Another variant might create groups based on the ratio of family income to the poverty line. That would allow for the determination of how likely poor children are to escape poverty themselves (with poverty defined in terms of absolute income thresholds). Bjorklund and Jantti (1997) create groups based on multiples of median earnings and conclude that 40 percent of sons below half the median as children end up below half the median as adults. The same percentage of those with at least 1.5 times the median in childhood end up above that threshold themselves. Acs, Elliott, and Kalish (2016) report that 35 percent of adults who were poor as children are poor as 30-year-olds.[38]

Alternatively, we might be interested in the average increase in inflation-adjusted income for children whose parents had a given income, or the average percentage increase. This can be estimated directly for different percentiles of parental income or for, say, children who started out in the bottom fifth. (See the end of this section.)

Or it can be modeled by imposing a linear or curvilinear relationship between parent and child incomes. As with relative mobility, this is typically done by assuming a linear relationship (though for technical reasons, between the *natural log* of parent and child incomes).[39] And as with relative mobility, the slope that is estimated is typically of more interest to researchers than the predicted average income (the subject of Section 3).

Sibling Similarity

Instead of comparing parent incomes to those of children in a transition matrix, there is no reason one cannot compare siblings' incomes in the same way. As far as I know, this kind of analysis has never been done before. Sibling similarity in income only indirectly reflects income mobility because it does not actually involve assessing individual changes in income between childhood and adulthood.

However, if we are interested in the extent to which family background inequalities persist into adulthood, then income is an incomplete indicator in two senses. Income captures neither everything that is important about family background nor everything that is important about child outcomes. The multidimensionality of child outcomes is an issue that is beyond the scope of this primer, but sibling comparisons provide a way to examine the importance of childhood inequalities that incorporates more than parental income and the factors associated with it.

The similarity of siblings' incomes indicates the extent to which all of the influences they have shared in common translate into similar adult incomes. Siblings share parental income in common, but they share many experiences and influences beyond parental income. Siblings share half of their genes (and they each share half their genes with each parent).[40] They share neighborhoods and schools. They share extended families and have overlapping peer groups.

To the extent that these shared influences are reflected in parental income, the relative and absolute mobility measures discussed above will incorporate them. But much of what siblings share in common and that affects their incomes is not likely to be reflected in parental income.

Of course, siblings do not share everything in common. Unless they are twins, parental income will tend to differ at least slightly between siblings over their respective childhoods. Half their genes differ, and residential moves mean that they can experience different neighborhoods and schools over at least part of their childhoods. Peer groups often overlap little. Sisters and brothers have vastly different experiences based on gender socialization and biological sex differences. Nevertheless, measures of similarity between sibling incomes capture a wide range of family background influences that affect income in adulthood.

Figures 6 through 8 present sibling transition matrices for brother earnings, sister earnings, and sibling family incomes. In all of these analyses, adults are linked each year to the same-sex sibling who is closest in age to them.[41] The samples used in these analyses were those that produced the strongest sibling associations in the analyses in Section 3.[42] Of importance, note that men and women who do not have siblings are excluded from these analyses, so that, for instance, the bottom quintile represents the poorest fifth of men or women *among those with a same-sex sibling*. Statements below about the percent of men or women with a brother or sister in some quintile should be interpreted as the share of men or women whose same-sex sibling is in some quintile *among those who have a same-sex sibling*. The charts confirm the general impression of limited mobility that the parent–child transition matrices conveyed.

In Figure 6, if a 40-year-old man is in the bottom fifth of male earnings, there is a 35 percent chance his brother is too. Nearly two-thirds of the time (63 percent), his brother is in the bottom two-fifths, meaning that a poor man's brother has only little more than a one-in-three chance of making it to the middle class (defined as the top three quintiles). An even stronger picture of "stickiness" at the top is apparent in Figure 6. Among men in the top fifth of male earnings, nearly half their brothers are also in the top fifth. It is rare for a rich brother to have a poor brother.

There is also a fair amount of immobility in the middle of the distribution of male earnings. One-third of men in the second-poorest fifth have brothers in the second-poorest fifth, and over half have brothers in the bottom two fifths. One-third of men in the middle quintile have a brother in the middle quintile. And over half of men in the second-richest fifth have brothers in the top two fifths.

In contrast, sisters appear not to be as similar to each other in terms of earnings as brothers are. Figure 7 displays many values close to 20, indicating substantial mobility. If shared influences had no effect on earnings, all of the labels in Figure 7 would be 20. There is clearly a tendency for poor women to have poor sisters and for well-off women to have well-off sisters, but family background seems to affect women less than it does men.[43]

Figure 8 pools men and women and compares them to their brothers and sisters, respectively, in terms of their size-adjusted family incomes.[44] There is considerable mobility in the middle of the income distribution but stickiness at the ends. Two-thirds of the time, a poor man or woman will have a brother or sister in the bottom two fifths of family income, and the odds are just as high that a well-off man or woman will have a brother or sister in the top two fifths.

Figure 8 indicates that in terms of family income rank, siblings resemble each other about as closely as or less than parents and children did in Figure 3. That suggests that after taking account of the things siblings share that are related to their parental income rank—whether investment, genes, or values—the rest of what they share is also substantively important in affecting income rank. If parental income rank (or its correlates) was the only thing shared between siblings that affected child income ranks, then some imperfect association between the income ranks of parents and one child, combined with an imperfect association between the income ranks of parents and a second child would produce a weaker association between the income ranks of the two children. In that case, the labels in Figure 8 would tend to be closer to 20 than those in Figure 3 (if the same families were in both samples).

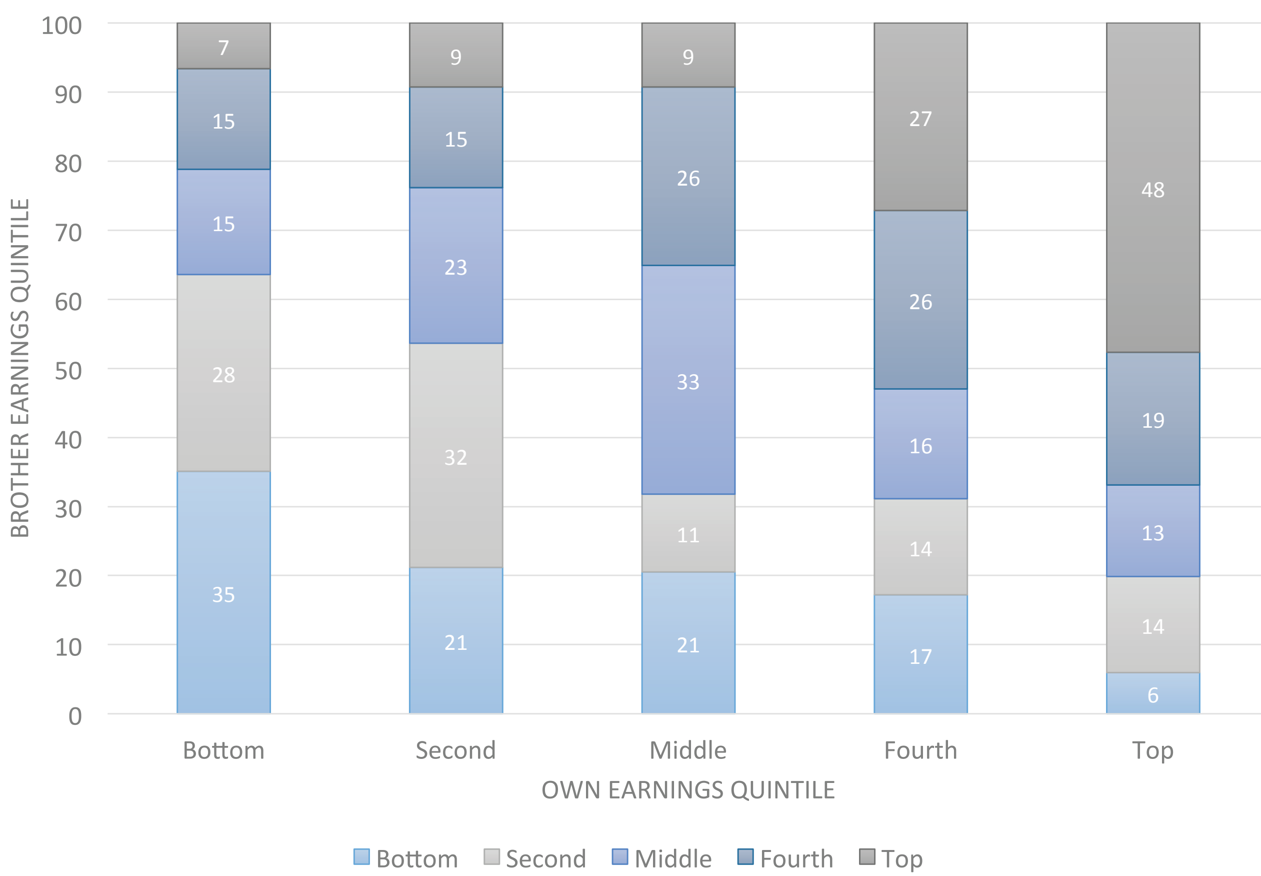
Since Figures 3 and 8 closely resemble each other, there must be other shared influences that strengthen sibling similarity. Comparing Figures 6 and 1 or Figures 7 and 2 leads to the same conclusion.

In this section, distributional measures of mobility were covered. The bottom line is that to an extent that ought to concern us, low-income families tend to have low-income children, and well-off families tend to have well-off children. However, it should not go unrecognized that perhaps three-fourths of the time, children are materially better off than their parents were, and that is true of the vast majority of children who grow up poor. Indeed, among those in the bottom fifth of parental income in Figure 3, even those who remained in the bottom fifth of family income as adults had size-adjusted income 27 percent higher than that of their parents, at the median. Some adults in the bottom fifth had an income high enough that they would have been deep within the second fifth of income in the previous generation, and some in the second fifth as adults would have been deep within the middle fifth of parental income.

Across all adults, median family income (size-adjusted) rose by 28 percent across the two generations (by over $11,000 using non-size-adjusted income). Similarly, the median child experienced an increase in family income (size-adjusted) of 28 percent between childhood and adulthood.

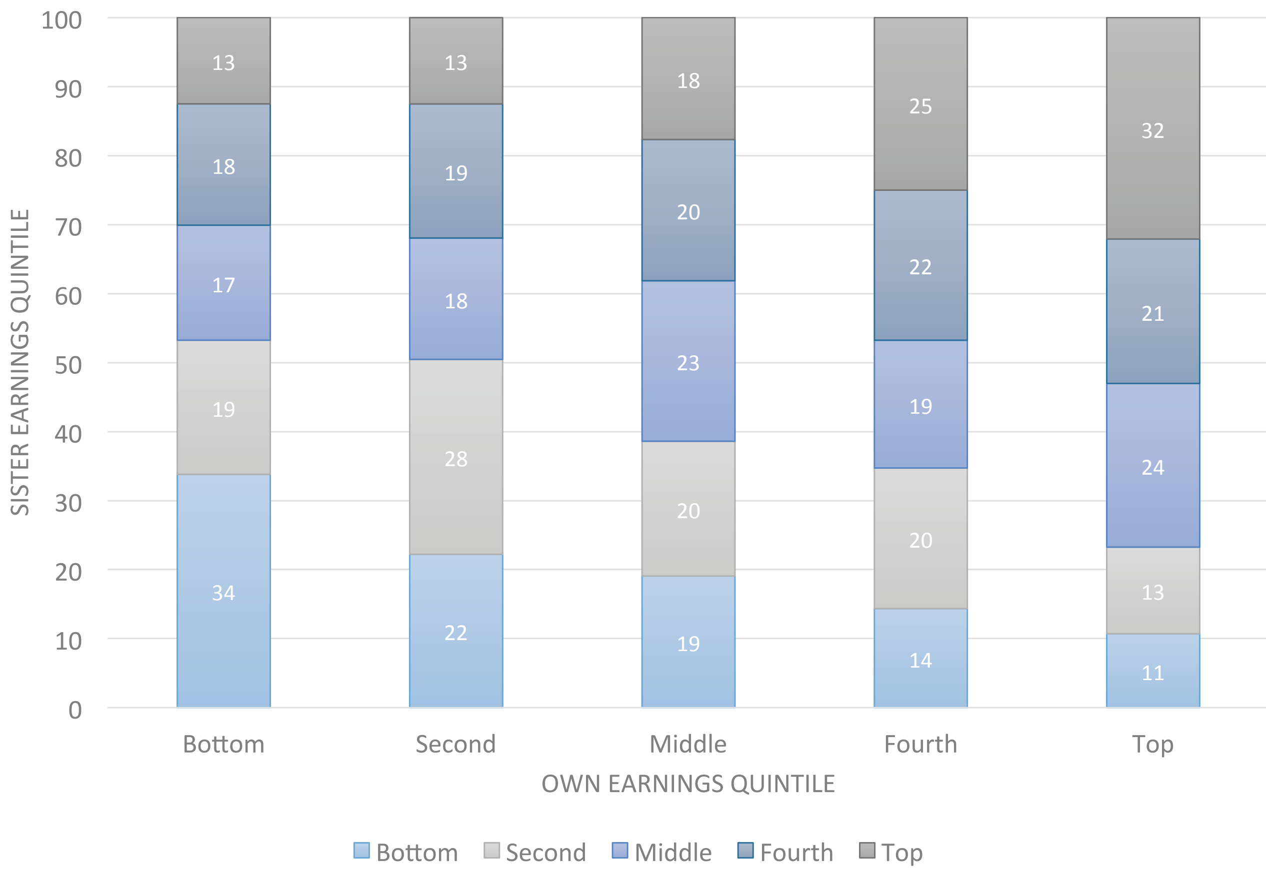
Just as absolute mobility is not everything, neither is relative mobility. But if we care about the ideal of equal opportunity, it is a better indicator than absolute mobility. The next section discusses summary measures that indicate the extent to which childhood income inequalities persist into adulthood. As we will see, not all of these measures primarily reflect relative mobility, and some are better than others at indicating the extent to which opportunities are or are not equal.

Figure 6. Percent of Brothers in Each Fifth of Male Earnings by Each Fifth of Own Earnings



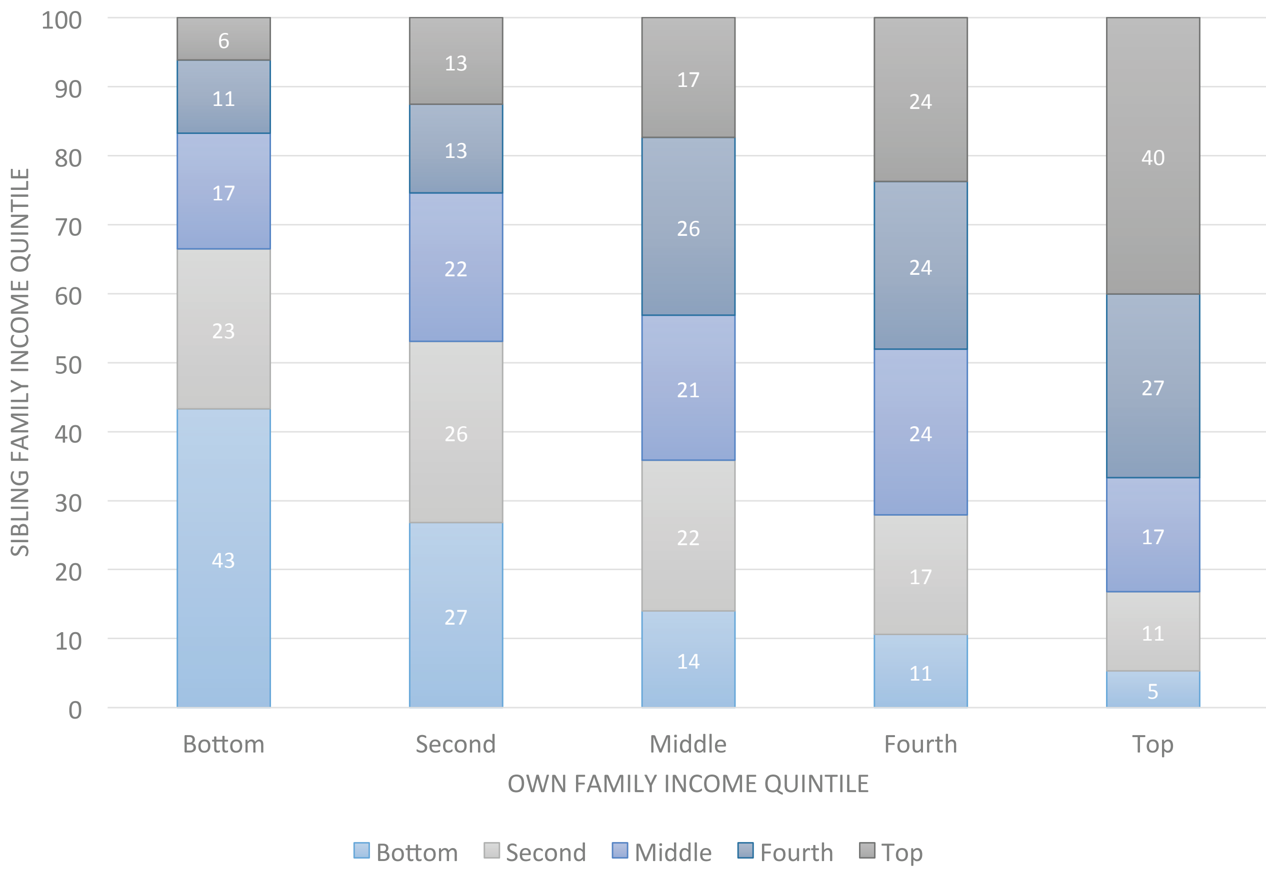
Source: Author's analysis of the Panel Study of Income Dynamics (PSID). The sample is restricted to the 755 brother pairs where each had at least 9 years of non-missing earnings. Quintiles are estimated using only brothers. See Appendix 1 for methodological details.

Figure 7. Percent of Sisters in Each Fifth of Female Earnings by Each Fifth of Own Earnings



Source: Author's analysis of the Panel Study of Income Dynamics (PSID). The sample is restricted to the 1,078 sister pairs where a woman had at least 4 years of non-missing earnings and her sister had at least 5. Quintiles are estimated using only sisters. See Appendix 1 for methodological details.

Figure 8. Percent of Siblings in Each Fifth of Family Income by Each Fifth of Own Family Income



Source: Author's analysis of the Panel Study of Income Dynamics (PSID). The sample is restricted to the 1,788 sibling pairs where an adult had at least 4 years of non-missing income and the sibling had at least 9 years. Brothers and sisters are never compared to each other. Incomes are adjusted for family size. Quintiles are estimated using only adults with same-sex siblings. See Appendix 1 for methodological details.

3. Summary Measures of the Persistence of Childhood Economic Inequality

The most commonly used summary measures of mobility tell us the extent to which movement between generations, in the aggregate, reduces adult income inequality between rich and poor children. In their common usage, these persistence measures take income as an indicator of revealed opportunity and then assess the extent to which a generation of parents with unequal opportunity have children with unequal opportunity. High values indicate more persistence of incomes and of opportunities and less reduction of childhood inequality.

These summary measures can describe the extent to which relative childhood gaps are reduced by relative mobility or the extent to which absolute childhood gaps are reduced by absolute mobility. Many analysts characterize an indicator of high persistence as evidence of "low mobility," but this convention is imprecise. In the case of relative mobility, where someone must move down for someone else to move up, inequality-reducing mobility (low persistence) is synonymous with "high" mobility. But absolute mobility can be "high" without reducing childhood inequalities. If economic growth raises everyone's income by 20 percent but childhood gaps between rich and poor children are 20 percent larger in adulthood, then childhood inequality will be persistent even as upward absolute mobility is substantial for rich and poor alike.

Table 2 summarizes the key results from the new estimates presented in this section.

Persistence of Relative Economic Inequality—The Intergenerational Rank Association

There has been pervasive confusion—even among experts—about what the most widely used "mobility" measure actually summarizes. While many researchers have believed mistakenly they or others were looking at relative mobility—a misunderstanding to be discussed below—researchers have only recently begun to emphasize actual summary measures of persistence that reflect relative mobility. The most common of these measures are the "rank-rank slope" and the "Spearman rank correlation," which are actually the same.[45] For ease of exposition and to help conventionalize the term, I will follow Dahl and DeLeire (2008) and use a third name—the "intergenerational rank association," or "IRA"—in what follows.

The easiest way to interpret intergenerational rank associations, which can range between -1 and 1, is as the number of percentiles by which the richest and poorest children will tend to be separated in adulthood. An IRA of 0.4 indicates that the richer of two children will tend to have an income placing her 40 percentiles higher than the poorer child. Because the two children started out 100 percentiles apart, 40 percent of the gap between them persists. This is another way to interpret the IRA—as the share of the percentile gap between two children that will tend to persist into adulthood. If two children are 20 percentiles apart, they will typically be 8 percentiles apart as adults (20 multiplied by 0.4), leaving a percentile gap 40 percent as large as the initial gap.

An IRA of 1 means that there is no relative mobility and that childhood income gaps persist completely—everyone ends up occupying the same rank in adulthood as in childhood. An IRA of 0 means that adulthood ranks are completely unrelated to childhood ranks, and initial percentile gaps tend to disappear. An IRA of -1 indicates that children are just as unequal in adulthood as in childhood, but this time rich and poor children have switched positions. The poorest children end up the richest adults and vice versa.

Table 2. Summary of Key Measures of Persistence

Measure	Men's Earnings	Women's Earnings	Family Income
Persistence of Relative Inequality			
Intergenerational rank association (rank–rank)	.44–.52 (.51)	.31–.40 (.37)	.51–.53 (.53)
Persistence of Absolute Inequality			
Intergenerational elasticity	.44–.78 (.77)	.27–.54 (.40)	.59–.66 (.66)
Intergenerational correlation	.38–.51 (.48)	.35–.42 (.39)	.51–.53 (.53)
Sibling Similarity			
Sibling rank association	.38–.39 (.39)	.24–.32 (.31)	.36–.43 (.43)
Sibling correlation	.33–.45 (.39)	.22–.31 (.30)	.35–.45 (.45)

Estimates are preferred ranges and, in parentheses, preferred point estimates. See the text for selection criteria. Women's earnings compare women to their mothers or sisters. Family incomes are adjusted for family size. All earnings and incomes are adjusted for inflation.

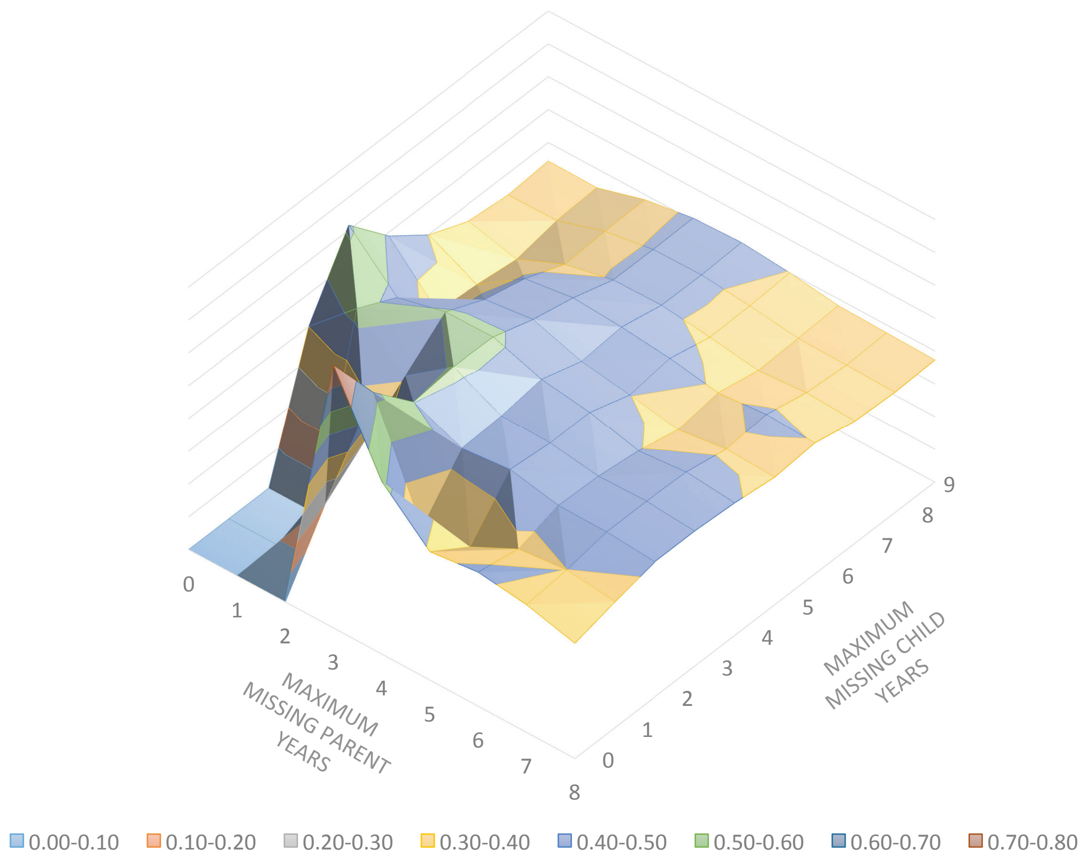
Using the PSID, more or less restrictive criteria for the number of years of income data that may be missing (out of a maximum of 15) produce a range of mobility estimates. Figure 9 displays the set of estimates produced for the father–son earnings IRA. The chart is intended to display the ambiguity in estimating mobility figures from imperfect data.[46] The *x* and *y* axes vary the number of years of parental and child income allowed to be missing, up to 8 years for parents and up to 9 years for children (out of as many as 15 in a 31-year span).[47] The vertical axis shows the IRA estimated using each of 67 samples.[48] Higher peaks indicate higher IRA estimates (less mobility and less inequality reduction). The legend indicates the range of estimates corresponding to different colors.

The most striking feature of the chart is the wide variation in mobility estimates depending on how restricted are the parent and child samples. On the "west" side of the chart are 5 missing samples with no parent–child pairs, but there is also a sample of 3 pairs that produces an IRA of 0.00 and a sample of 8 pairs with an IRA of -0.01 (which I have shown as zero for sake of presentation). Other samples along the southwest and northwest edges are similarly small, and taken together they produce a volatile set of IRA estimates, including one of 0.80 based on 4 parent–child pairs. These estimates obviously cannot be taken seriously.

Also notable is a clear tendency for IRA estimates to be higher on the west side of the chart than on the east side: higher moving from southeast to northwest or from northeast to southwest. The implication is that more complete income data results in higher IRA estimates. With ideal data, the IRA presumably would peak at the western point of the diamond, where parents and children both have 15 years of income available. Unfortunately, without ideal data, the most restrictive samples (with the most complete income measures) produce unreliable IRA estimates.

The analyses in this primer seek to offer a defensible range of estimates for various mobility measures that can guide policymaking and future research. After examining many charts like Figure 9 for many mobility measures, I settled on a range bounded by the highest association among the samples with at least 200 parent–child pairs and the highest association among the samples with 50 to 199 parent–child pairs. I also offer a single preferred estimate, averaging the two highest IRA estimates among samples with at least 50 parent–child pairs.

Figure 9. Changes in the Intergenerational Rank Association (IRA) for Male Earnings as the Number of Years of Missing Earnings is Allowed to Vary



Source: Author's analysis of the Panel Study of Income Dynamics (PSID). Each parent or child may have up to 15 years of income within a span of up to 31 years. See Appendix 1 for methodological details.

In practice, these decisions tend to highlight associations similar to those near the mouth of the volcano in Figure 9. I conclude, for instance, that the father–son earnings IRA is between 0.44 and 0.52, with a preferred estimate of 0.51. The five stable estimates around the mouth of the volcano range from 0.50 to 0.53. The extent to which this approach blends art and science should not be overlooked, but the researcher attempting to find the single true IRA is doomed to failure. The best that can be done with existing data is to give a sense of the "ballpark" that contains the estimate that best reflects social reality. The reader should maintain this sense of ambiguity throughout the length of the primer and in reading other mobility research.

An important general conclusion from the analyses in this section is that the amount of mobility—and the extent to which intergenerational mobility reduces childhood inequalities—has been overstated. There have been signs for over a decade that this has been the case, but the state of knowledge has spread slowly, and the recent research of Raj Chetty and his colleagues—vitally important as it is—has confused matters. More on this issue below.

If anything, the estimates in this primer overstate mobility too. With better—bigger—data, the estimates in Figure 9 would presumably continue to rise until reaching the western point of the chart. Indeed, two estimates bookending the five stable ones around the mouth of the volcano are 0.61 and 0.62, though they are based on samples of 15 and 16 parent–child pairs, respectively.

Furthermore, because of the way the restricted age of parents at childbirth in the western-most samples interacts with a feature of my analyses, even the plausible-looking IRAs are understated. The more restrictive of my samples, in terms of how many years of income parents and children must have, are made up of pairs in which parents were relatively young when their children were born. At the extreme, the sample with 15 years of income for both parent and child would be comprised solely of parents who were no older than 17 years old upon the birth of their child.[49] Ranking parents and children *within this sample* will produce higher IRA estimates than would be the case if the sample also included parents who were in their thirties or forties when their children were born. The reason is that parents' age at birth is associated with child outcomes.

Since young parents are poorer, one might be concerned that because of the association between parental age at birth and child outcomes, the samples on the western frontier of Figure 9 might be relatively poor. If the IRA was steeper for adults with poorer parents, then the high IRAs in the chart might simply reflect the changing composition of the samples as they become more restricted. However, the IRA has been shown to be more reasonably linear across the distribution of parental income.[50] In addition, across the set of moderately to highly restricted samples, there was little relationship between the degree of sample restriction and the magnitude of the IRA, on the one hand, and the median of male earnings on the other.

For technical reasons discussed in Appendix 1, my analyses only use every other year of income available within a window of up to 31 years. As a check on whether this decision prevents me from obtaining more reliable estimates, Figure 10 shows the same surface chart as Figure 9 but this time averaging all possible years within the 31-year range.[51] The west side of the chart is largely comprised of missing samples; few parent–child pairs have more than 20 years of parent and child income.[52] The volcano is still visible, though this time the adjacent peaks soar above it. Most importantly, though, other than these peaks, the IRA values are similar to those in Figure 9. Using my approach to identify the preferred estimates, I get a range of 0.43 to 0.53 and a point estimate of 0.53. The lip of the volcano is again consistent with these figures, varying around 0.50. Only about two dozen samples produced estimates of 0.60 or higher, and they were all based on fewer than 35 parent–child pairs.

Returning to Figure 9 after this reassuring check, the preferred range of 0.44 to 0.52 exceeds the estimates from three previous studies estimating the father–son IRA, including two that used administrative data.[53] A reasonable conclusion from past research was that the IRA in the United States is 0.4, but the preferred estimate here of 0.51 suggests that these studies overestimated mobility.

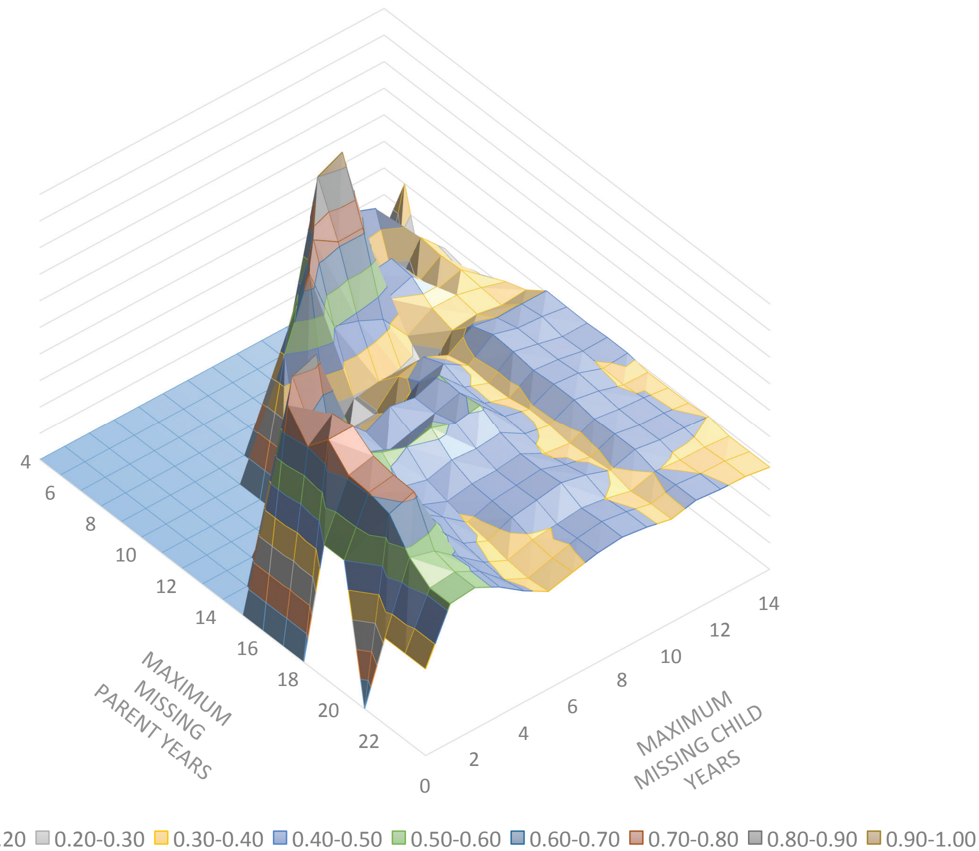
While mobility analyses comparing fathers and sons dominate the research literature, one problem with these studies is that they exclude sons whose fathers were not living with them. In my analyses, if a biological father is not present in the data between the ages of 38 and 42, then his son will be excluded.

One way of checking this is to see how the IRAs change when we replace biological father earnings with those of the biological mother's male partner (whether husband or cohabiter) when a father is absent and a partner is present. The IRA range when mothers' partners' earnings are used is 0.49 to 0.61, and the preferred estimate is 0.59. These are higher by 0.07 to 0.09 than the estimates for biological fathers and sons.

If the male partners of biological mothers are decent proxies for biological fathers, then it may be that the exclusion of fatherless sons from earnings mobility analyses overstates mobility somewhat. However, it would be inappropriate to draw a strong conclusion given the uncertainty in the estimates and in assuming these men are adequate stand-ins for fathers. In addition, fatherless sons whose mothers do not have male partners are still excluded from these analyses.

One way to include many of those sons too is to use mothers' (non-zero) earnings when there is an absent father and no male maternal partner. Doing so increases the range of IRA estimates to 0.47–0.65 and the preferred estimate to 0.63.

Figure 10. Changes in the Intergenerational Rank Association (IRA) for Male Earnings as the Number of Years of Missing Earnings is Allowed to Vary (Using Up to 31 Years of Income)



Source: Author's analysis of the Panel Study of Income Dynamics (PSID). See Appendix 1 for methodological details.

These different sets of estimates look more similar when zeroes are excluded from the earnings averages of men. Arguably, years in which men report no earnings should be discarded from the analyses. Most men who are out of the labor force are disabled, retired, or tell surveyors they do not want a job. Some of the latter are in school or taking care of home or family. Some have under-the-table earnings that they do not mention to surveyors. At the same time, excluding years without earnings for those with poor employment prospects who do want to work (or who would be without earnings even if they did want to work) makes less sense.

The range for the father–son earnings IRA when zeroes are excluded from averages is 0.48 to 0.55, and the preferred estimate is 0.53. When mothers' partners or mothers themselves are compared to sons in the absence of biological fathers, the range is 0.48 to 0.58 with a preferred estimate of 0.57.[54]

Turning to daughters, we find lower IRAs, indicating a looser connection between parental earnings and daughters' earnings. This is unsurprising. Given occupational segregation by sex, the relationship between father and daughter earnings is likely to be weaker than that for father and son earnings. And given the expansion of opportunities for women in the decades since the mid-twentieth century, we might expect that the mother–daughter IRA is lower than the father–son IRA.

The father-daughter earnings IRA range is from 0.22 to 0.26, with a preferred estimate of 0.26.[55] This is somewhat higher than in Dahl and DeLeire (2008), who use administrative data and find an IRA no higher than 0.23. When years without earnings are excluded, however, my range runs from 0.26 to 0.39, and the preferred estimate is 0.37. Given that women are more likely to spend time out of the workforce than men to take care of home and family, there is a strong case for emphasizing these figures.

That is even truer comparing mothers' and daughters' earnings. When zeroes are included, the range of IRA estimates goes from 0.31 to 0.40, and the preferred estimate is 0.37.[56] The estimates are a little higher after discarding years of no earnings: 0.36 to 0.42 and a preferred estimate of 0.39. Once zeroes are excluded from "permanent" earnings averages, daughters resemble their fathers about as much as they resemble their mothers.

Several studies have examined the IRA when parent and child family incomes are compared. These measures are often preferred to parent–child earnings estimates. On the one hand, parental family income may be a better indicator of the resources families have to promote child opportunity. On the other hand, part of the advantage or disadvantage that family background confers on children is a greater or lesser ability to attract a spouse (and a high-earning one). As a practical matter, survey respondents are also less likely to report no family income for an entire year than they are to report no earnings.

I find that comparing sons' parental family income to their own earnings produces IRAs ranging from 0.43 to 0.47, with a preferred estimate of 0.46.[57] Those figures are very similar if parental incomes are first adjusted for the number of family members. For daughters, the range of IRAs runs from 0.29 to 0.34 if years of zero earnings or zero family income are included, with a preferred estimate of 0.33.[58] But if zeroes are discarded, the range is from 0.35 to 0.44 (0.41 preferred).

These estimates, too, are higher than those that have been estimated in the past, which have generally been no higher than 0.40. In particular, they are notably higher than the relatively low estimates of Chetty et al. (2014) and Landerso and Heckman (2016). The Landerso and Heckman paper—as yet unpublished—claims that the United States has mobility rates similar to those of Denmark when measured consistently. However, the American mobility rates they estimate are fairly high versus the rest of the literature and those of Denmark fairly low. Mazumder (2015) has convincingly demonstrated that while the research of the Chetty team has been invaluable, their low IRAs are due to a number of features of the IRS data used in the analyses and methodological choices he and his coauthors made.[59]

My final IRA analyses compare child family income to that of parents. The IRAs for sons range from 0.51 to 0.60 (0.47 to 0.56 after size-adjusting incomes), with a preferred estimate of 0.58 (0.56). For daughters, the IRAs are somewhat higher. The range runs from 0.56 to 0.65 (0.65 preferred), or with size-adjustment from 0.54 to 0.62 (0.60 preferred). Pooling sons and daughters, the preferred estimate is 0.54 and the high and low bounds of the range also converge on 0.54. Adjusting for family size, the range is 0.51 to 0.53 and the preferred estimate is 0.53.[60]

These family income IRA estimates are also higher (mobility lower) than what has been estimated in the past. Chetty et al. (2014) report estimates ranging from 0.32 to 0.35, but as noted, these have been shown to be biased downward. Mazumder (2015) presents a wide range of IRA estimates for sons, ranging from 0.28 to 0.64, but nearly all of his estimates based on reliably large samples are no higher than 0.45 (with a range running from 0.40 to 0.45 or 0.50 being a reasonable conclusion from his results). Mazumder himself downplays the extent to which his IRA results differ from earlier studies.

In short, the evidence presented here suggests that relative mobility reduces earnings percentile gaps between poor and rich sons by 35 to 55 percent between generations (1.0 minus 0.45 or 0.65). Earnings percentile gaps between daughters are reduced by 55 to 70 percent, and family income gaps between children are reduced by 45 to 50 percent. On the one hand, these estimates indicate considerably less relative mobility and less reduction of childhood inequality than past research. On the other hand, they suggest a considerable amount of mobility in that a large share of childhood inequality is eliminated by adulthood.

However, as we have seen, transition matrices reveal that most of this mobility takes place in the middle of the parental income distribution. That is, the IRA overstates the extent to which percentile gaps between rich and poor children are reduced in adulthood, or even gaps between rich and poor children, on the one hand, and middle-class children on the other. This is a distinct disadvantage of using summary measures in mobility analyses: they potentially obscure dynamics of interest among subsets of children.[61]

Persistence of Absolute Economic Inequality—The Intergenerational Elasticity

The most widely used measure of absolute mobility—or of intergenerational income mobility generally—is the "intergenerational elasticity," or IGE. For at least a decade, however, since the absolute-relative distinction has been convention, analysts have tended to call the IGE a summary of *relative* mobility.[62] Even the recent popularity of the IRA inspired by the research of Chetty and his team has failed to clarify the issue. (And unfortunately, the Chetty team has furthered the confusion by using the term "absolute mobility" inconsistently.)[63]

The IGE is an absolute mobility analogue to the IRA, which is clearer if we think of both as being "regression slopes"—straight lines that best characterize the cloud of parent and child incomes when they are plotted on two axes of a graph. Both the IRA and the IGE come from statistical models that relate parent and child incomes and produce estimates describing how inequality between children is reduced in adulthood by mobility. The IRA summarizes all movement between childhood and adulthood *ranks*, indicating the extent to which percentile gaps—relative gaps—are reduced by the overall pattern of relative mobility. It tells us that children who start out *s* ranks apart will tend to end up *e* ranks apart, on average.

The corresponding summary measure relating to absolute mobility would summarize all movement between childhood and adulthood *dollar amounts*, telling us that children whose incomes start out *s* dollars apart will tend to end up *e* dollars apart, on average. However, the size of absolute movements will tend to depend on where a child starts out. A drop of $1,000 from a parental income of $100,000 is small relative to an increase of $1,000 from a parental income of $10,000. In contrast, a drop of 10 percent from $100,000 is more comparable to a rise of 10 percent from $10,000.

Therefore, to be meaningful, the summary measure of absolute mobility should express individual child movements and the reduction of absolute gaps in percentage terms. This is the essence of what the IGE does.

Specifically, the IGE tells us, roughly, that children whose incomes start out *s* percent apart will tend to end up *e* percent apart, on average.[64] Equivalently, it indicates that the ratio of adulthood incomes between children who grew up rich and poor tends to be lower by a factor of *f* compared with the ratio of their childhood incomes. And that is equivalent to saying that the ratio of adulthood-to-childhood income for the richer child tends to be lower by a factor of *f* compared with the ratio for the poorer child.[65] The IGE indicates, in other words, that absolute mobility (as a percent change from childhood income) tends to be less upward or more downward for the richer child than for the poorer child. As a result of this individual absolute mobility, inequality between rich and poor children tends to be lower in adulthood than it was in childhood.

Why have analysts insisted on calling the IGE a relative measure of mobility? It would appear that the expression of individual mobility in terms of percent changes has convinced many that it summarizes relative movement in contrast to the dollar-amount changes that would summarize absolute movement. But expressing mobility in terms of percentages scales movement not based on the relative positions (ranks) from which people start, but based on the absolute size of their childhood incomes.

To get concrete about the measure's interpretation, an IGE of 0.4 tells us, roughly, that a 10 percent difference in childhood income tends to shrink to a 4 percent difference in adulthood. This kind of simple approximation holds only for small initial income differences. Occasionally, some analyst suggests that an IGE of 0.4 indicates that a 100 percent difference in childhood incomes tends to shrink to a 40 percent difference, but the actual formula is more complicated.66 With an IGE of 0.4, a 100 percent initial difference would tend to fall to a 32 percent difference.

Analysts sometimes imply that the IGE, like the IRA, ranges between -1 and 1, but in theory it could exceed these bounds. An IGE greater than 1 would mean that initial inequality between rich and poor children is magnified—that rich children experience better absolute mobility (in percentage terms) than poor children. An IGE exceeding -1 would indicate that there is more inequality between rich and poor children in adulthood than in childhood, but the roles are reversed, and the poorest children have become the richest adults. In practice, IGEs are almost always above 0 and below 1, and when they are not, it usually indicates some sort of data or analytic problem.

New Estimates of Intergenerational Elasticities

As was the case for IRAs above, the analyses here include a range of estimates and a preferred one for each income type and group considered. The range for the father–son earnings IGE is much wider than was the case for the IRAs—from 0.44 to 0.78. The upper bound is from a sample of 61 father–son pairs. Averaging it with an IGE from a second sample above 0.7 leads to the preferred estimate of 0.77. Excluding zeroes from earnings averages produces even higher IGEs. The range is 0.62 to 0.96 and the preferred estimate is 0.93.[67]

While clearly imprecise, these preferred estimates are much higher than nearly all previous credible IGE estimates. In fact, out of 190 unique samples, there are 12 estimates between 0.50 and 0.59, 5 between 0.60 and 0.69, 3 between 0.70 and 0.79, none between 0.80 and 0.89, 3 between 0.90 and 0.99, and 3 above 1.00. Different ways of identifying "preferred estimates" yield different values, of course, but they generally remain high. Averaging the five largest IGEs, for instance, rather than the two largest produces estimates of 0.67 when zeroes are included in earnings averages and 0.76 when they are not.

If fatherless sons are added to the samples by comparing their earnings to those of maternal partners or to the non-zero earnings of mothers, the range I estimate runs from 0.60 all the way to 1.05.[68] When this latter IGE, from a 51-pair sample, is averaged with an IGE of 0.89 (from a 52-pair sample, and not one that simply adds to the 51-pair sample), the preferred estimate is a remarkable 0.97.[69] Six of my estimates are between 0.70 and 0.79 and four are between 0.80 and 0.89. These ten samples have, at most, 121 "father" –son pairs. Five other samples, with no more than 51 pairs, have IGEs over 0.90. Again, there is clearly some imprecision in these estimates, but if I exclude zeroes from earnings averages, the preferred estimate is still 0.90. If I include zeroes but take the average of the largest five IGEs, the preferred estimate is still 0.83.

To assess the plausibility of the IGE being this high, Figure 11 displays the range of estimates produced from more or less restrictive samples of "fathers" and sons (using maternal partner or mother earnings when fathers are not present).[70] While the west side of the chart is noisy, the steady rise of IGE estimates as more and more years of earnings are averaged is more apparent than the rise of IRAs in Figure 9. The surface rises moving closer to the southwest and—especially—the northwest walls. Since IGE estimates are more sensitive than IRAs to measurement problems in parental income, the IGE increases more with more complete data on parents than it does with more years of child income.

Worth mentioning is the fact that the IGE, unlike the IRA, is not sensitive to the restricted age of parents at childbirth found in the westernmost samples. The IGE, however, more so than the IRA, tends to show a stronger association between parent and child incomes among children who grew up poor. (Indeed, that is what the transition matrices revealed in Section 2.) If the westernmost samples—with parents who were relatively young at the birth of their child—tend to be poorer than other samples, then the high IGEs might simply reflect the poverty of the parents. However, in my tests, across the set of moderately to highly restricted samples, there was little relationship between the degree of sample restriction and the magnitude of the IGE, on the one hand, and the median of male earnings on the other.

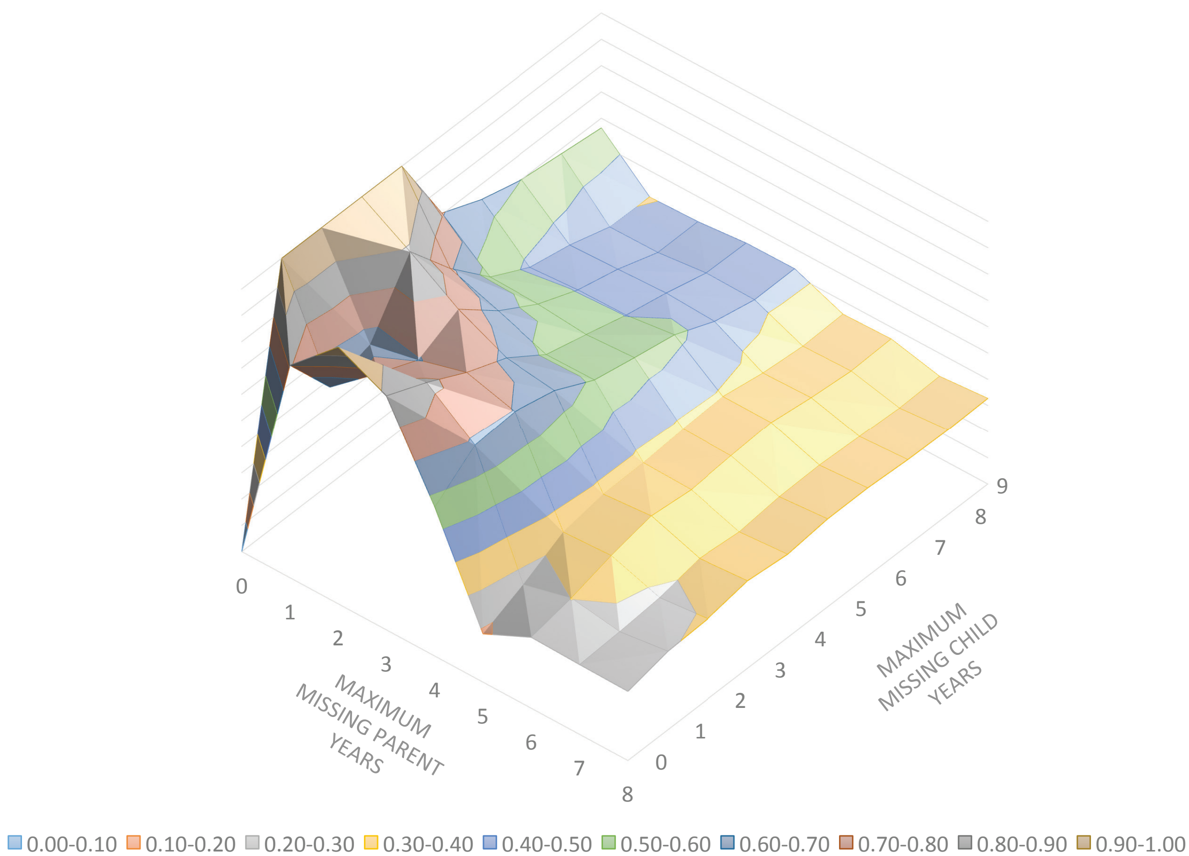
My IGE estimates for daughters are more earthbound. Comparing fathers and daughters, I find a range from 0.44 to 0.46, with a preferred estimate of 0.44.[71] Comparing mothers and daughters, the range is 0.27 to 0.44, and the preferred estimate is 0.40.[72] However, the linkage between mothers and daughters is stronger if years with no earnings are excluded: the range is 0.26 to 0.58 and the preferred estimate is 0.54. If I instead use as my preferred estimate the average of the five largest IGEs, I get estimates of 0.35 and 0.40 including and excluding years with no earnings.

When I compare parent family income to sons' earnings, the IGE ranges from 0.64 to 0.87, with a preferred estimate of 0.82. The estimates for daughters are 0.64 to 0.82 and 0.75.[73] The IGEs are lower but still substantial when years without income or earnings are excluded: 0.61 to 0.70 (0.69) for sons, and 0.44 to 0.52 (0.51) for daughters.[74]

Finally, the family income IGEs range from 0.60 to 0.84 (0.82 preferred) for sons, from 0.62 to 0.83 for daughters (0.76 preferred), and from 0.62 to 0.75 when they are pooled (0.72 preferred).[75] Excluding zeroes from income averages puts the pooled range between 0.63 and 0.83 and the preferred estimate at 0.77. Using size-adjusted incomes, the estimates are a bit lower: for sons, 0.58 to 0.82 (0.78), for daughters, 0.59 to 0.76 (0.72), and pooled, 0.59 to 0.66 (0.66). The preferred pooled estimates using the average of the five largest IGEs are 0.67 with no size-adjustment and 0.63 with the adjustment.

To summarize the results, these are remarkably high estimates. Some of them are unrealistically high. Many are similar to the upper-bound estimates found in some previous analyses, but well above those in most studies.[76] My results suggest that an IGE between 0.70 and 0.80 is a reasonable guess for sons' earnings, 0.35-0.55 is reasonable for daughter's earnings, and 0.65-0.75 for family income. The true IGEs, however, could be even larger. For one, research suggests that the greater likelihood of poor children with poor parents to drop out of the PSID biases the IGE downward.[77] In addition, in my data, the IGE tends to increase as the number of missing values parents and children may have falls. No one in my samples has complete income data, and the estimates become unstable and imprecise when parents and children are required to have no more than two or three missing years of income (out of 15). But with larger samples, the estimates would presumably rise further.

Figure 11. Changes in the Intergenerational Elasticity (IGE) for Male Earnings as the Number of Years of Missing Earnings is Allowed to Vary



Source: Author's analysis of the Panel Study of Income Dynamics (PSID). Each parent or child may have up to 15 years of income within a span of up to 31 years. See Appendix 1 for methodological details.

An IGE of 0.75 implies that a child with twice the income of another will still tend to have an income 68 percent higher than the poorer child in adulthood. Under the strong assumption that once we take parents into account, grandparents and great-grandparents do not matter for one's earnings, an IGE of 0.75 would indicate that the grandchild of the richer of two neighboring boys will have an income 34 percent higher than the grandchild of the poorer neighbor.[78] In contrast, an elasticity of 0.4—the consensus estimate for the IGE until recently—indicates that the grandchild from the richer family will have income less than 5 percent higher than the grandchild from the poorer family. Childhood inequalities apparently persist far into the future, despite the popular impression that the American Dream tends to quickly eliminate inequalities from one generation to the next.

Persistence of Absolute Economic Inequality—The Intergenerational Correlation

A related summary measure of the persistence of absolute inequality is the "intergenerational correlation," or IGC. To understand the difference between the IGC and IGE, remember that the IGE is a measure of the extent to which the absolute mobility experienced by individual poor children tends to be better than that experienced by individual rich children. The IGE depends not just on whether rich and poor children converge in their rankings (relative mobility). It also depends on how the dispersion of incomes changes between generations. That is, regardless of the extent to which people move up or down in terms of positions, if those positions become more spread out, that will push the IGE higher.[79]

The IGC effectively purges individual absolute mobility of the influence of changes in income dispersion before determining whether the pattern of individual absolute mobility tends to reduce inequalities between rich and poor children.[80] If the IGE is a summary measure of whether absolute mobility is diminishing childhood absolute income gaps, the IGC summarizes the absolute-gap reduction from that part of absolute mobility that is unrelated to changes in income dispersion. It summarizes the reduction in childhood gaps that would have occurred if income dispersion had not changed.[81] (Section 4 elaborates on how the difference between the IGE and IGC matters in practical terms.)

Another way to distinguish conceptually the IGC from the IGE is to recognize the distinction between *what* parent incomes predict about child incomes and *how well* parent incomes predict child incomes. The IGE can be high—indicating that rich parents tend to have rich children—without parent income providing good predictions of child income. After all, many things unrelated to parental income also affect child income. It may be that parental income (and the things that are correlated with it) is a relatively small factor affecting child income, in which case knowing parental income doesn't tell one very much about what a child's income will be. In particular, if the IGE rises over time simply because inequality is growing, parent income does not become any better a predictor of child income. Alternatively, the IGE can be low—indicating that childhood income gaps tend to narrow—even though parental income predicts the income of adult children very well.

The IGC indicates how well parent income predicts child income.[82] It ranges between -1 and 1; when the IGC is 1, then knowing parental income and knowing the IGE allows one to exactly predict child income.[83] The same is true when the IGC is -1. In that case, childhood inequalities are reversed in adulthood, with poor children doing better than rich children. When the IGC is 0 the IGE is also 0, and knowing parental income tells someone nothing about what child income is likely to be.

It is useful to interpret the IGC in terms of how the actual inequality in grown-child incomes compares to the inequality that can be predicted from the IGE and parental income. An IGC of 0.4 indicates that the inequality in predicted (logged) adult incomes is 40 percent of the inequality in actual (logged) adult incomes.[84]

Defining the preferred ranges and point estimates as above, I find a range of 0.38 to 0.51 for the male earnings IGC and a preferred estimate of 0.48.[85] If years with no earnings are excluded in averaging earnings, the range increases to between 0.45 and 0.57, with a preferred estimate of 0.55.[86] As with the IGE, the IGC estimates are lower for daughters, indicating that absolute mobility eliminates more of the childhood inequality between daughters than it does between sons. Excluding years without earnings from averages, the IGC comparing daughters to their fathers ranges from 0.29 to 0.32 (0.32 preferred), and comparing them to their mothers it ranges from 0.35 to 0.42 (0.39 preferred).[87]

The IGC comparing parental family income to sons' earnings ranges from 0.43 to 0.47 (0.45 preferred), and the corresponding estimates for daughters (after dropping years with no earnings) are 0.31 to 0.37 and 0.35. Finally, comparing parents' family incomes to those of their children, the range for sons is 0.45 to 0.55 (0.54 preferred), the range for daughters is 0.54 to 0.59 (0.59 preferred), and the range when sons and daughters are pooled is 0.51 to 0.52 (0.52).[88] Interestingly, parental family income appears to predict daughters' family incomes better than it does sons'.

These estimates are generally at least as large as those from previous analyses, and they are often larger. Only a handful of studies have looked at IGCs, however. (See Appendix 2.) The IGCs are generally lower than the IGEs shown above. This may reflect a real difference; if income inequality is rising, then the IGE will exceed the IGC. However, it is important to note that IGCs are more sensitive to measurement error than IGEs are, and thus they are more likely than IGEs to be biased downward.[89]

Persistence of Absolute Economic Inequality—Surname-Based Measures

In 2014, the economist Gregory Clark enjoyed a wave of publicity on the basis of his book, *The Son Also Rises: Surnames and the History of Social Mobility*. Clark used a novel approach to the measurement of economic persistence and found that absolute mobility in a variety of indicators reduces childhood inequality very little. Further, he argued that there was little variation over centuries or across countries in the extent to which success ran in families. Clark argued that indicators like income and educational attainment were noisy measures of an underlying "social competence" and that family inequalities in social competence were more persistent than inequalities in the various indicators. He concluded that family inequalities primarily reflected genetic contributions and that those inequalities were apparently impervious to policy efforts to reduce them.

Clark reached these conclusions by comparing children with surnames associated with high status in earlier generations to children with surnames associated with low status. Essentially, he looked at whether inequalities (in income, for instance) between children with "good" and "bad" surnames were diminished in adulthood as a result of absolute mobility patterns, estimating IGE- and IGC-like summary measures of social competence. Those estimates were consistently higher than those found in conventional mobility research, with IGEs typically running from 0.7 to 0.9.[90]

There are many reasons to be skeptical of Clark's conclusions.[91] Clark's interpretation of his results may be an example of the "ecological fallacy"—the mistaken inference that a group-level relationship (across surnames in this case) is the same as the individual-level relationship (across people). The fact that rich states tend to vote for Democrats while poor states tend to vote for Republicans does not mean that rich people tend to vote for Democrats and poor people for Republicans.[92] The relationship between income and party preference within states can differ from the relationship between states for any number of reasons.

In the same way, the relationship between parent and child social competence *within* "good" and "bad" surnames (and the relationship across all children) cannot necessarily be estimated from the relationship *across* good and bad surnames. Averaging income across children with the same surname doesn't simply filter out "noise," revealing the underlying social competence captured by income. It also filters out differences within same-surname children that are relevant to adult income. (And it actually doesn't even filter out the individual-level noise.)

Further, the approach results in a same-surname income average that reflects more than just the social competence of children with the surname. Children with the same surname tend to be of the same ethnic background, for instance. If the reason some surnames have been "bad" in the past (and continue to be today) is because they belong to children who experience systemic discrimination, then Clark's IGE will capture not individual-level but group-level persistence of inequality—a persistence driven by discrimination rather than genetically transmitted social competence.[93]

The point is not that Clark's IGE might be primarily capturing the effects of discrimination; it is that group-level persistence of inequality is a potentially bad indicator of individual-level persistence. And the problem is exacerbated when Clark specifically compares "good" surnames to "bad" surnames, as those surnames are likely to be imbued with historical experiences that cause them to be "good" or "bad." Crucially, the comparison of "good" and "bad" surnames also is likely to bias the IGE upward relative to conventionally estimated IGEs.

Torche and Corvalan (forthcoming) illustrate these issues elegantly by showing that the between-ethnic-group male family-income IGE in the National Longitudinal Survey of Youth (NLSY79) is 0.84 while the individual-level IGE is just 0.39. When they restrict the sample to children of English, German, French, Irish, or Italian backgrounds, these two IGEs are much closer (0.30 and 0.34), demonstrating that the high between-group IGE in the first sample reflects the inclusion of black and Hispanic children and the high inequality between them and children of European descent.

In addition, Vosters (in progress) and Nybom and Vosters (forthcoming) demonstrate that when multiple indicators of "social competence" are incorporated into a single measure, the IGE is not meaningfully larger than the IGE based on income alone, contrary to Clark's conjecture.

It would not be surprising if Clark's methods ultimately prove unsound. His most controversial and sweeping conclusions do not seem justified by his results. The statistical model underlying his approach hides a number of strong assumptions, and it would be remarkable if the low cross-individual IGEs that are produced in high-quality Scandinavian data mask a "true" IGE for social competence of 0.7 to 0.9. Nevertheless, an IGE in that range does appear to be consistent with the very highest United States estimates found in the literature, and with the 0.65 to 0.75 that I estimate.

Sibling Similarity in Terms of Relative Income—The Sibling Rank Association

Instead of comparing parent and child incomes, other summary measures of mobility compare the incomes of siblings. As noted in Section 2, sibling similarity measures only indirectly reflect income mobility. They capture the influence of the full set of shared family background factors that cause sibling incomes to resemble each other. As with summary measures of persistence, we can distinguish between sibling similarity measures that involve relative or absolute comparisons.

What I will call the "sibling rank association" (SRA) captures the extent to which relative gaps between two adults are matched by relative gaps between their siblings, or alternatively the extent to which the relative positions of siblings predict each other. It can be thought of as the slope of the straight line that best fits the cloud of points when the income ranks of sibling pairs are plotted on two axes of a chart.

The SRA ranges from -1 to 1, with 1 indicating that a given percentile gap between two people will tend to be matched by the same percentile gap between their siblings. An SRA of 0 indicates that there will tend to be no percentile gap at all between the siblings of people whose incomes differ, and an SRA of -1 indicates that two people will tend to be separated by the same percentiles as their siblings, but with the richer and poorer person switching places. An SRA of, say, 0.4 indicates that the percentile gap between the sibling of a richer person and the sibling of a poorer person will tend to be 40 percent of the percentile gap between the richer and poorer person. The siblings of the richest and poorest person would be expected to be separated by 40 percentiles in that case.

I estimate a brother rank association for earnings of 0.38 to 0.39 (0.39 preferred) and a brother rank association for size-adjusted family income of 0.28 to 0.43 (0.43).[94] The estimated sister rank association for earnings ranges from 0.24 to 0.32, with a preferred estimate of 0.31. The rank association for sisters' size-adjusted family incomes ranges from 0.43 to 0.44 (0.44 preferred).[95] Pooling brothers and sisters, the SRA for size-adjusted family income ranges from 0.36 to 0.43, with a preferred estimate of 0.43.[96] The SRAs for family income are higher than the one estimate of which I am aware from previous research, which found an SRA of 0.35 pooling men and women together.[97]

These estimates, like the sibling similarity estimates in Section 2, suggest that parents' income ranks—and the things correlated with them—are not the only shared influences affecting siblings' income ranks. Under a simple model in which siblings' adult income ranks reflect shared parental income rank (and its correlates), other shared influences, and influences not shared between siblings, and in which those three sets of factors are independent of each other, the SRA is the square of the IRA plus a component reflecting other shared influences besides parental income rank. Earlier in this section, we saw the IRA tends to be between 0.5 and 0.6 (lower for women's earnings). If shared parental income rank were the only family background factor affecting sibling income ranks, then we would expect the SRA to be (roughly) between 0.25 and 0.36 instead of the 0.40 to 0.45 estimated here (again, lower for women's earnings).

Sibling Similarity in Terms of Absolute Income—The Sibling Correlation

The sibling correlation (SC) is like the IGC, except it compares sibling incomes to each other rather than comparing parent and child incomes. It is like the SRA, except it correlates logged sibling incomes rather than sibling income *ranks*. A sibling correlation of 0.4 indicates that the inequality in logged incomes predicted from the logged income of siblings and the regression line through logged sibling incomes is 40 percent of actual logged-income inequality. Under reasonable assumptions, it means that 63 percent of logged-income inequality (the square root of 0.4) is between families and 37 percent is within families, between siblings.[98]

Using the PSID, I estimate that the SC for brother earnings ranges from 0.33 to 0.45, with a preferred estimate of 0.39. Similarly, the brother correlation for size-adjusted family income ranges from 0.27 to 0.45 (0.45 preferred). For sisters, the earnings correlation ranges from 0.22 to 0.31 (0.30 preferred), and the size-adjusted family income correlation ranges from 0.45 to 0.46 (0.46 preferred).[99] Pooling brothers and sisters, the size-adjusted family income correlation ranges from 0.35 to 0.45 (0.45 preferred).[100]

The preferred estimates imply that nearly half of female earnings inequality occurs between sisters within the same family (45 percent, which is 1 minus the square root of 0.3), while roughly one third of male earnings inequality and of family income inequality is within family.

These estimates are very similar to the SRA estimates, which use income ranks instead of logged income. They are well above the square of the IGC estimates, confirming that family background is not fully captured by parental income. The sibling correlations here are within the range—a fairly broad range—of estimates from past research; unlike for the other summary measures in this section, they do not tend to be higher than previous research indicates. This could reflect the possibility that my multi-year averages of income capture permanent income no better than the models that have tended to be used in research since 2000. Alternatively, some of those models could be overstating sibling similarity. However, much of the recent model-based research produces results that are entirely consistent with mine.

4. Assessing Equality of Opportunity

Mobility versus Opportunity

Is economic mobility equivalent to opportunity? Is the reduction of childhood income inequalities through intergenerational mobility equivalent to increasing equality of opportunity? At the individual level, mobility and opportunity are clearly not the same thing. As noted in the introduction, people who are well off as children can end up with lower incomes as adults because they choose vocations that appeal to them in nonmonetary ways. Some who start poorer than average might end up better off by taking an undesirable job that pays relatively well as an inducement to potential workers. Income is not the only thing that people care about; equal opportunity does not simply mean equal opportunity to achieve a high income.

However, the extent to which income differences between families persist from childhood into adulthood does indicate the degree of equality of opportunity. In a world of equal opportunity, the children of poor parents would be limited in their aims neither by their parents' inability to afford college nor by their parents' lack of encouragement to attend college. The children of upper-income parents would not have the diminished opportunities they experience today to pursue the aims of children from lower-income families. There are plenty of people from advantaged backgrounds who are dissatisfied with their spiritual life, their leisure time, and the state of their connection to family and friends, all of which may suffer because they were steered toward careerist goals. In a world of equal opportunity, everyone would have the same opportunities to pursue any aims, and so no childhood variable would predict any adult outcome.

Achieving such a result, however, would require that we sever all intergenerational transmission between parents and children, whether genetic, cultural, or economic. It would, in other words, require complete genetic, cultural, and economic leveling and homogeneity. That world would likely be a deeply unpleasant one in which to live, but in it, there would be no connection between the incomes of parents and the incomes of their children.

It is probably undesirable to have perfect equality of opportunity, but that does not mean that we should be satisfied with the current distribution of opportunity, nor the current distribution of the specific opportunity to achieve a high income.

Summary measures of the persistence of childhood income inequalities offer a way to assess how equal opportunities are, even if they do not tell us how equal they ought to be.

One other point worth emphasizing is that all of the estimates presented in this primer are *descriptive*. Just because children who are rich tend to become rich adults does not mean that it was their parents' incomes that led to their adult success. Parents with higher incomes have many other characteristics that could be causing the intergenerational association of incomes. They tend to have higher levels of education and to have attended better schools. They have higher wealth levels and more valuable cultural and social capital. They have higher intelligence levels, better health, and more advantageous personality traits. Their families are more stable. Assessing the causal importance of family income is beyond the scope of this paper, and a much more challenging task than simply describing the extent to which rich and poor parents have rich or poor children.

Which Summary Measure?—Conceptual Issues

Which summary measure provides the best indicator of equality of opportunity? To clarify this issue, imagine three countries that have had the same IRAs, IGEs, IGCs, SRAs, and SCs as each other in the past. That is to say, they have had the same opportunity levels as each other by any of these five measures. Imagine further that they have exactly the same income distribution across parents—neither CEOs nor janitors make more or less in one country than in another, and the inequality between them is the same in each. Economic growth rates are steady and identical in each country.

Now imagine that in Country A, public schools are abolished, government redistribution is ended, and how much schooling a child obtains is entirely dependent on what she and her parents can afford and how much they can convince others to subsidize. Imagine that in Country B, demand for high-skilled labor rises and demand for low-skilled labor falls—the gap in pay between CEOs and janitors widens. In Country C, the rate of income growth becomes 20 percent higher for CEOs and janitors alike.

What would our various summary measures say about how much more or less equal opportunity has become in these three countries? Set aside the sibling similarity measures for the time being. In Country A, because income has become more important in determining who gets to be a CEO, the incomes of rich and poor children will come to resemble more closely the incomes of their parents. The IGE will become higher than it used to be, and analysts would typically say that equality of opportunity in Country A has diminished.

In this case, the conclusion accords with our intuitions about opportunity. It does so because the new IGE in Country A will reflect lower absolute income losses experienced by rich children and lower absolute gains experienced by poor children. These absolute losses and gains will be patterned by the initial relative positions of children and result in less equalization in those positions than previously existed.

Country B will also have a higher IGE than before. In this scenario, it will have larger absolute income gains and smaller losses among all children who become CEOs, and vice versa among all children who become janitors. The new gains and losses will not be patterned by initial relative positions. Everyone who becomes a CEO will see better absolute income growth than in the past, everyone who becomes a janitor will see worse absolute income growth, and neither rich nor poor children will become more or less likely to be a CEO or a janitor. Change in relative positions will remain as before.

Interpreting the IGE is more complicated in this scenario than in the case of Country A. For some observers, if poor children in Country B still have the same chance of becoming a CEO as in the past, the fact that being a CEO has become more lucrative and being a janitor less so does not necessarily mean that poor children enjoy less opportunity than they did or that opportunity has become less equal. To others, however, the newer CEOs, by virtue of being richer in absolute terms than previous CEOs (and perhaps by virtue of being richer *relative* to janitors than in the past) are objectively better off and have more opportunity than CEOs did in the past. Similarly, newer janitors may be viewed as worse off than past janitors. In this view, inequality of opportunity will have worsened.

Other observers might argue that Country B's inequality of opportunity has become worse because the newly richer CEOs will pass on advantages to their children. That is, Country B will become Country A. That may well be—the question is always an empirical one—but even so, the IGE is a summary of the *past* generation's absolute mobility. A high IGE does not convey information about opportunity to reach a given station until unequal opportunities actually arise.

There is a third way in which we might think of Country B as having more inequality of opportunity than before. There may be children there who decided to become janitors early on but who would have tried to be CEOs if they had known how much more lucrative being a CEO would become (or how much less lucrative being a janitor would become). This kind of “inequality of opportunity” will have worsened if the children who thought the world was going to be like it always was and would have done things differently if they’d known it was going to be otherwise tend to come from poor families. Of the IRA, IGE, and IGC, the IGE alone reflects such diminished opportunity.

Finally, when everyone’s income rises by the same percentage and that percentage is higher than in the past, the IGE will be unaffected. From Country C’s IGE, we would conclude that inequality of opportunity was higher than in Countries A and B, but no higher or lower in Country C than it used to be. Everyone becomes better off than in the past in absolute terms and relative mobility will have not declined. By some conceptualizations, inequality will have not grown either—whatever the ratio of CEO-to-janitor pay was before, it has not changed.

However, the absolute gap in expected incomes between rich and poor children will have grown, so by this understanding of equality of opportunity, there will have been a deterioration that the IGE fails to reflect. Note, however, that unlike in Country B, it is not the case that the poor have become poorer—their absolute gains were just not as large as those of rich children.

What would the IGC imply about mobility in these three countries? By controlling for the widening pay ratio between CEOs and janitors in Countries A and B, the IGC might indicate that equality of opportunity is unchanged in all three countries. If so, the IGC would accord with the intuition that if Country B’s and Country C’s poor children have become no less likely to become CEOs, then inequality of opportunity has not worsened. But the IGC would obscure rising unequal opportunity to achieve a given living standard in these two countries, and it would not show Country B—where the poor grew poorer—as having less equality of opportunity than Country C (by the view that elevates absolute income inequality).

Most problematically, it would be perverse to interpret an unchanged IGC in Country A as meaning it has the same equality of opportunity as before. When opportunity became more dependent on parental income in Country A, rising inequality was a *consequence*. “Controlling for” this rise in inequality disposes of the evidence that income became more important to life chances.[101]

Of the IRA, IGE, and IGC, the IRA does the best job according with our intuitions about opportunity. It would show Country A as having more inequality of opportunity than before, something that might not be true of the IGC. It would show Country A as having more inequality of opportunity than Countries B and C, something that might not be true of the IGE. It would show Countries B and C having the same inequality of opportunity as in the past and as each other, conforming to the view that opportunity is about transcending one's station. It also would reveal if either country subsequently becomes akin to Country A, providing evidence about the two-generation concerns of IGE defenders.

The IRA fails only to the extent that we want to define opportunity as the opportunity to achieve a given living standard or as the opportunity to make human capital investment decisions with an accurate picture of future income potential. These are the only senses in which the IGE *uniquely* reflects differences in equal opportunity that derive from income inequality increasing "the consequences of the 'birth lottery.'"[102] But even then, the IGE cannot distinguish between rising "inequality of opportunity" in which poor children suffer losses and that within which poor children simply have smaller gains than rich children.

Indeed, the problem with defining "opportunity" as the opportunity to achieve a given standard of living—rather than as the opportunity to reach a given station—is that it is unclear that we ought to be primarily concerned with *inequality* of such opportunity as opposed to the *level* of opportunity enjoyed by poor children. The opportunity to reach a given living standard depends not just on inequality of absolute mobility, but on how much economic growth lifts incomes generally. A poor child in a country with a high IGE and rising living standards may do better than one in a country with a low IGE and stagnant living standards. That is, the level of opportunity (to reach a given living standard) itself may be more important for poor children than whether they have the *same* level of opportunity as rich children. Rather than caring about the IGEs of two countries or the IGEs of two eras, we might want to simply focus on the incomes poor children can anticipate.[103] Put another way, should we care more about the percentage of poor children who exceed their parents' income, or about whether this percentage is higher or lower for them than for rich children?

The SC and SRA turn out to share the strengths and weaknesses of the IGC and IRA, respectively.[104] They would yield the same conclusions about which of the three countries experienced declines in equality of opportunity and about which have the most and least inequality of opportunity. However, in the real world, the SC and SRA have the virtue of reflecting other kinds of inequality of opportunity not captured in the IGC or IRA—those caused by inequalities in influences unrelated to parent income.

Which Summary Measure?—Practical Issues

There are also practical reasons to prefer the IRA over the IGE as a measure of the degree of equality of opportunity. Several studies have concluded that the IRA is more robust to various sensitivity checks and measurement issues.[105] It is less sensitive to the number of years of parental and child income that are averaged to proxy "permanent income," suggesting that income rank may suffer from less volatility than logged income. It also appears to be less sensitive to the age at which sons' incomes are measured.[106]

For high and low parental incomes, the IGE is higher than in the middle of the income distribution. This feature combines with the log transformation of incomes (which makes IGEs sensitive to small income differences among poor children) to create instability in IGE estimates. Exacerbating the problem is that the log of zero is undefined, so individuals without income must be dropped or they must be given imputed values that (if low) unduly affect the IGE.[107]

The relationship between parent and child income ranks, on the other hand, is roughly linear even at the top and bottom of the parental income distribution.[108] Incomes of zero (or negative incomes) may be incorporated into IRA analyses, and the IRA is not sensitive to small differences in low incomes.

Furthermore, the IRA presents more opportunities for analyzing group differences and geographic differences in mobility. Everyone may, first, be ranked in the combined population, and then separate subgroup IRAs may be estimated and compared against one another. This answers the question of which subgroups have more mobility across the aggregate distribution of incomes. Alternatively, if one is interested in mobility within a subgroup, then parent–child pairs can be ranked within the subgroup. Then subgroups can be compared based on which have more mobility within their own income distributions. IGEs, in contrast, always indicate the degree of income persistence *within* the population represented by the sample. Comparing IGEs for blacks and whites means looking at whether childhood inequalities within the black community are narrowing more or less than childhood inequalities within the white community. IGEs cannot tell us whether blacks experience absolute mobility that reduces childhood inequalities between them and white children.

5. Conclusion

Contrary to some claims, the American Dream abides. The United States remains among the richest countries in the world, which are collectively the richest societies in world history. Poverty is much lower than it was in the "Golden Age" of the mid-twentieth century, to which so many people seem to want to return.[109] The pace of middle-class income growth has slowed, but beyond cyclical downturns, it has not reversed.[110] The productivity growth of the 1940s, 1950s, and 1960s was unusually strong, so its slowdown beginning in the 1970s was bound to produce disappointing income growth.

As we will see in the third installment of this primer, one consequence of slower growth is that upward absolute mobility has declined. Yet it remains the case that roughly three in four adults today are better off than their parents were at the same age. That share is higher among Americans who grew up poor, and it is lower among those who grew up in affluence. Well-off children can end up worse-off than their parents and still be quite comfortable. The typical adult is over 25 percent better off than her parents, as noted in Section 2. Unadjusted for declining family size, that translates into a $12,300 bump in my analyses, over and above parental income and after accounting for the rise in the cost of living.

However, while the ability of the American economy to lift incomes remains strong, our ineffectiveness in helping people to transcend their family origins continues to disappoint. The relative mobility estimates in this study affirm those from past research in finding that poor children are all too likely to remain poor in adulthood. At the same time, the limited downward mobility of children with well-off parents may indicate that our meritocracy tolerates a level of anti-competitiveness that is economically inefficient. In fact, past research has overstated the extent to which patterns of relative mobility reduce childhood relative income gaps. (It is this summary indicator—measured by the IRA—to which attention should be heeded if we are concerned about transcending family origins, not the indicator measured by the more popular IGE.)

In fairness, without benchmarks for assessing economic mobility levels, it is difficult to ascertain how much better we could do to promote equal opportunity. Supreme Court Justice Potter Stewart famously wrote, of his standard for judging whether art was obscene, “I know it when I see it.” We ought to have a more solid basis for determining whether our mobility levels are an obscenity. The remaining two installments of this primer will ascertain how we are doing in comparison with levels of mobility in our peer nations and in comparison with past levels of American mobility.

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Appendix 1:
Methodology

As noted in the text, I rely on the Panel Study of Income Dynamics (PSID). The PSID is the most widely used source for American intergenerational mobility statistics. Since it follows a nationally representative group of adults from nearly 50 years ago and their grown children up to the present day, the PSID is ideally suited to estimating intergenerational mobility measures. Even so, the relatively small number of people interviewed and the fact that it does not go back further in time mean that it is not the ideal dataset if lifetime incomes are to be approximated. However, it is probably the best available for American estimates, with the possible exception of difficult-to-access sources based wholly or in part on administrative data.

Originally the PSID included two samples, one of which was nationally representative and another that oversampled low-income households. Researchers have identified problems with the way that latter sample was selected.[111] I therefore only use the nationally representative sample. (Nor do I use a later sample of immigrants, for which data on parental income is generally unavailable because it was introduced in 1997.)

My analyses consider a variety of income measures, including the annual earnings of fathers, mothers, sons, and daughters and the annual family incomes of each. Earnings include wage and salary income, bonus and overtime pay, and tips, as well as self-employment income. Family income includes earnings, capital income, retirement income, and income from private and public cash transfers. Family income is before taxes, and it does not include the value of employer-sponsored or government-provided noncash benefits. In analyses where I adjust family incomes for the number of family members, I do so by dividing income by the square root of family size. All incomes and earnings are adjusted for inflation using the Bureau of Economic Analysis's Personal Consumption Expenditures (PCE) deflator, except in the exercise replicating the absolute mobility estimates of Chetty et al. (2016).[112]

I link all of the grown children in the PSID data to their biological parents and to the same-sex sibling in each year who was nearest in age to them. Parent, child, and sibling earnings are only available in the PSID if they are "family unit" heads, spouses, or long-term partners of the head. It is possible that at different ages, a child is matched to different siblings. The match to the same-sex nearest-age sibling occurs within each survey year. If a sibling drops out of the survey or fails to report income in some year, a child is matched to the sibling next-closest in age. It is also possible that, because of the interaction of interview date with birthdays, a child might get successively matched to a sibling one year older and one year younger than she. Brothers and sisters are never matched to each other. Siblings must share at least one biological parent to be matched.

Note that adults can enter into the sibling analyses more than once. They can enter in themselves, matched to their nearest-age same-sex sibling, and they can also enter in as the nearest-age same-sex sibling to someone else.

In my analyses, income amounts are multiyear averages. I begin with income at age 40 (or 41 or 39 if unavailable at 40, or 42 or 38 if also unavailable at those ages). I then search outward from age 40 (up and down) and capture income measured at other ages. In general, I capture as many years as are available between the ages of 25 and 55 (except for the absolute mobility analyses in Section2—see below). Because the survey was annually administered from 1968 to 1997 but only biennially thereafter, if income is missing for some age, I use income when one year older (if the age is above 40) or one year younger (if below age 40) when it is available. I then drop every other income observation, so that I only have incomes measured at age 26, 28, 30, …, 50, 52, and 54—up to 15 observations within the 31 years between 25 and 55.

I average across all non-missing observations to get income averages for each person. Parents who were older than 26 before 1967 and children who were younger than 54 by 2012 will not have 31 years from which to draw incomes, and they will lack a full 15 years of income. The same will be true of people who temporarily are absent from the PSID or who permanently attrite from the survey. I include years in which someone participated in the survey but reported no income, giving them an income of $0 for that year. (Similarly, reports of negative income are retained.) I occasionally highlight results when these years are excluded, particularly when looking at women's mobility. I generally exclude pairs in which the parent or child has a multi-year income average that is non-positive, noting exceptions.

Once these multi-year averages are computed, I de-mean averaged incomes by regressing them on the calendar year a person turns 40 (or 38, 39, 41, or 42 if they do not have income data for age 40). I do so to compare, in Section 3, summary measure estimates from samples with different restrictions on the amount of missing data. Depending on how many years of non-missing income are required, more restricted samples will draw incomes from a wider span of calendar years. Since the survey starts with 1967 income, and 2012 is the most recent year for which income is available, samples with larger spans will more often exclude people whose span is truncated by these end points. That means that more restricted samples will tend to have narrower variation in the year in which parents or children turned 40.

For instance, in samples requiring 15 years of parental income data, a parent must turn 40 no earlier than 1981, while in those requiring only one year of parent income data, parents can turn 40 any time from 1967 forward. Similarly, samples requiring 15 years of grown-child income data will include only children who turn 40 no later than 1998, while those requiring a single year of income data will include children turning 40 as late as 2012.

Because I pool parents and children with age-40 income measured in different years and then average their incomes to proxy permanent income, a sample's permanent income variance will partly reflect income growth between the years the oldest people turn 40 and the years the youngest people do. More restricted samples will have smaller permanent-income variances than less restricted samples, which will affect some of my mobility estimates.

De-meaning permanent incomes within calendar years reduces the heterogeneity in estimates between more and less restricted samples. It adjusts permanent incomes for the fact that some people turned 40 more recently than others and will have higher permanent income purely because incomes were higher in more recent years than earlier ones.

A second reason to de-mean my permanent incomes is to adjust for the fact that people turning 40 in the earliest years of the PSID will have permanent incomes based on averaging incomes primarily after age 40. Someone turning 40 in 1967 will have a permanent income that averages income at ages 40, 42, 44, 46, 48, 50, 52, and 54. Meanwhile, someone turning 40 in 1981 will have a permanent income that averages income at ages 26, 28, 30, 32, 34, 36, 38, 40, 42, 44, 46, 48, 50, 52, and 54. An analogous problems exists for children who turn 40 in the most recent years of the PSID; their permanent income will reflect mostly pre-40 incomes.

I analyze mobility using these de-meaned multi-year averages, estimating summary measures of persistence on different samples. I begin with a sample that has 15 years of income for grown children and parents (no missing data) or for grown children and same-sex nearest-age siblings. I then allow children to have a single year missing for parental income (or sibling income) and repeat the analysis. Next I allow them to have up to two years missing for parental (sibling) income, and so on until I include all children with at least one year of parent (sibling) income.

I then analyze a sample that lets children have one year of their own income missing, then one year missing from their own and from their parents' (sibling's) income, then one year missing from their own and two years missing from their parents' (sibling's) income, and so on. Then I repeat this process allowing up to two years of child income to be missing, then up to three years, and so on until the final sample includes all children with at least one year of child income data and at least one year of parental (sibling) income data.

That leaves me with nearly 225 mobility estimates from which to choose. (The square of 15. The number of samples is always less than 225 because some restrictions eliminate all parent–child pairs, and others do not change the sample.) I then select from these estimates to provide a preferred range and a preferred point estimate. As discussed in the text, this selection is somewhat arbitrary, but I settled on the approaches used after extensive study of surface charts of my estimates and careful attention to discontinuities across the nearly 225 estimates.[113] In general, estimates of persistence increase smoothly the more restrictive the sample is in terms of missing data, but if more than 13 non-missing years of income are required, the sample sizes become very small and estimates become very volatile. See Figures 9 and 10 in the text and the discussion of them.

My methods differ somewhat in the analyses of distributional measures of mobility in Section 2. Each of these analyses focuses on a single sample rather than nearly 225. I choose the sample among those with at least 400 parent–child pairs that maximizes persistence in the analogous summary measure in Section 3.

The absolute mobility analyses in Section 2 rely on permanent income measures that average only up to 7 years of income within a window of up to 13 years. I then include in the sample only grown children that turned 40 (or 38, 39, 41, or 42) before 2006 with parents who turned 40 after 1974. This ensures that everyone in the sample without missing data will have 7 years of income averaged. In the absence of this restriction, absolute mobility will tend to be understated. The income of parents observed near the start of the PSID in the late 1960s will be overstated (since it will mostly be their over-40 incomes averaged together). Similarly, the income of children observed recently in the PSID will be understated (since it will mostly be their under-40 incomes averaged).

I chose a 13-year window in order to ensure that my sample would include parents who were older at the time of their child's birth. Choosing larger windows—while requiring that everyone potentially be able to have complete income data—pushes forward in time the earliest year a parent can turn 40 and pushes backward in time the latest year a child can turn 40, which pushes downward the age of parents at the birth of their children. It also makes the child cohorts less contemporary.

In addition, while I "de-mean" income in most analyses in this paper, by partialing out calendar/survey year effects, for estimating upward mobility this adjustment is unwarranted.

In the replication of the Chetty et al. (2014) absolute mobility analysis, I use single-year measures of income in order to be consistent with that study.

Finally, note that I do not use survey weights in any analyses. If there were no attrition and no changes to survey administration rules, then survey weights would not be necessary, because the original 1968 sample was nationally representative and has followed children of the original sample members as they have left home (though recent immigrants and their children would go unrepresented). However, if there is a pattern in terms of who drops out of the survey over time, then my estimates may be subject to attrition bias. The key question is whether missing data is ignorable or not. If those with missing data have higher or lower mobility than others, then by leaving them out, I would be under- or over-estimating mobility. Research on attrition from the PSID has not identified obvious sources of bias.[114]

The weights available in the PSID also adjust for the effects of efforts in the early 1990s to bring attriters back into the survey, the effects of changes in the rules for following people who move, and the effects of a mid-1994 change that made more children eligible for inclusion in the survey.[115]

However, given that I am averaging income, potentially across many years, for each person I include in my analyses, and given that they turn 40 in different years, it is not at all clear that the available weights in the PSID do more good than harm in my case.

Differences from Mazumder (2015)

As noted in the introduction, my approach is inspired by that of Mazumder (2015). Mazumder also used the PSID and, in an important innovation, centered up to 15 years of individual incomes on age 40. He then tabulated 150 estimates corresponding to samples with different numbers of years averaged for parental and child income. That is, each sample includes parents with exactly *x* years of income and children with exactly *y* years of income, with no allowance for missing incomes. In contrast, my samples hold fixed *x* and *y* (up to 15 years in a period of up to 31 years for parents and children) and are distinguished by the number of years of income, *p* and *c*, that are allowed to be missing. My samples include people with 15-*p* years *or more* of parental income and 15-*c* years *or more* of child income.

Beyond this distinction, my analyses differ from Mazumder's in several ways. First, I explore many more mobility measures. I look at men and women separately, while Mazumder focuses on fathers and sons. I examine earnings mobility, family income mobility, and family-income-versus-child-earnings mobility; Mazumder looks at only the first two. I estimate the indicators that Mazumder does, but I also examine intergenerational correlations, sibling correlations, and absolute mobility.

Second, there are a number of methodological differences between our analyses. I take incomes from every other year when I average them, in order not to treat observations after 1997 and before then differently. (The PSID switched to biennial administration after 1997.) Mazumder weights his data in some way to account for attrition. I chose not to use weights, since it is unclear how to weight observations from multiple survey waves who, if they attrite, do so at different times. Mazumder includes standard errors as an indication of the extent to which sampling error affects his estimates. I chose not to compute them because given that the analyses include multi-year averages and exhibit such a large degree of *nonrandom* variability, standard errors seem uninformative (and difficult to compute correctly). Mazumder drops observations with "major" imputations estimated by PSID administrators while I leave them in. In his earnings analyses, he drops observations with no earnings. My estimates generally include them, but I report additional results when their exclusion matters.

Despite these differences, his and my estimates both indicate that IGEs have generally been underestimated by most researchers. My estimates, however, also suggest that is true of IRAs, while Mazumder downplays the impact of multiyear averaging on those estimates.

Appendix 2:

Up-To-the-Minute Review of Research on Mobility Levels in the United States[116]

Distributional Measures of Economic Mobility

Relative Mobility—The Transition Matrix

Mobility research rarely included transition matrices prior to 2005, but they have grown in popularity over the past decade. Seven studies have estimated transition matrices comparing fathers' and sons' earnings. Four used quartiles and found that between 36 percent and 40 percent of sons raised in the bottom fourth of father earnings remain in the bottom fourth as adults.[117] The corresponding range for the share raised in the top fourth who remain there is 36 percent to 43 percent. These studies included two using survey data and two using administrative data. Similarly, one study based on survey data and two based on administrative data converged on similar quintile-based estimates.[118] Between 29 and 32 percent of sons starting in the bottom fifth remained there in adulthood, compared with 38 to 43 percent of sons being immobile at the top.[119]

One study also included a decile-based transition matrix, finding that 22 percent of sons starting in the bottom tenth of father earnings were in the bottom tenth themselves. Likewise, 26 percent of sons with fathers in the top tenth remained there.[120]

Three studies compared daughters' earnings to those of their fathers, two based on survey data and one on administrative data.[121] Peters (1992) found that 31 percent of daughters with father earnings in the bottom fourth had adult earnings that put them in the bottom fourth of daughters. Similarly, 32 percent were immobile in the top fourth. Dahl and DeLeire (2008) concluded that 25 percent of daughters raised by fathers in the bottom *fifth* stayed there as adults, and 31 percent raised in the top fifth remained there.[122] Unsurprisingly, then, daughters' earnings resemble those of their fathers less than sons' earnings do.

Most of the research on family income mobility using transition matrices has focused on quintiles and pooled sons and daughters. These studies report a modestly wide range of estimates for upward and downward mobility. Seven use the PSID and find that between 37 and 44 percent of children starting in the bottom fifth of family income end up in the bottom fifth. At the top, the range is from 39 to 47 percent.[123]

Two studies use administrative data from the Internal Revenue Service and report somewhat higher mobility.[124] The range for immobility from the bottom fifth is from 30 to 34 percent, while it is 35 to 41 percent for immobility from the top fifth. Mazumder (2008) used the National Longitudinal Survey of Youth 1979 and found mobility rates similar to the studies using IRS data. He reported that 34 percent of children are immobile at the bottom and 38 percent are immobile at the top. Schoeni and Wiemers (2015) estimate a quartile-based transition matrix using the PSID and find that 40 percent are immobile at the bottom and 28 percent at the top. Charles et al. (2014) do the same but find estimates of 46 percent and 41 percent.[125]

Another PSID study included a decile-based transition matrix and found that 32 to 37 percent of children in the bottom tenth grow up to have family income in the bottom tenth, and 27 to 30 percent are immobile in the top decile.[126] Finally, Peters (1992) reported results separately for sons and daughters using quartiles. She found that 42 percent of sons and 46 percent of daughters who are in the bottom fourth of parental income end up in the bottom fourth of family income themselves. At the top, 40 percent of sons and 41 percent of daughters are immobile.

While the estimates vary considerably, it appears that family income mobility is at least as low as earnings mobility among men. Earnings mobility is higher for women when they are compared with their fathers, but it is unclear whether it would be higher if they were compared with mothers.

Absolute Mobility—Surpassing Parental Income

Surprisingly, there has been little research on absolute mobility, and almost all of it has looked only at upward mobility. A single study used the PSID to examine the share of sons who surpassed the earnings of their fathers at a similar age (after adjusting for inflation).[127] It found that 59 percent of sons had done so, ranging from 85 percent of sons with fathers in the bottom fifth, 51 percent of those starting in the middle fifth, and 46 percent of sons in the top fifth as children.

Corak, Lindquist, and Mazumder (2014) report the average absolute gain for sons who surpass their fathers' earnings. That average is roughly $11,000 for sons starting in the bottom fifth and about $9,500 for sons starting in the bottom half. Similarly, the average absolute loss among sons who do worse than their fathers is about $50,000 for sons starting out in the top fifth and $35,000 for those starting in the top half. These averages are likely to be driven by sons whose parents have very large earnings.

A widely publicized working paper by Chetty et al. (2016) looking at the personal pre-tax and -transfer income of fathers and sons (that is, including all income of fathers and sons—not just earnings—but no income from other family members) found that 40 percent of 30-year-old sons have exceeded their father's income.

Nearly all the research on absolute mobility in terms of family income has looked at upward mobility specifically, usually using the PSID. Three PSID studies find that between 63 and 67 percent of adults exceed their parents' parental income.[128] For adults raised in the bottom, middle, and top fifths, the ranges are 81 to 83 percent, 60 to 70 percent, and 43 to 54 percent. The highest estimate in each of those three groups comes from a study that averaged 15 years of parental income.[129] Another PSID study adjusted incomes for family size and found that 84 percent of adults are better off than their parents were. The shares for the bottom, middle, and top fifths were 93, 88, and 70 percent.[130] Davis and Mazumder (2016) use the NLSY79 and find that 53 to 58 percent of daughters exceed their fathers' family income.

Chetty et al. (2016) look at pre-tax and -transfer family income and find that just 50 percent of 30-year-olds have exceeded their parents' income. That includes about 70 percent of adults whose parents were at the 10th percentile, about 45 percent of those whose parents were at the median, and roughly 30 percent of those raised at the 90th percentile. However, the percentage with upward absolute mobility rises from 50 to 55 percent when the PCE deflator is used to adjust incomes for inflation or when incomes are measured at age 40 instead of age 30. It rises to 60 percent if incomes are size-adjusted first. Combining these three modifications would likely indicate that at least two-thirds of 40-year-olds are better off than their parents.

A handful of other papers also address absolute mobility. Davis and Mazumder (in progress) report the average change in income that adults experience in relation to their parents' income, by parental income percentile. Bjorklund and Jantti (1997) create groups based on multiples of median earnings and conclude that 40 percent of sons below half the median as children end up below half the median as adults. The same percentage of those with at least 1.5 times the median in childhood end up above that threshold themselves. Acs, Elliott, and Kalish (2016) report that 35 percent of adults who were poor as children are poor as 30-year-olds.[131]

Summary Measures of the Persistence of Childhood Economic Inequality

Persistence of Relative Economic Inequality—The Intergenerational Rank Association

There are relatively few IRA estimates in the United States, and essentially all of them come from the past ten years. Based on three studies comparing the earnings of fathers and sons, the IRA was previously estimated to be between 0.3 and 0.4, and a single study comparing father earnings to daughter earnings found an IRA between 0.08 and 0.17.[132] The IRA comparing child earnings to parent family income appears to be consistent with these findings.[133] Out of three studies, none reported an IRA higher than 0.4 for sons or for sons and daughters pooled together, with the single study done for daughters showing an IRA of 0.25. Three studies comparing family incomes of sons to their parents found an IRA ranging from 0.33 to 0.45, two comparing daughters to parents found an IRA between 0.34 and 0.4, and a single study found a range of 0.32 to 0.34 for sons and daughters pooled together.[134]

Persistence of Absolute Economic Inequality—The Intergenerational Elasticity[135]

The vast majority of studies on intergenerational mobility have used the IGE, and IGE-based research extends back to the 1970s. Because the IGE is more sensitive to various methodological decisions (discussed above) than the rank correlation, the earlier studies that were ignorant of these issues tended to produce results indicating that mobility in the United States reduces childhood inequality by far more than it does. The earliest research, from the late 1970s and 1980s, also suffered from an absence of quality longitudinal data (tracking people from adolescence into adulthood) that represented the entire US population. Typically based on comparisons of sons' earnings to parent family income and finding low IGEs (ranging from 0.15 to 0.28), the early mobility literature reinforced the idea that family background does not impede opportunity all that much.[136]

The early 1990s marked a second wave of research, more sophisticated than the first. This wave drew from two national longitudinal surveys: the PSID and the National Longitudinal Survey–Original Cohorts (NLSOC). Researchers began to appreciate that they needed to approximate the long-run ("permanent") incomes of parents and children rather than use a single year of income for each. When single years are used, the idiosyncrasies of a good or bad year can make mobility look more inequality-reducing than it is. Similarly, researchers became better aware of the fact that incomes are measured with error, either because people misreport how much they make or because subsequent attempts by survey administrators to deal with missing income data distort the true picture. Using multiple years of income averages out some of these errors—those that are equally likely to overstate as to understate income. Finally, it became clear that using local samples to proxy the national population also overstated the extent to which mobility reduces childhood inequalities.

This second wave of research produced IGE estimates that ranged from 0.18 to 0.80. However, the NLSOC estimates tended to be significantly lower than those from the PSID. IGEs estimated from two PSID studies ranged from 0.37 to 0.80.[137] One NLSOC study estimated an IGE of between 0.25 and 0.68, but it was restricted to men employed fulltime, year-round. That restriction pushes the IGE higher than it would be if men less attached to the labor force were included.[138] The other two NLSOC studies yielded IGEs ranging from 0.14 to 0.34.[139]

In the third wave of IGE research, researchers came to realize that they needed to worry about the age at which incomes were measured. Workers make more when they are older than they do when they are younger. Mobility estimates will differ depending on when parents' incomes are measured and when the incomes of adult children are measured. Ideally, we would be able to measure lifetime incomes in both generations, but this is generally not practical due to data limitations. Absent the ideal data, analysts must look at income taken at ages that best reflect lifetime income (at ages around 40 years old, as it turns out). Further, "life-cycle bias" is also a problem if the ages at which incomes are measured differ between children and parents.

All but four of 12 PSID-based father–son earnings IGEs from studies between 1994 and 2004 were 0.3 or higher. All but five were 0.4 or higher, and three studies included IGE estimates above 0.5 (the highest being 0.77).[140] All but one of four PSID-based father–daughter earnings IGEs were between 0.41 and 0.45.[141] The four PSID-based studies looking at family income IGEs included estimates ranging from 0.3 to 0.78, with the best estimates generally above 0.4.[142] And three studies using the PSID and comparing parent family income to children's earnings found IGEs ranging from 0.37 to 0.54.[143] Meanwhile, two studies using the NLSOC mirrored the earlier studies using that dataset. One produced a very low IGE of 0.15, indicating high mobility while the other, restricted to men working fulltime, year-round, found a higher IGE (0.37—still lower than the PSID estimates in the same study).[144]

In the latest wave of mobility research, since 2004, researchers have begun to take advantage of administrative data on earnings and income, which is generally thought to be less error-prone than survey responses and often includes more years of income per adult. Research using surveys has also grown more sophisticated, and analysts have added the National Longitudinal Survey of Youth 1979 (NLSY79) as a workhorse survey to supplement the PSID research.

Six PSID-based studies comparing fathers' and sons' earnings find IGEs between 0.32 and 0.79, with all six including estimates above 0.45, five of them including estimates above 0.55, four of them including estimates above 0.6, and two including estimates above 0.7.[145] These results mirror two studies using tax data, where both have IGEs ranging from 0.45 to 0.65 and include high-end estimates exceeding 0.6.[146] None of the PSID studies examine earnings IGEs for daughters, but the two tax studies do. Mazumder (2005a) estimates mobility for daughters as being comparable to that for sons, with the best estimates ranging from 0.45 to 0.85. However, Dahl and DeLeire (2008) show estimates that are no higher than 0.27, and the low end is slightly below zero, suggesting that childhood inequalities partly reverse in adulthood.

Another six PSID-based studies compare parent and child family incomes and find IGEs ranging from 0.45 to 0.71.[147] Those are well above the estimates in the one study based on tax data, by Raj Chetty and his colleagues.[148] The best estimates in that paper are between 0.34 and 0.35. However, the Chetty paper has been convincingly shown to underestimate the IGEs (to overestimate mobility).[149] One NLSY79-based study reports family income IGEs of 0.37 for both sons and daughters. A second reports IGEs of 0.43 for sons and 0.52 for daughters.[150]

Only two studies using the PSID compared parent family income to child earnings. One found IGEs of just 0.29 for sons and 0.25 for daughters; the other found an IGE of 0.29 for sons.[151] The estimate for men is lower than all the estimates in five NLSY79 studies and the one study based on tax data. The NLSY79 estimates range from 0.33 to 0.54 comparing parental income to sons' earnings, and the best estimates from the paper using tax data range from 0.4 to 0.59.[152] The PSID and NLSY79 estimates for daughters agree, however, as the latter range from 0.25 to 0.31 in two studies.[153] Those are lower than the range for daughters in the tax data, which goes from 0.33 to 0.54.[154] A fifth NLSY79 study finds an IGE of 0.43 pooling sons and daughters together.[155]

Persistence of Absolute Economic Inequality—The Intergenerational Correlation

There has been little research estimating IGCs in the United States, and the research that has been done mostly predates the state-of-the-art studies on IGE measurement. Unfortunately, the IGC is even more sensitive to measurement error and year-to-year fluctuations in income than the IGE. Therefore, the existing estimates in the literature underestimate how strongly parent income predicts child income. The IGCs comparing father and son earnings range from 0.2 to 0.53. Three of the seven studies include high-end estimates above 0.4; three include estimates no higher than 0.26.[156] Only four studies compare fathers' earnings to daughters' earnings, and the IGCs range from 0.02 to 0.42.[157] Two studies compared mother earnings to daughter earnings, producing a range from 0.16 to 0.28 in one and an estimate of 0.01 in the other.[158]

Two studies compare child earnings to parental family income. Jantti et al. (2006) use the NLSY79 and find the IGC for sons is 0.35 to 0.36, compared with 0.15 to 0.16 for daughters. Landerso and Heckman (2016) pool sons and daughters in the PSID and obtain IGCs ranging from 0.21 to 0.26. As with their IGE estimates, Landerso and Heckman's results are outliers compared with other PSID studies, indicating that mobility leads to a large reduction in childhood inequality.

Finally, three studies compare child family incomes to parental family incomes. These IGCs range from 0.3 to 0.43.[159] The IGC research as a whole largely relies on the NLSOC and PSID, with a single study from the NLSY79 and another using administrative data.[160]

Sibling Similarity in Terms of Relative Income—The Sibling Rank Association

The only study of which I am aware that estimated a sibling rank association found a correlation in family income (pooling sons and daughters) of 0.35 using the PSID.[161]

Sibling Similarity in Terms of Absolute Income—The Sibling Correlation

Solon (1999) reviewed early studies, which tended to use a single year of parent and child income. The four with national samples (conducted between 1979 and 1986) reported estimates ranging from 0.11 to 0.31 for brothers.[162] Four studies between 1988 and 1997 averaged multiple years of income together using the PSID or NLSOC. They found brother correlations within a relatively small range, from 0.30 to 0.45, but sister correlations ranged widely between 0.26 and 0.73 in the two studies that examined them.[163]

Since 2000, studies using the PSID and NLSY79 have also found reasonably consistent estimates of brother correlations, though tending to be higher than in the pre-2000 research. Brother correlations in annual earnings and in family income are generally found to fall between 0.35 and 0.55, with three of eight studies including estimates above 0.5.[164] The picture is less clear for sisters, owing in part to fewer studies having been conducted. The PSID and NLSY79 yield relatively comparable estimates for sister correlations in family income, ranging from 0.43 to 0.63 across four studies.[165] But the PSID estimates of sister correlations in annual earnings range from 0.14 to 0.29 across three studies, compared with 0.29 to 0.34 in the single NLSY79 study.[166]

End Notes

[1] U.S. House Republican Task Force on Poverty, Opportunity, and Upward Mobility (2016).

[2] AEI/Brookings Working Group on Poverty and Opportunity (2015).

[3] Boteach, Vallas, and Schultz (2016).

[4] Trumble and Hatalsky (2014).

[5] YG Network (2014).

[6] See http://www.pewtrusts.org/en/projects/financial-security-and-mobility, http://www.aecf.org/work/economic-opportunity/, https://www.macfound.org/press/article/how-housing-matters-research-briefs/, http://www.pgpf.org/what-we-are-doing/grants/grantee-list/policy-options-for-improving-economic-opportunity-and-mobility, https://freopp.org/, and https://archbridgeinstitute.org/. Disclosure: I am an honorary adviser of the Archbridge Institute, which funded this report, as well as the Foundation for Research on Equal Opportunity

[7] Isaacs, Sawhill, and Haskins (2008); Pew Charitable Trusts (2012); Chetty et al. (2014).

[8] For reviews of the literature on intragenerational mobility, see Burkhauser and Couch (2009) and Jantti and Jenkins (2014).

[9] Panel Study of Income Dynamics, public use dataset. Produced and distributed by the Survey Research Center, Institute for Social Research, University of Michigan, Ann Arbor, MI (2016). Acknowledgement: The collection of data used in this study was partly supported by the National Institutes of Health under grant number R01 HD069609 and the National Science Foundation under award number 1157698.

[10] Averaging multiple years addresses “attenuation bias,” while measuring incomes near age 40 addresses “life-cycle bias.” On the former, see Jenkins (1987), Solon (1992), Mazumder (2005a), Grawe (2006), and Mazumder (2015). On the latter see Haider and Solon (2006), Baker and Solon (2003), Mazumder (2005a), and Mazumder (2015).

[11] See Appendix 1 for differences between my approach and Mazumder's.

[12] If respondents drop out of the PSID, were older than 25 before the PSID started, or were younger than 55 in the most recent wave of the PSID, they can have less than 31 years over which to average income.

[13] For a fuller and more technical treatment of the various ways of measuring mobility, see Fields and Oks (1999) and Jantti and Jenkins (2014).

[14] Reeves (forthcoming); Reeves and Howard (2013).

[15] The estimates are similar when years without earnings are excluded from permanent earnings averaging.

[16] Another benefit of using family income is that few survey respondents report no annual income, while more report having no earnings. Estimates may differ depending on whether years without income are included or excluded in averaging incomes, so the rarity of these reports for family income removes one source of ambiguity.

[17] Non-size-adjusted family income results show more downward mobility from the middle and less upward mobility, but otherwise the estimates do not change meaningfully. Nor do they change when years without income are included in permanent income averaging.

[18] Pew Charitable Trusts (2012); Dahl and DeLeire (2008); Corak, Lindquist, and Mazumder (2014).

[19] The other PSID studies with transition matrices are Hertz (2005); Hertz (2006); Isaacs, Sawhill, and Haskins (2008); Pew Charitable Trusts (2012); Bengali and Daly (2013); and Acs, Elliott, and Kalish (2016).

[20] The National Longitudinal Survey–Original Cohorts (analyzed in Peters, 1992) is the outlier dataset, as discussed in Appendix 2. The study that seems to have been superseded is Mazumder (2008a). Mazumder (2014) uses the National Longitudinal

Survey of Youth 1979, as does Mazumder (2008a), but the more recent study reports significantly less mobility for African Americans than did the earlier study. Unfortunately, it does not include updated mobility estimates for the population as a whole.

[21] Chetty et al. (2014).

[22] Schoeni and Wiemers (2015).

[23] O'Neill et al. (2007).

[24] One last point bears mentioning. Transition matrices are conventionally estimated by basing quintiles or quartiles on the incomes of (1) a group of adults in one generation who have children subsequently observed in the same data (ignoring those adults who do not, including non-parents) and (2) a group of adults in a later generation who have parents previously observed in the data (ignoring those adults who do not, including recent immigrants). An alternative approach would be to base quintiles or quartiles on broader groups of adults. If, for instance, less-skilled immigration were strong enough, many adults who are in the bottom fifth in analyses like those here might end up in the second fifth of the income distribution when immigrants are included. (Of course, high-skilled immigration might result in more downward mobility by this approach.) This conceptualization of relative mobility and of a person's rank has the attractive feature that relative mobility is not strictly zero-sum. Otherwise, for someone to have upward relative mobility, someone else must fall downward. That said, it is not clear how ranking parental income with non-parents included in the distribution would affect our understanding of mobility.

[25] Bhattacharya and Mazumder (2011); Mazumder (2014).

[26] Corak, Lindquist, and Mazumder (2014).

[27] Davis and Mazumder (in progress); Bratberg et al. (2017).

[28] These estimates average a different span of incomes than the others in this primer. Specifically, I center incomes on age 40 as in the rest of the paper but I average every other year within a 13-year window, up to 7 years of income. I then include in the sample only grown children that turned 40 before 2006 with parents who turned 40 after 1974. This ensures that everyone in the sample without missing data will have

7 years of income averaged. In the absence of this restriction, absolute mobility will tend to be understated. The income of parents observed near the start of the PSID in the late 1960s will be overstated (since it will mostly be their over-40 incomes averaged together). Similarly, the income of children observed recently in the PSID will be understated (since it will mostly be their under-40 incomes averaged). In addition, while I “de-mean” income in most analyses in this paper, by partialing out calendar/survey year effects, for estimating upward mobility this adjustment is unwarranted. My estimates include years of no reported income, but the estimates are little changed if those years are excluded. Nor do they change if average incomes of $0 are included.

[29] The estimate for men is no different if the earnings of mothers’ male partners are used when a biological father is not present in the home. The estimates for women are no different if years with no earnings are included in the earnings averages.

[30] Pew Charitable Trusts (2012).

[31] Three use family income unadjusted for family size: Isaacs, Sawhill, and Haskins (2008), Bengali and Daly (2013), and Acs, Elliott, and Kalish (2016). When I use non-size-adjusted income, my results are close to theirs (not shown).

[32] On the superiority of the PCE deflator, see Winship (2016), Appendix 2.

[33] See their Online Data Table 4: http://www.equality-of-opportunity.org/data/.

[34] In these analyses, I include years without income in averages, and I include averages of $0.

[35] In results not reported, I confirm that the differences are not a function of averaging incomes in Figure 4 instead of using single-year measures as I do in Figure 5.

[36] Winship (2013).

[37] Aghion et al. (2017).

[38] See also Corcoran (2001).

[39] Income distributions are skewed, with many people having low to moderate incomes and a few having very high incomes, which means that parent and child incomes will not typically be linearly related. Taking the natural log of incomes, however, pulls the skew in, making the distribution of income look more bell-shaped and making the relationship between parent and child incomes more linear. See Mitnik et al. (2015) for an important explanation of why these "log–log" models do not technically allow one to estimate the average income conditional on parental income.

[40] Technically, they share half the genes that vary across humanity.

[41] From year to year, someone may be matched to different siblings if the one closest in age changes. That could happen either because of closely timed sibling births or because a sibling drops out of the PSID. Siblings must share one biological parent in common. Note that adult incomes appear in my data both when matched to a sibling (averaged with other incomes as the adult's permanent income) and when a sibling is matched to them (within the average of his sibling's "sibling permanent income").

[42] Brothers must have at least 9 earnings observations out of a possible 15 over a period up to 31 years. A woman must have at least 4 earnings observations and her sister must have at least 5. In the pooled sample, an adult must have 4 family income observations, and the sibling must have at least 9.

[43] Excluding years with no reporting earnings produces similar results.

[44] The results for men and women separately are very similar.

[45] The rank-rank slope is technically the coefficient on parental income rank in the bivariate regression of child income rank on parent income rank. If there are 1,000 pairs of children and parents, the poorest parent is assigned a rank of 1 and the richest a rank of 1,000, and child incomes are also assigned ranks so that they too are ordered. These ranks can be expressed as "percentile ranks" (roughly, dividing each rank by the number of parent-child pairs), so that the ranks range between just above 0 and just below 100, but doing so does not affect the slope.

The Spearman rank correlation is the "Pearson correlation" after converting incomes to ranks. As will be discussed below, a Pearson correlation is simply a regression coefficient multiplied by the ratio of the standard deviation of parent income to the standard deviation of child income. If there are no ties—i.e., if everyone has at least slightly different incomes—then the standard deviations of the ranks of parent and child income will be the same. In this case, the ratio of the two standard deviations will be one and the correlation will equal the regression coefficient (the rank correlation will equal the rank-rank slope). To the extent there are ties, the two measures will differ modestly.

Davis and Mazumder (in progress) estimate "rank mobility" as the average intergenerational change in percentiles conditional on parental percentile. This equals the rank-rank slope (or rank coefficient) minus 1.

[46] Charts for other mobility estimates discussed below, such as for family income, are available from the author.

[47] Very few parent–child pairs are added using even less restricted samples.

[48] 8 times 9, minus 5 sets of restriction criteria that left no parent–child pairs to analyze.

[49] This occurs because in order to appear in the PSID data for 31 years with income centered on age 40, a parent cannot turn 40 before 1981, and a child cannot turn 40 later than 1998. The problem is that the PSID data start in 1968 (with income data for 1967) and end in 2013 (with income data for 2012).

[50] Chetty et al. (2014); Davis and Mazumder (in progress); Bratberg et al. (2017); Dahl and DeLeire (2008).

[51] To improve the readability of the chart, I have deleted a small number of estimates that were below zero or that were equal to 1.0, all of them based on a tiny number of parent–child pairs.

[52] One reason is that the PSID switched to biennial interviews after 1997, which is the reason I only use incomes from every other year.

[53] The sample sizes for the lower- and upper-bound estimates are 179 and 61, respectively.

[54] Given the number of years covered, relatively few people have average "permanent" incomes of zero. It makes little difference whether they are included in estimating IRAs, but the results I present exclude them.

[55] Sample sizes for the lower and upper bounds are 547 and 105.

[56] Sample sizes for the lower and upper bounds are 214 and 73, respectively.

[57] The sample sizes for the lower and upper bounds are 580 and 78, respectively.

[58] Sample sizes for the lower and upper bounds are 511 and 75, respectively.

[59] Specifically, Mazumder argues that (1) child incomes are measured at too young an age; (2) child incomes are based on too few years of data, which when combined with the fact that they are measured during a period of high unemployment means that too many children must be dropped due to their having no reported income; (3) parent incomes are measured at too old an age; (4) parent incomes are based on too few years of data; (5) parents who do not file are given imputed incomes of $0, which lowers the IRA; (6) incomes do not include government transfers; and (7) administrative data may have more measurement error among low-income people. This latter contention is one with which I do not agree based on my read of the literature. For more criticism of Chetty et al.'s estimates, see Mitnik et al. (2015).

[60] The sample sizes for the lower and upper bounds are 201 and 130 for sons, 215 and 112 for daughters, and 217 and 159 pooled.

[61] A number of studies assess the association between parent and child incomes using nonlinear models or describe the way that different quantiles of child income change as parental income changes. For studies using either rank associations or elasticities in this fashion, see Peters (1992); Eide and Showalter (1999); Minicozzi (2003); Hyson (2003); Fertig (2003); Couch and Lillard (2004); Grawe (2004); Hertz (2005); Bratsberg et al. (2007); Lee et al. (2009); Torche (2013); Chetty et al. (2014); Mitnik et al. (2015); Landerso and Heckman (2016); Bratberg et al. (2017); and Davis and Mazumder (in progress).

[62] See Chetty et al. (2014), for instance, who describe the IGE as the "canonical measure of relative mobility" before rejecting it for the IRA.

[63] In Chetty et al. (2014), they used "absolute mobility" to refer to the expected percentile rank in adulthood of a child raised at the 25th percentile. This measure incorporates not only the rank-correlation (the slope of the regression of child income rank on parent income rank) but the intercept in the regression equation. Because this measure focuses on ranks, it is a relative mobility measure. In Chetty et al. (2016), as we have seen, they properly use "absolute mobility" to refer to the share of children with a higher inflation-adjusted income than their parents at the same age.

[64] While the IRA is the coefficient on parental income rank when child income rank is regressed on it, the IGE is the coefficient on the natural logarithm of parental income when the natural logarithm of child income is regressed on it. Transforming both parent and child incomes by taking natural logarithms allows one to interpret regression coefficients as the percentage change in child income for a one-percent change in parental income. For small percentage changes, this approximation is reasonable, but it becomes decreasingly so for large changes in parental income. The discussion in this paragraph is intended to provide the intuition for treating the IGE as a summary measure of absolute mobility's impact on childhood inequalities. It is not a literal description of the mathematics behind log–log regressions.

[65] Consider the simple table below, where the childhood incomes of a poor and a rich child are represented by A and B, respectively, and their adult incomes are represented by C and D. The ratio of their incomes in childhood is B/A, while the ratio of their incomes in adulthood is D/C. In adulthood, this ratio tends to be smaller, so that f = (D/C) / (B/A) = (DA/CB) < 1. The ratio of adulthood-to-childhood income for the rich child is given by D/B, and it is C/A for the poor child. The absolute mobility experienced by the rich child (expressed as a ratio or, equivalently, in percentage terms) tends to be smaller than the absolute mobility experienced by the poor child, so that 1 > (D/B) / (C/A) = (DA/CB) = f.

	Poor	*Rich*
Childhood Income	*A*	*B*
Adult Income	*C*	*D*

[66] An initial 100 percent difference in incomes translates into a ratio of 2.0. To get the expected percent difference in adulthood, one raises 2.0 to the power *b*, where *b* is the IGE. To get the expected percentage difference in adulthood given a 50 percent difference in childhood, one computes 1.5^b.

[67] The sample sizes for the lower and upper bounds are 243 and 61 when zeroes are included in averages and 296 and 54 when they are excluded.

[68] Sample sizes for lower and upper bounds are 205 and 51.

[69] It is only a bit lower if I exclude years with zero income from the averages (0.90).

[70] The vertical axis in Figure 11 is cut off at 1.00 for sake of presentation.

[71] The sample sizes for the lower and upper bounds are 547 and 131.

[72] Sample sizes: 214 and 73.

[73] The sample sizes for the lower and upper bounds are 214 and 78 for sons and 104 and 65 for daughters.

[74] The results are very similar adjusting parental income for family size.

[75] Sample sizes: 235 and 88 for sons, 297 and 69 for daughters, and 307 and 88 pooled.

[76] Mulligan (1997); Abul Naga (2002); Mazumder (2005a); Gouskova et al. (2010); Chau (2012); Eberharter (2014); Mazumder (2015).

[77] Schoeni and Wiemers.

[78] There has been a wave of recent multigenerational mobility studies. See Solon (2015) and Pfeffer (2014) for reviews.

[79] To see this, return to end note 65, above, and multiply D by 1.2 and C by 0.8. The resulting ratio of ratios is 1.5 times DA/CB rather than DA/CB.

[80] The IGC can be thought of as the IGE after logged parent and child incomes are "standardized"—that is, after the generational means are subtracted from each logged income value and then those "centered" logged incomes are divided by the generational standard deviation of logged income.

[81] The IGC can also be thought of as the IGE multiplied by the ratio of the standard deviation of logged parental income to the standard deviation of logged child income. Of course, the standard deviation is a particular summary measure of inequality, so changes in inequality that are not fully captured by this summary measure can still affect the IGC. It is also worth remembering that what is being controlled for is not how the standard deviation of income changes, but how the standard deviation of *logged* income changes.

[82] The IGC is an indicator of how well the linear IGE predicts income. It may be that a low IGC masks a strong nonlinear relationship between parent and child income, so that if some curvilinear "IGE" were available, it would allow for strong predictions of child income. The estimation of nonlinear summary mobility measures is an active field of research that I ignore in this primer. See end note 61.

[83] Technically, one needs to know the intercept of the regression line too, not just the slope. Without the intercept, one would still be able to predict the size of gaps between children. Further, the prediction would be perfect only if the relationship between parent and child incomes really was linear. If the relationship is curvilinear, then it is possible that the IGC might be estimated as 1, yet most of the predictions made using it would be off the mark.

[84] If "inequality" is measured as the standard deviation of logged income. The "proportion of variance explained" is the square of the IGC rather than the IGC itself, but this is simply a statistical concept, and it is only convention that uses it to express the fraction of inequality in the dependent variable explained by the independent variable. This "coefficient of determination," or "R^2" is the variance of the predicted adult income divided by the variance of actual income. The IGC is the standard deviation of the predicted income divided by the standard deviation of actual income. See Rodgers and Nicewander (1988) and Ozer (1985).

[85] The sample sizes for the lower and upper bounds are 243 and 61, respectively.

[86] The estimates are similar if the earnings of mothers' partners and the non-zero earnings of mothers are used for fatherless children.

[87] Sample sizes: 517 and 52 for the comparison to fathers, 201 and 52 for the comparison to mothers.

[88] The estimates are very similar using size-adjusted family income. Sample sizes: 328 and 78 for the family income versus sons' earnings comparison, 220 and 65 for family income versus daughter earnings, 750 and 130 for sons' family income, 297 and 69 for daughter family income, and 273 and 159 for pooled family income.

[89] IGEs are affected by classical measurement error only if it applies to parental income, while classical error in children's as well as parents' incomes will also diminish the IGC.

[90] A small number of papers relied on surname-based methods before Clark's book was published.

[91] See Torche and Corvalan (2016); Chetty et al. (2014); Vosters (2015); Vosters (forthcoming); Solon (2015).

[92] Gelman (2009).

[93] The foregoing argument is elaborated most fully by Torche and Corvalan (2016), but Chetty et al. (2014) were the first to make it. (See their Appendix D.)

[94] Most contemporary analyses of sibling similarity rely on models that decompose income into a family component and permanent and transitory components of individual income. They then purge the individual transitory component and assess the share of permanent income accounted for by the family component. My sibling similarity analyses instead rely on using multiyear averages as proxies for permanent income, directly estimating sibling permanent income correlations. Note that I match adults to the same-sex sibling nearest in age to them in each survey; in different survey years, adults may be matched to different siblings. Furthermore, adult incomes appear in my data both when matched to a sibling (averaged with other incomes as the adult's permanent income) and when a sibling is matched to them (within the average of his sibling's "sibling permanent income"). Siblings must share at least one biological parent in my analyses.

[95] The sample sizes for the lower and upper bounds are 416 and 76 for brother earnings, 187 and 808 for brother family income, 209 and 65 for sister earnings, and 80 and 804 for sister family income. The SRA estimates are similar if zeroes are excluded from averages and if averages of zero are excluded.

[96] Sample sizes for the lower and upper bounds are 168 and 1734. Results are similar if estimates are unadjusted for family size.

[97] Conley, Glauber, and Olasky (2004).

[98] These interpretations measure inequality by the standard deviation of incomes. In a simple model of sibling similarities commonly used by researchers, the sibling correlation is the share of the variance of permanent income accounted for by the variance of the family component of permanent income that siblings share. Taking the square root of that gives the share of the standard deviation of permanent income accounted for by the standard deviation of the family component. The convention of using the variance to measure inequality is just that—convention. The variance has useful statistical properties, but it squares the units of the quantities being compared (income in this case).

[99] Sample sizes: 563 and 76 for brother earnings, 187 and 808 for brother family income, 540 and 65 for sister earnings, and 204 and 164 for sister family income. The estimates are similar if family incomes are not adjusted for size or if years without earnings or income are excluded from averages. All of these correlations used logged average earnings or income.

[100] Sample sizes: 168 and 1737.

[101] This point has been articulated by Mazumder (2015).

[102] Chetty et al. (2014).

[103] That is, rather than comparing the slopes in the two countries' regression equations, we might prefer estimating the predicted incomes of poor children (which are affected by regression intercepts too). Mitnik et al. (2015) show that the slope and intercept do not actually allow for the estimation of expected income conditional on parent income, if by expected income is meant the arithmetic mean. They offer a new estimand that does allow for such a computation.

[104] If the only thing that were important for child incomes (besides luck and other random factors) were parental incomes, then the sibling correlation would equal the IGC squared. Similarly, the SRA would equal the IRA squared. In the case of the SC, sibling incomes are scaled by the level of inequality before the sibling association is assessed. That means that the higher income inequality created by diminished opportunity in Country A would be "controlled" away, and the sibling correlation might not rise. That would perversely suggest that equality of opportunity had not changed. The SRA avoids this problem because when ranks are used instead of incomes, the level of inequality (in ranks) does not change over time, so scaling by that inequality leaves untouched the increased sibling association produced by the policy changes. The SRA would rise appropriately.

[105] See, e.g., Chetty et al. (2014); Dahl and DeLeire (2008).

[106] Mazumder (2015).

[107] Chetty et al. (2014); Mitnik et al. (2015).

[108] Chetty et al. (2014); Davis and Mazumder (in progress); Bratberg et al. (2017); Dahl and DeLeire (2008).

[109] Winship (2016).

[110] Winship (2013).

[111] See Brown (1996). Furthermore, including the oversample would make my analyses overly reflective of disadvantaged families. The usual solution to this problem is to use survey weights that down-weight the members of the oversample. However, because I average incomes over as many as 31 years (and in different years for each sample member), it is not obvious how to correctly weight the sample. I therefore do not use any weights.

[112] See Winship (2016), Appendix 2 for the superiority of the PCE deflator.

[113] My preferred estimates are always ones that indicate relatively low mobility within similarly restricted samples and within samples of similar sizes. One justification for this choice is that ideal data that included a full 15 years of income for

everyone would likely produce even lower mobility estimates. Another is that the more restrictive the sample, the more missing data there will be, and the more homogeneous the sample is likely to be. That will make relative mobility look stronger than it is by some of my measures. The interaction of missing data and the end points of the PSID creates a specific problem related to homogeneity. For a parent and child to each have 15 years of income data, the parent must turn 40 no sooner than 1981 and the child must turn 40 no later than 1998. The parent in such parent–child pairs would be 17 years old at the time of a child's birth. That means that more restricted samples requiring more years of non-missing income data will tend to feature parents and children closer in age to each other. The samples are likely to be relatively disadvantaged. If parent and child incomes are more strongly related among the disadvantaged, that could create artificially low estimates of mobility. However, in my tests, I found that parental incomes were not especially low in more restrictive samples and not strongly correlated with the degree of restrictiveness or the estimated mobility rate.

[114] Fitzgerald, Gottschalk, and Moffitt (1998a); Fitzgerald, Gottschalk, and Moffitt (1998b); Lillard and Panis (1998); Zabel (1998); Becketti et al. (1988). See Nichols and Zimmerman (2008), however, for evidence that from year to year, attriters are different from those included in volatility samples in terms of the joint distribution of a number of demographic variables.

[115] See Winship (2009).

[116] The estimates I report from previous research are the ones that seem, in my view, to reflect the best methodological choices. Most studies provide a range of estimates—preferred ones, naïve (known to be inferior) ones, and ones from sensitivity checks. In general, authors' preferred estimates tend to show less equalization of opportunity and lower relative mobility than naïve estimates. Sensitivity checks can show estimates higher or lower than the preferred ones. Some subjectivity in summarizing the "best" estimates from each study is inevitable, but those I emphasize are generally ones that are larger, indicating less equalization of opportunity.

This review includes only those cross-national studies and trend studies with well-estimated mobility estimates for the US; these literatures will be reviewed more fully in future installments of the primer.

My review was facilitated by earlier reviews by Solon (1999); Corak (2006); Black and Devereux (2011); Jantti and Jenkins (2014); and Torche (2015).

[117] Zimmerman (1992) used the National Longitudinal Survey–Original Cohort, as did Peters (1992). Mazumder (2005b) used the 1984 Survey of Income and Program Participation, linked to administrative data from the Social Security Administration. Corak, Lindquist, and Mazumder (2014) used the same source, this time drawing from more SIPP panels. Zimmerman (1992) and Peters (1992) exclude years in which the father or son reported having no earnings. Corak, Lindquist, and Mazumder (2014) retain some years where the son has no earnings. It is not clear how Mazumder (2005b) treats zeroes.

Zimmerman (1992) includes a transition matrix for men's hourly wages, finding more upward mobility and less downward mobility.

[118] Pew Charitable Trusts (2012) relied on the PSID, while Dahl and DeLeire (2008) and Corak, Lindquist, and Mazumder (2014) used the SIPP-SSA data. It is not clear how Pew (2012) treats zeroes, but Dahl and DeLeire (2008) retain them. Corak, Lindquist, and Mazumder (2014) include up to three years of $0 earnings in sons' averages, as long as there are two years of non-zero earnings, but they exclude fathers with any zeroes.

[119] Another quintile-based study using the PSID—Fertig (2003)—finds less upward and downward mobility, with 52 percent who were raised in the bottom fifth staying there and 46 percent of those raised in the top fifth remaining there. Fertig includes years with zero earnings.

[120] Mazumder (2005b), using the SIPP-SSA data.

[121] Peters (1992), Fertig (2003), and Dahl and DeLeire (2008).

[122] Fertig's results are quite different but seem implausible. She finds that hardly anyone raised in the bottom or top fifth remains there, whether daughters are compared to fathers or mothers.

[123] Hertz (2006); Isaacs, Sawhill, and Haskins (2008); Pew Charitable Trusts (2012); Bengali and Daly (2013); Eberharter (2014); Acs, Elliott, and Kalish (2016). Hertz and Eberharter exclude years with no income reported. Isaacs, Sawhill, and Haskins; Pew

Charitable Trusts; and Acs, Elliott and Kalish include them. It is unclear how Bengali and Daly treat zeroes. Eberharter's income measure is after taxes and transfer, the rest are pre-tax, post-transfer.

[124] Auten et al. (2013); Chetty et al. (2014). Chetty et al. retain years with no income reported. It is unclear how Auten et al. treat them.

[125] Charles et al. (2014) also estimate a transition matrix for expenditures.

[126] Hertz (2005). It is unclear how he treats years with no income reported, but Hertz (2006) excludes them.

[127] Pew Charitable Trusts (2012).

[128] Isaacs, Sawhill, and Haskins (2008); Bengali and Daly (2013); Acs, Elliott, and Kalish (2016).

[129] Bengali and Daly (2013).

[130] Pew Charitable Trusts (2012).

[131] Pew Charitable Trusts (2012) examine absolute wealth mobility, finding that half of adults exceed their parents' wealth. That includes 72 percent of those raised in the bottom fifth, 55 percent of those starting in the middle fifth, and 25 percent of those with parents in the top fifth.

[132] Dahl and DeLeire (2008), using the 1984 SIPP linked to the SER and DER, report a father–son IRA ranging from 0.29 to 0.4. Corak, Lindquist, and Mazumder (2014) report a father–son IRA of 0.3 using multiple SIPP panels linked to the SER. This estimate is probably biased downward due to their primary goal of making it comparable to Canadian and Swedish estimates based on other data. Using the NLSY79, Bratberg et al. (2017) report a father–son IRA of 0.4. Dahl and DeLeire (2008) report the father–daughter IRA.

[133] Chetty et al. (2014) find an IRA of 0.31 for sons, while Bratberg et al. (2017) find 0.40 (using ranks that include daughters too, however). Chetty et al. (2014) report an IRA of 0.25 for daughters. Landerso and Heckman (2016) report an IRAof 0.23-0.32 pooling sons and daughters, with the lower estimate excluding observations of $0. Bratberg

et al. (2017) report a pooled IRA of 0.40. Landerso and Heckman (2016) find larger IRAs using child individual or market income instead of earnings, ranging from 0.27 to 0.37.

[134] See Chetty et al. (2014) for estimates for sons, daughters, and children pooled together. Mazumder (2015) provides estimates for sons. Davis and Mazumder (2016) provide estimates for sons and daughters separately.

[135] In addition to these studies, several have addressed the persistence of wealth inequalities or otherwise looked at wealth mobility, including Mulligan (1997), Charles and Hurst (2003), Conley and Glauber (2008), Pew Charitable Trusts (2012), and Pfeffer and Killewald (2016).

[136] Sewell and Hauser (1975); Behrman and Taubman (1985); Bielby and Hauser (1977); Becker and Tomes (1986)

[137] Behrman and Taubman (1990) and Solon (1992). Behrman and Taubman pooled sons and daughters while Solon focused on sons.

[138] The study is Zimmerman (1992). Grawe (2004) analyzes the NLSOC and PSID and finds that no more than half the difference between the two can be attributed to the older age of fathers in the NLSOC when their incomes are measured. Couch and Lillard (1998) analyze the NLSOC and PSID and produce comparable estimates restricted to men who work fulltime, year-round. They measure father earnings in 1970 (when they are 54 on average in the NLSOC and 44 in the PSID) and son earnings in either 1980 (NLSOC) or 1984 (PSID), when they are 32 on average in the NLSOC and 29 in the PSID). They find that the IGE from the NLSOC is 0.26, half that in the PSID (0.52). The highest of 10 estimates from the NLSOC that measure father earnings between 1965 and 1970 is 0.37. The lowest of 6 estimates from the PSID that measure father earnings between 1968 and 1970 is 0.48 (and the highest is 0.55).

It is worth pointing out that the NLSOC sample must be created by linking fathers in the National Longitudinal Survey of Older Men with their sons in the National Longitudinal Survey of Young Men. These two surveys began at different times, meaning that some sons or fathers may have moved out by the time the sons were interviewed. Other technical details of the sampling process for sons also may create lower IGEs in the NLSOC.

[139] Peters (1992); Altonji and Dunn (1991). For sons the estimates ranged from 0.14 to 0.22. For daughters, they ranged from 0.13 to 0.34.

Solon (1992), Zimmerman (1992), and Altonji and Dunn (1991) all showed IGE estimates for hourly wages as well, which were generally similar to the corresponding earnings IGEs.

[140] The four with IGEs below 0.3 were Lillard and Reville (1996), Couch and Dunn (1997), Shea (2000), and Fertig (2003). Couch and Dunn (1997) average six years of earnings for fathers and children, but they include years without earnings in the averages. This is a problem because their sample includes college-age children, and because they measure earnings in the 1980s for both generations, which makes the fathers relatively old. Both students and retirees can be expected to have several years without earnings, so mobility looks stronger than it is. Shea (2000) controls for race in his regression model (and includes parametric controls for age). Lillard and Reville (1996) was an unpublished paper.

The other two PSID-based papers with IGEs no higher than 0.39 are Buron (1994) and Eide and Showalter (1999). The three with IGEs above 0.5 are Bjorklund and Jantti (1997); Couch and Lillard (1998); and Abul Naga (2002). Other PSID-based papers during this period include Reville (1996); Mulligan (1997); and Hyson (2003).

[141] Couch and Dunn (1997); Minicozzi (1997); Shea (2000); Hyson (2003). Couch and Dunn is the outlier and is too low. See the previous note.

Reville (1995), Mulligan (1997), and Shea (2000) included IGEs for men's hourly wages, and Shea (2000) included IGEs for daughters and with sons and daughters pooled. Shea also included IGEs comparing parent family income to child hourly wages.

[142] Mulligan (1997); Shea (1997); Shea (2000); Abul Naga (2002); and Chadwick and Solon (2002). Estimates for sons range from 0.47 to 0.63. See Mulligan (1997); Shea (1997); Chadwick and Solon (2002). Estimates for daughters range from 0.39 to 0.49. See Shea (1997); Chadwick and Solon (2002). Pooled estimates range from 0.3 to 0.78. See Mulligan (1997); Shea (1997); Shea (2000); Abul Naga (2002).

Mulligan (1997) includes estimates for consumption too. Charles et al. (2014) estimate IGEs for expenditures.

[143] Shea (1997); Couch and Lillard (1998); Eide and Showalter (1999); Shea (2000). Estimates for sons ranged from 0.37 to 0.45. See Shea (1997); Couch and Lillard (1998); and Eide and Showalter (1999). The single study for daughters (Shea, 1997) produced an estimate of 0.54. Shea (1997) and Shea (2000) produced estimates of 0.47 pooling sons and daughters. One other PSID-based study produced a lower estimate—0.29 (Levine and Mazumder, 2002).

[144] For the low IGE (0.15), see Grawe (2004). For the high IGE (0.37), see Couch and Lillard (1998).

[145] See Mazumder (2005a); Gouskova et al. (2010); Muller (2010); Chau (2012); Mazumder and Acosta (2015); and Mazumder (2015). Only Muller (2010) has a range entirely below 0.55, and Mazumder and Acosta (2015) estimate an IGE below 0.6. The two that include IGEs above 0.7 are Gouskova (2010) and Mazumder (2015).

Torche (2011) includes IGEs comparing fathers' hourly wages to sons' and daughters' hourly wages.

[146] The estimates for are remarkably similar in these two studies: 0.45 to 0.65 in Mazumder (2005a) and 0.48 to 0.63 in Dahl and DeLeire (2008). A third study using tax data estimates an IGE of 0.4, but this estimate is low because of the authors' attempting to make it consistent with estimates from Canada and Sweden. See Corak et al. (2014).

[147] Hertz (2006); Torche (2011); Rothwell and Massey (2014); Eberharter (2014); Mazumder (2015); and Schoeni and Wiemers (2015). Hertz, Eberharter, Rothwell and Massey, and Schoeni and Wiemers pool sons and daughters, while Mazumder looks at sons. Torche looks at sons and daughters separately. Four other PSID studies are primarily concerned with estimating trends at the expense of producing the best level estimates. They find lower IGEs. See Mayer and Lopoo (2005), Harding et al. (2005), Lee and Solon (2009), and Hertz (2007) (though Hertz shows estimates as high as 0.64).

[148] Chetty et al. (2014).

[149] See Mazumder (2015) and Mitnik et al. (2015). There are several issues involved. One is that Chetty's data allows him to only use a small number of years of income

observations, which makes for noisy estimates of permanent income and which, given the young age at which children's incomes are observed, makes his estimates sensitive to observations with no income. Second, because of the young age of the adult children and the old age of parents in the data, life-cycle bias pushes the estimates downward. Mazumder shows that the checks implemented by Chetty et al. are inadequate for showing that their estimates are unbiased.

[150] Torche (2016) reports the lower estimates. The higher ones are from Davis and Mazumder (2016).

[151] Landerso and Heckman (2016). Levine and Mazumder (2002).

[152] The NLSY79 studies include Levine and Mazumder (2002); Mazumder (2005b); Jantti et al. (2006); Bratsberg et al. (2007); and Raaum et al. (2007). The latter three have considerable overlap in authorship. The tax study is Mitnik et al. (2015). Levine and Mazumder (2002) also report IGEs from the NLSOC ranging from 0.21 to 0.24.

[153] Jantti et al. (2006); Raaum et al. (2007).

[154] Mitnik et al. (2015).

[155] Bratberg et al. (2017).

[156] The estimates are 0.22-0.39 (Altonji and Dunn, 1991), 0.2 (Peters, 1992), 0.42 (Reville, 1996), 0.31-0.41 (Bjorklund and Jantti, 1997), 0.53 (Couch and Dunn, 1997), 0.2 (Fertig, 2003), and 0.26 (Corak et al., 2014).

Two studies estimate the IGC in hourly wages: Zimmerman (1992) finds a range of 0.31 to 0.41 (with a sample restricted to men who worked fulltime year-round). Altonji and Dunn (1991) estimate a range of 0.32 to 0.42 for sons, 0.31-0.43 comparing fathers and daughters, and 0.25-0.35 comparing mothers and daughters.

[157] Father–daughter estimates include 0.21-0.42 (Altonji and Dunn, 1991), 0.1 (Peters, 1992), 0.14 (Couch and Dunn, 1997), and 0.02 (Fertig, 2003).

[158] The range is from Altonji and Dunn (1991) while the implausibly low estimate is from Fertig (2003).

[159] The estimates include 0.3-0.56 (Altonji and Dunn, 1991), 0.3-0.33 (Peters, 1992), and 0.42-0.43 (Hertz, 2006).

[160] The NLSOC studies include Altonji and Dunn (1991) and Peters (1992). The PSID studies include Reville (1996), Bjorklund and Jantti (1997), Couch and Dunn (1997), Hertz (2006), and Landerso and Heckman (2016). Jantti et al. (2006) use the NLSY79 and Corak et al. (2014) use the Survey of Income and Program Participation (SIPP) linked to Social Security data.

[161] Conley, Glauber, and Olasky (2004).

[162] Bound et al. (1986); Corcoran and Datcher (1981); Corcoran and Jencks (1979); Griliches (1979).

[163] Altonji (1988); Altonji and Dunn (1991); Solon et al. (1991); Ashenfelter and Zimmerman (1997). Altonji and Dunn (1991) and Solon et al. (1991) look at sisters. Altonji and Dunn (2000) extends their earlier paper.

[164] Bjorklund et al. (2002); Page and Solon (2003); Conley, Glauber, and Olasky (2004); Conley and Glauber (2008); Mazumder (2008b); Mazumder (2011); Schnitzlein (2014). Levine and Mazumder (2002) estimate lower brother correlations in earnings and family income using the NLSOC.

[165] Conley, Glauber, and Olasky (2004); Conley and Glauber (2008); Mazumder (2008b); Mazumder (2011). Conley, Glauber, and Olasky also estimate at sibling correlation pooling brothers and sisters of 0.36 to 0.43.

[166] The PSID estimates are Conley and Glauber (2008); Mazumder (2011); and Schnitzlein (2014). The NLSY study is Mazumder (2008b).

About the Author

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