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A VERY UNEVEN PLAYING FIELD:

ECONOMIC MOBILITY IN THE UNITED STATES[*]

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Abstract

We present results from a new data set, the Statistics of Income Mobility Panel, that has been assembled from tax and other administrative sources to provide evidence on economic mobility and persistence in the United States. This data set allows us to take on the methodological problems that have complicated previous efforts to estimate intergenerational earnings and income elasticities. We find that the elasticities for women's income, men's income, and men's earnings are as high as all but the highest of the previously reported survey-based estimates. Because the intergenerational curves are especially steep within the parental-income region defined by the 50th to 90th percentiles, approximately two-thirds of the inequality between poor and well-off families is passed on to the next generation. This extreme persistence cannot be attributed to any single factor. Instead, the U.S. is exceptional with respect to virtually all factors governing economic persistence, including the returns to human capital, the amount of public investment in the human capital of low-income children, the amount of socioeconomic segregation, and the progressiveness of the tax-and-transfer system. For each of these four factors, the U.S. has opted for policies that are mobility-reducing, with the implication that any substantial increase in mobility will likely require a wide-ranging package of reforms that cut across many institutions.

The ideal of equal opportunity has played a central role in U.S. history, with various formulations of this ideal appearing in drafts of the country's founding documents (Reeves 2015), fictional odes to the "American Dream" (e.g., Alger 1868), and a long line of political philosophy (e.g., Dworkin 2000; Rawls 1999; Roemer 1998). Because equal opportunity is arguably one of the country's most fundamental values, and because measures of economic mobility and persistence have long been viewed as especially important indicators of opportunity, much depends on delivering high-quality measurements of economic mobility and persistence.[1] The simple purpose of this paper is to provide just such measurements and to use them to assess our options for increasing economic mobility.

It might be thought that, given the amount and prominence of recent research on economic mobility, we are already awash in high-quality measurements and that little is left to be done. That would be a mistaken conclusion. We will show, to the contrary, that existing estimates fall short in various ways and that the problems cannot be addressed without improving on existing approaches for measuring persistence. After introducing these improvements and describing the new data set used to implement them, we will provide evidence on (a) the extent of intergenerational persistence for women and men, and (b) the relative importance of credit constraints and complementarities between parental investments and other factors (e.g., neighborhood quality) in explaining persistence. We will conclude by interpreting these results with a theoretical model of persistence that reveals the type of policy approach that would be needed to substantially increase equality of opportunity in the U.S.

How could there still be an evidence deficit despite decades of research on economic mobility? The evidence coming out of surveys on mobility is problematic because the samples are small, attrition is high, income reports are unreliable, and the full income distribution is typically not well covered (especially the top of the distribution). For these and other reasons, administrative data

have been increasingly used to examine economic mobility, with Mazumder (2005) and Chetty et al. (2014a; 2014b; 2017; 2018a; 2018b) reporting especially influential results. We will show that this new stream of administrative-data research, although immensely important, faces its own set of difficulties and biases. Because these problems have made it difficult to estimate intergenerational earnings and income elasticities, some scholars have now turned away from analyses based on elasticities, even though they have long served as workhorse measures of economic mobility (e.g., Solon 1999; Black and Devereaux 2011; Corak 2006) and are especially useful for theoretical and policy analyses (e.g., Benabou 2000; Durlouf an Seshadri 2018; Mitnik 2018; Solon 2004). The ongoing turn to administrative data has not, then, solved all or even most of the problems of interest.

The evidence deficit is partly attributable to a data deficit. We thus rely here on a new data set, the Statistics of Income Mobility (SOI-M) Panel, that is based on tax and other administrative data. The SOI-M panel can be used to provide new benchmark estimates of intergenerational elasticities in the present-day U.S. and, in the course of doing so, speak to some of the most important unanswered questions about economic mobility and the transmission of economic advantage across generations. We examine whether past analyses have understated the amount of reproduction because of selection bias and other methodological problems, whether there are regions of the parental-income distribution in which intergenerational persistence is especially high, and whether intergenerational persistence differs by gender. We then consider the implications of this new portrait of intergenerational reproduction for efforts to equalize opportunity in the U.S.

In most of our analyses, we will measure persistence with intergenerational income and earnings elasticities (IGEs), which refer to a child's percent increase in expected income or earnings given a one percent increase in the income of her parents.[2] There is of course a long history of studying intergenerational mobility that encompasses a wide variety of measurement approaches (see Fox, Torche, and Waldfogel 2016; Grusky and Cumberworth 2010; Jäntti and Jenkins 2015). The IGE has, however, played an especially important role because it provides a very concrete

quantification of the extent to which money affects outcomes and because it can be easily expressed in terms of structural parameters that identify levers for social policy (e.g., Solon 2004).[3] For these reasons and others, the IGE has long had a prominent role in basic research, public-policy discussions, international comparisons, and "Great Gatsby" analyses of the effects of income inequality (e.g., Corak 2006, 2013; Björklund and Jäntti 2011; Blanden 2009; Helsø 2018; Solon 2002).

We will focus laser-like on the task of providing benchmark estimates of IGEs because much rides on doing so. If intergenerational persistence is very high, it may explain why many Americans feel left out and are disaffected, why they may be unwilling to treat current levels of economic inequality as the outcome of a fair and open competition, and why they are rejecting business-as-usual economic policy (see Manza and Brooks 2016; Hochschild 2016). We also look to mobility statistics to understand the extent to which the country's existing social and economic policies fall short. To this point, the intergenerational elasticity measure has yielded a wide range of estimates, indeed the range is wide enough to sustain at once the conflicting views that (a) the departure from equal opportunity, while inconsistent with the high ideals of the "American Dream," is at least no worse than what prevails in most other late-industrial countries, or (b) the departure is so large that a fundamental rethinking of our policies is indicated. If the latter were the case, no one would be served by pretending that our existing reform efforts are adequate to the task.

Although U.S. "mobility policy" is complex, most of this complexity can be understood as variations around an overriding focus on programs intended to increase human-capital formation among children of low-income families. These programs either provide low-income parents with the means to invest in their children (e.g., child tax credits, earned income tax credits) or directly provide services that are targeted to low-income children (e.g., home visiting programs, means-tested early education) or are distributed universally to all children (e.g., free public education). It is not always appreciated that, although our *explicit* mobility policy is narrowly focused on human-capital

formation, the U.S. also has a great many other policies, like "residential segregation policy," that are very consequential for persistence and might thus be termed implicit mobility policy. We have chosen to conduct our analyses with the intergenerational elasticity in part because it can be used to reveal the effects of a full range of such explicit and implicit policies—not just human-capital policy—on persistence. We will use this feature to examine the policies that could bring U.S. persistence into line with the country's avowed commitment to equality of opportunity.

Because estimates of intergenerational persistence speak to the distribution of opportunity, a distribution that is especially central to the "self-image" of the U.S., it is important to address all of the methodological problems that have historically made it difficult to deliver such estimates. It is not enough to take on some of these problems and to ignore others. The current estimates range so widely in part because previous studies have taken on partial subsets of the full complement of methodological problems. If we want credible estimates, we need to turn to a new data set and develop improved methods that make it possible to credibly address all methodological problems at once. We note below four key issues that will require special attention (although these issues do not exhaust our efforts to improve the methodological foundations of mobility research).

Estimating the correct IGE. The conventional approach to estimating the IGE has rested on a misinterpretation of what is being estimated. In almost all cases, mobility scholars have assumed that they are estimating the elasticity of the expectation of children's earnings or income, whereas they actually are estimating the elasticity of the geometric mean of children's earnings or income (Mitnik and Grusky 2017). We use a new approach that recovers the estimand that scholars have long intended to estimate (see Mitnik and Grusky 2017 for a detailed justification of this approach).

Avoiding selection bias. As an unfortunate by-product of the conventional approach, it has been standard practice to drop offspring without adult earnings or income, a practice that can be expected to generate substantial selection biases (Mitnik and Grusky 2017). In our analyses with the correct estimand, it is no longer necessary to drop those children, thus making it possible to estimate

elasticities pertaining to the full population. The resulting estimates, which are very robust, also resolve recent concerns about the marked sensitivity of tax-data estimates of the conventional elasticity to the treatment of nonfilers (Chetty et al. 2014a).

Nonlinearities. Although mobility scholars have widely assumed that the elasticity is constant across levels of parental income, this assumption has been adopted more as a matter of necessity (given the small available samples) than by virtue of any strong prior that it in fact holds. It is an open question whether the many social and economic forces in play do in fact combine to produce a relationship between parental income and the income of children that is well approximated by a straight line (in log-log space). We address this question by estimating nonparametric and spline models and then use the results to compute persistence measures that do not rely on the constant-elasticity assumption.

New types of intergenerational elasticities. We then take into account these nonlinearities by defining and estimating new "region-specific IGEs" that characterize the extent of persistence in particular zones of the parental-income distribution. We also introduce new elasticities that allow us to estimate, for the first time, the level of economic persistence between families that are far apart in the income distribution. The traditional "point elasticities," although useful for many purposes, compare families that are close together in the income distribution and thus cannot speak to the amount of economic persistence across long distances.

These methodological advances, when applied to a recently-built administrative data set (see Mitnik et al. 2015), allow us to provide state-of-the-art estimates of intergenerational elasticities. Because of recent methodological problems with IGE-based analyses, there has been increasing interest in alternative mobility measures, especially the rank-rank slope (a measure of positional mobility). There are surely research situations in which the rank-rank slope provides answers to the most important questions at stake. There are, however, also many situations in which the concreteness of a dollar-based measure, and its easy embedding within theoretical models of

intergenerational processes (e.g., Benabou 2000; Durlauf and Seshadri 2018; Mitnik 2018; Solon 2004), is invaluable. For these research situations, we need a new line of IGE-based analyses that are consistent with the interpretations wrongly attached to the old IGE, that solve the selection bias problem, and that can accommodate nonlinearities. The analyses presented here satisfy all of these strictures.

What do we find? The results from our analyses will show that intergenerational persistence is substantially higher than some of the most prominent administrative-data analyses (Chetty et al. 2014a; Chetty et al. 2014b) have recently suggested. We will show that (a) approximately half of economic advantages are transmitted from parents to children (when we average across all levels of parental income), and that (b) about two-thirds of the economic differences between well-off and poor families reappear among their children. These results make it clear that the U.S. has a profound mobility problem and that any authentic effort to address it will require a much more substantial policy response than has been evinced to date. We will also show that the constant-elasticity assumption typically invoked in the literature conceals that the intergenerational curves are actually convex and indicate a very high level of economic persistence *within* the "middle-upper class" region. This suggests that complementarities may be playing an important role in the transmission of economic advantages from parents to children.

The balance of the paper is organized as follows. We first review the literature on U.S. elasticities, examine why the range of estimates is still so wide, and ask why there is still a troubling deficit of evidence on other key features of the mobility process. We next discuss how the shape of intergenerational curves can inform our understanding of the role of credit constraints and complementarities in the mobility process. After completing this review of the literature, we introduce the SOI-M Panel, describe the sample and variables employed in our analyses, and discuss the methodological problems that our data solve. We then introduce our estimators, report our estimates of global elasticities, explore the effects of selection bias, provide estimates of region-

specific and "long-distance" elasticities, conduct robustness checks, and examine the shape of intergenerational curves. We conclude by discussing the policy levers that could be used to reduce economic persistence in the U.S.

The Evidence Deficit

Given how central IGEs have been in previous research, and given their role in informing international comparisons and policy, it is unfortunate that the available evidence does not allow us to establish the size of the most important IGEs with confidence. The reasons for this state of affairs are best appreciated by examining how our understanding of economic mobility and persistence have shifted, often quite dramatically, as results on intergenerational elasticities have accumulated. Although one might have hoped that the use of administrative data would have settled matters and provided more definitive results, we will show that, at least as regards the size of key IGEs, this has not happened. The latest wave of administrative-data analyses has instead yielded new results that, while immensely important, nonetheless raise as many questions as they answer.

The first stream of research on IGEs, which began some 40 years ago, suggested intergenerational correlations and IGEs of approximately 0.2 (or even less), a value that implies that only one-fifth of the inequality in origin incomes are passed on to sons (e.g., Sewell and Hauser 1975; Behrman and Taubman 1985; Becker and Tomes 1986:Table 1; Becker 1988).[4] This early research thus led to the consensus view that the U.S. is a quite mobile society. The consensus view started to shift, however, when Solon (1989) showed that previous estimates had been downwardly biased by the use of homogeneous samples and attenuation bias, where the latter refers to the bias resulting from measurement error in parental earnings or income. The growing availability of representative samples with repeated measures of parental income and earnings allowed analysts to reduce that bias by using averages of parental annual measures.[5] The resulting IGE estimates increased as expected: The earnings IGEs for men from the Panel Study of Income Dynamics (PSID)

and from the National Longitudinal Surveys (NLS) both came in at approximately 0.4 (Solon 1992; Zimmerman 1992).

The ensuing stream of research on the IGE of men's earnings generated estimates that were not always consistent with an IGE of about 0.4. As this research developed, different samples were often drawn from the same data sources, and alternative estimators and specifications were sometimes employed, with the result that a wide range of estimates was obtained. In Solon's (1999) and Corak's (2006) reviews of research based on the PSID and NLS, the post-1990 estimates of the men's earnings IGE range from 0.13 to 0.54, a dispiritingly large range.[6] As both Solon and Corak nonetheless stressed, much of this variability could be attributed to the various biases identified in the literature, including (a) attenuation bias (as discussed above), (b) the lifecycle biases that result from measuring earnings when children or parents are either too young or too old to reflect lifetime differences well (e.g., Mazumder 2005, pp. 236-240), and (c) the instrumental-variable bias likely to result when an invalid instrument (e.g., father's education) is employed to estimate the IGE (Solon 1992, Appendix; see also Mitnik 2017b, pp. 8-10). After taking into account these various biases, Solon concluded that "all in all, 0.4 or a bit higher … seems a reasonable guess of the intergenerational elasticity in long-run earnings for men in the United States" (1999, p. 1784).[7] Based on this assessment of the men's earnings IGE and on similar (but much sparser) results for men's and women's income IGEs (e.g., Solon 1992; Mulligan 1997; Chadwick and Solon 2002), a new consensus view that the U.S. is quite immobile developed and consolidated.[8]

This consensus nonetheless papered over real ambiguities in the survey evidence. It is troubling, for example, that the PSID and NLS tend to produce systematically different results: The estimates from the NLS are generally lower than those from the PSID when early cohorts are analyzed, whereas the opposite pattern obtains when later cohorts are studied (Corak 2006, p. 53).[9] The PSID and NLS surveys are further affected by an unusually long list of additional problems or limitations: (a) neither survey covers the institutionalized population (e.g., people in prison); (b) the

PSID only collects full income information for household heads and their spouses; (c) post-1968 immigrants and their descendants are not represented in the PSID samples available to study intergenerational mobility; (d) both surveys are affected by substantial attrition;[10] (e) both surveys can only deliver small samples (with some estimates based on one hundred observations or less); (f) neither survey covers the upper tail of the income and earnings distributions well; (g) neither survey provides enough years of parental information to address attenuation bias satisfactorily;[11] and (h) neither survey allows after-tax measures of income to be reliably computed.

The third research stream on IGEs, which is based on administrative data, is marked by the publication of Mazumder's (2005) well-known article. By matching the Survey of Income and Program Participation (SIPP) to Social Security Administration (SSA) earnings records, Mazumder was able to average parental earnings over many more years than before, yielding estimated elasticities of approximately 0.6 for both men and women. These results thus suggested that (a) the true value of the earnings IGE is markedly larger than previously believed, and (b) the downward bias due to measurement error is even more substantial than previously reported (with the implication that the true value of the IGE can only be recovered by using many years of parental information). This research has been very influential. Although its results were consistent with the previous "consensus view" that immobility was quite high, it nonetheless led to a substantial upward recalibration of the consensus IGE value. In his 2006 review of the literature, which was strongly influenced by Mazumder's work (see Online Appendix A), Corak selected 0.47 as his "preferred estimate" of the IGE of men's earnings. In 2008, Solon updated his previous assessment, concluding that once all "downward biases in the estimation of the intergenerational elasticity are considered, it becomes plausible that the intergenerational elasticity in the United States may well be as large as 0.5 or 0.6" (Solon 2008, p. 4).

It may then seem that, with Mazumder's (2005) very important and influential contribution, administrative data have delivered on their promise and, at the very least, have established a hard

lower bound for the true value of U.S. IGEs (if only for earnings). There have, however, been two important developments since Mazumder's research that strongly militate against this conclusion. The first of these developments is a paper by Dahl and DeLeire (2008) revealing that estimates based on SSA earnings data are very sensitive to the choice of sample and different ways of treating fathers with zero reported earnings. The Dahl-DeLeire estimates range from 0.26 to 0.63 for men and from 0 to 0.27 for women.[12] Although the estimates for men might be consistent with those from Mazumder (2005), the estimates for women clearly cannot be. Even for men, it is troubling that the IGEs vary so much across different samples and different ways of computing parental earnings, a result that raises questions about the robustness of the estimates provided by Mazumder.[13] The Dahl-DeLeire results also call into question Mazumder's finding that the earnings IGEs for men and women are similar. There are good reasons to expect, just as Dahl and DeLeire's (2008) results suggest, that the earnings IGE for women should be smaller than that for men. In a later section, we will be discussing these "good reasons" for expecting a gender gap, but for now it is important to flag that Mazumder's (2005) results are inconsistent with them (although his results are not entirely unprecedented in the survey-based literature).

The second post-Mazumder development of interest is the release of the influential Chetty et al. (2014a) study.[14] This study has cast doubt on the previously accepted conclusion that measurement error in parental income can only be addressed by using many years of parental income. Although this research is based on tax data and focuses on intergenerational mobility in family income, it is still relevant that Chetty et al. (2014a) reported that attenuation bias essentially disappeared when as few as five years of parental information were used. According to Chetty et al. (2014a, Online Appendix E), Mazumder's (2005) decision to impute father's earnings with measures of race and education was tantamount to resorting to instrumental-variable estimation, which is expected to yield unduly high elasticities (as noted above). Because Mazumder (2005) was obliged to rely disproportionately on imputed data when including additional years of father's earnings, Chetty

et al. (2014a) argued that the *appearance* of substantial attenuation bias was accordingly created (see Chetty et al. 2014a, Online Appendix E; cf. Mazumder 2016).

The administrative-data analyses of Chetty et al. (2014a) also revealed the fragility of IGE estimates (as Dahl and DeLeire 2008 also reported). For Chetty et al. (2014a), such fragility emerged when addressing the "non-filer problem," which arises when (a) children do not file (and therefore no tax return is available for them), and (b) other possible sources of administrative information on their income are unavailable as well.[15] When Chetty et al. (2014a) adopted different assumptions about such missing income (all consistent with it being very low), the resulting IGE estimates varied widely, a result that in turn motivated Chetty et al. (2014a) to conduct the bulk of their analyses using the rank-rank slope instead of the IGE. At the same time, Chetty et al. (2014a) do report a preferred IGE income estimate that is as low as 0.34 (for men and women pooled), a surprisingly low estimate that has not attracted as much attention as it should. The latter estimate, which was obtained by dropping children without income data, suggests a level of persistence that is (a) much closer to the very low estimates coming out of the first round of research on elasticities in the U.S., and (b) very close to the intergenerational persistence found in other late-industrial countries.[16]

Is this low estimate on the mark? In a recent paper, Mazumder (2016) argued that it is affected by attenuation and lifecycle biases, a conclusion based in part on his new PSID-based estimates of the IGE of men's family income and earnings that are as high as 0.68 and 0.75 respectively (when 15 years of parental information are used).[17] Likewise, Mitnik (2017b) shows that, when 25 years of parental information are used, the PSID estimate of the family-income IGE comes in very high. Because these estimates are based on relatively recent cohorts, they are affected by substantial attrition and involve small samples, thus reducing our confidence in the results. In other recent NLS-based studies, very high IGEs have again been reported, although they are so high—when adjusted to account for attenuation bias—that it becomes unclear whether they can be relied upon.[18]

The foregoing suggests substantial disarray on the seemingly simple matter of the size of U.S. income and earnings IGEs. The consensus forged over time to the effect that U.S. IGEs are very high seems now to rest on a rather fragile foundation. As large-sample and high-quality administrative data have increasingly been analyzed, new low-end IGE estimates of 0.26 (men's earnings) and 0.34 (family-income, men and women pooled) have emerged, and once commonly-accepted arguments about the effects of attenuation bias are now in dispute.[19] The overall effect of the increased use of administrative data has, paradoxically, been to increase the range of prima-facie plausible estimates.

There is also substantial disarray on the matter of gender differences in earnings IGEs. As already indicated, there are conflicting conclusions coming out of the two articles using administrative data to compute earnings IGEs, with Mazumder (2005) reporting similar IGEs for men and women and Dahl and DeLeire (2008) reporting substantially higher IGEs for men than for women (across their various specifications). There are likewise conflicting results in the survey-based literature, with some researchers reporting broadly similar IGEs for men and women (Altonji and Dunn 1991; Peters 1992), but others reporting either a higher value for women (Shea 2000) or a higher value for men (Fertig 2003; Minicozzi 1997; Jäntti et al. 2006; Raaum et al. 2007).[20]

It follows, then, from our review that there is a quite troubling "evidence deficit" on U.S. IGEs. The post-1990 survey estimates of income and men's earnings IGEs range very widely and are sensitive to operational decisions, inconsistent across data sources, difficult to conciliate with existing evidence on the magnitude of attenuation bias, and often based on strikingly small, and possibly unrepresentative, samples. The only administrative-data estimates of family-income IGEs (Chetty et al. 2014a, 2014b) are very likely affected by lifecycle and attenuation biases and are very sensitive to assumptions about the income of children with missing income data. The administrative-data estimates of earnings IGEs (Mazumder 2005; Dahl and DeLeire 2008) are likely affected by instrumental-variable bias or are disturbingly sensitive to the treatment of zero reported earnings.

Finally, there has been surprisingly little research on the IGE of women's earnings, and such research as is available provides conflicting evidence, with some studies suggesting that earnings IGEs for men and women are the same and others suggesting that they differ. The main purpose of our paper is to present benchmark estimates of economic mobility that rest on new data and new semiparametric and nonparametric models that allow us to overcome the various methodological problems affecting previous estimates.

The Shape of the Intergenerational Curve

It is especially important that our benchmark estimates will not be based on the conventional assumption that the intergenerational relationship takes a simple linear form (in log-log space). Because of sample-size constraints, relatively few researchers have tested for nonlinearities with the NLS (Lillard 2001; Bratsberg et al. 2007; Couch and Lillard 2004) or the PSID (Solon 1992; Behrman and Taubman 1990; Mulligan 1997). On balance, these studies suggest that the IGE increases with parental income (i.e., a convex curve), but their assessments are based on restrictive parameterizations and are inconclusive in other ways (see Online Appendix B for details). By contrast, Chetty et al.'s (2014a) recent study has been more conclusive, as it is based on nonparametric methods applied to population-level tax data. This study shows that the family-income curve is for its most part convex when the data for women and men are pooled (Chetty et al. 2014a, Figure 1; see also their Online Appendix Figure 1). Although this is a very important result, the analysis nonetheless needs to be extended by examining (a) whether there are also nonlinearities in the earnings curve, (b) whether the nonlinearities take the same form for women and men alike, and (c) whether the same pattern of nonlinearities also appears for older offspring.

Why are these additional analyses of interest? The first point to be made is that nonlinearities cannot be ignored even when the objective is simply to provide a single-value summary measure of intergenerational persistence. When departures from linearity are large, a constant-IGE estimate cannot be "saved," in other words, by reinterpreting it as the average IGE across levels of parental

income, as the latter is neither equal to nor necessarily well approximated by the former (as we will show). It follows that cross-national or over-time comparisons may mislead when they rely on summary measures that wrongly assume that a straight line (in log-log space) adequately characterizes the intergenerational relationship (Bratsberg et al. 2007).

We also care about the shape of the intergenerational curve because it casts light on the mechanisms underlying economic persistence and thus the policies that are likely to reduce it. If one is willing to assume that the underlying "constraint-free" and "complementarity-free" curve is linear, then the empirically observed shape of the curve allows us to adjudicate between (a) the hypothesis that credit constraints lead low-income parents to under-invest in the human capital of their children (i.e., the "credit-constraint hypothesis"), and (b) the alternative hypothesis that low-income families live in disadvantaged neighborhoods and other social contexts that reduce the returns to investing in human capital (i.e., the "complementarities hypothesis"). We have presented the stylized curves pertaining to each of these two hypotheses in Figure 1. The y-axis in this figure pertains to men's earnings because, as will become evident, the mechanisms behind these hypotheses are more relevant for individual earnings than for family income and are likely to be more pronounced for sons than for daughters.

The top intergenerational curve in Figure 1, a concave curve, has the slope declining as parental income increases. Although a concave curve of this sort could be the result of many mechanisms, a leading hypothesis is that it arises because low-income parents cannot borrow the money needed to make optimizing human-capital investments in their children (e.g., Becker and Tomes 1986; Corak and Heisz 1999; Bratsberg et al. 2007).[21] For many low-income families, loans for college tuition and expenses are available, but there aren't equally well-developed markets for borrowing the money needed to buy high-quality childcare and early childhood education, high-quality primary and secondary education, and after-school training and test preparation. Moreover, these types of human capital are often not directly purchased, but instead are part of a "package deal"

in which parents secure access to high-quality education and advantageous networks by buying or renting a home in an expensive neighborhood.[22]

The intergenerational curve should take a concave form insofar as the "credit-constraint hypothesis" is correct. If low-income parents cannot afford the optimizing human-capital investments or cannot borrow the money to make them, the left part of the intergenerational curve will be depressed and the left-side slope will be accordingly steep but decreasing (reflecting the growing capacity to make human-capital investments as income increases). This implication will only hold, of course, under the assumption that the underlying relationship is linear in the absence of credit constraints.[23]

The second curve of Figure 1 reflects the alternative "complementarities hypothesis" that higher-income families secure, on average, higher returns to their human-capital investments in their children (e.g., Becker et al. forthcoming; Durlauf and Shesadri 2018). There are two possible reasons for this heterogeneity in returns: (a) higher-income parents have the human capital needed to make better investment decisions (e.g., to identify high-payoff schools) as well as to "catalyze" the investments they've made (e.g., to help with homework); or (b) higher-income parents live in neighborhoods that provide higher-return outlets for their investments (e.g., better schools) and that can "catalyze" the investments they've made (e.g., well-educated peers). The first version of the complementarities hypothesis is expressed in Becker et al.'s argument (forthcoming p. X) that highly-educated parents can expect higher returns because they are better at "choosing more effective inputs in order to achieve the same outcome," "navigating the intricacies of public school systems," or helping their "children with their schoolwork" (see also Lareau 1989; 2003). The second version is expressed in well-known arguments that low-income parents reap lower returns because they often live in neighborhoods that are stressful, provide few opportunities, and entail ongoing discrimination and bias (Schonkoff et al. 2012; Evans and Kim 2012). The expected payoff to a human capital investment may be reduced, for example, in neighborhoods in which children are exposed to crime

(Sharkey and Torrats-Espinosa 2017), environmental toxins (Clark-Reyna et al. 2016), eviction (Desmond 2016), incarceration (e.g., Western 2018), disadvantageous social networks (Chetty et al. 2018c), and stereotype threat and racial animus or bias (Chetty et al. 2018c). The key implication of both types of complementarities is that, even if low-income parents had the capacity to make more substantial investments in their children, it would not help them that much because they're typically living in contexts in which the returns to such investments are suppressed. This alternative hypothesis, insofar as it's on the mark, leads us to expect a convex curve rather than a concave one (see Figure 1).[24]

The shape of the intergenerational curve thus speaks to the mechanisms generating intergenerational inequality and to the types of policies that might reduce this inequality. It is important, then, to extend Chetty et al.'s (2014a) analyses by examining curvilinearities for earnings as well as family income, for men and women separately, and for older offspring. Because the key hypotheses in play often rest on mechanisms that are gender-specific (e.g., rates of incarceration), and because they pertain to earnings rather than family income, our analyses will allow us to provide more persuasive evidence.

A New Administrative Data Set

We rely here on the SOI-M Panel, a new administrative data set described by Mitnik et al. (2015), because it allows us to take on the various methodological problems that have complicated previous efforts to estimate intergenerational earnings and income elasticities.[25] Although Chetty et al.'s (2014a) data set is well-suited for the purpose of uncovering geographic variability in income-rank mobility, it is not tailor made for the purpose of estimating the IGE, mainly because of the problem of lifecycle bias. Because the full-population tax records used by Chetty et al. (2014) are only available starting in 1996, the oldest children in their analyses are just 29-32 years old, which is too early in their careers to yield good IGE estimates.[26] The children in the SOI-M Panel are, by

contrast, observed up to their late 30s. The SOI-M Panel also allows us to improve on prior estimates by reducing attenuation bias, measuring the total resources of the family, and avoiding selection bias (as discussed below).[27]

The SOI-M Panel is based on a sample of 1987 tax returns that can be used to represent all children born between 1972 and 1975 who were living in the U.S. in 1987. These children are old enough (i.e., 35 to 38 years old in 2010) to substantially reduce lifecycle bias.[28] The SOI-M Panel also allows us to minimize attenuation bias (by averaging across nine years of parental information) and to measure the total family resources that may be invested in children.[29] In prior research, the family's economic status has often been measured via the father's earnings (e.g., Mazumder 2005; Dahl and DeLeire 2008), a practice that is very problematic. It is preferable to estimate earnings elasticities with respect to family income because doing so (a) incorporates the income of mothers and thus better indexes the full complement of economic resources available to invest in children, (b) reflects the ability of families to draw on income sources other than earnings in response to transitory earnings shocks, (c) avoids any selection bias that may result from omitting children with absent fathers (as they are likely to be comparatively disadvantaged), and (d) eliminates the sensitivity of IGE estimates to varying approaches to treating fathers with zero earnings.[30]

The backbone of the data set used here, the 1987-1996 Statistics of Income Family Panel, is based on a stratified random sample of 1987 tax returns with a sampling probability that increases with income. The SOI-M Panel includes all dependents in the 1987 tax returns of the SOI Family Panel who were born between 1972 and 1975. For the purposes of our research, our objective is of course to represent *all* children born in those years, yet the SOI Family Panel doesn't meet this objective insofar as it under-represents children whose parents fall below the filing threshold and are not required to file tax returns. In the SOI-M Panel, the sample of children from the SOI Family Panel was thus supplemented with additional children who (a) were born in 1972-1975, and (b) are listed as dependents in the returns of the "refreshment segment" of the Office of Tax Analysis (OTA)

Panel. This segment of the OTA Panel represents those in the 1987 non-filing population who appeared in a return in at least one year between 1988 and 1996 (i.e., the "nonpermanent nonfilers").[31] We refer to the resulting sample of children, all of whom were drawn either from the SOI Family Panel or the OTA Panel, as the "base sample" of the SOI-M Panel. A comparison with the Current Population Survey (CPS) shows that the base sample represents its target population well (see Online Appendix E).

The offspring in the base sample of the SOI-M Panel were tracked into 1998-2010 tax data and other administrative sources. However, almost all of the analyses in this paper pertain to 2010 (when the children were 35-38 years old), as this allows us to minimize lifecycle bias. The selected records include information on (a) children's gender and their age in 2010, (b) their annual family income and individual earnings in 2010, and (c) parental age and income when the children were 15 to 23 years old.[32] In constructing the SOI-M Panel, the Data Master File (see Chetty et al. 2014, Online Appendix A) was used to identify the age of parents, the age and gender of the children, and the year of death of deceased children. The parental income data were drawn from the SOI Family Panel, the OTA Panel, and the 1997-1998 population tax data. The income data for children (and their spouses when they filed "married filing separately") were drawn from the 2010 population tax data. These data were supplemented with additional information on earnings, self-employment income, and unemployment-insurance income from W-2, 1040SE, and 1099G forms respectively. For nonfiling children without any available administrative data, we used information on likely nonfilers from the CPS (as discussed below).

We employ three different income concepts: total family income, disposable family income, and individual earnings (which are only available for children).[33] Due to differences in data availability, the SOI-M Panel does not measure these concepts identically for parents and offspring, but the differences are only minor. We also use annual measures for offspring (pertaining to 2010) and nine-year averages for parents (when their children were 15 to 23 years old) to minimize

attenuation bias. The measure of annual parental total income is the sum of (a) pre-tax "total income" in Form 1040 (which includes labor earnings, capital income, unemployment insurance income, and the taxable portion of pensions, annuities, and social security income), and (b) nontaxable interest. For filing children, total income also includes nontaxable earnings, which is the difference between gross ("Medicare") and taxable wages from the W-2 form. For nonfiling children, when earnings are available from either the W-2 form or the 1099-G form (i.e., UI income), total income is defined as the sum of those sources (see Chetty et al. 2014a for a further discussion of this approach). Whenever both W-2 and UI information were unavailable, we used data from the CPS on likely nonfilers without UI income or earnings to conduct mean imputation (see Online Appendix C).[34] This approach makes it possible to avoid the selection bias that would likely result if instead the children without W-2 and UI information were dropped. After-tax income, which we denote as "disposable income," is measured by subtracting out net federal taxes (which include refundable credits) from total income.[35] We use this measure of parental income to estimate our earnings elasticities, and we also use it for parents and children alike in some of our robustness analyses. The earnings for children are defined as the sum of W-2 wages and 65 percent of self-employment income (with the other 35 percent assumed to be the return to capital). We express all income variables in 2010 dollars using the Consumer Price Index for Urban Consumers - Research Series (CPI-U-RS), and we use sampling weights to estimate our models.[36]

The sample used for our analyses excludes children with (a) negative income, (b) income or earnings over $7,000,000, (c) more than 3 years of missing parental information, (d) nonpositive average parental income, or (e) average parental income over $7,000,000.[37] In Tables 1 and 2, we provide descriptive statistics for the SOI-M sample, including statistics on its demographic makeup, income sources, missing information, and income under different income concepts.

New Estimates of Intergenerational Persistence

As discussed earlier, there is much uncertainty about just how large U.S. intergenerational elasticities are, with some survey-data estimates of income or earnings elasticities coming in as high as 0.75 (Mazumder 2016) and others, based on administrative data, coming in as low as 0.34 (Chetty et al. 2014a) or even 0.26 (Dahl and DeLeire 2008). This is a wide range of estimates. We seek to narrow it here by offering new estimates that (a) rely on high-quality administrative data, (b) reduce all types of bias (functional-form, selection, lifecycle, and attenuation), and (c) are based on estimators that deliver the correct elasticity.

A key advantage of our approach is that it estimates an elasticity that is consistent with the interpretations that mobility scholars have long attributed to the IGE. The starting point for most IGE-based studies is the following population regression function (PRF):

$$E(\ln Y\,|x) = \beta_0 + \beta_1 \ln x, \qquad [1]$$

where Y is the offspring's long-run income or earnings, X is the long-run parental income or father's earnings, and β_1 is the IGE estimated in the literature.[38] The parameter β_1 is not, in the general case, the elasticity of the conditional expectation of the child's income, a point that mobility scholars have generally ignored. This would hold as a general result only if $E(\ln Y|x) = \ln E(Y|x)$. But, due to Jensen's inequality, the latter is not the case. Instead, as $E(\ln Y\,|x) = \ln \exp E(\ln Y\,|x)$, and $GM(Y|x) = \exp E(\ln Y\,|x)$, Equation [1] is equivalent to

$$\ln GM(Y|x) = \beta_0 + \beta_1 \ln x, \qquad [2]$$

where GM denotes the geometric mean operator. It follows that β_1 is the elasticity of the conditional geometric mean. It should be interpreted, in other words, as the percentage differential in the geometric mean of children's long-run income with respect to a marginal percentage differential in parental long-run income.[39] Although the IGE has been widely construed as pertaining to the conditional expectation of the offspring's income, this result means that it in fact pertains to the

conditional geometric mean of the offspring's income. The conventional approach is thus problematic because it rests on a misinterpretation of the IGE (Mitnik and Grusky 2017). Moreover, because the geometric mean is undefined for variables including zero in their support, mobility scholars have typically resorted to the expedient of dropping children with zero income or earnings from samples. The resulting estimates are almost certainly affected by selection bias (Mitnik and Grusky 2017).

These problems can be solved by estimating the IGE of the expectation (IGE_e) rather than the IGE of the geometric mean (IGE_g). This approach entails estimating the IGE that is of conceptual interest and that scholars long thought they were estimating. By estimating this IGE, we can also retain all children in our analyses, thereby eliminating the (likely substantial) selection bias affecting conventional estimates.

To estimate the IGE_e under the constant-elasticity assumption, we posit the following PRF:

$$\ln E(Y|x) = \alpha_0 + \alpha_1 \ln x, \qquad [3]$$

where $\alpha_1 = \frac{d \ln E(Y|x)}{d \ln x}$ is the IGE_e. This elasticity can be interpreted as the share of the parental income inequality that is found among the expected income (or earnings) of the parents' offspring (see Mitnik and Grusky 2017).[40] As with Equation [1], Equation [3] cannot be estimated directly, given that short-run proxy variables need to be substituted for the unavailable long-run measures. After replacing *Y* and *X* by these short-run counterparts, we estimate Equation [3] with the Poisson Pseudo Maximum Likelihood (PPML) estimator (Santos Silva and Tenreyro 2006).[41]

We present the constant IGE_e estimates on the left side of Figure 2 (with Table 3 providing the estimates themselves). This figure provides point estimates (and the upper and lower bounds of the confidence intervals) for men's earnings, men's total income, and women's total income.[42] The main conclusion from this first set of results is that, even assuming a constant elasticity, the resulting estimates are quite high, ranging from 0.45 to 0.49. These results approach the high end of the

existing range of survey estimates (with the exception of those from Mazumder 2016 and Mitnik 2017b). It is especially noteworthy that our income estimates of 0.45 and 0.47 for women and men respectively are approximately 30 percent larger than the tax-data estimate of 0.34 (for the IGE_e) that Chetty et al. (2014a) report for men and women pooled.[43] This difference arises mainly because our estimates are much less affected by attenuation and lifecycle biases.

The foregoing constant-elasticity estimates are important to present because the literature has almost always defaulted to the constant-elasticity assumption. It does not follow, of course, that such estimates are to be preferred. The assumption of a constant IGE has been adopted more as a matter of necessity (given the small available samples) than by virtue of any strong prior that it in fact holds. The SOI-M Panel is large enough, however, to now test that assumption, an important advantage because there is good reason to believe that this conventional assumption may bias our estimates downward.

We use two approaches to investigate possible nonlinearities (in log-log space) in the relationship between parental income and the expected income or earnings of children. In the first approach, we modify Equation [3] by including the right-side terms that permit a linear spline effect with knots at the 10th, 50th, and 90th percentiles.[44] The resulting PRF is:

$$\ln E(Y|x) = \alpha_0 + \alpha_1 \ln x + \sum_{\substack{j= \\ 10,50,90}} \alpha_j \, I\big(ln\, x > q_j(ln\, X)\big)\big[ln(x) - q_j(ln\, X)\big], \qquad [4]$$

where *I* is the indicator function and the operator $q_j(.)$ returns the j^{th} percentile of the variable in its argument (the logarithm of average parental income in this case). The model of Equation [4] assumes that (a) the IGE_e is constant within each of the four regions of parental income defined by the 10^{th}, 50^{th}, and 90^{th} percentiles of this variable, (b) the IGE_e may vary across regions, and (c) the intergenerational curve is continuous. We again estimate the parameters of the model by PML and using the likelihood function of a Poisson regression.

The second approach that we take, a nonparametric one, relies on local polynomial regression or loess (Cleveland, Devlin, and Grosse 1988; Cleveland and Grosse 1991). Here the model is:

$$\mathrm{E}(Y|x) = \mathrm{F}(\mathrm{x}), \qquad [5]$$

where F is an unknown smooth function. The selection of the "smoothing parameter"—the parameter that determines the fraction of observations to be included in the computation of each local polynomial regression—is completed automatically (within the range of [.08, 1]) by finding the global minimizer of $\mathrm{AIC_c}$, a version of the Akaike Information Criterion specifically tailored to nonparametric regression (Hurvich, Simonoff, and Tsai 1998; see also Li and Racine 2004). We use degree 1 polynomials and a tricube weight function.

The results for each of these two specifications are shown in the middle and right side of Figure 2. For both sets of results, we have presented the "global $\mathrm{IGE_e}$," which is our single-parameter summary of the degree of persistence over the full parental-income distribution. This measure, which is defined as the expected value of the $\mathrm{IGE_e}$ over the parental-income distribution, reduces to the constant-elasticity IGE when a constant-elasticity model is fit. When the constant-elasticity assumption is relaxed, our global measure can be interpreted as the weighted average slope of the estimated intergenerational curve, with the weights set at the density of each parental-income value.[45]

The key result from Figure 3 is that, as anticipated, both the spline and nonparametric estimates are higher than the constant-elasticity estimates. The estimates based on the constant-elasticity assumption are lower by approximately 10 percent for the men's estimates and by less than 5 percent for the women's total-income estimates. For men, the spline and nonparametric models show income and earnings elasticities above 0.5, with the estimates for earnings (i.e., 0.54, 0.56) slightly larger than those for income (i.e., 0.51, 0.52). The corresponding results for women reveal an income elasticity that is not much below 0.5 (i.e., 0.46, 0.47).

Which of the estimates presented in Figure 2 best represent the level of earnings and income persistence? When we test whether the constant-elasticity assumption can be rejected, the results are in fact straightforward: We find that it is rejected in all three cases (see Table 3). It follows that our preferred estimates, all of which relax the constant-elasticity assumption, are above 0.5 for men and just below 0.5 for women. Because the constant-elasticity assumption can be rejected, we will not be reporting any further constant-elasticity estimates from the SOI-M data.

Persistence of Long-Distance Inequality

We next introduce a new set of measures that allow us to assess the persistence of long-distance inequality. These measures are useful because the main descriptive questions of interest are very often long-distance questions. We would like to know, for example, the share of inequality between poor and well-off families that is found between their children. This share cannot, however, be read off the elasticities pertaining to particular points in the curve (nor can it be read off the averages of such elasticities). If point elasticities are wrongly used for the purpose of characterizing the persistence of long-distance inequality, it can lead to misstatements of the extent to which inequality is just a carry-over of the preceding generation's inequality.

The degree of persistence across long reaches of the parental-income distribution is properly measured with standard (i.e., Allen's) arc elasticities. In the case at hand, these take the following form:

$$AIGE_e(Y|x,i,j) = \frac{E(Y|q_j(X)) - E(Y|q_i(X))}{E(Y|q_j(X)) + E(Y|q_i(X))} \left[\frac{q_j(X) - q_i(X)}{q_j(X) + q_i(X)}\right]^{-1}, \quad [6]$$

where $100 \geq j > i \geq 0$. The arc elasticity pertaining, for example, to the children's expected income (Y) between the 10th and 90th percentiles of parental income (X) is $AIGE_e(Y|x, 10{,}90)$. As equation [6] indicates, this arc elasticity depends on (a) the proportional increase in a child's expected income when her or his parents are at the 90th rather than 10th percentile (of parental income), and (b) the

corresponding proportional increase in parental income between those two percentiles. The arc elasticity is the ratio of these proportional increases, where each proportional increase is computed by dividing the absolute difference by the average between the two values defining the difference. We have improved precision in our analyses by estimating the $AIGE_e$ of children's total income between points "around" the ith and the jth percentiles of parental income. In estimating $AIGE_e(Y|x, 10{,}90)$, for instance, we replace the percentile $q_j(X)$ of parental income by the average of the values between $q_j(X) - 5$ and $q_j(X) + 5$ (i.e., the average value for percentiles 5 to 15), and we likewise replace the percentile $q_i(X)$ with the corresponding average (i.e., the average value for percentiles 85 to 95). We apply a similar replacement for the children's conditional expectations.[46]

We can use arc elasticities to define a new single-value summary measure of persistence. The global IGE$_e$ is the expectation of a point elasticity across all values of parental income, whereas the global AIGE$_e$ is the expectation of an arc elasticity across all possible pairs of values of parental income. If U and V are independent variates with a uniform distribution on [0, 100], our new persistence measure is defined as follows:

$$\text{Global AIGE}_e = E(AIGE_e(Y|x, \min(U,V), \max(U,V)),$$

where the expectation is taken over the joint distribution of U and V (for $u \neq v$). The interpretation of this summary measure is straightforward: It is the expected share of inequality (as defined by the arc elasticity) that is passed on to children across all possible random draws of pairs of families. Unlike the global IGE$_e$**,** the global AIGE$_e$ compares each family with *all* families in the population, not just contiguous ones. This summary persistence measure is computed using numerical approximations (see Appendix F).

The main results of interest are provided in Table 4. Here, we present our AIGE$_e$ estimates, computed from the conditional-expectation estimates of the spline and nonparametric models. We estimate the global AIGE$_e$ as well as the AIGE$_e$ for arcs defined by families with income in the 5th-

15th percentiles ("poor families") and 85th-95th percentiles ("well-off families"). We find that persistence between children born into poor and well-off families is strikingly high: The tables indicate that about two-thirds of the difference in total income between these families is passed on to sons. The persistence of advantage is equally high for sons' earnings, but somewhat lower for daughters' total income (where our estimates are 0.57 under the spline model and 0.60 under the nonparametric model).[47]

The second column in Table 4 presents our estimates of the global $AIGE_e$. As shown here, the global $AIGE_e$ is larger than the global IGE_e (see Table 3), a conclusion that holds across genders, models, and income measures. Under the global $AIGE_e$, the share of economic advantages passed on from parents to children ranges from 0.55 to 0.60 for men (which is 5 to 11 percent higher than under the global IGE_e), while it ranges from 0.49 to 0.50 for women (which is 4 to10 percent higher than under the global IGE_e). These results underscore our main conclusion that a very large share of U.S. inequality is passed on from parents to children.

Robustness

It is important to assess whether our results, which reveal that persistence is more extreme than implied by previous tax-return estimates (Chetty et al. 2014a), hold up under alternative models and measures and pass other related checks. We report here on some of our most important checks.

We first ask whether our estimates are free of attenuation bias. The history of estimating intergenerational elasticities has in large part been a history of coming to terms with severe attenuation bias.[48] When we reestimate our elasticities with measures of parental income based on one to nine years of data (see Online Appendix H), we find that the 9-year averages reduce attenuation bias very substantially. The results also suggest that our estimates are reaching a plateau once nine years of parental information are used. This indicates that the benefit of including additional years may be minor and that the residual attenuation bias in our baseline estimates are likely to be small. Because we do not have parental-income measures based on additional years of

information (beyond 9 years), we cannot, however, rule out the possibility that some bias remains and that our estimates may therefore understate persistence.[49]

The other threat to validity extensively discussed in the literature is that of lifecycle bias.[50] The best available evidence suggests that, when estimates are based on information taken at approximately age 40 (for parents and children alike), they come closest to representing lifetime IGEs. If instead income measurements are taken when the children are younger, a downward bias is generated (see Haider and Solon 2006; Böhlmark and Lindquist 2006; Nyborn and Stuhler 2016; Mitnik 2017a). Consistent with this evidence, our supplementary analyses with the SOI-M data (see Online Appendix I) show that (a) the IGE_e of men's earnings continues to increase substantially as men move into their late thirties, while (b) the IGE_e of income continues to grow past age 30 and perhaps until later years (for men but not women).[51] This suggests that, by using a sample of children observed at 35-38 years old, we are able to reduce lifecycle bias relative to what would obtain with a sample of younger children. Nevertheless, because the children in our sample fall short of age 40, our estimates may still understate at least some lifetime elasticities.

We have also estimated the earnings IGE_e for women. To this point, we have presented both income and earnings elasticities for men, but only income elasticities for women. We have not yet reported earnings elasticities for women because, given that their labor supply at midlife remains lower than that of men, their earnings at that time are not a good measure of their lifetime economic status (e.g., Chadwick and Solon 2002, p. 335). It is nonetheless useful to examine the earnings IGE_e for women because doing so provides a check on the quality of our data and our estimation approach. We anticipate a low earnings elasticity (compared to men's) because women from relatively affluent backgrounds tend to have higher-income partners and to work fewer hours (or not at all) when they have young children (Raaum et al. 2007; see Online Appendix J for a further discussion of the rationale behind this expectation). As noted in the introduction, there are very few estimates of women's earnings IGEs, with such results as are available yielding contradictory estimates. In Figure

3, we present measures based on parental income (rather than father's earnings), thus eliminating any biases arising from ignoring the income of mothers as well as sources of income other than earnings. We find that the elasticity for women is, as anticipated, substantially lower than that for men. The IGE_e for women, estimated at 0.31 (spline model) and 0.32 (nonparametric model), is about 43 percent lower than the corresponding estimates for men, which are 0.54 and 0.56 respectively.[52] These results, unlike those reported elsewhere (e.g., Mazumder 2005), accord well with expectations.

We have carried out other robustness checks (see Online Appendix K) that likewise support our conclusion that the key global elasticities are approximately 0.5 and may well be larger. When all forms of bias are addressed, the resulting estimates of economic persistence come in at the high end of existing survey estimates and are much larger than the tax-based estimates of Chetty et al. (2014a). In the case of men's earnings, our estimates are even close to Mazumder's (2005) very high estimates based on SSA administrative data, even though Mazumder used far more years of parental information.[53] These robustness checks are important because, whereas prior results have suggested that there's much volatility and variability in estimates of persistence, our results show that all of that ambiguity disappears once the main threats to validity are addressed. We can thus conclude with high confidence that intergenerational persistence in the U.S. is very high.

The Missing Poor

We have not yet explicitly addressed the possible effects of selection bias on our results. The most prominent example of the biasing effects of selection on education and labor market statistics arises when prison inmates are excluded from analyses. As Pettit (2012) and others (e.g., Western 2006) have shown, the exclusion of prison inmates has produced statistics that understate the black-white racial gap in educational attainment, work force participation, earnings, and many other labor market outcomes. Although the possible effects of selective processes for survey-based estimates of economic mobility have been largely ignored (Schoeni and Wiemers [2015] is a notable exception), there are nonetheless good reasons to worry about them. It is not just that the institutionalized

population is typically excluded in survey-based studies.[54] The other main sources of unrepresentativeness in the samples used in survey-based studies are (a) very high attrition rates,[55] and (b) the unavailability of income data for adults who are not household heads (and who are therefore dropped from samples).[56] The combined effects of excluding the institutionalized population, high attrition, and household-head bias may well be substantial. Worse yet, these three effects are all likely working in the same direction, as they tend to select out the offspring from low-income households who themselves have low income. It follows that survey-based analyses are affected by forces that pull estimates downward (but of course whether they are actually downwardly biased will depend on what other biasing forces are present).

The turn to administrative data is promising in part because these three sources of possible bias are eliminated. The SOI-M data, like Chetty et al.'s (2014a) data, have the distinct advantages of covering the institutionalized population, including those who are not household heads, and avoiding the problem of attrition. The critics of administrative data will, however, point out that these advantages are traded off against the disadvantage of failing to collect income and earnings information for those who engage in informal work or do not file tax returns even if they do work in formal sector.[57] Because these missing data are not "missing completely at random" (e.g., Little and Rubin 2002), they can be expected to generate selection bias. This is a troubling problem that must be overcome given that administrative data are becoming the go-to source for intergenerational mobility analyses.

The purpose of this section is to show precisely how this problem *can* be overcome. The first step in doing so is to recognize that, given the mechanisms generating nonfiling (i.e., income below the filing threshold, work in the informal sector), offspring with missing data will typically have income or earnings that are very low (if not zero) and that are unlikely, moreover, to vary much by parental income. This in turn suggests estimating IGEs by imputing a low income or earnings value to children with missing data. Unfortunately, this approach has proven unviable in past analyses, as

the IGE_g has been found to be wildly sensitive to the exact imputed amounts (Chetty et al. 2014a:Table 1; Mitnik et al. 2015:Tables 9 and 10). It was precisely this sensitivity that, in part, motivated Chetty et al. (2014a) to turn away from the IGE_g and instead estimate the rank-rank slope. However, this decision is not without its costs, as the IGE answers the main questions of interest in many research contexts (e.g., Bénabou and Ok, 2001) and, as we have pointed out earlier, can be (and has been) embedded within policy-relevant theoretical models of intergenerational processes. The rank-rank slope, by contrast, does not have the same theoretical apparatus standing behind it (and it would be quite daunting to successfully develop it).

Is there a way forward? In Table 5, we show that indeed there is, as all volatility virtually disappears under the IGE_e. The first five rows of Table 5, which pertain to different constant-income imputations for those without any administrative information, show estimates ranging from 0.53 to 0.54 for men and from 0.48 to 0.49 for women.[58] The next five rows of Table 5 pertain to our favored approach in which we carry out mean imputations by gender and age using CPS data (see Online Appendix C for details). Among CPS "likely nonfilers" without earnings or UI income, approximately one-third have zero income, which prevents us from using mean imputation to estimate the IGE_g. These zeroes are, however, unproblematic when the estimand is the IGE_e. We have thus presented the estimates under the assumption that these zeroes are true zeroes as well as under the alternative assumption that the true income is low (ranging from $1 to $3,000) but is not zero. This alternative assumption is plausible because of possible recall failures among CPS respondents reporting zero income (see, e.g., Moore et al. [2000]). The important result from Table 5 is that, no matter what is assumed about the true income of respondents who report zero income, under mean imputation all of the male estimates come in at exactly 0.52, and all of the female estimates come in at exactly 0.47.

We can conclude that, when the IGE_e is estimated, the volatility problem is essentially eliminated. It follows that, as useful as the rank-rank slope often is, one should turn to it only if it

answers the questions of interest. If instead the policy or substantive questions in play dictate estimating an IGE, the results of Table 5 show that the IGE_e solves the volatility problem and is the estimator of choice (see Online Appendix L for a similar analysis pertaining to the earnings elasticity).

The last two rows of Table 5 speak to the selection bias that arises when (a) the sample only includes offspring who filed a tax return, and (b) the sample only includes offspring who either filed a tax return or had other administrative information on income. Relative to the estimates for tax return filers, our preferred estimates are 21 percent higher for men ([0.52-0.43]/0.43 = 20.9) and 15 percent higher for women ([0.47-0.41]/0.41 = 14.6). The same type of conclusion holds for the earnings elasticity: That is, when those without reported W-2 earnings are retained in the sample, the IGE_e increases approximately 19 percent (see Online Appendix L for details). Although the results of Table 5 pertain to the IGE_e, they nevertheless suggest that the IGE_g reported by Chetty et al. (2014a) was low partly because of selection bias. We can conclude that, while administrative data are *potentially* vulnerable to the problems that arise when the "missing poor" are not represented, the IGE_e provides a straightforward pathway to addressing these problems and studying intergenerational persistence within the full population of interest.

The Implications of Curvilinearity

We have focused to this point on delivering high-quality estimates of economic persistence. We have shown that, whereas previously available estimates have varied widely, the volatility disappears when the main threats to validity are removed, especially those that address the "missing poor" problem. The simple conclusion: Under all credible models and robustness checks, intergenerational persistence in the U.S. is very high.

The balance of our analyses and discussion turns to the question of policy. Although here the evidence is more limited and less dispositive, we do nonetheless have two tools at our disposal. The first is empirical evidence on the shape of the intergenerational curves, and the second is an

empirically-grounded model of the determinants of economic persistence. We will rely on these two tools to reveal why persistence is so high in the U.S. and, by implication, how it might be reduced.

We begin, then, by considering the evidence on the shape of the intergenerational curves. This evidence, which is based on our nonparametric models, allows us to speak to the credit-constraint and complementarities hypotheses that were outlined at the beginning of our paper. The credit-constraint hypothesis implies that children from low-income families are disadvantaged because their families don't have the money needed to invest in them, whereas the complementarities hypothesis implies that they are disadvantaged because they find themselves in social contexts in which the payoffs to human capital investments are lower on average. If one is willing to assume that the intergenerational relationship is linear in the absence of credit constraints and complementarities (as is conventional within the literature), then these hypotheses entail clear and contrasting predictions about the observed shape of the curves.

What do we find? In Figures 4 through 6, the intergenerational curves under our nonparametric models are presented (for men's income, women's income, and men's earnings), with each figure showing the expected value of the IGE for regions of the parental-income distribution defined by the 10^{th}, 50^{th}, and 90^{th} percentiles. These four elasticities can be interpreted as the average slope of the intergenerational curve in each of those regions (where, as before, these are weighted averages).[59] The black dots and grey segments in the figures represent point estimates and confidence intervals corresponding to equidistant quantiles of parental income.[60] In Table 6, the estimates under our spline and nonparametric models are presented, using the four region-specific IGEs defined by the three percentiles of parental income indicated above. The tables also include p-values from the bootstrap test of the null hypothesis that the expected IGE_e for the P50-P90 region is not larger than that for the P10-P50 region.[61]

The first result to be gleaned from Figures 4-6 and Table 6 is that, even in the bottom half of the parental income distribution, the family income and earnings gradients are positive. As shown in

Table 6, the point estimates for the below-P10 and P10-P50 regions are positive in all 12 cases, and 9 of 12 estimates are significantly different from zero. The three non-significant estimates all pertain to the below-P10 region. It follows that children born into the lower half of the distribution face heterogeneous life chances.

The second result is that, even though money matters below the 50th percentile of family income, it matters even more among families between the 50th and 90th percentiles. For men, the estimated total-income elasticities are comparatively low on the left side of Figure 4, with point estimates of 0.32 for the below-P10 region and 0.43 for the P10-P50 region. The elasticity then increases and reaches its highest level of 0.68 in the P50-P90 region (i.e., parental incomes between $57,000 and $128,000). Although our region-specific results are imprecise and must be treated cautiously, it is nonetheless striking that, according to the latter point estimate, a full two-thirds of parental-income advantages within the middle to upper-middle-class region persist into the next generation.[62] We can also formally reject the "non-convexity hypothesis" for the interior regions of the curve (with a p-value of 0.03). At the top decile of parental income, the curve appears to flatten, but here the IGE_e estimate is particularly imprecise (see Table 6). It is reassuring that the point estimates from the spline model are, save for one exception (the below-P10 region), very similar to those obtained with the nonparametric model.[63]

The shape of the intergenerational curve for women's income, which is shown in Figure 5, takes on a broadly similar pattern. Because the left tail appears less flat, the overall curve is closer to being linear, but the point estimates again imply that the curve steepens as it moves from the P10-P50 region to the P50-P90 region (although here the p-value is only 0.06 for the test of the non-convexity hypothesis). As with the income curve for men, the point estimate for the P50-P90 region is especially large (0.63), indeed it implies that almost two-thirds of parental-income advantages within that region persist into the next generation. In the corresponding spline model, the confidence intervals are smaller, and the hypothesis of non-convexity is more definitively rejected (with a p-

value of 0.01). The IGE_e estimate for the above-P90 region is also larger under the spline model (0.42) than the nonparametric model (0.25), but even that higher value is still well below the corresponding estimate for the P50-P90 region (0.63). While the nonparametric estimate for the above-P90 region is extremely imprecise, the confidence interval for the corresponding spline-model estimate is smaller (i.e., 0.33-0.52), giving us some confidence that there is a real tapering-off in the elasticity at the highest levels of parental income.

The nonparametric estimate for the men's earnings curve, presented in Figure 6, speaks more directly to the credit-constraint and complementarities hypotheses. It suggests more prominent convexity than emerged in the income curves. The left tail is almost flat, and the slope of the curve continues to increase steadily up to the 98th percentile of parental income. Because the conditional expectations in the upper tail are imprecisely estimated, the estimate for the above-P90 region is uninformative (see Table 6). The estimate for the P50-P90 region again implies that approximately two-thirds of parental-income advantages persist into the next generation's earnings. In this case, the point estimate for the P10-P50 region is again lower (0.56), but the difference is smaller and the hypothesis of non-convexity cannot be rejected. There is, however, a gain in precision with the spline model (Table 6). As with the income curves, the non-convexity hypothesis is now rejected, and the estimate for the above-P90 region clearly indicates that the curve becomes flatter in that region. The point estimate for the P50-P90 elasticity, 0.75, is larger than any other we've presented and implies very extreme intergenerational persistence within that region.

We have also carried out additional bootstrap tests over slightly different regions of the intergenerational curve for men's earnings. In Table 7, we present the results of these additional tests, with the "left-tail" of our curves now represented by the below-P10 and below-P20 regions, and the "bulk of the curve" represented by the complement to these regions up to P90. These tests are even more conclusive: The p-values shown here are all small and again consistent with our conclusion that

persistence is more extreme at higher levels of parental income (at least up to the 90th percentile of the parental-income distribution).

The evidence of Figures 4-6 and Tables 6-7 means that income matters less for children of families below the median than for children above it. This result is inconsistent with the argument that credit constraints will produce a concave curve (see Becker and Tomes 1986). To the contrary, the "returns to parental income" increase with income, just as the complementarities hypothesis suggests. It should not of course be concluded that low-income families are unaffected by credit constraints. The convexity of the intergenerational curves only suggests that, at the bottom of the family income distribution, the effects of complementarities dominate those of possible credit constraints.

If Figures 4-6 and Tables 6-7 were the only evidence suggestive of complementarities, we would have to be even more cautious with this interpretation than we already are. There is, however, a substantial body of evidence that opportunities for mobility vary by neighborhood after controlling for parental income and other parental characteristics (e.g., Chetty et al. 2018d; Sharkey 2016; Sampson 2012). Although there are many other possible sources of complementarities (e.g., Becker et al., forthcoming; Caucutt, Lochner, and Park 2017), it is especially plausible that some neighborhoods do not provide much of a payoff for conventional human capital investments. As we pointed out earlier, the return to human capital investments is likely reduced, for example, in neighborhoods that bring frequent exposure to crime, environmental toxins, discrimination, incarceration, racial animus, crime, substandard housing, and other sources of chronic stress and reduced cognitive functioning. The convex curves of Figures 4-6 are thus consistent with the conclusion that conventional human-capital policy (e.g., public provision of early childhood education) is undermined when low-income families face environments that don't support human capital investments in their children.

The Sources of High Persistence

The foregoing discussion positions us for a more formal analysis of the sources of high economic persistence in the U.S. and the types of policies that might reduce it. As we have noted previously, a virtue of using IGEs to analyze persistence is that they can be embedded within theoretical models, which then help to identify the full range of policies that could be used to reduce persistence. The purpose of this concluding section is to apply a theoretical model of the determinants of the IGE_e to understand why economic persistence is comparatively high in the U.S. and to identify policy options for reducing it.

Although policy-relevant theoretical models for the IGE_g have long been available (e.g., Solon 2004), a corresponding model of the determinants of the IGE_e has only recently been developed by Mitnik (2018).[64] For our purposes here, a main result of this model is that it identifies the following determinants of persistence:

$$\alpha_1 = \theta\,(1+\tau) + \theta\,\pi\,(1-\delta_1)(1-\varphi_1), \qquad [7]$$

where α_1 is the constant IGE_e of the offspring's expected market income with respect to the parents' market income, θ is the elasticity of human capital to the investment in human capital (with $\theta > 0$), π is the elasticity of labor income to human capital (with $\pi > 0$), φ_1 indicates the relative progressivity of public investments in human capital (with $\varphi_1 \geq 0$), τ is an index of socioeconomic residential segregation (with $0 < \tau < 1$), and δ_1 determines the progressivity of the tax system and the generosity of the social safety net (with $0 < \delta_1 < 1$), where the ratio between the Gini coefficients for disposable and market income is inversely related to δ_1.[65]

It follows from Equation [7] that, all else equal, the IGE_e of market income will fall with (a) a decrease in the productivity of private and public investments in human capital, (b) a decrease in the return to human capital, (c) an increase in the progressivity of public investments in human capital, (d) a decrease in socioeconomic residential segregation, and (e) an increase in the degree to

which taxes and transfers curtail cross-sectional income inequality. If the IGE_e is to be reduced, we must choose from among some mix of these five policy levers. In practice, our policy choices can be further simplified, as it's hard to defend a policy of reducing persistence by making it more expensive to produce a unit of human capital (i.e., to reduce θ). Because such a policy would almost certainly reduce total economic output, it seems unlikely that any substantial constituency around it could emerge.

The four remaining levers are, by contrast, the standard stuff of social policy debates, although not necessarily social policy debates *about economic persistence*. For instance, Solon's (2004) highly cited theoretical model only identifies the three "human-capital factors" discussed above (θ, π, and φ_1), which leaves only two policy levers available. We can examine a more encompassing and realistic set of causes and levers via Equation (7).

To make this discussion concrete, it is useful to compare economic persistence for the U.S. with that for Denmark, an attractive comparison because Denmark is a conventional exemplar of a low-persistence country and thus provides the starkest possible contrast (e.g., Corak 2006; Björklund and Jäntti 2011; Blanden 2009; Helsø 2018).[66] It is also an attractive contrast because Helsø (2018) has produced administrative-data estimates of the IGE_e for Denmark that are as comparable as possible to our estimates.[67] When our estimates are compared to those of Helsø (2018), we find that the U.S. elasticities are between 1.6 and 2.6 times larger than the corresponding Danish elasticities (see Figure 7).[68]

Why are the mobility regimes of the U.S. and Denmark so different? The difference arises because, for each of the factors of Equation (7), the U.S. consistently takes on scores that raise persistence. The return to human capital (π) is higher in the United States because Denmark's labor market institutions do much more work propping up pay in low-skill and mid-skill jobs (see Broecke 2016, Figure 2; OECD 2017, Chart A6.1; Peracchi 2006, Table 6; Hanushek and Woessmann 2011,

Figure 2.10; Jaumotte and Osorio Buitron 2015; Pontusson et al. 2002; Rueda 2015). The public investment in human-capital formation (φ_1), especially for preschool and college, is much less progressive in the U.S. than in Denmark (e.g., Esping-Andersen et al. 2012; OECD 2013, pp. 228-229; Andrade and Thomsen 2018; see also Porter 2013).[69] The level of income segregation among schools, which is a good proxy for socioeconomic residential segregation (τ), is much lower in Denmark because of its more egalitarian housing programs and other integrative policies (Chmielewski and Reardon 2016, Figure 4; Nielsen 2017; Scanlon et al. 2015).[70] And the tax-and-transfer system (δ_1) reduces household income inequality by 66 percent more in Denmark than in the U.S. (Gornick and Milanovic 2015, Figure 1).[71]

The upshot is that economic persistence is very high in the U.S. because it has adopted persistence-promoting policies in nearly *all* of the institutional domains where persistence is determined. This across-the-board institutional embedding of high persistence has not been widely acknowledged in U.S. policy circles. The U.S. intervention industry is, to the contrary, mainly oriented toward one-factor solutions that are predicated on a very partial analysis of the sources of unequal opportunity. This analysis focuses on inequalities in human capital investments and thus supports increased public investment in home visiting programs, early childcare, preschool, and schools in low-income neighborhoods. The focus, in short, is on raising φ_1, with a particular stress on doing so by investing early in the lifecourse, as current levels of early-childhood investment are seen as suboptimal (e.g., Heckman 2013). Because these early interventions have much evidence behind them (e.g., Cunha et al. 2006), it is unsurprising that the exemplar low-persistence countries, like Denmark, make very extensive investments in them (e.g., Esping-Andersen 2004; Landersø and Heckman 2017).

It's not well appreciated, however, that Denmark doesn't stop with raising φ_1. Most importantly, Denmark also (a) reduces π and τ through wage-compression and housing policies, and

(b) increases δ_1 through a progressive tax-and-transfer system. It follows that all four policy levers identified in Equation [7] play an important role in driving down Denmark's economic persistence. Why haven't policy analysts in the U.S. likewise advocated a comprehensive four-factor approach? It certainly isn't for lack of evidence on the effectiveness of policy addressing the payoff to lower-skill labor (π), neighborhood integration (τ), and progressive taxes and transfers (δ_1). It is well established, for example, that policies addressing the return to human capital affect persistence (e.g., Corak 2013; Davis and Mazumder 2017), that low-income families are locked out of opportunity in part because they're clustered in unfavorable neighborhoods (e.g., Chetty et al. 2018a; 2018b; 2018d), and that children from families receiving cash and near-cash transfers tend to have higher math and reading scores, more years of schooling, higher rates of college enrollment, and ultimately higher earnings (e.g., Aizer et al. 2014; Dahl and Lochner 2012; Duncan, Morris, and Rodrigues 2011; Maxfield 2015; Duncan and Brooks-Gunn 1997; also see Duncan et al. 2010; Hoynes, Miller, and Simon 2015; Hoynes, Schanzenbach, and Almond 2016). The near-exclusive focus on human-capital policy in the U.S. may partly reflect a wholly pragmatic calculation that other types of persistence-reducing policy, although likely to be very effective, would be politically infeasible.

It would be child's play, of course, to create the misleading impression that the U.S. is working seriously on all four fronts by listing the government policies that are relevant for each of them. This list would be long. The point that we are making isn't that there is *no* U.S. policy on these other fronts, but only that (a) the scale of the effort pales in comparison with the scale in other countries, and (b) we need to own up to the implications of this very limited approach for the future of economic persistence in the U.S. If the objective is truly to drive down U.S. persistence to the lower levels observed in most other well-off countries, it will likely be necessary to move beyond our one-factor approach and begin to develop more comprehensive policy.

In the absence of any such ramp-up, the theoretical model of Equation (7) suggests that persistence in the U.S. will likely only grow more extreme, as rising income inequality has the effect of increasing the amount of private resources that high-income families can direct to human capital investments, high-amenity neighborhoods, and other "reproductive" efforts (see Schneider, Hastings, LaBriola 2018; Mitnik, Cumberworth, and Grusky 2016). The U.S. is exceptional, in other words, not just because of its limited commitment to policies aimed at moving φ_1, π, τ, and δ_1 toward "low-persistence values," but also because it's facing the challenge of an especially rapid increase in income inequality. If this increase continues apace and high-income families continue to exploit it to increase their reproductive investments, Equation 7 implies that—in the absence of a corresponding increase in public expenditures on the safety net—δ_1 will become smaller and the elasticity will become larger. It follows that our current "treading-water" approach to policy, if left intact, may lead to further increases in our already-high levels of economic persistence.

Conclusions

The intellectual backdrop to our paper is a long history of research, beginning some 40 years ago (e.g., Sewell and Hauser 1975), on intergenerational earnings and income elasticities. Although one might think that little is left to be done, in fact there is quite striking dissensus on such key issues as the extent of intergenerational persistence and the pattern of gender differences in intergenerational persistence. It would have been reasonable to expect that our knowledge would firm up as high-quality administrative data became increasingly available. This has not happened. If anything, the uncertainty has only increased with the latest wave of research using administrative data.

We have sought to address this evidence deficit here. Because it is partly attributable to a data deficit, we have turned to a new administrative-data panel, the SOI-M Panel, that allows us to take on the full set of biases that have affected previous estimates. The "missing poor" problem, as

we have come to call it, is an especially important methodological threat. Although administrative data are attractive precisely because they eliminate many selection problems, they are still vulnerable insofar as they fail to represent those whose information is unavailable both in tax returns and other administrative records. If this problem goes unaddressed, we are left with a very undesirable elasticity that pertains to "when things are going well" (Couch and Lillard 1998). We have shown that, when the "missing poor" problem is addressed, intergenerational persistence increases substantially, a result that explains in part why some administrative-data results (Chetty et al. 2014a) have created the appearance of relatively weak persistence.

The evidence deficit arises as much from methodological problems as from data problems. The conventional IGE, which we have dubbed the IGE_g, cannot of course be estimated with zeroes in play (as doing so would require taking the logarithm of zero). Moreover, if one resorts to the approach of replacing zeroes with small nonzero values, the resulting estimates are wildly sensitive to the values chosen. We have shown that, by turning to the IGE of the expectation, we can eliminate this volatility and recover very attractive interpretations of the IGE (also see Mitnik and Grusky 2017).

It is important in this regard to bear in mind why the rank-rank slope has become so fashionable. This development was not based on some argument to the effect that it is the most appropriate measure given the questions of interest (although it may often be). Rather, it was introduced as a fallback approach, a fallback that became necessary because of methodological difficulties that arose in estimating intergenerational elasticities with administrative data. Although the rank-rank slope is a very welcome addition to the stable of mobility measures, it should be used when the question at hand demands its use, not as a fallback to which one resorts to solve methodological problems that, as we have shown, can be addressed without abandoning the elasticity.

What have we learned by analyzing the new SOI-M Panel and estimating the elasticity of the expectation? Although we cannot review all of our findings here, there are five that stand out and that bear highlighting.

- **Approximately half of parental income advantages are passed on to children.** The total-income IGE is estimated at 0.52 for men and 0.47 for women (under our preferred nonparametric estimates). These estimates are at the high end of the range of estimates reported in the existing literature on economic persistence.
- **The men's earnings IGE is even larger.** The earnings IGE for men, estimated at 0.56, is again at the high end of existing estimates of persistence. When the "missing poor" are excluded from the SOI-M sample, the estimate comes in at a much lower level.
- **A large share of the inequality between poor and well-off families persists into the next generation.** The preceding estimates, expressed as they are in terms of point elasticities, only pertain to persistence between families with very similar incomes. The arc IGE, by contrast, pertains to the share of inequality that persists for families that may be separated by large distances in the income distribution. The arc IGE for families at the 10^{th} and 90^{th} percentiles, which comes in at 0.65 for men and 0.60 for women, indicates that a very large share of income inequality between poor and well-off families persists from one generation to the next.
- **Parental income matters more for men's earnings than for women's earnings.** The earnings IGE for men (0.56) is more than 40 percent higher than that for women (0.32). This result is consistent with the expectation that, because well-educated married women are more likely than well-educated married men to reduce their labor supply, they "profit" much less from an advantaged birth (in the labor market).
- **The intergenerational curves are convex.** The intergenerational curves for men's income, women's income, and men's earnings are especially steep within the parental-income region defined by the 50^{th} to 90^{th} percentiles. Although money does matter in the bottom half of the parental-income distribution, it matters more in the region between the middle- and upper-middle class.

These five fundamental facts reveal a U.S. mobility regime that is sharply different than what prevails in other well-off democracies. What accounts for this particular brand of U.S. exceptionalism? Because we relied on the intergenerational elasticity to characterize mobility, we were able to make headway on this question by applying a new theoretical model of economic persistence, an approach that would not have been possible with the rank-rank slope (as it does not have the same theoretical apparatus standing behind it). We have showed that U.S. persistence is very high because, across nearly all of the institutional domains where persistence is determined, the U.S. has opted for persistence-raising policy. This consistent resort to persistence-raising policy is at odds with the conventional diagnosis attributing our mobility problem exclusively to inequalities in human capital investments. Although these inequalities are of course profound and consequential, the sources of U.S. "mobility exceptionalism" are more far-reaching than this one-factor diagnosis implies.

If the sources of high persistence are understood in one-factor terms, of course our policy options will be seen as limited as well. The analyses presented here make it clear that mobility in the U.S. is affected by a diverse constellation of institutional practices that are often represented as quite irrelevant to mobility. Because we don't typically understand policies aimed at wage compression, neighborhood integration, or income redistribution as bona fide *mobility policies*, the constituency that cares about those policies has come to be more constricted than it should be. We have shown that, insofar as there's an authentic interest in bringing U.S. mobility into line, we have no alternative but to own up to the role of these hidden mobility policies.

Notes

[1] It is of course well understood that equal opportunity is difficult to realize in full and that, even if it could be realized in full, doing so may compromise other normative commitments, such as respecting cultural diversity and allowing for familial intimacy (e.g., Hausman 2015). Nevertheless, different societies depart from equal opportunity to different degrees, and ascertaining the extent of this departure has long been understood as relevant to assessments of distributive justice in the U.S. and elsewhere (e.g., Lamont and Favor 2017).

[2] Although the IGE is strictly a measure of the persistence of economic differences across generations (Jäntti et al. 2006, p. 8), it is commonly interpreted as a measure of economic mobility (Solon 1999; Black and Devereux 2011) that indexes the degree of departure from equal opportunity (Mulligan 1997, p. 25).

[3] Because the IGE is unit-free, it can be compared across time and countries. The sensitivity of the IGE to changes in cross-sectional income distributions should be borne in mind when considering the sources of differences or changes in IGEs. This sensitivity is nonetheless an asset insofar as one seeks (as we do here) a simple descriptive benchmark indicating the share of economic advantages that is transmitted from parents to children.

[4] The discussion in this section pertains exclusively to IGEs estimated under the constant-elasticity assumption.

[5] It should be stressed that not all of the early literature was subject to the same level of measurement-error bias. Most notably, Sewell and Hauser (1975, p. 47) averaged parental income over four years, while research conducted after Solon's critique typically used between 3 and 5 years of parental information (e.g., Solon 1992; Zimmerman 1992). In other research using the Wisconsin Longitudinal Study (e.g., Hauser 1982 [1979]; Tsai 1983), much attention was paid to measurement-error bias.

[6] With the term "post-1990," we are referring to the publication year of the study, not the year in which children's earnings were measured.

[7] When we conducted a formal meta-analysis of the survey-based studies in Corak (2006), we obtained an IGE estimate of close to 0.4 using both 10 and 15 years of parental information (see Online Appendix A, Table A1). This result is in agreement with Solon's (1999) qualitative analysis.

[8] This conclusion is also supported by more recent survey-based estimates of the IGE of men's earnings (e.g., Jäntti et al. 2006; Bratsberg et al. 2007; Gouskova et al. 2010; Mazumder 2016) and of family income (e.g., Mayer and Loopo 2005; Hertz 2005 and 2007; Lee and Solon 2009; Mazumder 2016; Mitnik 2017b).

[9] Contrary to this generalization, Solon (1992) and Zimmerman (1992) report similar IGE estimates. See Grawe (2004a, p. 71) for an explanation of this "exception to the rule."

[10] Attrition is addressed by adjusting the weights of the remaining respondents. When these adjusted weights are used to compute IGEs, the implicit (and strong) assumption is that attrition is independent of children's earnings or income (after controlling for the variables on which the weights are based). Against this assumption, Shoeni and Wiemers (2015) have shown that PSID-based IGE estimates are biased due to selective attrition.

[11] We are referring here to results from past PSID studies. With the addition of new years of data, the PSID can address attenuation bias more successfully (provided the focus is on more recent cohorts).

[12] The fragility of their IGE estimates motivated Dahl and DeLeire (2008) to turn to more robust rank-rank slope estimates.

[13] We would not be much troubled if the operational decisions that yielded IGE estimates similar to Mazumder's were clearly better, from a methodological point of view, than those producing low estimates. This does not seem to be the case. For an argument to the contrary, see Mazumder (2016, pp. 99-100).

[14] We will not discuss Chetty et al. (2014b) in any detail because they report IGE estimates similar to those reported in Chetty et al. (2014a).

[15] In any given year, many individuals are not required to file tax returns, as they fall below the filing threshold.

[16] After excluding the United States, the average IGE among the countries in Corak's (2013, Figure 1) Great Gatsby Curve, which only includes late industrial economies, is 0.30.

[17] In making this point, Mazumder (2016) draws on our own argument to this effect, an argument that appears in the working-paper version of the present article (which circulated with a different title). Although Mazumder (2016, Tables 1 and 2) provides estimates based on averaging children's earnings over 1 to 10 years, we restrict our attention here to his one-year estimates (as otherwise his samples become very small).

[18] In recent research based on the NLS, Bratsberg et al. (2007) report a men's earnings IGE of 0.54 using two years of parental information, and Jäntti et al. (2006) report estimates of 0.52 and 0.53 employing, respectively, one and two years of parental information. These estimates are very large indeed when corrected for attenuation bias. If we assume, perhaps conservatively, that estimates based on one year of parental information underestimate the true value by 30-50 percent (Solon 1999, p. 1778), Jäntti et al.'s estimate would entail an IGE of between 0.73 and 1.04. These estimates are not just substantially larger than the upper bound of the updated consensus range but also substantially larger than Mazumder's (2005) highest estimate based on 16 years of parental information and Mazumder's (2016, Table 2) new PSID-based estimates. The latter only become comparable in magnitude to those of Bratsberg et al. (2007) and Jäntti et al. (2006) when Mazumder (2015) uses between eight and ten years of parental information.

[19] We focus in this paragraph on Dahl and DeLeire's (2008) lower-bound estimate of the men's earnings IGE. We do not consider the earnings IGE for women, which is even lower, because women's earnings are typically not considered to be a meaningful measure of their overall economic status (e.g., Chadwick and Solon 2002:335).

[20] In the case of Fertig (2003), the claim applies to her cohort-specific estimates.

[21] The large body of quasi-experimental and observational evidence on borrowing constraints has, to date, yielded mixed results (e.g., Bulman et al. 2016; Caucutt and Lochner 2017; Carneiro and Heckman 2002).

[22] Although there is a credit market for the purchase of housing, such loans are limited by the parents' capacity to pay, and repayment cannot be postponed until the children start making money.

[23] Although this assumption is commonly made in the literature, it has not received any explicit justification (Grawe 2004b, p. 818).

[24] Another possible source of convexity is ability-based complementarities. If ability is largely endogenous to neighborhood and family environments (e.g., Branigan, McCallum, and Freese 2013), then it's of course appropriate to stress complementarities based on neighborhood or family.

[25] All results from the SOI-M Panel presented in this article are based on statistics originally reported in the Statistics of Income Working Paper "New estimates of intergenerational mobility using administrative data" (Mitnik et al. 2015).

[26] Because parental income is best measured when the children are relatively young, Chetty et al. (2014a) could not use older children in 2011-2012. In 1996-2000, the children selected by Chetty et al. (2014a) were ages 14-18 (1982 cohort), 15-19 (1981 cohort), and 16-20 (1980 cohort).

[27] Although Chetty et al. (2014a) could have used more than five years of parental information, they opted against doing so on the basis of evidence that five years of data were enough to eliminate most attenuation bias.

[28] The income and earnings data for the children in our sample, which come from population records, are only available up to 2010 in the SOI-M Panel.

[29] Because the SOI-M Panel is based on a sample of 1987 tax returns (rather than the full population), it is not large enough to disaggregate by small geographic units (e.g., commuting zones). We are trading off, in effect, the attractiveness of disaggregation for a reduction in lifecycle and attenuation biases.

[30] This is one of those rare cases in which virtue and necessity coincide, as the SOI-M Panel does not include individual earnings data for parents. Although earnings elasticities are more commonly estimated with respect to fathers' earnings, it is hardly novel to use a measure of family income (e.g., Behrman and Taubman 1990; Bratsberg et al. 2007; Chadwick and Solon 2002; Jäntti et al. 2006; Levine and Mazumder 2002). Corak (2006, p.54) and Mazumder (2005, p.250) also noted some of the advantages of using family income over father's earnings.

[31] For details on the two 1987 panels, see Nunns et al. (2008).

[32] See Online Appendix C for (a) a discussion of how the children's parents were defined using the information available in tax returns, and (b) the rules used to pool income across parents' returns (when necessary).

[33] For the sake of readability, we will refer to these concepts as "total income" (or just "income"), "disposable income," and "earnings."

[34] Chetty et al. (2014a) code these children's income as zero. We discuss the rationale for mean imputation (instead of multiple imputation) in Online Appendix F.

[35] In the SOI-M Panel, state taxes were not excised from this measure, and some non-taxable transfers (e.g., Temporary Assistance for Needy Families) were not included. It follows that the measure of "disposable income" used here can only provide an approximation to true disposable income.

[36] See Online Appendix D for a discussion of sampling weights and Online Appendix G for a discussion of weights in model estimation.

[37] Whereas condition (a) is only relevant in the case of income models (i.e., not earnings models), condition (b) applies either to income or to earnings, depending on the model. In carrying out our income analyses, we dropped cases in which these conditions were not met either for total or disposable income,

as doing so allows us to compare results across the two types of income when conducting robustness analyses.

[38] We use expressions like "Z|w" as a shorthand for "$Z|W = w$."

[39] The parameter β_1 is (also) the IGE of the expectation only when the error term satisfies very special conditions (Santos Silva and Tenreyro 2006; Petersen 2017; Wooldridge 2002:17).

[40] Assuming without loss of generality that $x_2 > x_1$, it follows immediately from Equation [3] that the IGE_e can be written as:

$$\text{IGE}_e = \alpha_1 = \frac{\ln E(Y|x_2) - \ln E(Y|x_1)}{\ln x_2 - \ln x_1} \cong \frac{E(Y|x_2) - E(Y|x_1)}{E(Y|x_1)} \left[\frac{x_2 - x_1}{x_1}\right]^{-1},$$

where the approximated equality on the right holds as long as the ratio between x_2 and x_1 is not much larger than 1. This is the rationale for the "share interpretation" of the IGE_e just introduced.

[41] If the mean function is correctly specified, PML estimators are consistent estimators regardless of the distribution of the dependent variable (Gourieroux, Monfort, and Trognon 1984). For all of our models, we replace *Y* and *X* by their short-run counterparts in order to estimate them. See Mitnik (2017c) for information on the estimation of the constant IGE_e using the PPML estimator and the statistical package Stata. It is well known that, by measuring income close to age 40 and averaging parental income over several years, lifecycle and attenuation biases for the IGE_g can be substantially reduced, if not eliminated. The same strategies are appropriate for the IGE_e estimated with the PPML estimator (see Mitnik 2017a).

[42] We use a 95 percent level in constructing confidence intervals and testing hypotheses (here and throughout the analyses reported below). The IGE_e of women's earnings is not presented here because women's earnings are typically not considered to be a meaningful measure of their overall economic status.

[43] This estimate can be found in Chetty et al. (2014a:1574 and Online Appendix C).

[44] The location of the knots is partially based on independent evidence on the likely patterning of nonlinearities (see Chetty et al. 2014a, Online Appendix Figure 1).

[45] We employ numerical approximations to estimate the global IGE_e when nonparametric models are used. In analyses based on the spline and nonparametric models, all inference (save the F test described in the note to Table 3) relies on the nonparametric bootstrap, using 2,000 bootstrap samples. See Online Appendix F for details.

[46] Why is standardization achieved here by dividing by the average value instead of dividing by either the smaller or larger of the values defining a difference? If these two alternative approaches are employed,

the results differ very substantially (unless the differences involved are very small). This has been long deemed an undesirable property (Allen 1934). The value of the $AIGE_e$ always falls between the values that would be obtained if standardization were achieved by dividing, in turn, by the larger or smaller of the values defining differences.

[47] Although these estimates are computed with data from families that are *near* the 10^{th}, 50^{th}, and 90^{th} percentiles, the point estimates are much the same when we instead rely on families *exactly* at those percentiles.

[48] The well-known analytic results and evidence regarding attenuation bias pertain to the estimation of the IGE_g by OLS (e.g., Solon 1992; Haider and Solon 2006). As already indicated, however, Mitnik (2017a) has obtained similar results and evidence for the estimation of the IGE_e with the PPML estimator.

[49] In his empirical analysis with PSID data, Mitnik (2017a) reports that about 13 years of parental information are needed to nearly eliminate attenuation bias. When administrative data are used, he also shows that fewer years of information are most likely needed, as the signal to noise ratio is larger with these data.

[50] The well-known analytical results on lifecycle biases pertains to the IGE_g, but Mitnik (2017a) has obtained similar results for the estimation of the IGE_e with the PPML estimator.

[51] The evidence on income elasticities is less clear because some of the estimates appear to have been driven downward by the Great Recession. Indeed, the relevant plots reveal a marked dip in the IGE_e for male children and pooled children in 2008 and 2009, a result that is consistent with the hypothesis that the recession led to unusually low income elasticities in those years (see Online Appendix I for a discussion of the rationale for pooling men and women in some analyses).

[52] The null hypothesis of no positive difference is clearly rejected (p=0.000) in both cases (see Online Appendix F for details on these bootstrap tests).

[53] Of course, nearly all previous studies based on survey and administrative data (i.e., Mazumder 2005) measure persistence with the IGE_g, not the IGE_e.

[54] Using the 2010 American Community Survey, we found that approximately 3 percent of the population represented in our male SOI-M sample was institutionalized, a subpopulation that PSID and NLS analyses fully exclude.

[55] Within the PSID, the relevant attrition rate is as high as 41 percent, with attrition "particularly high among low-income adult children with low-income parents and particularly low for high-income adult

children with high-income parents" (Schoeni and Wiemers 2015, p. 351). The net effect of this pattern of attrition on IGE estimates is to push them downward.

[56] In the PSID, full income information is only collected for household heads and their spouses (or long-term partners). This should lead to negative selection bias because those who are not household heads (a) are often in a more compromised labor market situation, and (b) are disproportionately drawn from low-income families. In the PSID, approximately six percent of the men who are 35-40 years old in the 1987-2012 data are excluded from mobility analyses, as they are not household heads and thus do not have full income information.

[57] Even after supplementing tax data with information from other administrative sources (e.g., earnings reports), 6.1 percent of children in Chetty et al.'s core sample (2014a: Online Appendix, Table III), and 7.1 percent of children in our core sample (see Table 1), have no income data. We expect that some of those without income information will have positive incomes (as they may make less than the filing threshold or work in the informal economy).

[58] This is the approach taken by Chetty et al. (2014a) to assess the robustness of their IGE estimates. When they used the values \$1 and \$1,000 to estimate the IGE_g for men and women pooled, their estimates were 0.34 (dropping children with zero income), 0.41 (recoding \$0 to \$1,000), and 0.62 (recoding \$0 to \$1).

[59] The approach of computing region-specific IGEs is similar to that of Couch and Lillard (2004). We employ numerical approximations to estimate them. See Online Appendix F for details.

[60] Between any two of these quantiles, 0.5 percent of the children in the population will be found. The curves shown in the figures pertain to parental income between the first and the 99^{th} percentiles.

[61] The intervals reported in Table 6 cannot be used to test this hypothesis because estimates across regions are correlated within bootstrap samples.

[62] We are of course referring here to the expectation across levels of parental income within that region (a caveat that will not be repeated from hereon in).

[63] The seeming discrepancy in the below-P10 region is not necessarily troubling. When we examined the cases in that region, we found that the spline model is affected by a small number of outliers. If those outliers are dropped, the nonparametric and spline models deliver similar estimates (i.e., 0.34 and 0.38 respectively) for the below-P10 region (see Online Appendix M for details).

[64] This theoretical model gives priority to (a) identifying as many important policy levers as possible, and (b) allowing straightforward interpretation of its policy-relevant results. By not taking complementarities

into account, it is able to focus the analysis on a very tractable summary measure of persistence (i.e., the constant IGE_e) and to deliver unambiguous policy results. Although models that do take into account complementarities (e.g., Becker et al. forthcoming; Durlauf and Seshadri 2018) are highly illuminating, they do not satisfy the above requirements. Moreover, Mitnik's (2018) model *does* take into account the role of neighborhoods and parental education, which are the main concerns of both Becker et al. (forthcoming) and Durlauf and Seshadri (2018). Unlike Becker et al. (forthcoming) and Durlauf and Seshadri (2018), these roles are not built in by positing heterogeneity in parameter values across neighborhoods or levels of parental education, but by including the parents' human capital and a key neighborhood characteristic (average human capital) among the inputs used in the production of the offspring's human capital.

[65] The progressivity of the tax system and the generosity of the welfare system are jointly determined by δ_1 and a second parameter, but δ_1 alone determines the ratio between the two Gini coefficients.

[66] In opposition to a very extensive literature, Landersø and Heckman (2017) recently claimed that the low level of economic persistence in Denmark is a "fantasy," mainly because (a) intergenerational educational mobility in Denmark and the United States are remarkably similar, and (b) the difference in intergenerational economic persistence across the two countries is very sensitive to the income measure used to compute it (and, in particular, is small when computed with a measure of before-tax-and-transfers income). Andrade and Thomsen (2018) and Helsø (2018) have provided very strong empirical evidence against both arguments (see our Figure 7 for some of Helsø's results).

[67] Like our estimates, Helsø's (2018) estimates (a) pertain to children who were 35-38 years old in 2010, and (b) are based on 9 years of parental income information collected when the children were between 15 and 23 years old. The earnings IGEs are, like our IGEs, computed with respect to parental disposable income. Similarly, the family-income IGEs are, like our IGEs, computed with pre-tax measures of income.

[68] The constant-elasticity estimates for Denmark are from Helsø (2018, Table 2 and Figure 3). The nonparametric estimates are based on the same sample and income measures and were provided to us by Helsø.

[69] As Loeb and Socias (2004) point out, the federal contribution to the funding of K-12 education in the United States is quite regressive, although it's commonly and incorrectly viewed as compensatory.

[70] There is also very strong evidence that residential income segregation has increased markedly in the United States since the 1970s (see Galster and Sharkey [2017]).

[71] We have not addressed here the productivity of investments in human capital because, even if we focus exclusively on formal education, the differences between the educational systems of the United States and Denmark make a comparison difficult. The available literature suggests, nonetheless, that Denmark is not likely to be less efficient than the United States in producing human capital (see Online Appendix N). It follows that this factor is unlikely to account for the low intergenerational persistence in Denmark.

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Figure 1. Stylized Representations of the Relationship between Parents' Income and Men's Earnings

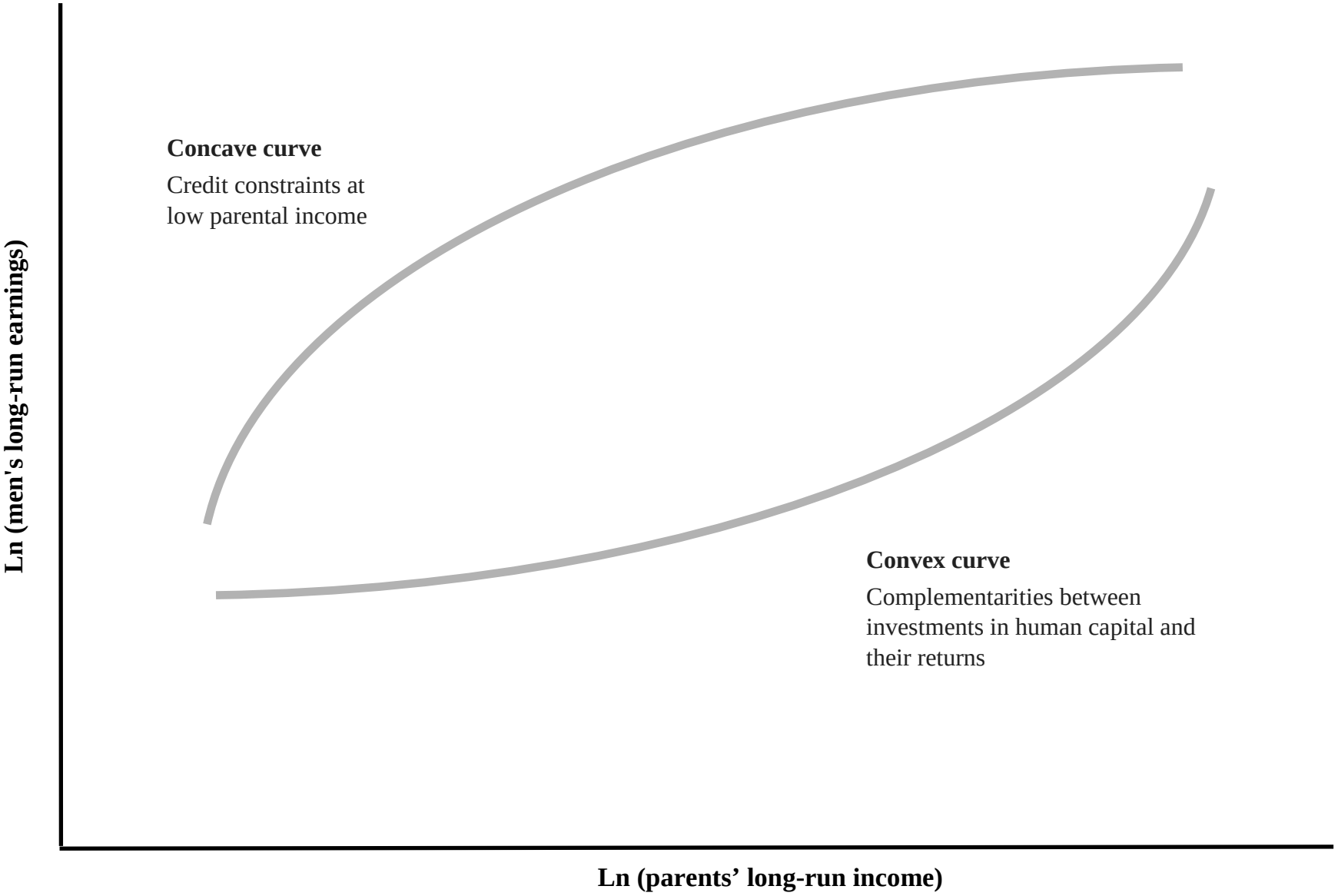
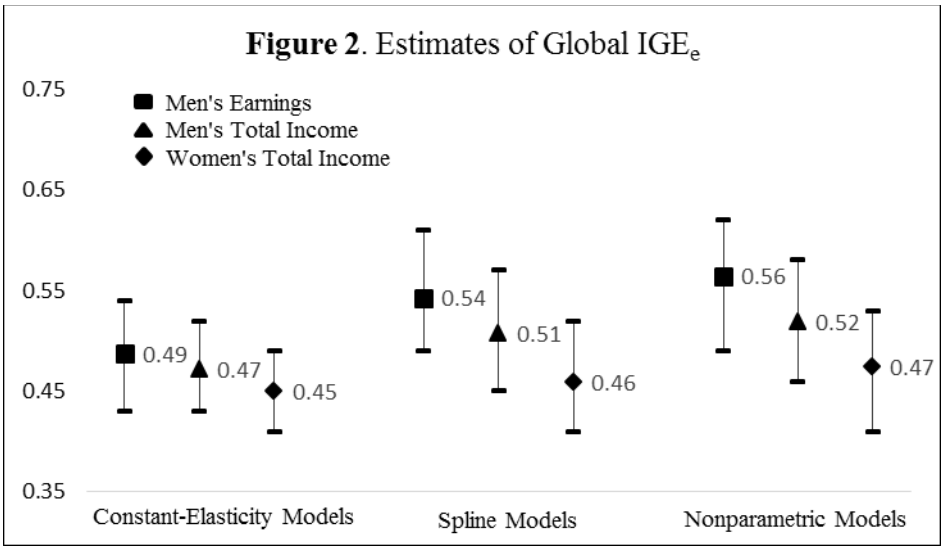


Figure 2. Estimates of Global IGE_e



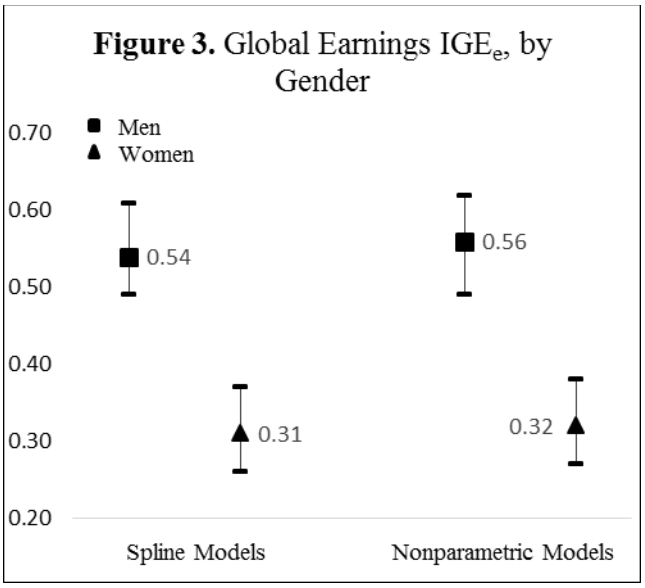


Figure 3. Global Earnings IGE$_e$, by Gender
Men
Women
0.70
0.60
0.50
0.40
0.30
0.20
0.54
0.31
0.56
0.32
Spline Models
Nonparametric Models

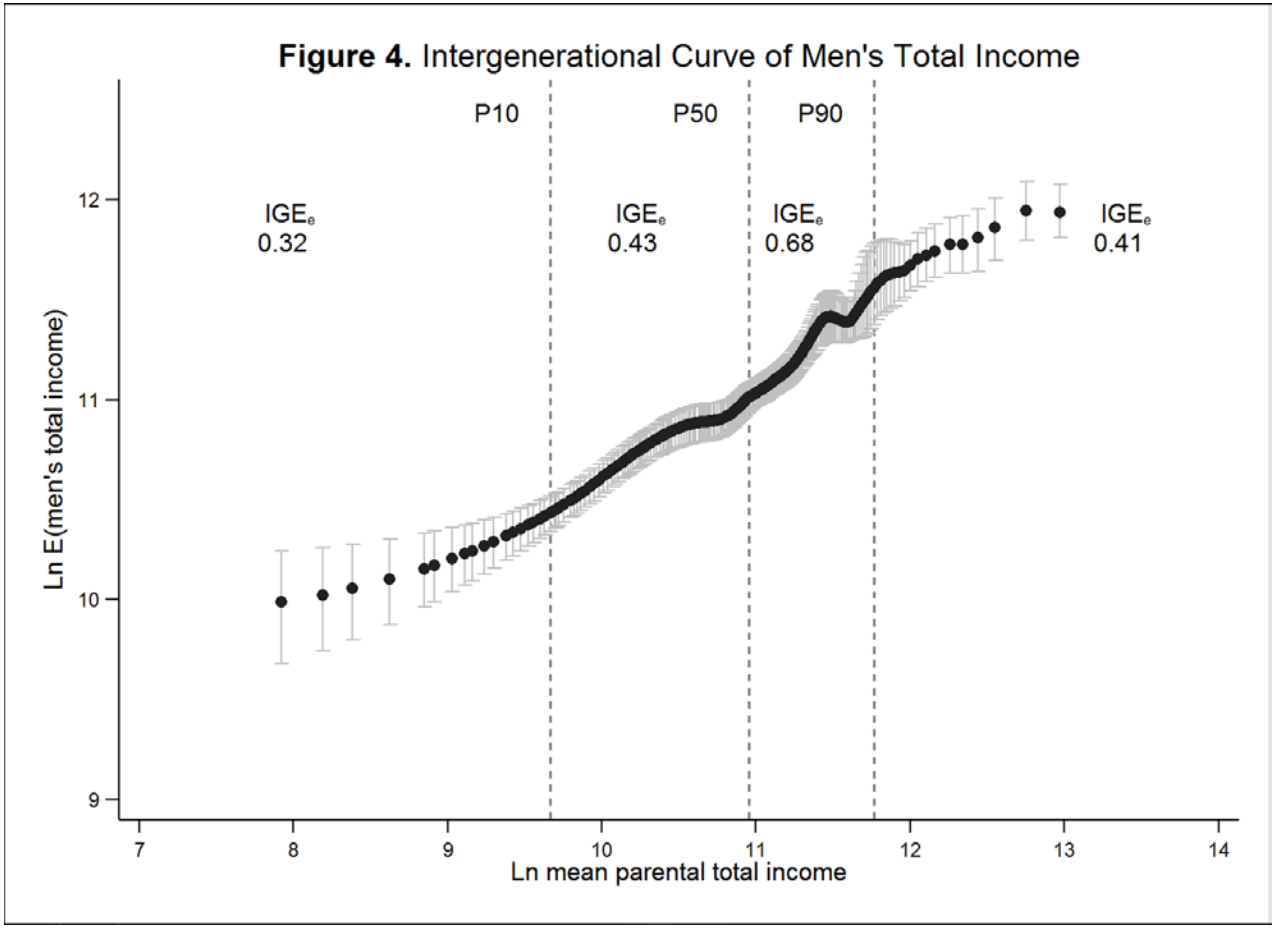


Figure 4. Intergenerational Curve of Men's Total Income
P10
P50
P90
IGE_e 0.32
IGE_e 0.43
IGE_e 0.68
IGE_e 0.41
Ln E(men's total income)
12
11
10
9
7
8
9
10
11
12
13
14
Ln mean parental total income

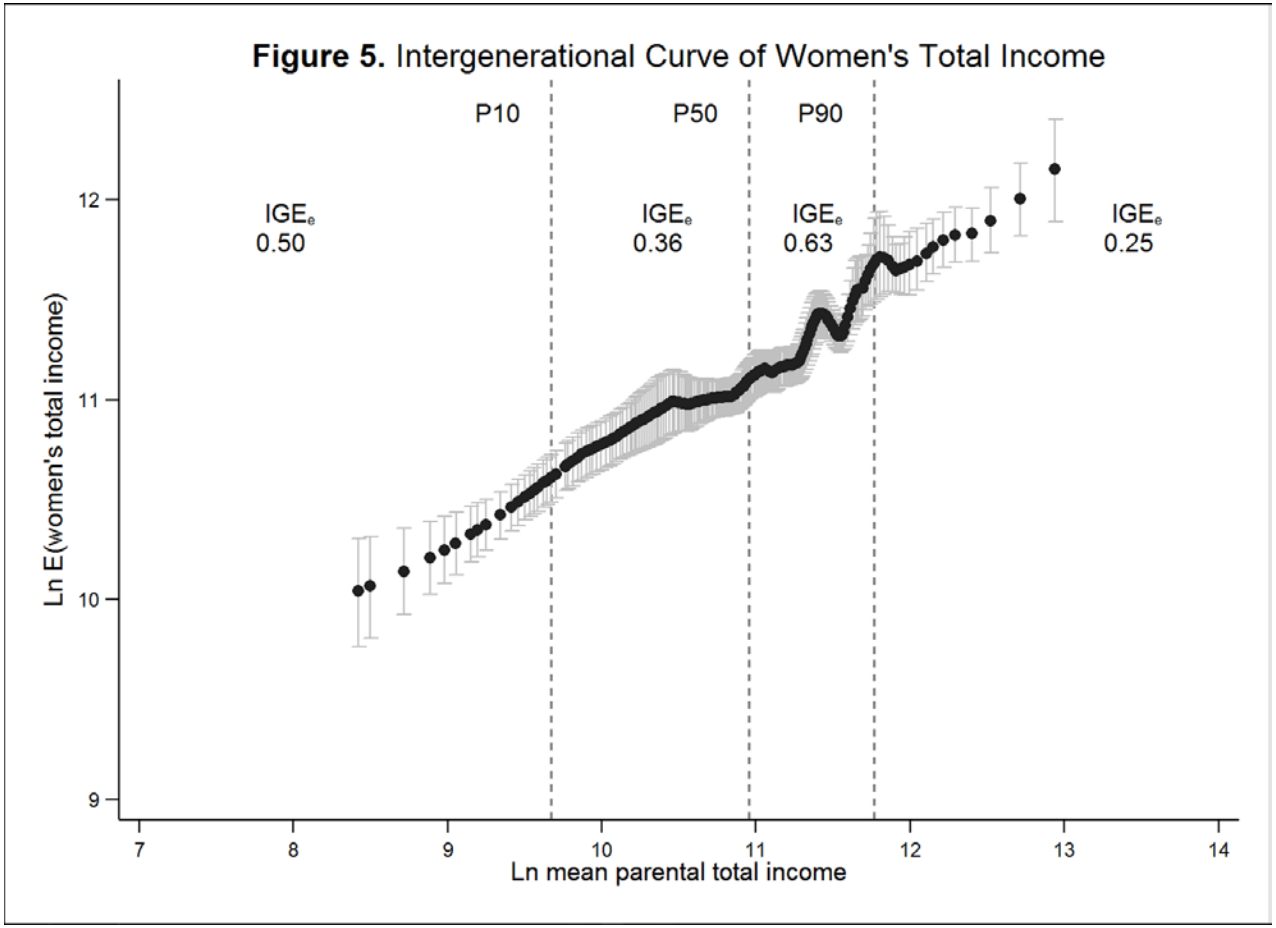


Figure 5. Intergenerational Curve of Women's Total Income
P10
P50
P90
IGE_e
0.50
IGE_e
0.36
IGE_e
0.63
IGE_e
0.25
Ln E(women's total income)
12
11
10
9
7
8
9
10
11
12
13
14
Ln mean parental total income

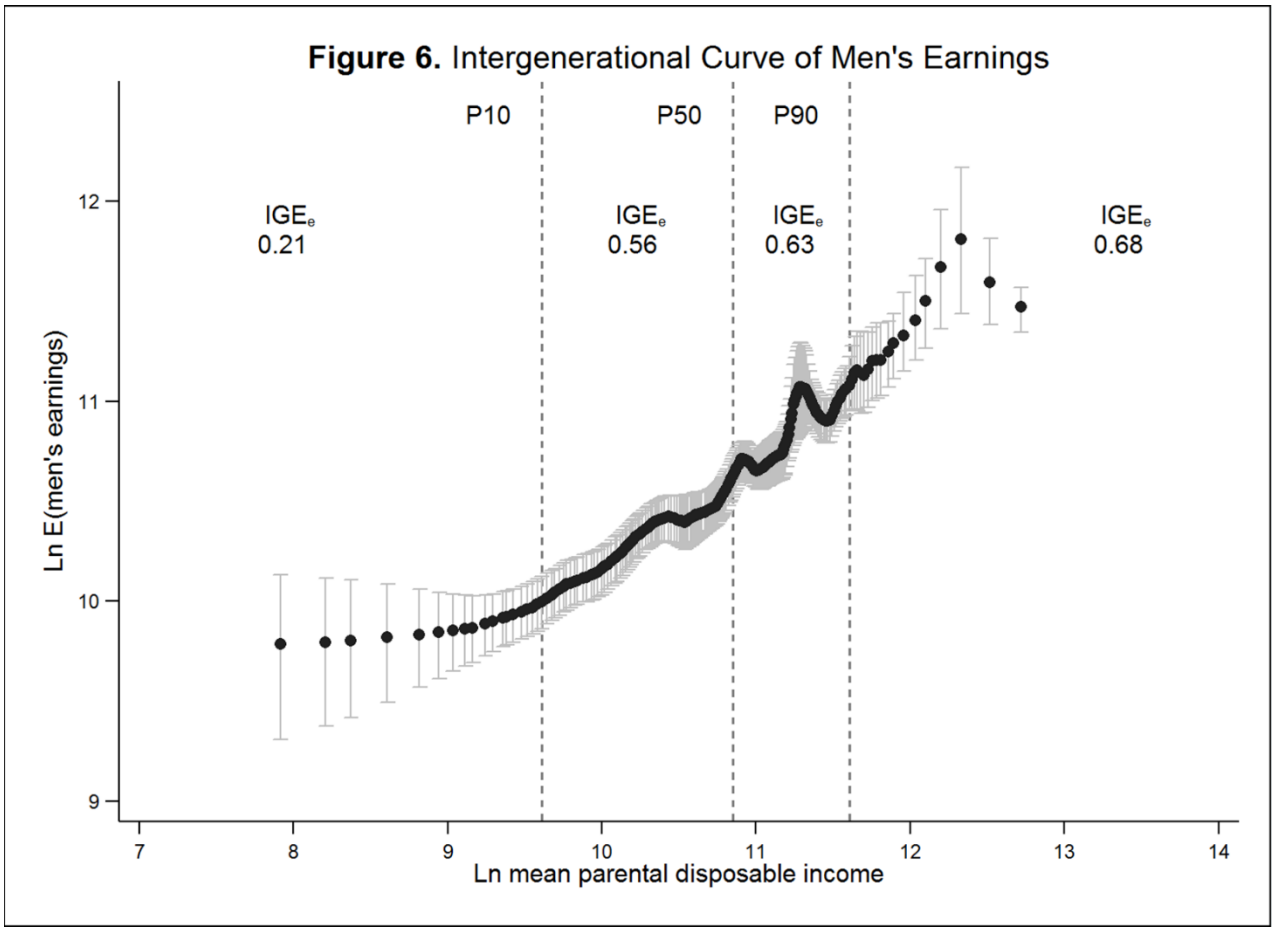


Figure 6. Intergenerational Curve of Men's Earnings
P10
P50
P90
IGE$_e$ 0.21
IGE$_e$ 0.56
IGE$_e$ 0.63
IGE$_e$ 0.68
Ln E(men's earnings)
12
11
10
9
7
8
9
10
11
12
13
14
Ln mean parental disposable income

Figure 7: Global IGE_e, United States and Denmark

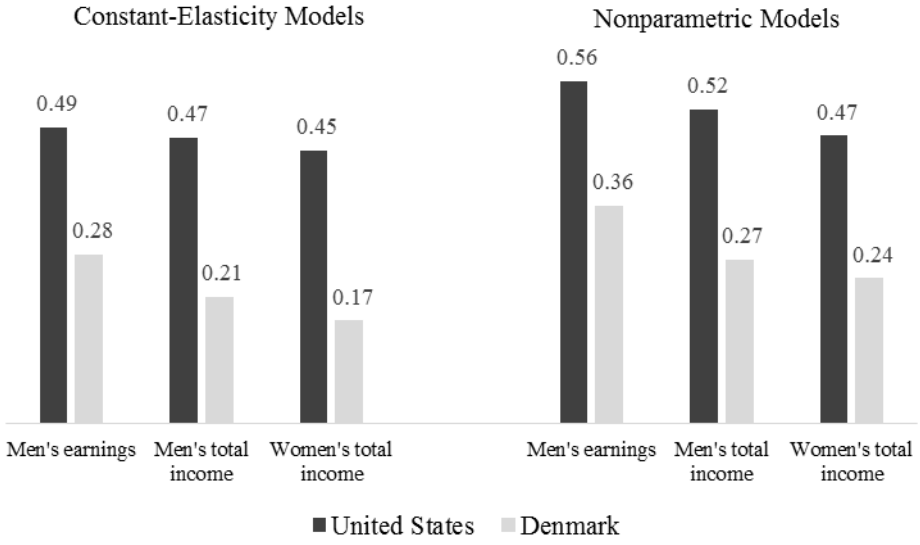


Table 1. Demographic Statistics, Income Sources, and Missing Information (Unweighted Percentages)

	Models	
Variables	Income	Earnings
Child's gender (% fem.)	49.4	49.1
Child's age		
35	23.7	23.7
36	24.1	24.1
37	25.1	25.1
38	27.1	27.1
Child's income information		
Return	88.8	NA
W-2 + UI	4.1	NA
CPS-based imputation	7.1	NA
Number of missing years of parental information		
0	95.2	95.3
1	2.5	2.5
2	1.5	1.5
3	0.8	0.8
Sample size	12,469	12,872

Notes: Children with more than 3 missing years of parental information are excluded from all samples. NA = Not Applicable (variable not relevant).

Table 2. Income and Parental-Age Statistics (Weighted Values)

	Models	
Variables	Income	Earnings
Child's total income		
Mean	69,329	NA
Standard deviation	107,061	NA
Child's disposable income		
Mean	59,239	NA
Standard deviation	80,890	NA
Child's earnings		
Mean	NA	36,547
Standard deviation	NA	56,436
Average parental total income over 9 years		
Mean	74,826	NA
Standard deviation	115,622	NA
Average parental disposable income over 9 years		
Mean	63,530	64,183
Standard deviation	84,744	91,706
Average parental age over 9 years		
Mean	45.3	45.4
Standard deviation	6.2	6.2

Notes: Monetary values in 2010 dollars (adjusted by inflation using CPI-U-RS). NA = Not Applicable (variable not relevant).

Table 3. Global IGE_e Estimates and Tests

	Constant-Elasticity Models	Spline Models	Nonpara-metric Models	P-value from CE test
Men's earnings	0.49	0.54	0.56	0.000
	(0.43-0.54)	(0.49-0.61)	(0.49-0.62)	
Men's total income	0.47	0.51	0.52	0.011
	(0.43-0.52)	(0.45-0.57)	(0.46-0.58)	
Women's total income	0.45	0.46	0.47	0.016
	(0.41-0.49)	(0.41-0.52)	(0.41-0.53)	

Note: The constant elasticity (CE) test is an F-test of the null hypothesis that all coefficients of the spline model of Equation [4], save α_0 and α_1, are zero.

Table 4: Estimates of the Arc IGE_e

	Well-off / Poor Families	Global
Spline Models		
Men's earnings	0.68	0.60
	(0.61-0.75)	(0.54-0.67)
Men's total income	0.64	0.56
	(0.57-0.71)	(0.50-0.63)
Women's total income	0.57	0.50
	(0.49-0.64)	(0.44-0.56)
Nonparametric Models		
Men's earnings	0.64	0.59
	(0.56-0.71)	(0.53-0.66)
Men's total income	0.65	0.56
	(0.57-0.73)	(0.50-0.63)
Women's total income	0.60	0.49
	(0.52-0.67)	(0.43-0.55)

Note: "Poor families" pertain to the 5^{th}-15^{th} percentiles and "well-off families" pertain to the 85^{th}-95^{th} percentiles.

Table 5. Sensitivity of Nonparametric Estimates of theTotal-Income Global IGE_e to the Treatment of Nonfilers

Selection Rule	Imputation for Nonfilers w/o Adm. Infor.	Treatment of CPS $0s in Computing Means	Men's IGE_e	Women's IGE_e
All people				
All people	$0		0.54	0.49
All people	$1		0.54	0.49
All people	$100		0.54	0.49
All people	$1,000		0.53	0.49
All people	$3,000		0.53	0.48
All people	CPS means	CPS $0 → $0	0.52	0.47
All people	CPS means	CPS $0 → $1	0.52	0.47
All people	CPS means	CPS $0 → $100	0.52	0.47
All people	CPS means	CPS $0 → $1,000	0.52	0.47
All people	CPS means	CPS $0 → $3,000	0.52	0.47
Selected Sample				
Filers only	NA	NA	0.43	0.41
Filers + nonfilers w/ adm. inf.	NA	NA	0.45	0.42

Table 6. Region-Specific Estimates of the IGE_e

	Up to P10	P10-P50	P50-P90	Above P90	P-value from Bootstrap Test of H0: P10-P50 ≥ P50-P90
Spline Models					
Men's earnings	0.00	0.52	0.75	0.35	0.040
	(-0.21-0.41)	(0.37-0.66)	(0.58-0.91)	(0.25-0.46)	
Men's total income	0.14	0.45	0.69	0.37	0.045
	(-0.07-0.50)	(0.33-0.58)	(0.50-0.90)	(0.26-0.47)	
Women's total income	0.22	0.36	0.63	0.42	0.013
	(0.02-0.67)	(0.21-0.49)	(0.47-0.78)	(0.33-0.52)	
Nonparametric Models					
Men's earnings	0.21	0.56	0.63	0.68	0.361
	(-0.05-0.51)	(0.39-0.73)	(0.41-0.81)	(0.22-1.04)	
Men's total income	0.32	0.43	0.68	0.41	0.027
	(0.17-0.48)	(0.32-0.54)	(0.48-0.90)	(-0.07-0.81)	
Women's total income	0.50	0.36	0.63	0.25	0.060
	(0.28-0.73)	(0.20-0.52)	(0.40-0.87)	(-0.27-0.68)	

Table 7. Additional Assessments of Convexity (Men's Earnings)

Null Hypothesis	P-value from Bootstrap Test	
	Spline model	Nonparametric model
IGE_e up to P10 $\geq$ IGE_e in P10-P90	0.004	0.009
IGE_e up to P20 $\geq$ IGE_e in P20-P90	0.001	0.023

Online Appendices

A VERY UNEVEN PLAYING FIELD:

ECONOMIC MOBILITY IN THE UNITED STATES

CONTENTS

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B. Survey-Data Evidence on Nonlinearities

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D. Construction of Sampling Weights

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F. Additional Information on Nonparametric and Spline-Based Estimation and Bootstrap-Based Inference

G. Use of Sampling Weights in Model Estimation

H. Evidence on Attenuation Bias

I. Evidence on Lifecycle Bias

J. Mechanical Relationship between the IGE_e, Dispersion and the Rank-rank Slope

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L. Earnings Elasticities and the Missing Poor

M. Supplementary Estimates of Region-Specific IGEs

N. Productivity of Investments in Human Capital in Denmark and the United States

A. Meta-Analysis of Survey-Based Studies in Corak (2006)

In an influential paper, Corak (2006) carried out both (a) a quantitative meta-analysis of the existing estimates in the literature of the (constant) IGE of men's earnings, and (b) a qualitative analysis of that literature. We argued in the main text that these analyses were strongly influenced by the results published in Mazumder (2005). This is for two reasons. First, Corak (2006) selected Grawe's (2004) PSID-based estimate of 0.47 as the preferred estimate for the IGE of men's earnings, largely because this value was consistent with Mazumder's estimates with a similar number of years of parental information. Second, when we repeated Corak's (2006) meta-analysis excluding Mazumder's (2005) results, we obtained substantially lower predicted IGEs. We provide here additional details on Corak's meta-analysis and our follow-up analysis.

The right-hand variables in Corak's meta-analysis were the number of years of parental information employed, the age at which father's earnings are measured, and a dummy variable indicating whether OLS or IV estimation was conducted. Because Corak included only those studies in which all three of these right-hand variables were available, his meta-analysis is based on 22 of the 41 estimates he listed in his review. We repeated Corak's (2006) meta-analysis using the same right-hand variables but kept only the survey-based estimates. That is, we excluded the administrative-data results reported in Mazumder (2001), which is a preliminary version of Mazumder (2005). As in Corak's analysis, we generated predicted IGEs under the assumptions that (a) father's earnings were measured around age 45, and (b) 5, 10, and 15 years of parental information were used. Table A1 shows that Corak's (2006) predictions are substantially higher that those we obtain after excluding Mazumder's estimates from the meta-analysis.

B. Survey-Data Evidence on Nonlinearities

We noted in the main text that, because of sample-size constraints, only a few researchers have used survey data to examine possible nonlinearities in the relationship between parents' and

children's income and earnings in the United States (in log-log space). We also noted that the results of these studies were inconclusive. Here we discuss the evidence behind those claims.

In his seminal PSID-based paper, Solon (1992) made a heroic attempt to assess nonlinearities with two very small samples, an attempt that proceeded by adding the square of log parental earnings into the standard specification. The corresponding point estimates suggested that the IGE of men's earnings increased with parental earnings, but those estimates did not reach statistical significance (Solon 1992, p. 404). In another analysis of PSID data using the same approach, Behrman and Taubman (1990) reported statistically significant results that were qualitatively similar to Solon's, but they were obtained by pooling men and women. By contrast, Mulligan's (1997) analysis of PSID data did not find evidence of nonlinearities, which he suggested might be the result of the PSID's underrepresentation of very rich individuals. Also using the PSID, Hertz (2005) analyzed the Black and White subsamples separately, employing nonparametric methods to uncover the relationship between children's and parents' log income. The estimated curves show a convex pattern that is particularly marked for Blacks, but Hertz neither estimated the pooled curve nor provided any inferential information for the curves he did estimate.

The NLS-based findings on nonlinearities are also inconclusive. Although some scholars have reported results indicating that the men's earnings IGE increases with father's earnings (Lillard 2001; Bratsberg et al. 2007; Couch and Lillard 2004), those results appear highly sensitive to the details of the specification, indeed several specifications suggest that the IGE decreases in at least some ranges of father's earnings.[1] There is also very substantial variability across the studies in the implied values of the IGEs under the estimated curves.[2]

It follows that neither the PSID nor the NLS have delivered clear evidence on the question at hand. To be sure, the available studies suggest convex curves on balance, but the evidence is very far from definitive. Moreover, the approaches typically employed to assess nonlinearities relax the

constant-elasticity assumption in a quite limited way, with the implication that the actual pattern of nonlinearities in the data remains ambiguous.

C. Construction of Income and Parental-Age Variables

We provide here additional information on the income and parental-age variables used in our analyses. We discuss (a) how the children's parents were defined from the information available in tax returns and the rules used to pool income across parents' returns (when necessary), and (b) the data used to conduct mean imputation for nonfiler children with no W-2 earnings and no UI income.

Parental income and age. The person or persons who claim a child as a dependent in 1987 were defined as the child's parents. If only one nondependent adult claimed the child (i.e., there was no "secondary filer" in the return), that nondependent adult was typically defined as the (single) parent of the child. However, whenever the adult claiming the child was married and the spouse filed separately, both spouses were defined as parents. It is of course possible that one or both parents of a child will in subsequent years file with someone else (as with most divorces). In that case, annual parental income was computed by pooling resources across the relevant returns, where the pooling is based on the following rules:

(a) If the two parents divorced, and each filed jointly with a new spouse, parental income was defined as the sum of half the income appearing in each parent's return (as pooling the full income appearing on the returns for each of the remarried parents would have overstated the income of the child's parents).

(b) If only one of the two parents filed with a new person, that parent's imputed income (calculated again by dividing by two) was combined with the full income of the other parent.

(c) If a parent was single in 1987 but later married, the pooled income of the parent and his or her spouse was used.

Parental age in each year was computed by averaging the age of the parents listed in any return that year. Average parental income and age when the children were 15-23 years old were computed by averaging the corresponding annual measures.

Imputations for nonfiler children. The Annual Social and Economic Supplement of the Current Population Survey (CPS-ASEC) identifies likely nonfilers using a tax simulation model. Although this information is available for the entire period covered by the SOI-Panel, the CPS-ASEC data after 2003 have serious inconsistencies and cannot be used. We therefore used pooled CPS-ASEC data from 1999 to 2003 to compute the mean income of nonfilers (without earnings or UI income) by gender and age group. The resulting values, which we used for mean imputation, are as follows (all in 2010 dollars): 26-30 year-old men: $4,910; 31-35 year-old men: $5,815; 36-40 year-old men: $6,706; 26-30 year-old women: $5,372; 31-35 year-old women: $6,574; 36-40 year-old women: $7,560.

D. Construction of Sampling Weights

As indicated in the main text, the SOI Family Panel is based on a stratified random sample of 1987 tax returns, with a sampling probability that increases with income. The sampling weights of the SOI-M Panel take into account (a) the foregoing sampling probability from the SOI Family Panel, (b) the probability that a return will enter the refreshment segment of the OTA Panel, and (c) estimates of the probability that a dependent child will have a valid Social Security number (and hence will be included in the base sample of the SOI-M Panel) given that he or she is included in a "qualifying return" (i.e., a 1987 return from the SOI Family Panel or a return from the refreshment segment of the OTA Panel). The latter estimates were computed separately for each sampling stratum, with the refreshment segment of the OTA Panel treated as a separate sampling stratum.

E. Comparison between the base sample of the SOI-M Panel and the CPS

Because the SOI-M Panel is new, it is especially important to compare it against known high-quality samples, such as the CPS. In Table A2 and Figure A1, we evaluate the representativeness of

the base sample of the SOI-M Panel against 1987 data from the CPS-ASEC. The purpose of Table A2 is to show that the weighted age distribution and gender distribution in the SOI-M Panel approximate well the corresponding distributions in the CPS-ASEC. It is equally important to compare the 1987 weighted family income distributions in the base sample and in the CPS-ASEC (for children 12 to 15 years old).[3] Although Figure A1 reveals that the two distributions are very similar, an important difference is that the share of children at the extreme right tail in the SOI-M Panel is almost twice as large as the corresponding CPS-ASEC share. This result is consistent with research documenting an underreporting of top incomes in the CPS (Fixler and Johnson 2014). We also find that the SOI-M, as compared to the CPS-ASEC, has a larger share of children in families with parental income between $10,000 and $30,000 but a smaller share of children in families with parental income between $50,000 and $70,000. These differences are relatively minor and, overall, the results are again satisfactory. It is especially reassuring that there is no evidence of a deficit of poor children in the SOI-M Panel.

F. Additional information on nonparametric and spline-based estimation and bootstrap-based inference

We provide here some additional information regarding nonparametric and spline-based estimation and bootstrap-based inference. For reasons that will become clear, we also explain here why we opted for using mean instead of multiple imputation for nonfilter children with no W-2 earnings and no UI income.

Nonparametric estimation of global and region-specific elasticities. We employ numerical approximations to estimate either a global or region-specific IGE_e when nonparametric models are used. The estimated nonparametric curve is divided into 196 segments (between the 1st and the 99th percentiles of parental income) such that each segment covers the same share of children in the population (i.e., 0.5 percent). We then use finite differences in logarithms to approximate the average point elasticity in each segment. This allows us to compute the global and region-specific elasticities

by averaging the estimated elasticities across all relevant segments. Our computation of the global and region-specific IGEs ignores the curve's final left and right segments (each covering 1 percent of children). Because the curve is estimated less precisely at the boundaries, these "trimmed estimators" are often more efficient. The point estimates from the trimmed and untrimmed estimators are, however, very similar.

Estimation of global arc elasticities. We employ a numerical approximation to estimate the global AIGE_e. This summary persistence measure can be written as:

$$\text{Global AIGE}_\text{e} = \int_0^{100} \int_0^{100} AIGE_e(Y|x, \min(u,v)\,, \max(u,v))\; du\; dv\,,$$

where to simplify notation we have ignored that the space of integration has to exclude those cases in which $u = v$. We approach the value of this integral by computing:

$$\text{Global AIGE}_\text{e} \cong \frac{1}{40{,}000} \sum_{i=1}^{40{,}000} AIGE_e(Y|x, min(u,v)_i, \max(u,v)_i),$$

where the $(u,v)_i$ are obtained by randomly drawing one value from each of 40,000 cells spanning the space of integration. These cells are defined so that each contains the same share of "pairs of children." The procedure is as follows. First, the estimated curve is divided into 200 segments, such that each segment covers the same share of children in the population (i.e., 0.5 percent). A matrix with 40,000 cells is then generated by cross-tabulating a variable indexing the segments of the curve with itself. Next, for each cell, a value of parental income is randomly drawn from each of the two segments of the curve defining the cell. With these 40,000 pairs of parental-income values in hand, we compute for each one the pair of corresponding conditional children's expectations. We do so in each case by linearly interpolating the needed expectations from the estimated expected values associated to the values of parental income defining the corresponding segments of the curve. Finally, we compute the arc elasticity for each sampled pair of families, and the average arc elasticity across all pairs. The latter is our estimate of the global AIGE_e.

Bootstrap-based inference. In analyses based on the spline and nonparametric models, all inference (save the test of the constant-elasticity assumption, which is described in the note to Table 3) relies on the nonparametric bootstrap, using 2,000 bootstrap samples. We generate bootstrap samples via simple random sampling of primary sampling units, with replacement, within strata. When estimating any nonparametric model with bootstrap samples, we keep the smoothing parameter fixed at the value selected for the original sample. This is equivalent to keeping the functional form fixed when carrying out bootstrap-based inference with parametric models (Racine and Parmeter 2014, p. 313 and note 12).

To assess the uncertainty of our spline-model estimates, we use the percentile method to compute confidence intervals (Efron and Tibshirani 1986). However, we can only compute "variability bounds" (Racine 2008) or "confidence bands" (Wasserman 2006) for our nonparametric estimates (for which we also use the percentile method). Indeed, with a nonparametric regression the bootstrap does not deliver true confidence intervals, as bias does not disappear asymptotically. It follows that the bootstrap-generated intervals are not centered around the true "parameters."[4] Under the reasonable assumption that the bias is small, the variability bounds nonetheless provide approximations to true confidence intervals (e.g., Wasserman 2006, p. 89).

We test one-sided null hypotheses by computing "type-2 p-values," which were developed specifically for bootstrap-based tests (Singh and Berk 1994; see also Liu and Singh 1997; Efron and Tibshirani 1998). These type-2 p-values, which we call "p-values" for simplicity, are computed as the proportion of bootstrap samples in which the null hypothesis is true. They can be interpreted as standard p-values.

Mean imputation instead of multiple imputation. Nonparametric estimation, numerical approximation, and bootstrap-based inference are all computer intensive. Using them together, as we do here, is therefore very demanding, as it requires conducting nonparametric estimation and numerical approximation with each bootstrap sample. In the context of bootstrap-based inference,

multiple-imputation needs to be nested within the bootstrap resampling process, thus multiple imputation greatly exacerbates "computing costs." For instance, conducting bootstrap inference with 2,000 samples and five imputed datasets would have required us to repeat 10,000 times the nonparametric estimation of each model we estimate, plus the associated numerical approximations. This may be compared to repeating it a mere 2,000 times with mean imputation. For this reason, we opted for using mean imputation instead of multiple-imputation when imputing the income of nonfiler children with no W-2 earnings and no UI income. The use of mean imputation should lead to a small underestimation of the uncertainty of our estimates (Little and Rubin 2002).

G. Use of Sampling Weights in Model Estimation

Although there are circumstances in which the use of weights in model estimation is not the best approach (e.g., Winship and Radbill 1994), we apply sampling weights in all of our analyses. This decision to use weights is partly an empirical one. As Pfeffermann and Sverchkov (2009, p. 455) clearly put it, if the sampling weights are "related to the values of the model outcome variable even after conditioning on the model covariates," ignoring the information contained in sampling weights "may yield large biases and erroneous inference." When we tested for bias in constant-elasticity models without weights (see Du Mouchel and Duncan 1983; Nordberg 1989), the null hypothesis of no bias was systematically rejected, a result that informed our decision to apply weights in our analyses.

As is well known, using weights in model estimation entails a loss of precision (e.g. Winship and Radbill 1994), a loss that increases when the weights cover a wide range (e.g., Skinner and Mason 2012). This is unfortunately the case in the SOI-M Panel (i.e., final weights go from 1 to 5,400). In our analyses, we carry out standard Horvitz-Thompson (e.g., Fuller 2009) estimation with sampling weights, which is the usual approach employed in the social sciences. Alternative approaches proposed by Pfeffermann and Sverchkov (1999) and Skinner and Mason (2012), which

were developed to improve precision when weights cover a wide range, did not lead to consistent improvements in precision with our data.

H. Evidence on Attenuation Bias

Since the early 1990s, mobility researchers sought to reduce attenuation bias by using averages of 3-5 years of parental income or earnings, with the assumption that such averages proxied reasonably well for lifetime average parental income or earnings. In his now-classic article, Mazumder (2005) argued that averaging over 3-5 years was not nearly enough, indeed he suggested that about 16 years of parental information, and perhaps more, are needed to eliminate or nearly eliminate attenuation bias. When Mazumder used progressively more years of parental information (from SSA records), the estimated earnings elasticities increased substantially. This research led to a growing consensus that a long time series of parental data was needed to secure good estimates. Against this consensus, Chetty et al. (2014) reported that (constant-elasticity) estimates from tax data nearly stabilize once 5 years of information are employed: The income IGE_g increased only 6.4 percent, from 0.344 to 0.366, when Chetty et al. used 15 years of parental information instead of 5 (2014, Table 1 and Online Appendix E).

We revisit here the issue of attenuation bias with the SOI-M Panel and the IGE_e. We proceed, as is conventional, by examining how constant-elasticity estimates change as additional years of parental information are incorporated. We complete our analyses with two different approaches, one based on a common sample, and another based on common sample-inclusion rules (shortened to "common rules" from hereon in).[5]

The goal of the first approach is to use the same sample as we increment the number of years of parental information (for any income measure and gender). We would of course ideally use precisely the samples employed in our main research to reestimate the IGE_e as we successively increment the number of years of parental income. However, because a 9-year average of parental income may be positive while an average based on fewer years may not, some observations have to

be dropped when using parental variables computed with fewer than 9 years. As a result, the goal of this approach cannot be fully attained, but it can be approximated to a very large degree.

Under the common rules approach, we apply our sample-selection rules with each of the parental measures (based on one through nine years of parental information), with the implication that the samples will not likely be the same across these measures. The observations that are excluded, for example, because parental income is above $7,000,000 for a 3-year average may stay in the sample when the average is computed with a different number of years. A further complication arises when applying our sample-inclusion rule that children should have at least 6 years of parental information available (when they are 15-23 years old). This rule may be interpreted to require an absolute number of 6 years or to require two-thirds of the maximum number of years. We implemented both interpretations, but here we only report the results generated under the second (as the results are much the same for either approach).[6]

We start by examining the results obtained with the common sample approach. In Figures A2 and A3, we present the estimates of the total-income IGE_e and earnings IGE_e for men, and of the total-income IGE_e for women, respectively. We also present corresponding total-income estimates after pooling the samples for men and women (see Figure A4).[7]

For both genders, the income and earnings estimates increase over the full range of years of parental information, but they increase more rapidly between one and 4-5 years of parental information than for further increases. The estimates for women increase only marginally after 7 years of information, whereas the estimates for men increase at a slower rate after four years but then jump up noticeably between the 7-year and 8- and 9-year parental measures. For men and women pooled, the estimates increase smoothly at a markedly decreasing rate over the full range of years of parental information.

We next consider the common rules approach (see Figures A5-A7). With this approach, the total bias that results from using a one-year instead of 9-year variable is much smaller than with the

common sample approach, regardless of sample (men, women, all) and income measure. The relationship between the estimated elasticities and the number of years of information is also closer to linear. The estimates, however, still increase substantially in all cases when comparing 4-5 years of parental information with 8-9.

The evidence in Figures A2-A7 suggests a plateau by year 9. Although we might conclude that the 9-year measure eliminates the bulk of attenuation bias, we cannot rule out that the curves continue growing very slowly but without reaching any plateau (or reaching it much later). There is also an alternative explanation for the tapering-off that emphasizes the deteriorating quality of the additional parental years that are incorporated. After 6 years of parental information are used to compute parental income in the SOI-M data, measures that include additional years go "in the wrong direction," as they pertain to parents who are increasingly advanced in their earnings lifecycle. As Mazumder (2005, pp. 247-248) noted, attenuation bias is best reduced by adding parental information from parents' prime-age period, not by adding information when they are in their fifties. It is possible that the results that Chetty et al. (2014) report are likewise affected by the noisiness of the additional years they are incorporating.[8]

In summary, our evidence indicates that (a) attenuation bias is greatly reduced by using 9 years of parental information, (b) it is possible that some bias still remains, and (c) a decision to use 5 years instead of 9 years would result in a non-negligible increase in bias.

I. Evidence on Lifecycle Bias

We consider here "left-side" lifecycle bias. This refers to the lifecycle bias that arises because children from different parental-income backgrounds have different age-income or age-earnings profiles. In particular, measuring children's income or earnings too early in their lifecycle leads to underestimating the corresponding elasticity. As we indicated in the main text, there is evidence suggesting that estimating IGEs with parents' and children's information taken around age 40 come closest to representing lifetime IGEs (see Mitnik 2017 for the IGE of the expectation; and Haider and

Solon 2006; Böhlmark and Lindquist 2006; Nyborn and Stuhler 2016 for the conventional IGE). Chetty et al. (2014) claim, however, that income IGEs stabilize around age 30, both in the case of the IGE_g (p. 1580 and Online Appendix Figure IIa) and the IGE_e (Online Appendix C and Online Appendix Figure Ib). We revisit this issue here with our SOI-M data.

Unlike in the attenuation-bias analysis, here we have included confidence intervals in all figures, as this helps interpret some of the results. Also to facilitate interpretation, we have added a second horizontal axis at the top of the figures, showing the mean age across our four cohorts in each year. We begin by considering lifecycle bias for earnings elasticities. In Figure A8, we present our estimates of the (constant-elasticity) IGE_e of men's earnings from 2001 to 2010. This figure, which reveals that the men's earnings IGE_e rises swiftly and nearly continuously, is in close agreement with the findings in the previous literature.

Is there evidence of an earlier stabilization when we turn to total-income elasticities? In Figures A9 and A10, we present our estimates of the total-income IGE_e for men and women respectively, again from 2001 to 2010. The elasticities for women grow at first but indeed seem to stabilize when women are in their early 30s (although there is a very small uptick at the end of the series). Although the elasticities for men exhibit a pattern that may seem more difficult to interpret, the key consideration to keep in mind is that they are likely affected by period as well as lifecycle effects. We advance the twofold hypothesis that (a) the dip in the income IGE_e in 2008 and 2009 is the result of the short-term income compression produced by the Great Recession, and (b) the subsequent uptick of the income IGE_e in 2010 reflects the growth of inequality and the restoration of more nearly normal age-income profiles. Based on a larger sample that is less affected by sampling variability, the results for all children, presented in Figure A11, exhibit a clearer pattern that is consistent with this hypothesis. The figure shows that the IGE_e increases quite smoothly between 2001 and 2007, falls in 2008 and 2009, and then returns to its pre-recession level in 2010.

Given the evidence in the previous literature and our results, it seems clear that measuring the elasticity of men's earnings when they are relatively young should generate substantial lifecycle bias. In addition, taking into account that the cohorts represented in the SOI-M Panel were 29-32 years old in 2004, our results for the pooled income IGE_e suggest that estimates of that elasticity around age 30 involves a substantial amount of lifecycle bias.

J. Mechanical Relationship between the IGE_e, Dispersion and the Rank-rank Slope

In the main text, we stated that we anticipated a low earnings elasticity for women (compared to men's), as women from relatively affluent backgrounds tend to have higher-income partners and to work fewer hours (or not at all) when they have young children (Raaum et al. 2007). We now provide a second justification for this expectation that is based on the mechanical relationship between the IGE_e, dispersion, and the rank-rank slope.

It is well known that the (constant) IGE_g is equal to the product of (a) the linear correlation between children's and parents' log incomes, and (b) the ratio between the standard deviations (SD) of the children's and parents' log incomes. Similarly, we will show here that the (constant) IGE_e (a) increases with the SD of children's income and with the correlation between children's income and log parental income, and (b) decreases with the SD of log parental income.

Mitnik (2017) has shown that the probability limit of the PPML estimator of the constant IGE_e, α_1, is approximately equal to:

$$\alpha_1 \cong \frac{1}{Cov(Y, \ln X)} - \left[\left[\frac{1}{Cov(Y, \ln X)}\right]^2 - \frac{2}{Var(\ln X)}\right]^{\frac{1}{2}},$$

where $\left[\frac{1}{Cov(Y,\ln X)}\right]^2 > \frac{2}{Var(\ln X)} > 0$.[9] We may then write:

$$\alpha_1 \cong \frac{1}{SD(Y)SD(\ln X)Corr(Y, \ln X)} - \left[\left[\frac{1}{SD(Y)SD(\ln X)Corr(Y, \ln X)}\right]^2 - \frac{2}{[SD(\ln X)]^2}\right]^{\frac{1}{2}}$$

$$\cong \frac{1}{SD(\ln X)}\left\{\frac{1}{SD(Y)Corr(Y,\ln X)} - \left[\left[\frac{1}{SD(Y)\ Corr(Y,\ln X)}\right]^2 - 2\right]^{\frac{1}{2}}\right\}$$

$$\cong \frac{1}{SD(\ln X)}\left\{A - [A^2 - 2]^{\frac{1}{2}}\right\},$$

where $A = \frac{1}{SD(Y)Corr(Y,\ln X)}$.

It is apparent that α_1 falls when $SD(\ln X)$ increases. The partial derivative of α_1 with respect to A is:

$$\frac{\partial \alpha_1}{\partial A} = 1 - \frac{A}{(A^2 - 2)^{\frac{1}{2}}} < 0.$$

As A is inversely related to both $SD(Y)$ and $Corr(Y, \ln X)$, it follows that α_1 increases with both $SD(Y)$ and $Corr(Y, \ln X)$.

The rank-rank slope between two variables in levels is closely related to the linear correlation between the logarithms of the variables (Chetty et al. 2014, p. 1561). It's reasonable to assume that the correlations between children's earnings and log parental income are ordered across genders in the same way in which the rank-rank slopes and the correlations between the logged income variables are ordered. From the fact that the rank-rank slope relating parental income to children's individual earnings is flatter for women than for men (Chetty et al. 2014, Table 1), it follows that $Corr(Y, \ln X)$ should be smaller for women than for men. In addition, the distribution of women's earnings is substantially less dispersed than that for men (e.g., Weinberg 2007, Table 5), so $SD(Y)$ can be expected to be smaller for women than for men. Therefore, as $SD(\ln X)$ should be approximately the same for men and women, we expect the women's IGE_e to be much smaller than the men's IGE.

K. Additional Robustness Checks

The purpose of this appendix is to describe some of our supplementary analyses of robustness. We first consider whether controls for parental age should be included. In our baseline

analyses, these controls were always omitted, as the age at which parents have their children is not likely to be exogenous to their income. Because high-income parents are more likely to delay childrearing, and because children with older parents have better life chances and higher lifetime income (e.g., Liu et al. 2011, Myrskylä and Fenelon 2012, Myrskylä et al. 2013, Powell et al. 2006), some of the association between parental income and children's income is a consequence of those delay-of-childrearing decisions. When other scholars control for parental age, they are eliminating that indirectly-generated portion of the total association, and their estimates may therefore be downwardly biased. For this reason, our baseline approach omits controls for parental age, even though other scholars often include them to account for age-related differentials in the measurement error for lifetime income.

These concerns with endogeneity are likely, we think, to trump any benefits that accrue to accounting for differential error in proxying lifetime income. It is nonetheless reassuring that the position one takes on this issue does not much matter. In Table A3, we present estimates from the constant elasticity and spline models (see Equations [3] and [4] respectively), where those models now include a third-degree polynomial in parental age. This table also provides p-values from the corresponding constant-elasticity tests.[10] We find that the results with the parental age controls are much the same as our baseline results and, moreover, consistent with other key conclusions reached on the basis of the estimates in Table 3 (and Figure 2). Most importantly, the constant-elasticity assumption is still rejected, and the estimates for women are still lower than the corresponding ones for men.

In a second set of supplementary analyses, we reconsider the standard approach of calculating total-income IGEs, an approach that may mislead because total income is an imperfect measure of the capacity to consume and invest. Because this capacity is more directly measured by disposable income than total income, we have used the SOI-M Panel to provide an approximate measure of disposable income (by subtracting net federal taxes from total income).[11] This measure

can then be used to answer the following simple question: Does the conventional total-income IGE misrepresent the extent to which economic advantage is transmitted across generations?

In Figure A12, we compare our estimates of total-income and disposable-income global elasticities, again applying our baseline spline and nonparametric specifications (for men and women).[12] The estimates are very similar across these two income measures: When averaged across models, the effect of using disposable-income measures is to reduce the IGE_e by about 5 percent for men and by less than 3 percent for women.[13] The total-income IGE_e, while slightly higher than its disposable-income counterpart, does not misrepresent in any fundamental way the persistence of economic status.

L. Earnings Elasticities and the Missing Poor

In this appendix, we examine the effects of the "missing poor" on the estimation of the IGE of men's individual earnings, thus supplementing our analysis of selection bias for total-income elasticities (as presented in the main text). The IGE_e estimates presented in the main text were based on the assumption that those without reported W-2 earnings have zero earnings. We were able to retain offspring with zero earnings in our analyses because the IGE_e, unlike the conventional IGE_g, is defined for variables including zero in their support. This assumption is almost surely false because some people without W-2 earnings receive earnings from work in the informal economy. Even so, our hypothesis is that it provides a good approximation, and that IGE_e estimates are robust to the exact assumption that is made as long as this assumption is consistent with the notion that the typical earnings of those with zero W-2 earnings are low and do not vary much by parental income. We test this hypothesis here. In doing so, we also assess the effects of following the practice, conventional in the literature, of dropping not only nonearners but also low earners from the analysis.

While conventional IGE_g-based analyses yield wildly different estimates under different assumptions about the earnings of nonearners (Mitnik et al. 2015, Tables 12 and 13), the nonparametric estimates of the global IGE_e, presented in Table A4, are very robust. In fact, when

low-earners are retained, the estimates hardly change under the various assumptions about the mean earnings of those without reported earnings. The estimates do fall substantially when low-earners are excluded (something that also happens when the estimand is the IGE_g), attesting to the importance of avoiding this conventional practice.

The foregoing results thus indicate that excising nonearners and low-earners produces downward biases, but that, as long as nonearners are retained, estimates of the global IGE_e are very robust to the values imputed to those with zero W-2 earnings. This implies that those estimates are likely to be close to the mark even though W-2 reported earnings exclude earnings from work in the informal economy. We can conclude that using administrative information does solve the problem of the missing poor for the estimation of the IGE of men's earnings, but only as long as that elasticity is the IGE_e .

M. Supplementary Estimates of Region-Specific IGEs

We present here supplementary estimates of all region-specific IGEs after dropping six observations (four for men and two for women). These observations had a very disproportionate influence on the spline-based estimates for the below-P10 region (as indicated in the main text). The spline-model estimates are presented in Table A5, while the nonparametric estimates are presented in Table A6.

N. Productivity of Investments in Human Capital in Denmark and the United States

We pointed out in the main text that, although it is difficult to compare the efficiency of the U.S. and Denmark in transforming money into human capital, the available literature suggests that Denmark is not likely to be, overall, any less efficient. We briefly discuss the evidence here.

The key results are in Bogetofl et al. (2014, Table 2), where it is reported that (a) the cost per primary student was similar in Denmark and the United States, (b) the cost per secondary student (lower and upper) was somewhat larger in the United States than in Denmark, (c) the cost per tertiary student was substantially larger in the United States than in Denmark, and (d) graduation rates were

very similar or somewhat higher in Denmark. In a related article, Hanushek and Woessmann (2011, Figure 2.3) show that the cumulative educational expenditure per student ages 6-15 is higher in the United States than in Denmark, but that the math performance of students, as measured by their PISA scores, is higher in the latter. Sutherland et al. (2009, Figure 1b) report a similar result for student performance in the combined reading, scientific, mathematical, and problem solving PISA scales.

Some studies have employed the data-envelopment-analysis (DEA) technique (e.g., Thanassoulis et al. 2008) to compare the relative efficiency of different countries. Bogetofl et al. (2014, Table 5) report that Denmark and the United States are very similar in terms of the efficiency of their overall educational systems when the output in consideration is the number of students enrolled, but that Denmark is more efficient than the United States at the secondary-education level. They also report that Denmark is in general more efficient than the United States when the output in consideration is the graduation rate in a cohort, although they are similarly efficient at the upper secondary education level when differences in the PISA test scores of the entering students are taken into account (Bogetofl et al. 2014, Table 7). Lastly, they report that the two countries are similarly efficient or that the U.S. is more efficient (depending on education level), when the output in consideration is the completion rate in relation to the number of students who begin an educational program (Bogetofl et al. 2014, Tables 9 and 10).

Using the same technique, Dolton and Marcenaro-Gutierrez (2016, Table 2) report that both countries are similarly efficient when the output in consideration is PISA scores. Sutherland et al. (2009, Table 2) report estimates of efficiency at the school level across countries, using a synthetic indicator of the average school-level PISA scores of 15-year-old students. Depending on the exact specification, the median school in Denmark is slightly or somewhat more efficient than the median school in the United States. Several other results they report are also consistent with the notion that the United States is not more efficient than Denmark.

Notes

[1] For example, some of Couch and Lillard's specifications (2004, Tables 8.4 and 8.5) imply a decreasing IGE across quintiles of father's earnings, whereas others imply an IGE that first increases and then decreases.

[2] When Couch and Lillard (2004) modify the usual specification by adding a quadratic term in the log of father's earnings, the average elasticity within each of the quintiles of father's earnings are 0.12, 0.21, 0.23, 0.25, and 0.29 (from the lowest to the highest). When Bratsberg et al. (2007) estimate a similar model, they report IGEs that are about three times larger: 0.49 (10^{th} percentile of parental earnings), 0.58 (50^{th} percentile of parental earnings), and 0.65 (90^{th} percentile of parental earnings).

[3] For this comparison, we use the 1988 CPS-ASEC, as it provides annual family-income information for 1987.

[4] The true "parameters" of interest include, for example, the expectation of children's income at some value of parental income or the expected IGE_e in some region of parental income.

[5] Mazumder (2005) reports results using both approaches (see his Tables 4 and 5).

[6] We implemented the second interpretation by allowing, for each parental income variable, the following maximum number of years of missing information: 3 missing years for the 9-year variable; 2 missing years for the 6-, 7-, and 8-year variables; 1 missing year for the 3-, 4-, and 5-year variables; and no missing year for the 1- and 2-year variables.

[7] The main reason for producing estimates with pooled data is that the larger sample size makes the trend clearer.

[8] It should be noted that Chetty et al. (2014) consider this possibility but reject it on the argument that their Figure IIb (in their Online Appendix) shows that "estimates of mobility are not sensitive to varying the age in which parent income is measured over the range observed in our dataset" (2014a, Online Appendix E). The evidence in the figure in question, however, pertains to the rank-rank slope. The rank of parents may remain the same as they get older even as the differences between their incomes do not.

[9] The expression assumes, without any loss of generality, that $E(Y) = E(\ln X) = 1$. There is no loss of generality because this can always be achieved by simply changing the monetary units used to measure income (i.e., by dividing the children's income variable by its mean, and the parental income variable by the exponential of the mean of its logarithmic values minus one).

[10] We also estimated constant-elasticity and spline models including (a) cohort dummies (thus simultaneously controlling for cohort and children's age), and (b) cohort dummies and a third degree polynomial in parental age. We have not reported results from these specifications because they are very similar to those that are reported.

[11] It should be recalled that, because we do not subtract state taxes and do not include some non-taxable transfers (e.g., TANF), our measure only provides an approximation to true disposable income. This measure does, however, incorporate the Earned Income Tax Credit (EITC) and other refundable credits.

[12] When estimating nonparametric models with disposable income, we do not select the smoothing parameter by finding the global minimizer of AIC_c (Hurvich, Simonoff, and Tsai 1998) within the range [.08, 1], as we do with all other nonparametric models. Instead, we reuse the same smoothing parameters selected for the corresponding total income models. Proceeding this way ensures that any difference is due to the change in the income measure (rather than the change in the smoothing parameter).

[13] We tested the null hypothesis that the global IGE_e for disposable income is not smaller than the global IGE_e for total-income. For the nonparametric model, this hypothesis is rejected for men ($p = 0.000$), but it cannot be rejected for women ($p = 0.259$). For the spline model, it is rejected for both men ($p = 0.000$) and women ($p = 0.005$).

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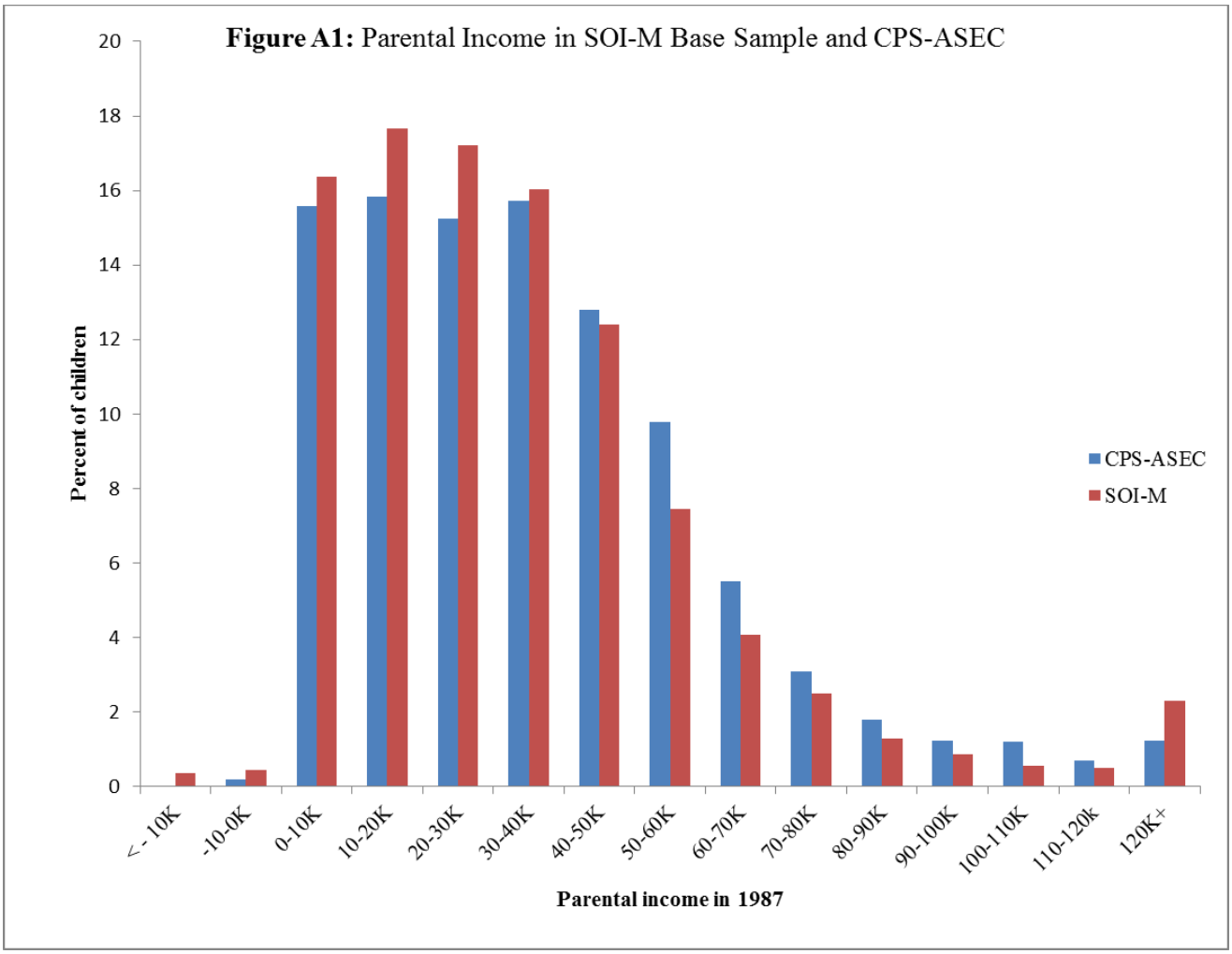


Figure A1: Parental Income in SOI-M Base Sample and CPS-ASEC
Percent of children
20
18
16
14
12
10
8
6
4
2
0
CPS-ASEC
SOI-M
< - 10K
-10-0K
0-10K
10-20K
20-30K
30-40K
40-50K
50-60K
60-70K
70-80K
80-90K
90-100K
100-110K
110-120k
120K+
Parental income in 1987

Figure A2: IGE_e as a function of years of parental information, men (common-sample approach)

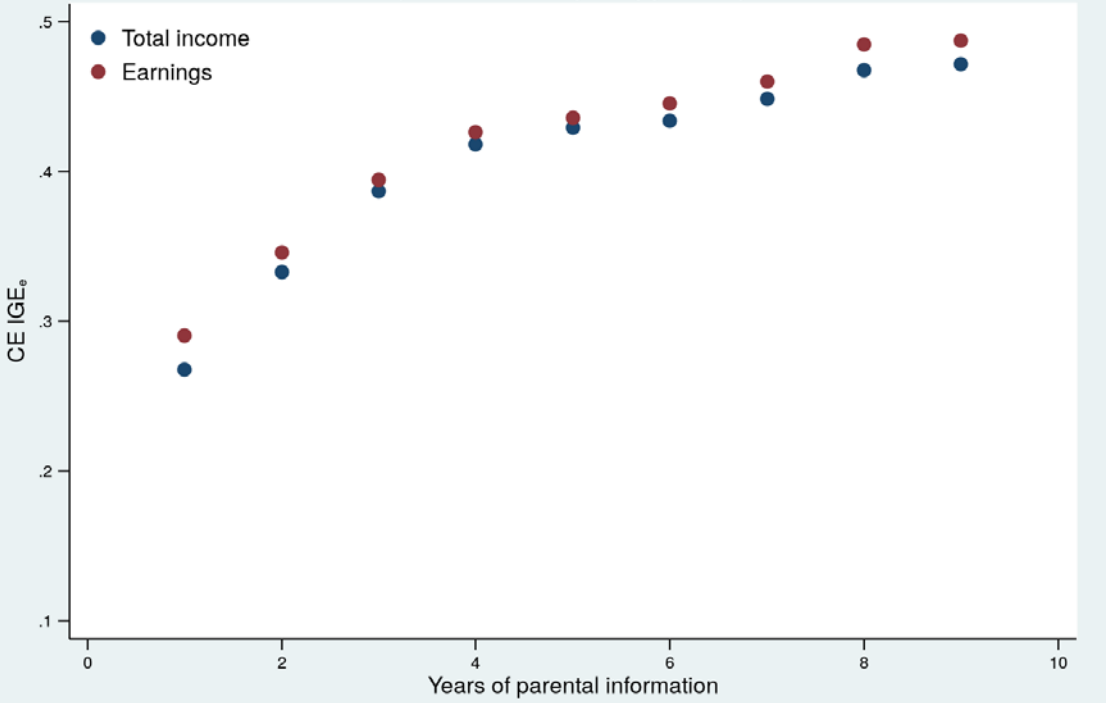


Figure A3: Total-income IGE_e as a function of years of parental information, women (common-sample approach)

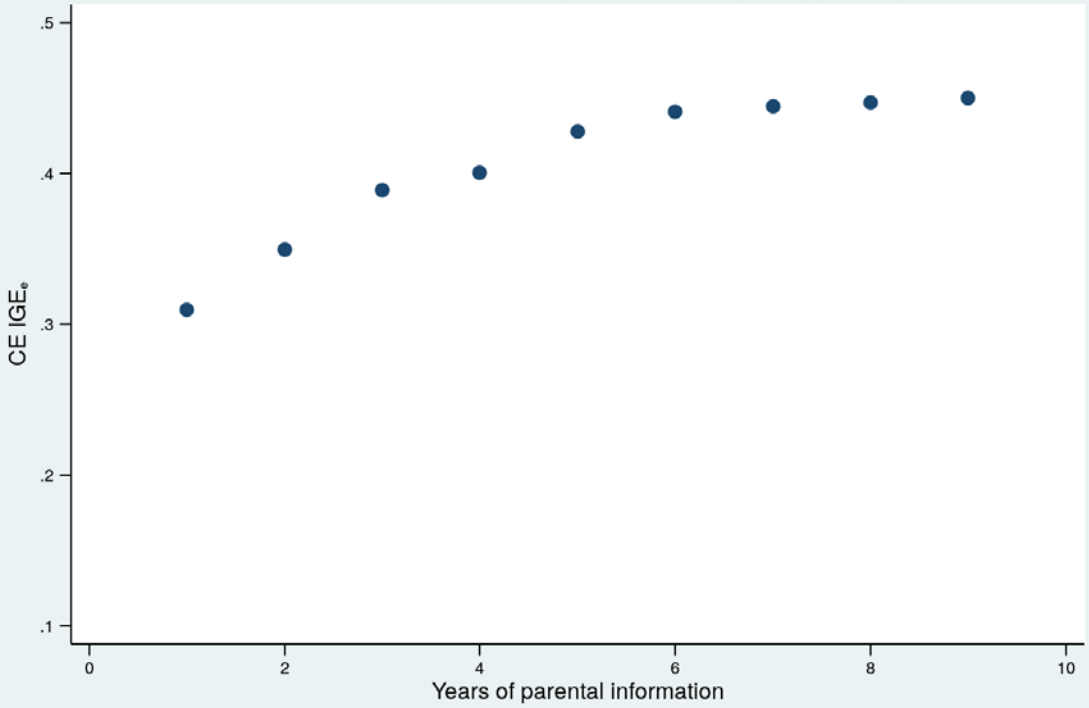
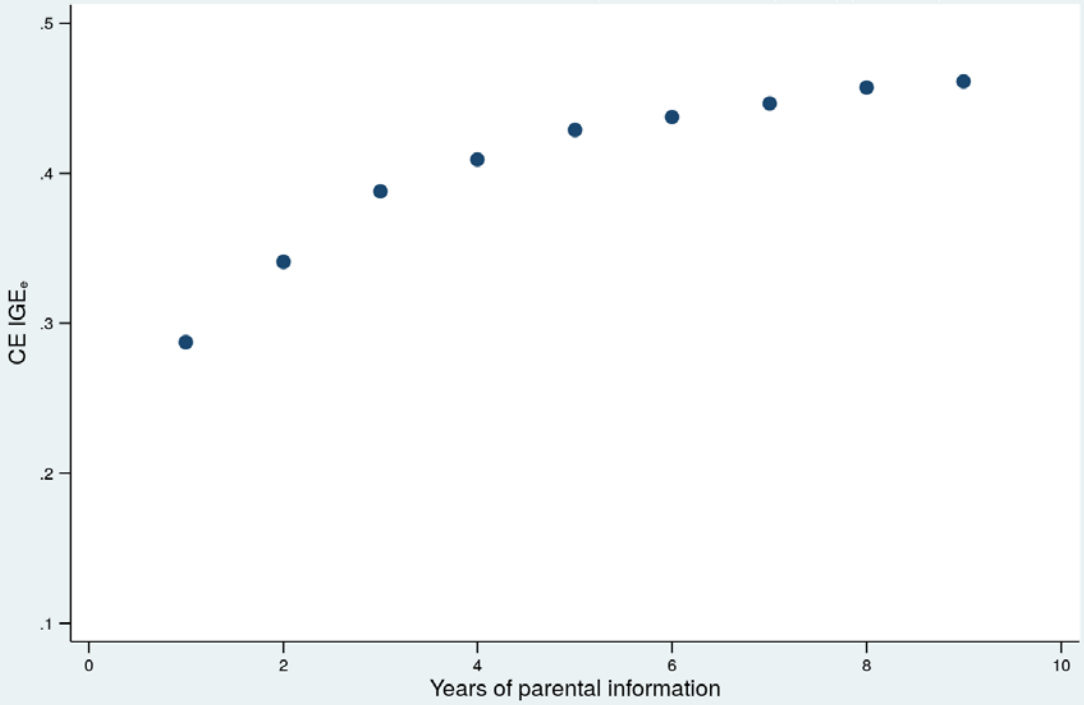


Figure A4: Total-income IGE_e as a function of years of parental information, all children (common-sample approach)



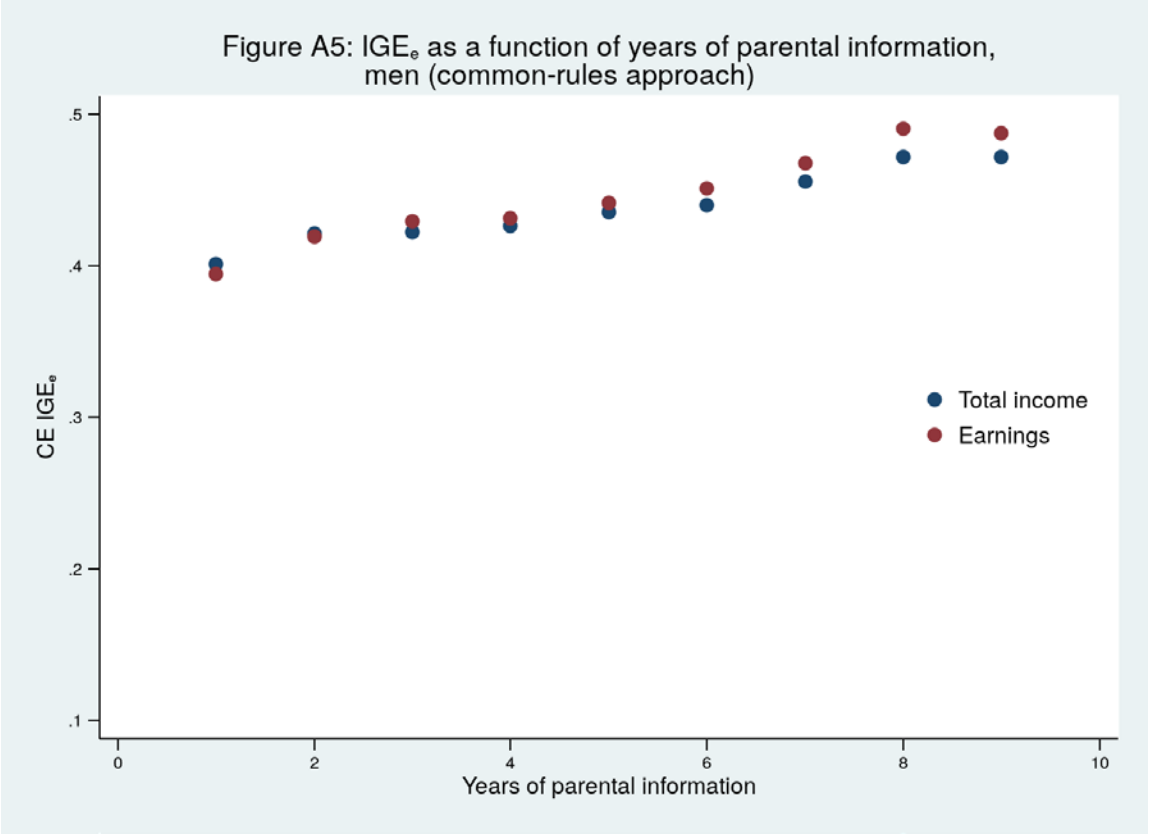


Figure A5: IGE$_e$ as a function of years of parental information, men (common-rules approach)
CE IGE$_e$
.1
.2
.3
.4
.5
0
2
4
6
8
10
Years of parental information
Total income
Earnings

Figure A6: Total-income IGE_e as a function of years of parental information, women (common-rules approach)

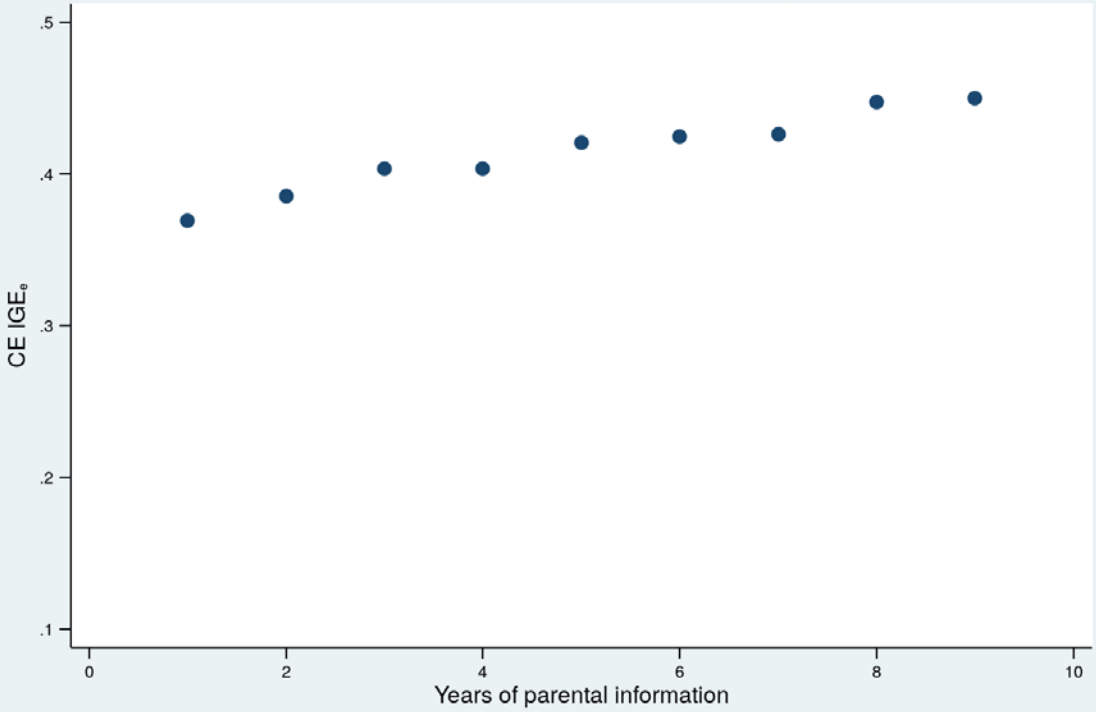
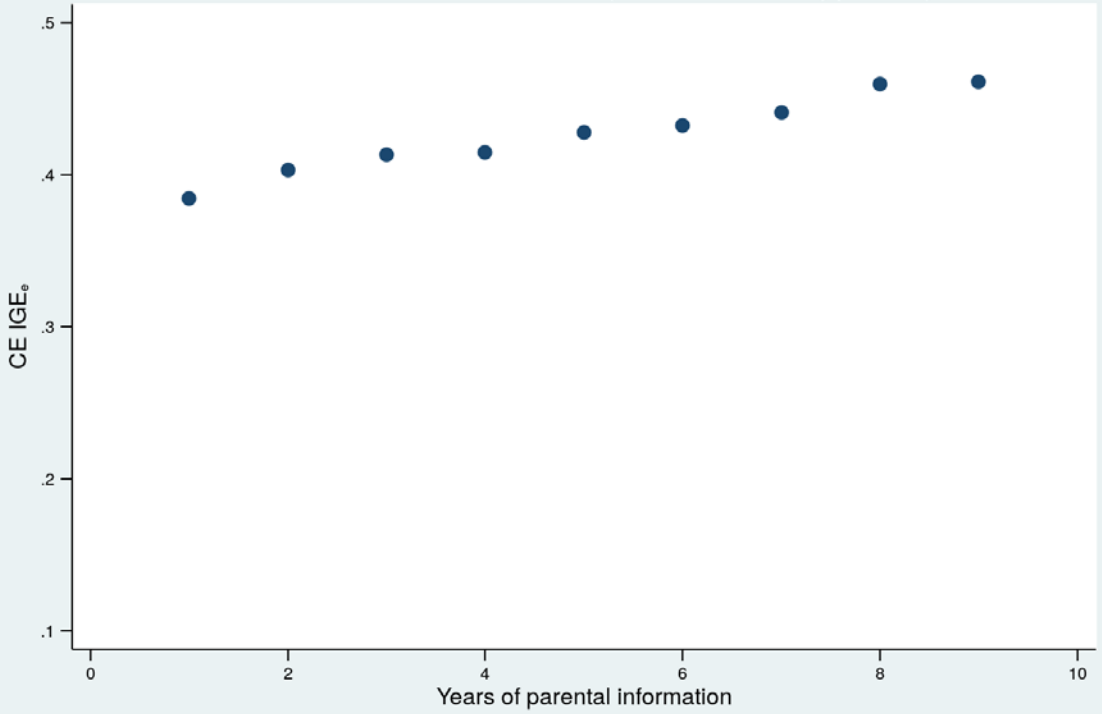


Figure A7: Total-income IGE_e as a function of years of parental information, all children (common-rules approach)



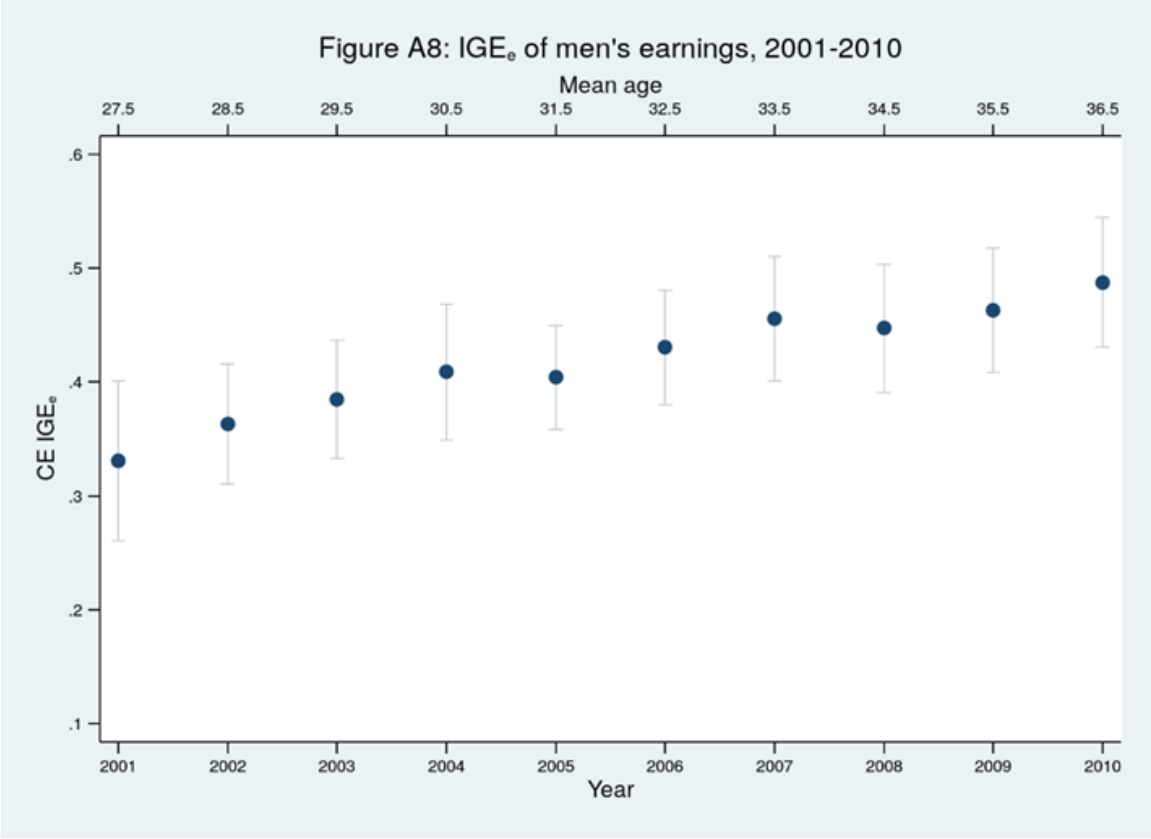


Figure A8: IGE$_e$ of men's earnings, 2001-2010
Mean age
27.5
28.5
29.5
30.5
31.5
32.5
33.5
34.5
35.5
36.5
.6
.5
.4
.3
.2
.1
CE IGE$_e$
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
Year

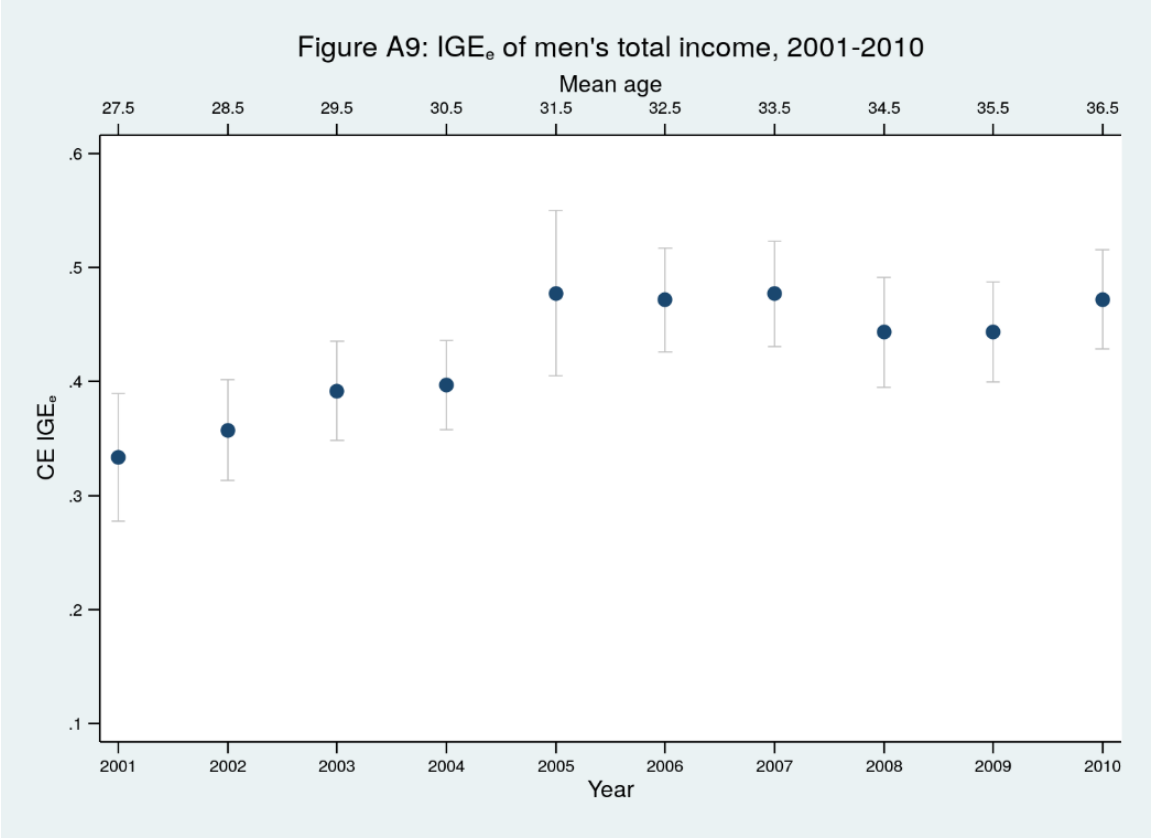


Figure A9: IGE$_e$ of men's total income, 2001-2010
Mean age
27.5
28.5
29.5
30.5
31.5
32.5
33.5
34.5
35.5
36.5
.6
.5
.4
.3
.2
.1
CE IGE$_e$
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
Year

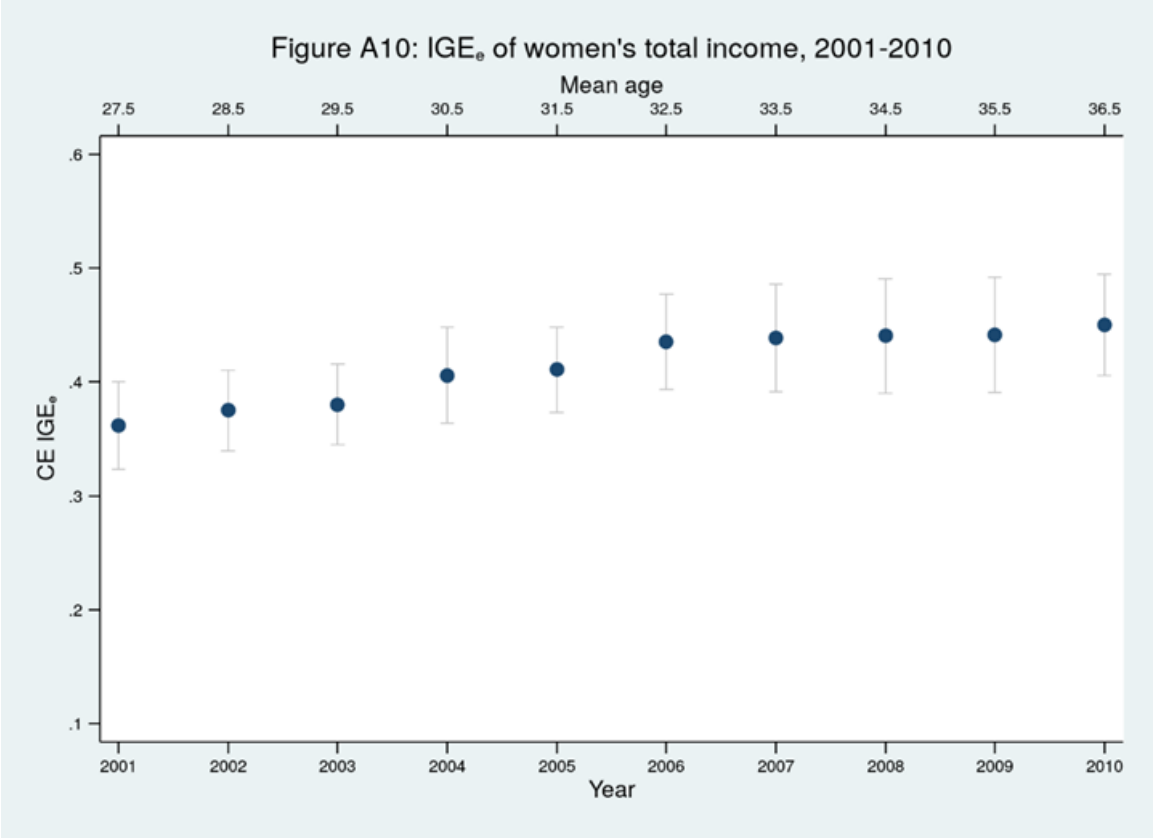


Figure A10: IGE$_e$ of women's total income, 2001-2010
Mean age
27.5
28.5
29.5
30.5
31.5
32.5
33.5
34.5
35.5
36.5
.6
.5
.4
.3
.2
.1
CE IGE$_e$
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
Year

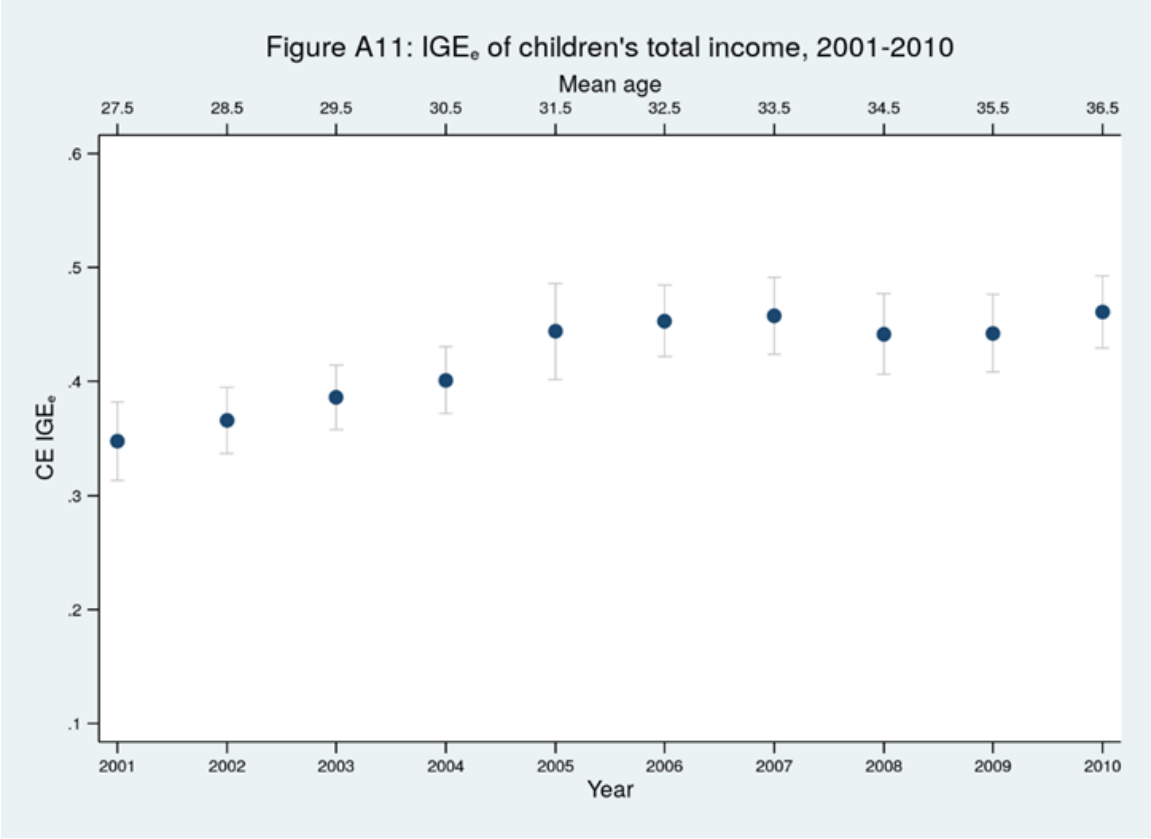


Figure A11: IGE$_e$ of children's total income, 2001-2010
Mean age
27.5
28.5
29.5
30.5
31.5
32.5
33.5
34.5
35.5
36.5
.6
.5
.4
.3
.2
.1
CE IGE$_e$
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
Year

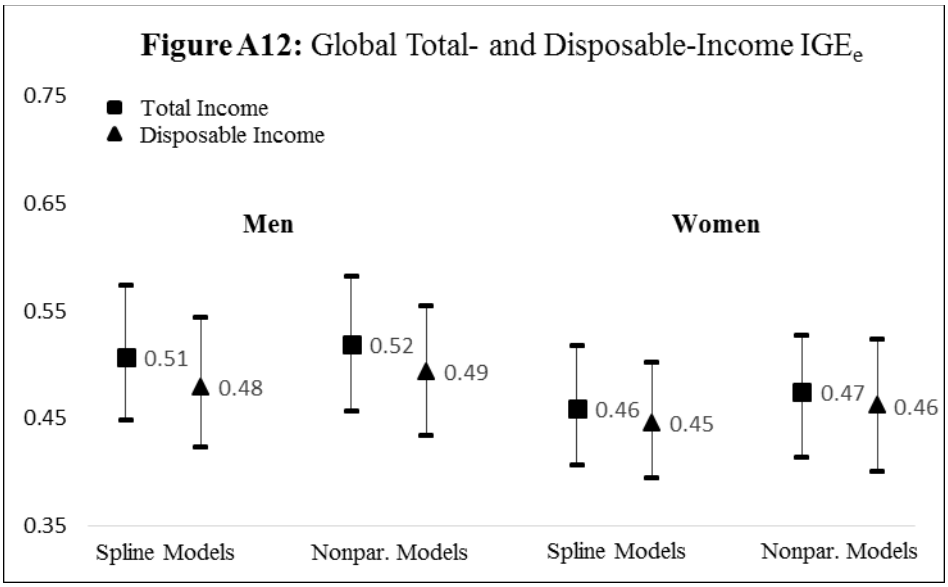


Figure A12: Global Total- and Disposable-Income IGE$_e$
0.75
0.65
0.55
0.45
0.35
Total Income
Disposable Income
Men
Women
0.51
0.48
0.52
0.49
0.46
0.45
0.47
0.46
Spline Models
Nonpar. Models
Spline Models
Nonpar. Models

Table A1: Predicted IGE_g from meta-analyses

	Included IGE Estimates	
	All	Survey-based
Years of parental information		
5	0.40	0.38
10	0.46	0.39
15	0.52	0.39

Table A2: SOI-M Base Sample and 1987 CPS-ASEC, Weighted Frequencies

Child's Age in 1987	SOI-M		1987 CPS	
	Frequency	Weighted Frequency	Weighted Frequency	Difference SOI-M – CPS
Males				
12	1,768	1,806,303	1,662,106	144,197
13	1,784	1,745,662	1,656,149	89,513
14	1,875	1,861,325	1,721,148	140,177
15	1,926	1,703,241	1,844,131	-140,890
Total	7,353	7,116,532	6,883,534	232,998
Females				
12	1,629	1,656,620	1,582,240	74,380
13	1,692	1,586,241	1,581,609	4,632
14	1,729	1,671,537	1,643,910	27,627
15	1,949	1,811,879	1,751,337	60,542
Total	6,999	6,726,277	6,559,096	167,181

Table A3: Global IGE_e Estimates and Tests, Models with Parental-Age Controls

	Constant-Elasticity Models	Spline Models	P-value from CE test
Men's earnings	0.46	0.51	0.000
	(0.40-0.52)	(0.45-0.58)	
Men's total income	0.45	0.48	0.013
	(0.40-0.49)	(0.42-0.55)	
Women's total income	0.41	0.42	0.041
	(0.37-0.46)	(0.37-0.48)	

Note: The constant elasticity (CE) test is an F-test of the null hypothesis that all coefficients of the spline model of Equation [4], save α_0, α_1 and the age coefficients, are zero.

Table A4: Sensitivity of Nonparametric Estimates of the Global IGE_e of Men's Earnings to the Treatment of Low and Nonearners

Included Men	Imputation for Nonearners	IGE_e
Earnings above $ 3,000		0.45
Earnings above $ 1,500		0.47
Earnings above $ 600		0.47
Earnings above $ 100		0.47
Earnings above $ 0		0.47
All	$0	0.56
All	$1	0.56
All	$100	0.56
All	$1,000	0.56
All	$3,000	0.55

Table A5: Region-Specific Estimates of the IGE_e, Spline Models (Outliers Dropped)

	Up to P10	P10-P50	P50-P90	Above P90	P-value from Bootstrap Test of H0: P10-P50 ≥ P50-P90
Men's earnings	0.19	0.48	0.76	0.34	0.022
	(-0.10-0.64)	(0.34-0.63)	(0.59-0.93)	(0.25-0.46)	
Men's total income	0.34	0.42	0.70	0.37	0.019
	(0.07-0.68)	(0.29-0.55)	(0.51-0.91)	(0.26-0.47)	
Women's total income	0.52	0.31	0.64	0.42	0.003
	(0.27-0.80)	(0.18-0.44)	(0.49-0.80)	(0.33-0.52)	

Table A6: Region-Specific Estimates of the IGE_e, Nonparametric Models (Outliers Dropped)

	Up to P10	P10-P50	P50-P90	Above P90	P-value from Bootstrap Test of H0: P10-P50 ≥ P50-P90
Men's earnings	0.31	0.56	0.62	0.68	0.370
	(0.03-0.63)	(0.39-0.73)	(0.41-0.82)	(0.22-1.05)	
Men's total income	0.38	0.44	0.68	0.41	0.033
	(0.23-0.55)	(0.33-0.55)	(0.48-0.90)	(-0.07-0.81)	
Women's total income	0.53	0.36	0.63	0.25	0.058
	(0.31-0.75)	(0.20-0.52)	(0.40-0.87)	(-0.27-0.68)	