New Estimates of Intergenerational Mobility Using Administrative Data

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July 8, 2015

Abstract

This report presents analyses of a new data set, the Statistics of Income Mobility Panel, that has been assembled to provide new evidence on economic mobility and the implications of tax policy for economic mobility. Because the data are of unusually high quality, they provide the foundation for a comprehensive report on intergenerational income and earnings mobility in the United States. We describe here (a) the rationale for measuring economic mobility and for using intergenerational income and earnings elasticities to do so; (b) the current state of the evidence on the size of intergenerational elasticities; (c) the rationale for assembling the Statistics of Income Mobility (SOI-M) Panel and the key features of this new panel; (d) the main reasons why it's necessary to abandon some of the field's long-standing methodological conventions in favor of a new approach for specifying and estimating intergenerational elasticities; (e) the new estimates of average intergenerational elasticities for individual earnings and for family income; (f) the extent to which intergenerational persistence varies across regions of the parental income distribution; (g) the extent of intergenerational persistence among families that are very far apart in the income distribution; (h) the robustness of our estimates to the treatment of nonfilers; (i) the extent to which estimates of after-tax elasticities are consistent with estimates of pre-tax elasticities; (j) the effects of low-income tax credits on economic persistence; and (k) the role of gender and marriage in generating total-income elasticities.

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Introduction

The amount and patterns of social mobility in the U.S. are affected by a complicated constellation of institutions and policies, but tax policy is often taken to be an especially important lever (see Steuerle 2012; Steuerle et al. 2008; Chetty et al. 2014a). In standard economic models of intergenerational mobility, tax credits to low-income households are typically understood to relax credit constraints, thus allowing parents to make investments in their children's human capital that will then increase their opportunities for mobility (e.g., Ichino et al. 2011; Mayer and Lopoo 2008; Becker and Tomes 1979). As with much tax policy, low-income tax credits have been supported for a host of reasons, including of course an interest in simply helping disadvantaged families make ends meet. However, low-income tax credits are also supported because they facilitate mobility and promote equal opportunity, indeed this is one reason why they are often mandated to increase in size for households with dependent children. The same interest in facilitating mobility often rests behind tax policy relevant to high-income or high-wealth households. The supporters of the Estate Tax, for example, often value it not just because it raises revenues but also because it is presumed to limit the transmission of economic advantage between generations (see Jacobson, Raub, and Johnson 2007).

This interest in tax policy as a "mobility lever" operates against a backdrop of assumptions about the amount of economic mobility in the U.S. The validity of these assumptions has proven difficult to establish. Although one might imagine that the U.S. has a well-developed infrastructure for monitoring mobility, in fact there are ongoing concerns that the existing survey evidence is not based on high-quality income reports and relies on samples that are too small and do not cover the full income distribution. These concerns have made it hard to evaluate the increasingly popular hypothesis that the United States does not have as much economic mobility as had long been assumed (see Mitnik, Cumberworth, and Grusky 2015; Long and Ferrie 2013). Because of such concerns, administrative data have been increasingly used to examine economic mobility, with Mazumder (2005) and Chetty et al. (2014a; 2014b) reporting especially influential results (also see Chetty and Hendren 2015). This new research stream is immensely important but, as we discuss in detail below, it has not yet provided clear evidence on such key questions as the size of U.S. intergenerational elasticities (IGEs), the extent to which they vary across different regions of the parental income distribution, and the vulnerability of administrative-data estimates of the IGE to different types of biases. The purpose of our report is to use tax and other administrative data to provide new high-quality estimates of IGEs and other measures of economic mobility in the present-day United States and, in the course of doing so, speak to some of the more important unanswered questions about such mobility.

The intergenerational elasticity, which we estimate throughout our analyses, has long been the workhorse measure of economic mobility. Although the IGE is, strictly speaking, a measure of the persistence of economic differences across generations (e.g., Jäntti et al. 2006, p.

8), it has been commonly interpreted as a measure of economic mobility (in which a high IGE signifies low mobility). Because the IGE quantifies the extent to which adult income or earnings are related to economic circumstances when growing up, it is often understood as also indexing the degree of departure from equal opportunity.[1] Under the simplifying (and very common) assumption that the IGE is constant across levels of parental income, it further provides a single summary measure of the degree of mobility in a country. For these and other reasons, the IGE has long had a prominent role in basic research, public-policy discussions, and international comparisons (e.g., Björklund and Jäntti 2011; Blanden 2009; Corak 2006; Solon 2002). The analyses presented here will accordingly focus, in large part, on IGEs.

We report seven key results on economic mobility in the U.S.[2] Using high-quality administrative data and samples substantially larger than those of survey-based studies, we document the following:

- The elasticities for men's earnings and for men's and women's total income are at the higher end of the wide range of estimates reported in the literature and imply a very high level of economic persistence.
- The constant-elasticity assumption, which has typically been invoked in the literature, conceals the especially high persistence of economic differences within the "middle to upper middle class" zone (i.e., parental income between the 50th and 90th percentiles).
- Because of moderate-to-strong persistence within the lower zones of the parental distribution and heightened persistence within the "middle to upper middle class" zone, a large share of income differences between low-income (10th percentile) and high-income families (90th percentile) persist into the next generation.
- The elasticities for after-tax income are slightly smaller than those for total income and take on a pattern suggesting that the recent growth of low-income tax credits has reduced economic persistence among very low-income families.
- For women and men alike, total-income elasticities are driven higher because children from higher-income families are (a) more likely to be married, and (b) more likely to have higher-earnings partners when they are married.
- The earnings elasticity for women is substantially lower than that for men, in large part because married women tend to withdraw from employment as their spouses' earnings increase.

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[1] See, for example, Mulligan (1997, p. 25). This interpretation ignores well-known caveats to the effect that some economic persistence may result from differences in talent and preferences that, although correlated with origins, might not be understood as producing inequality of opportunity (see, for example, Swift 2005; Roemer, 2012). It has also been pointed out that children of wealthy parents may opt for lower-paying jobs or opt not to work at all (Chetty et al. 2014a, p. 1559). By virtue of such ambiguities, many scholars seem to assume that levels of economic persistence are best understood as first approximations of how unequal opportunities are.

[2] For the sake of simplifying the exposition, the term "elasticity" is used in this summary to refer to expected elasticities across levels of parental income. We occasionally use this simplifying language in other sections of the report as well.

- The total-income elasticity is nearly as large for women as men, but the processes through which that elasticity is generated differ by gender. The direct pathway (via one's own earnings) accounts for much of the total-income elasticity for men, whereas the indirect pathway (via marriage and spouse's earnings conditional on marriage) accounts for much of the total-income elasticity for women.

Although a key rationale for this report is to exploit a new high-quality data set, the report also discusses various methodological problems and introduces new methodological approaches that should be of general interest for mobility scholars, policy analysts and makers, and other users of intergenerational elasticities. The following three methodological issues are particularly important:

Wrong estimand: We show that a very problematic practice within the field of mobility studies is the widespread use of the "OLS log-log estimator." The simple problem: This estimator provides estimates for the wrong estimand. For purposes of comparison, we will present some estimates of the traditional estimand, but most of our results pertain to the estimand that is consistent with the way in which scholars in the field have interpreted their results. We will provide an extensive discussion of this problem and our approach to addressing it.

Selection bias and fragility of results: As an unfortunate by-product of the use of the traditional estimator, it has been conventional to drop cases without earnings or income, with the resulting elasticities thus pertaining to intergenerational persistence among those who are "doing well" (see Couch and Lillard 1998). We argue that substantial selection biases are likely generated by this practice. We further show that, in analyses with the correct estimand, it is no longer necessary to drop those cases, thus making it possible to estimate elasticities pertaining to the full population. The resulting estimates, which are very robust, resolve recent concerns about the sensitivity of IGE estimates to the treatment of nonfilers (Chetty et al. 2014a).

Nonparametric and spline models: It has long been suggested that intergenerational persistence may be more pronounced in some zones of the parental-income distribution than others. However, because the available survey samples are rather small, it has been difficult to assess hypotheses of this sort. We estimate nonparametric and spline models and use the results to compute average elasticities in different regions of the parental-income distribution.

The core of our report rests on presenting a set of key IGEs characterizing intergenerational mobility in the U.S. Because the IGE plays such a central role in this report, it is useful to begin by rehearsing some reasons why it has become a prominent measure in the field, indeed arguably one of a small handful of fundamental measures. We also review the literature on U.S. intergenerational elasticities and offer some additional reasons why it is especially important to estimate them with administrative data. After addressing these issues, we

turn to a discussion of the structure and data sources of the new Statistics of Income Mobility (SOI-M) Panel, the models and estimators we employ, and the main results from our analyses.

Measuring Mobility

There is a long history of studying intergenerational mobility that encompasses a wide variety of approaches to measuring it (see Grusky and Cumberworth 2010; Jäntti and Jenkins 2013). The most important approaches include (a) using mobility tables or transition matrices to measure the proportion of the population moving among social or income classes; (b) estimating log-linear and log-multiplicative models of the chances of ending up in different social or income classes conditional on different origins; (c) calculating simple linear correlations between the income (or log-income) of parents and children; (d) estimating the percentile-point increase in children's position given a one percentile-point increase in their parents' position (i.e., "rank-rank slope"); (e) estimating copula-based measures of association between the income of parents and children; and (f) estimating the percent change in children's income given a one percent increase in parental income (i.e., "intergenerational elasticity").

There is no need to review here the history of these measurement approaches or to carry out any comprehensive assessment of the various advantages or disadvantages of any given one (see Fox, Torche, and Waldfogel forthcoming; Jäntti and Jenkins 2013). Rather, our objective is simply to establish that the intergenerational elasticity is *one* useful measure of mobility, albeit one that in our view is important enough to be taken into account in any effort to assess the state of mobility in the U.S. The following are some well-known conceptual and pragmatic virtues of this measure:

- *Comparability:* Because IGEs are unit-free, they can be compared across time and across countries or other geographic units.
- *Robustness to classical measurement error*: Unlike other measures (e.g., linear correlation), IGEs are not affected by classical measurement error in children's income or earnings.
- *Convenient functional form*: The relationship between the income of children and parents is not well approximated by a simple straight line, whereas the relationship between proportional changes in the income of children and parents is closer to linear in form. It follows that elasticities facilitate estimation and interpretation.
- *Concreteness of interpretation:* Unlike many other measures (e.g., correlation, rank-rank slope), the elasticity speaks very concretely to a child's percent increase in expected income accruing to a percent increase in the income of his or her parents. This type of concreteness implies that the elasticity is sensitive to changes in cross-sectional income distributions. If, for example, the children's income distribution becomes more unequal, then the elasticity will become larger (all else equal). Although this sensitivity must be borne in mind when considering the sources of differences or changes in elasticities, it is

nonetheless an asset insofar as one seeks, as we do here, a simple descriptive benchmark of the extent to which economic background conveys economic advantage.

The preceding virtues of the IGE are well-known, long-standing, and apply equally to the analysis of survey and administrative data. We will nonetheless argue below that, given the important limitations of the survey data employed to study mobility in the past, our uncertainty about the current size of IGEs is much higher than it should be. This makes it especially important to estimate IGEs with administrative data. Indeed, given the workhorse status of IGEs in the literature and in policy discussions, it seems indispensable to estimate them. The task of doing so is fortunately eased by the wealth of methodological knowledge produced by the substantial literature on IGEs over the last 40 years. As with all analyses, a large number of methodological decisions have to be made in estimating IGEs from administrative data, decisions that can be partly guided by this knowledge.

We obviously do not mean to gainsay the value of estimating other measures of mobility with administrative data. The recent research of Chetty et al. (2014a; 2014b), which is mainly based on the rank-rank slope, is a landmark contribution to the field and has advanced our understanding of economic mobility in many important ways. Likewise, a recent study by Auten, Gee, and Turner (2013) used tax data to examine intergenerational transition tables, an effort that again yielded important results. In listing some of the merits of IGEs, we seek simply to reaffirm that they are an important complement to existing and ongoing research that focuses on other measures.

The Current State of the Evidence

Given how important IGEs have been in previous research, and given their role in informing policy discussions, it is unfortunate that the available evidence does not allow us to establish the size of key IGEs with confidence. The reasons for this state of affairs are best appreciated by examining how our understanding of economic mobility has 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. This conclusion will emerge very clearly in the following brief review of the history of research on IGEs in the U.S.

 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 percent differences in origin incomes are passed on to sons (e.g., Sewell and Hauser 1975; Behrman and Taubman 1985; Becker and Tomes 1986:Table 1). This early

research thus led to the consensus view that the U.S. is a quite mobile society. As Gary Becker put it in his 1988 presidential address to the American Economics Association, "low earnings as well as high earnings are not strongly transmitted from fathers to sons" (1988, p.10).

The consensus view shifted, however, when Solon (1989) showed that previous estimates had been downwardly biased by the use of homogeneous samples or by the measurement error generated by transitory fluctuation in measured earnings or income (i.e., "attenuation bias"). 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 measures.[3] 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 men's earnings IGE generated estimates that were not always consistent with an IGE of about 0.4. In much of this subsequent research, different samples were drawn from the same data sources, and alternative estimators and specifications were 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-1990s estimates of the men's earnings IGE range from 0.13 to 0.54, a dispiritingly large range.[4] 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) lifecycle biases that result from measuring income or earnings when children or parents are either too young or too old (see, e.g., Mazumder 2005, pp. 236-240), and (c) instrumental-variable (IV) bias likely to result when an instrument (e.g., father's education) is employed to estimate the IGE (e.g., Solon 1992, Appendix). After taking into account that estimates based on young adults are likely to be downward biased, while estimates using IV estimators are likely to be upward biased, 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" (1999:1784).[5] Based on this

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[3] 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.

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

[5] We have carried out a formal meta-analysis of the *survey-based studies* in Corak (2006) that suggests a preferred IGE estimate of close to 0.4. This result is in agreement with Solon's (1999) qualitative analysis. In his own meta-analysis, Corak (2006) included one study based on administrative data (Mazumder 2001), a study that we excluded to secure an estimate based on survey results alone (see Mazumder 2005 for a revised version). The right-hand variables in Corak's 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 the estimator used is OLS or IV. Because Corak included only those studies in which all three of these right-hand variables were available, his meta-analysis is based on only 22 of the 41 estimates he listed in his review. In our own follow-up analysis, we repeated Corak's meta-analysis after dropping the administrative-data estimates from Mazumder (2001), with the result being predicted

assessment of the men's earnings IGE, and based on a similar (but much sparser) set of results for men's and women's income IGE (e.g., Solon 1992; Mulligan 1997; Chadwick and Solon 2002; Peters 1992), a new consensus view that the U.S. is quite immobile developed and consolidated.[6]

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).[7] The estimates remain inconsistent when an effort is made to define the PSID and NLS samples very similarly and also when the PSID and NLS samples cover birth cohorts that are relatively close to one another.[8] The PSID and NLS surveys are further affected by an unusually long list of additional problems or limitations: (a) the PSID only collects full income information for household heads and their spouses and is not fully representative of the country's population (mainly because post-1968 immigrants and their descendants are not represented);[9] (b) both the PSID and the NLS are affected by substantial attrition;[10] (c) both surveys can only provide small samples (with some estimates based on a few hundred observations); (d) neither survey covers

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IGEs of 0.381, 0.387, and 0.394 under the assumptions that (a) father's earnings was measured around age 45, and (b) 5, 10, and 15 years of parental information were used (with the three estimates listed here corresponding to these five-year increments in parental information). These assumptions are similar to those of Corak (2006).

[6] This conclusion is also supported if one takes into account further studies that were not included in Solon's or Corak's review. We are referring here to more recent PSID-based studies of men's earnings IGEs (e.g., Gouskova et al. 2010) as well as relatively recent analyses of trends in income IGEs (Mayer and Loopo 2005; Hertz 2007; Lee and Solon 2009). It bears noting that two recent NLS-based studies of men's earnings (Jäntti et al. 2006; Bratsberg et al. 2007) also offer IGE estimates at or close to the upper bound of estimates covered in the reviews by Solon and Corak. We comment on these two studies below.

[7] Contrary to this generalization, Solon (1992) and Zimmerman (1992) report similar IGE estimates. As argued by Grawe (2004a), similar estimates may have been obtained in this case because Zimmerman restricted his NLS sample to full-time and (mostly) year-round workers.

[8] Employing very similar sample-inclusion rules, Grawe (2004a) reports men's earnings IGEs of 0.47 (PSID) and 0.15 (NLS). In part, this discrepancy is attributable to differences in parental age at the time of measuring parental earnings, yet Grawe nonetheless concludes that "life-cycle differences cannot explain the whole gap between the two samples; the PSID sample exhibits substantially less earnings mobility than the NLS sample" (2004, p. 72). See Mayer and Lopoo (2004, p. 95) for a discussion of differences in PSID estimates across cohorts. As regards the NLS, it is instructive to contrast Grawe's (2004a) estimate for men's earnings of 0.15 with Jantti's (2006) corresponding estimate of 0.52. Although these pertain to different cohorts, and cross-cohort differences are not necessarily surprising, the magnitude of this difference is seemingly too large to be believable (especially given that the higher estimate, 0.52, is based on only one year of parental information, an issue we further discuss below).

[9] The failure to represent post-1968 immigrants and their descendants pertains to the PSID samples that have been available to study intergenerational mobility.

[10] This attrition probably generates a weaker version of the sample-homogeneity bias affecting earlier data sets (Solon 2008, p. 4). In studies employing both the PSID and the NLS, attrition is addressed by adjusting the weights of the respondents that remain in the sample, an approach that rests on the strong assumption that attrition is independent of children's earnings or income (after controlling for the variables on which the weights are based).

the upper tail of the income and earnings distributions well; (e) neither survey provides enough years of parental information to address attenuation bias satisfactorily;[11] and (f) 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 transitory fluctuations and 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 that consensus IGE value. In his 2006 review of the literature, which was strongly influenced by Mazumder's work, Corak selected 0.47 as his "preferred estimate" of the IGE of men's earnings.[12] 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. First, in a paper that has not been much cited, Dahl and DeLeire (2008) also employ SSA earnings data to estimate elasticities with nearly career-long earnings histories, with the important result that the estimates proved to be very sensitive to the choice of sample and the definition of father's lifetime earnings. These Dahl-DeLeire estimates range from 0.26 to 0.63 for sons and from 0 to 0.27 for daughters.[13] Although the estimates for sons might well be

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

[12] In his review, Corak carried out not just a quantitative meta-analysis of the literature (as mentioned in footnote 5), but also a qualitative analysis of that literature. In the qualitative analysis, Corak selected Grawe's (2004a) 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. In addition, Grawe's estimate was consistent with the predictions from his meta-analysis, which yielded IGEs of 0.40, 0.46, and 0.52 under the assumptions specified in footnote 5. These values are substantially higher that those we obtained after excluding Mazumder's estimates from the meta-analysis.

[13] The volatility of their IGE estimates motivated Dahl and DeLeire to turn to (much more robust) rank-rank slope estimates.

consistent with those from Mazumder (2005), the estimates for daughters 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.[14] The Dahl-DeLeire results also call into question Mazumder's finding that the earnings IGEs for men and women are similar. As we discuss below, there are indeed good reasons to expect the earnings IGE for women to be smaller than that for men, just as Dahl and DeLeire's (2008) results suggest. In this context, it is perhaps surprising that Mazumder found similar earnings IGEs for men and women, although this result is not unprecedented in the survey-based literature.

The second post-Mazumder development of interest is the release of the influential Chetty et al. (2014a) study. This study has cast doubt on the previously accepted conclusion that transitory fluctuations 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 *income* mobility, 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, Section E), Mazumder's (2005) decision to impute parental income with measures of race and education was tantamount to resorting to IV estimation, which, as we already noted, is known to yield unduly high IGEs. Because Mazumder (2005) was obliged to rely disproportionately on imputed data when including additional years of parental income, Chetty et al. (2014a) argue that the *appearance* of substantial attenuation bias was accordingly created (see Chetty et al. 2014a, Online Appendix, Section E).[15]

The evidence on U.S. mobility became even less clear when Chetty et al. (2014a) further reported that, as in Dahl and DeLeire (2008), IGE estimates can be very volatile. For Chetty et

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[14] 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 example, Dahl and DeLeire (2008) report son's IGEs of 0.30 and 0.51 respectively when parental earnings are measured as (a) average earnings from age 20 to 55 (including years of zero earnings), and (b) average *positive* earnings from age 20 to 55. The highest estimate, 0.63, is obtained when (a) the sample only includes fathers with earnings at every age from 25 to 55, and (b) average earnings are measured over ages 20-55 (including years with zero earnings). An intermediate estimate, 0.48, is obtained when (a) the sample includes fathers with at least 16 years of earnings, and (b) average earnings are measured over ages 20-55 (including years with zero earnings). The operational decisions leading to the higher estimates do not appear to be methodologically superior.

[15] Given the importance of this argument, it seems useful to reproduce it at length: "Mazumder imputes parent income based on race and education for up to 60% of the observations in his sample to account for top-coding in social security records. These imputations are analogous to instrumenting for parent income using race and education, an approach known to yield higher estimates of the IGE, perhaps because parents' education directly affects children's earnings (Solon 1992). Because the SSA earnings limit is lower in the early years of his sample, Mazumder imputes income for a larger fraction of observations when he averages parent income over more years (Mazumder 2005, Figure 3). As a result, Mazumder's estimates effectively converge toward IV estimates as he uses more years to calculate mean parent income, explaining why his estimates rise so sharply with the number of years used to measure parent income. Consistent with this explanation, when he drops imputed observations, his IGE estimates increase much less with the number of years used to measure parent income (Mazumder 2005, Table 6)" (Chetty et al. 2014a, Online Appendix, Section E).

al. (2014a), such volatility emerged in 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.[16] When Chetty et al. (2014a) adopted different assumptions about such missing income (all consistent with it being low), the resulting IGE estimates varied widely, a result that in turn motivated them 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), an estimate obtained by dropping children without income data.

The most recent NLS-based estimates of men's earnings IGEs are also difficult to conciliate with the existing estimates of the effects of attenuation bias (both pre and post Mazumder 2005). We are referring here to recent research by Bratsberg et al. (2007) that yields an estimate of men's earnings IGE of 0.54 using two years of parental information, and by Jäntti et al. (2006) that yields 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 lead to underestimation of between 30 and 50 percent (Solon 1999, p. 1778), Jäntti et al.'s estimate would entail a true IGE of between 0.73 and 1.04, which is 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).

The foregoing suggests not only high uncertainty but also some amount of 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 evidentiary foundation. As large-sample and high-quality administrative data have increasingly been analyzed, a new low-end estimate of 0.34 has emerged for the income IGE (for men and women pooled), and once commonly-accepted arguments about the effects of attenuation bias are now in dispute. The overall effect of the increased use of administrative data has, paradoxically, been to increase the range of plausible estimates.

A similar disarray is evident 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

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[16] In any given year, many individuals are not required to file tax returns, as they fall below the filing threshold. For more details on filing requirements, see the Form 1040 Instructions, available at www.irs.gov/pub/irs-pdf/i1040.pdf.

women (Shea 2000) or a higher value for men (Fertig 2003; Minicozzi 1997; Jäntti et al. 2006; Raaum et al. 2007).[17]

We have to this point ignored the further complicating matter of the possibility of nonlinearities (in log-log space) in the relationship between parental and children's income and earnings. Because of sample-size constraints, only a few researchers have examined such nonlinearities in the United States. In his seminal PSID-based paper, Solon made a heroic attempt 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 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.[18] There is also very substantial variability across the studies in the implied values of the IGEs under the estimated curves.[19] It follows that neither the PSID nor the NLS have delivered clear evidence on this question. To be sure, the available studies suggest convex curves on balance, but the evidence is 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 (if indeed there are any nonlinearities).

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

[18] 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.

[19] 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 (10th percentile of parental earnings), 0.58 (50th percentile of parental earnings), and 0.65 (90th percentile of parental earnings).

Why have we reviewed the literature on nonlinearities in some detail? Although one might imagine that they are a relatively unimportant issue, in fact some of the most consequential evidence on mobility rests hard on assumptions about nonlinearities (or the lack thereof). We review below three reasons why issues of functional form matter.

Summary measures: The first point to be made is that possible nonlinearities are a concern even if the objective is simply to provide a single-value summary measure of persistence. When departures from linearity are large, an estimate based on the constant-IGE assumption cannot be "saved" 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 is accordingly unclear – in the absence of good evidence on nonlinearities – whether the constant-IGE estimates reviewed above provide valid summary measures of persistence.

Cross-national and over-time comparisons: The so-called Gatsby Curve, which relates mobility to income inequality and suggests that the U.S. has less mobility than many well-off countries, has increased public concern about opportunity in the U.S. (see Corak 2013; Krueger 2012). It bears emphasizing, however, that the frequently-advanced claim that the U.S. has less mobility than other countries usually rests on IGE estimates that assume linearity. If the underlying function is instead nonlinear, these cross-national comparisons may be misleading (Bratsberg et al. 2007). The same conclusion holds for trend analyses: We may well reach incorrect conclusions about trends in mobility when we rely on summary measures that wrongly assume that a straight line adequately characterizes the relationship between parental and children's income and earnings (in log-log space).

Borrowing constraints: The absence of firm evidence on nonlinearities has also made it difficult for researchers to assess standing theories about the effects of borrowing constraints on parental investments in the human capital of their children. It has long been claimed that the shape of the relationship between parental resources and children's earnings provides evidence on who is constrained and how those constraints affect human capital formation (e.g., Becker and Tomes 1986; Mulligan 1997; Han and Mulligan 2001; Corak and Heisz 1999; Bratsberg et al. 2007). If one accepts the line of reasoning behind these hypotheses, the shape of the curve is of interest because it speaks directly to the type of borrowing constraints in play.[20]

It follows from our review that there is a quite troubling *evidence deficit* on U.S. IGEs. The main features of this deficit can be summarized as follows:

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[20] This line of argumentation rests on the strong assumption that the underlying relationship is "linear in the absence of credit constraints" (Grawe 2004b, p. 818). If, as Grawe argues, there is "no basis whatsoever" for that assumption, then scholars can be led astray by attempting to infer the nature of the constraints from the estimated curves (Grawe 2004b, p. 818).

- The post-1990 survey estimates of income and men's earnings IGEs are in the 0.13-0.54 range. Although careful reviews of the survey-based results suggest IGEs of at least 0.4, the data sets upon which the estimates are based have real limitations. The estimates are highly sensitive to the way in which the sample is defined, are inconsistent across data sources, and are sometimes difficult to conciliate with existing evidence on the magnitude of attenuation bias.
- The new administrative-data studies of economic mobility have not focused on estimating income IGEs, and the few available estimates are very sensitive to assumptions about the income of children with missing income data.[21]
- There is far more administrative-data evidence on earnings IGEs. However, it has been argued that Mazumder's (2005) very influential estimates of earnings IGEs are upward biased, while Dahl and DeLeire's (2008) estimates are very sensitive to the choice of sample and the way in which earnings are measured.
- There is no available research on IGEs for after-tax measures of income. We therefore don't know whether the persistence of advantage in "available resources" is similar to or different from the persistence of advantage in total income.
- There has been very 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.
- There is very little research on potential nonlinear patterns in the relationship between children's and parental income or earnings (in log-log space). The existing evidence is compromised by small sample sizes and the associated need to resort to highly parameterized models.

The main purpose of our report is to address this evidence deficit. We will do so by introducing semiparametric and nonparametric models that make it possible to overcome some of the methodological problems that have beset earlier efforts to estimate IGEs with administrative data. Before introducing these models, we will first describe the characteristics of our data set, the advantages that it offers for the study of intergenerational economic mobility, and some of our efforts to document its representativeness.

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[21] We have not discussed Chetty et al. (2014b) in any detail because they report IGE estimates similar to those reported in Chetty et al. (2014a). Likewise, we have not discussed the findings of Auten et al. (2013) and Chetty and Hendren (2015), as they do not estimate IGEs.

The SOI-M Panel

The SOI-M Panel, which is described in this section of the report, is the basis of our new estimates of economic mobility in the U.S. It might of course be asked why, given the pathbreaking research of Chetty et al. (2014a), a new administrative data set is needed. We address this question here by laying out the rationale for the SOI-M Panel.

The research by Chetty et al. (2014a) rested on the full population of 1996-2012 electronic tax records. The analysis entailed (a) identifying U.S. citizens born in 1980-1982 and alive in 2012, (b) calculating their average income across the years 2011 and 2012, (c) finding the first return in which they were listed as dependents (to identify their parents); and (d) computing the average income of their parents from 1996 to 2000. The resulting data set is large enough to study mobility within quite small geographic areas.

Although this approach has yielded important evidence on geographic variability in mobility, it is less attractive for the purpose of estimating national-level IGEs. Because one of our main objectives is to secure the best possible estimates of income and earnings IGEs, it is important to guard against lifecycle bias. It is difficult, however, to do so when one relies exclusively on the population of tax records to obtain information on parents and children. This is because these data are only available starting in 1996. Even if one selects the oldest children in 2011-12 for whom parental information can be collected, they are still only 29-32 years old then and thus too early in their careers to yield good IGE estimates.[22] It is likewise important to reduce attenuation bias as much as possible by averaging parental income across many years.[23]

We have therefore constructed a new panel, based on a sample of 1987 tax returns, that makes it possible to address both of these biases.[24] This new panel represents all children born between 1972 and 1975 who were living in the U.S. in 1987. Because it is based on 1987 returns, the children who are identified in these returns are old enough in 2010 (i.e., 35 to 38 years old) to substantially reduce lifecycle bias. We carry out most of our analyses with 2010 data precisely to ensure that such lifecycle bias is minimized.[25] We are also able to minimize attenuation bias by

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[22] Because parental income is best measured when the children are relatively young, it is not possible to 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). We discuss subsequently their claim that, even though children's income was measured at ages 29 to 32, there is no lifecycle bias in their analyses.

[23] 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. We discuss this argument subsequently.

[24] This panel is only available for internal SOI use.

[25] The income and earnings data for the children in our sample, which come from population records, were available only up to 2010 (when we started this research).

averaging across nine years of parental information. Although the SOI-M Panel is accordingly well suited for the purpose of estimating elasticities at the national level, the obvious trade-off is that, because it is based on a sample of 1987 tax returns (rather than the full population), it is not large enough to conduct studies at the subnational level.

The rest of this section discusses (a) the "base sample" of the SOI-M Panel, (b) the rules followed to map return-based information into parental income, (c) the data sources used to build the SOI-M Panel, (d) the structure of the final SOI-M Panel and the "late-30s sample" that underlies most of the analyses in this report, (f) the income concepts and measures, (g) the data from the Current Population Survey (CPS) used to impute mean values for some nonfilers, and (h) the descriptive statistics for the "late-30s sample."

Base Sample

The backbone of the SOI-M Panel is the 1987-1996 Statistics of Income Family Panel. The latter panel, which SOI drew in 1988, 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 this study, our objective is of course to represent *all* children born between 1972 and 1975, yet the SOI Family Panel doesn't allow us to meet this objective insofar as it under-represents children whose parents fall below the filing threshold and are not required to file tax returns. We therefore supplemented the sample of children from the SOI Family Panel with additional children who were born in those years and 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"). 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.[26]

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 1 and Figure 1, we evaluate the representativeness of the base sample against 1987 data from the CPS Annual Social and Economic Supplement (CPS-ASEC). The purpose of Table 1 is to show that the 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 family income distributions in the base sample and in the CPS-ASEC (for children 12 to 15 years old).[27] Although Figure 1 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-

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

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

ASEC share. This result is consistent with research documenting an underreporting of top incomes in the CPS (Fixler and Johnson 2012). 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. The records from the refreshment segment of the OTA Panel, which mostly pertain to parents whose income fell below the filing threshold for 1987 (but filed in a subsequent year), served to eliminate a shortfall of children in the SOI Family Panel with less than $10,000 of parental income.

Measuring Parental Income

The person or persons who claim a child as a dependent in 1987 are defined, for our purposes, as the child's parents. If only one nondependent adult claims the child (i.e., there is no "secondary filer" in the return), we typically define that nondependent adult as the (single) parent of the child. However, whenever the adult claiming the child is married and the spouse files separately, both spouses are 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, we compute parental income by pooling resources across the relevant returns, where the pooling is based on the following rules:

(a) If the two parents divorce, and each files jointly with a new spouse, parental income is 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 overstate the income of the child's parents).

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

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

Data Sources

The data sources are presented in Table 2. As shown here, the Data Master File was used to identify the age of parents, the age and gender of the children, and the year of death of deceased children.[28] The parental income data, which pertain to years when the children were 15 to 23 years old, 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

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[28] See Chetty et al. (2014, Online Appendix Section A) for a description of the Data Master File. We compute parental age each year by averaging the age of the parents listed in any return that year.

filing separately") were drawn from the 1998-2010 population tax data. We supplemented these tax data 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, an imputation that is discussed below in more detail (see "Imputations for Nonfilers and their Spouses"). We also explain in a subsequent section how the marital status of children was determined (for the analyses in which doing so was necessary).

Structure of the SOI-M Panel and the Late-30s Sample

The SOI-M Panel represents all children born between 1972 and 1975 who were living in the U.S. in 1987. As indicated in Figure 2, the panel covers the years from 1998 to 2010, with each record in the panel representing a child-year. These child-year records include parental information pertaining to the years in which the children were 15 to 23 years old. When the children are 26 years old, they enter the panel and remain there until 2010 (unless they die before then). As noted above, most of our analyses are carried out with the "late-30s sample," which pertains to children ages 35 to 38 in 2010. This restriction, which will be discussed in more detail below (see "Attenuation and Lifecycle Biases"), serves to reduce lifecycle bias. For analyses based on the late-30s sample, we use income or earnings reports for 2010.

Income Concepts and Measures

We employ three different income concepts: total family income, disposable family income, and individual earnings (which are only available for children).[29] These concepts are not measured identically for parents and children due to differences in data availability (see Table 3). We express all income variables in 2010 dollars using the Consumer Price Index for Urban Consumers - Research Series (CPI-U-RS).

We measure parental total income as 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.[30] For nonfiling children, we follow Chetty et al. (2014a) by summing earnings from the W-2 form and UI income from the 1099-G form,

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[29] For the sake of readability, in what follows we refer to these concepts as "total income," "disposable income," and "earnings."

[30] Because W-2 information is only available starting in 1999, our measure of children's total income in 1998 does not include nontaxable earnings. Likewise, because nontaxable earnings are not available for parents in 1987-1998, they are not included in our measure of parental income in any year.

when at least one of these forms is available.[31] Unlike Chetty et al. (2014a), whenever both W-2 and UI information are unavailable, we use data from the CPS on likely nonfilers to conduct mean or multiple imputation. This CPS-based imputation is described below.

As Table 3 again shows, we measure after-tax income by subtracting out net federal taxes (which include refundable credits) from total income. Throughout our analyses, we refer to this concept as "disposable income." We have not, however, subtracted out state taxes, nor have we included some non-taxable transfers (e.g., Temporary Assistance for Needy Families). It follows that our measure of "disposable income" can only provide an approximation to true disposable income.

Starting in 1999, we measure the earnings of all children (and of the spouses of filing children) 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 (see Johnson 1954). For the earnings of spouses of nonfiling children, we again resort to CPS-based imputation, as described below.

Imputations for Nonfilers and their Spouses

The CPS-ASEC identifies likely nonfilers using a tax simulation model. Although this information is available for the entire period covered by our data, the CPS-ASEC data after 2003 have serious inconsistencies and cannot be used.[32] We therefore use 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 use 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.[33]

Because mean imputation of marital status is not feasible, we employ the same CPS-ASEC data to carry out multiple imputation by gender and exact age in models that distinguish between single and married children. For nonfiling children without W-2 or UI information, we

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[31] We proceed in this way for all years but 1998 because W-2 information and 1099-G information are only available starting in 1999. Although Chetty et al. (2014a) do not discuss this point, the sum of (own) earnings and UI income can be expected to provide a good approximation to total income when the nonfiler is single or is legally married but with a spouse who does not contribute, or contributes very little, to total income. The latter condition will hold when (a) the spouse has no or little income of his or her own, or (b) the spouses are formally (e.g., through an interlocutory decree of divorce) or informally (i.e., de facto) separated. In an analysis based on CPS data, we have found that this condition is very frequently met, especially for the late 30s sample. The CPS analysis indicated that only 10 percent of likely nonfilers who are 35-38 years old and with earnings or UI income are legally married and not separated. The rest are single or are separated.

[32] For a discussion of some (but not all) of the problems, see http://users.nber.org/~taxsim/to-taxsim/cps/cps-feenber.

[33] It will become clear subsequently why we impute mean income values rather than mean log-income values.

carry out a joint imputation of marital status, total income, and spouses' earnings. For nonfiling children with W-2 or UI information, we only impute marital status. We discuss these imputations in more detail below (where it will become clear why we do not use the multiple-imputed variables for all analyses).

Descriptive Statistics

Because most of our analyses are based on the late-30s sample, we provide descriptive statistics pertaining to this sample rather than the full SOI-M dataset. The late-30s sample 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.[34]

The resulting descriptive statistics for our various models are shown in Tables 4 and 5. Table 4 reports the number of observations and the gender and age of the children included in each analysis, the origin of their income information, and the number of missing years of parental information among those retained. Table 5 shows the weighted mean and standard deviation of (a) the income variables for the children and their spouses, and (b) the various parental variables (age, total income, disposable income) after averaging them over the 9-year span from which they were taken.

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[34] Whereas condition (a) is only relevant in the case of income models (i.e., not for earnings models), condition (b) applies either to income or to earnings, depending on the model. In conducting 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. We also estimated IGEs with samples in which (a) only children with 9 years of parental information were retained, and (b) only children with at least 8 years of parental information were retained. The point estimates were typically slightly larger with these samples. The results from these supplementary analyses are not reported here.

Methodological Issues

The purpose of this section is to describe our statistical models and estimation approaches, and discuss other key methodological issues. We divide this section into eleven parts that address in turn (a) the "wrong estimand" problem and how it can be overcome, (b) our approach to investigating the structure of nonlinearities in the data, (c) the summary measures of persistence that we use to characterize nonlinear curves, (d) our measures of economic persistence for families that are far apart in the income distribution, (e) our procedures for multiple-imputing the marital status of nonfiling children, (f) the ways in which we conduct inference, (g) the selection bias that is introduced by excluding children without income or earnings, (h) the rationale for estimating models that treat parental age as endogenous, (i) our approach to reducing the attenuation and lifecycle biases that have long preoccupied scholars, (j) our analysis of the "channels" through which the total-income IGE_e is generated, and (h) some of the more important remaining methodological decisions.

This section is extensive not just because there are necessarily many operational details but also because our report introduces new approaches and methods as well as new data. It is accordingly important to lay out why these approaches and methods will yield improved estimates of mobility in the U.S.

The "Wrong Estimand" Problem

We begin by showing that the IGE, as conventionally estimated, has been misinterpreted. The key problem here is that the conventional estimator does not estimate what scholars assumed they were estimating. Although the estimated IGE is typically assumed to refer to the *expectation* of children's earnings or income (IGE_e), it actually pertains to the *geometric mean* of children's earnings or income (IGE_g).

The latter point is easily demonstrated by considering the following standard population regression function:

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

where Y is the children's annual income (or earnings) when they're adults, and X is the average of the parents' income (or father's earnings) over several years.[35] The parameters of this regression function are usually estimated by Ordinary Least Squares (OLS), but other approaches have also been employed. Regardless of the estimation approach, the resulting estimate of β_1 is

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[35] We use expressions like "$Z|w$" as a shorthand for "$Z|W = w$." The right-hand side of Equation [1] typically includes polynomials on the age of parents and children at the time their income is observed (as will be discussed below). We have not included them here because doing so would not affect any of the arguments in this subsection. Likewise, the average of parents' log-income (across years) is sometimes used instead of the logarithm of the average, but our arguments in this subsection are unaffected by which of these two is employed. (When the average of log parental income is used, that is equivalent to using the log of the geometric mean of parental income.)

typically stipulated to be the estimate of the (constant) IGE. When OLS is used, we refer to the estimator of β_1 as the "OLS log-log estimator" of the IGE.

As Y is a random variable, an IGE must be the elasticity of a specific property of Y's conditional distribution, most conventionally the expectation. Although mobility researchers typically assume that their estimate of the IGE indeed pertains to the expectation (see Mitnik and Grusky 2015), the estimate of β_1 is not, in the general case, an estimate of the elasticity of the conditional expectation of children's income. This would be the case only if $E(\ln Y|x) = \ln E(Y|x)$, but of course the latter does not hold (due to Jensen's inequality).[36] Instead, as $E(\ln Y|x) = \ln \exp E(\ln Y|x)$, Equation [1] is equivalent to:

$$\ln GM(Y|x) = \beta_0 + \beta_1 \ln x,$$

where GM denotes the geometric mean operator. It follows that the reported coefficient from the estimation of Equation [1] corresponds to the following estimand:

$$\beta_1 = \frac{d \ln GM(Y|x)}{d \ln x}.$$

This means that β_1 is the *elasticity of the conditional geometric mean*. It is the percentage differential in the geometric mean of children's income with respect to a marginal percentage differential in parental income.[37] There has not been any discussion in the literature as to why the elasticity of the geometric mean may be of interest in assessing the level of intergenerational mobility. As we argue below, this is in fact a very unattractive estimand.

Although we will provide some estimates of the IGE of the conditional geometric mean (or IGE$_g$), the bulk of our analyses focus on the IGE of the conditional expectation (or IGE$_e$). To estimate the latter under the constant-elasticity assumption, we posit the following population regression function:

$$\ln E(Y|x) = \alpha_0 + \alpha_1 \ln x \qquad\qquad [2]$$

where $\alpha_1 = \frac{d \ln E(Y|x)}{d \ln x}$ is the IGE$_e$.[38] The parameters of this regression function may be estimated using a variety of approaches. These include nonlinear least squares, generalized method of

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[36] The OLS log-log estimator can be a consistent estimator of the IGE of the expectation, but only when the error term satisfies very specific conditions (Santos Silva and Tenreyro 2006). The misuse of this estimator for the purpose of estimating the elasticity of the expectation has been examined in the context of Cobb-Douglas production functions (Goldberger 1968) as well as international trade (Santos Silva and Tenreyro 2006).

[37] We are grateful to Joao Santos Silva for pointing out to us that the OLS log-log estimator estimates the elasticity of the conditional geometric mean. This point was not made in Santos Silva and Tenreyro (2006), but was recently mentioned by Manning (2012) in the context of health economics. Likewise, Goldberger (1968) identified the de facto estimand of the OLS log-log estimator as the elasticity of the conditional median, which is the same as the conditional geometric mean in the context of his analysis (due to the assumption of a lognormal distribution). In their recent comprehensive review of the mobility literature, Jäntti and Jenkins (2013:53) note in passing that the IGE "measures the degree of regression to the (geometric) mean in income," but they do not pursue the issue further.

moments, and pseudo maximum likelihood (PML) based on a variety of distributions in the linear exponential family (e.g., Poisson, gamma, inverse Gaussian).[39] However, given the arguments and evidence offered by Santos Silva and Tenreyro (2006, 2011), we employ here the Poisson PML (PPML) estimator.[40] The IGE_e is estimated with this semiparametric estimator almost as easily as the IGE_g is estimated with the OLS log-log estimator.

We will be examining the advantages of estimating the IGE_e throughout this report. The simple but important point for now is that mobility researchers have interpreted their estimates in ways that misrepresent what they actually are estimating (see Mitnik and Grusky 2015). Although this disjuncture between interpretation and practice could be addressed by simply repairing the language with which IGE estimates are described, the usual interpretation is in fact a desirable one, and hence the field would do better by estimating precisely what researchers have assumed they were estimating. We suggest, in other words, that the language should be retained and the practice changed. A key payoff to doing so, as will be shown subsequently, is that it then becomes possible to deliver estimates that are freed of the selection bias that arises when children without earnings or income are dropped from the analysis.

Nonlinearities

We noted earlier that mobility scholars have typically assumed that the IGE 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, however, whether the many social and economic determinants of mobility do in fact combine to produce a relationship between parental income and the expected income of children that is well approximated by a straight line (in log-log space).

We use two approaches to investigate possible nonlinearities in this relationship. In the first approach, we modify Equation [2] by including the right-side terms that permit a linear spline effect with knots at the 10th, 50th, and 90th percentiles.[41] The resulting population regression function is:

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[38] This is the reason why, for the bulk of our analyses, the relevant quantity to impute for nonfilers without administrative information is mean income rather than mean log-income (when using mean imputation).

[39] In contrast to the OLS log-log estimator, PML estimators are consistent estimators of the constant IGE_e regardless of the distribution of the error term (provided that the mean function is correctly specified). See Gourieroux, Monfort, and Trognon (1984).

[40] We have also experimented with the gamma PML estimator. Although it provides estimates similar to those delivered by the PPML estimator, we had convergence and other computational difficulties for some of the more complex models. These difficulties have not arisen with the PPML estimator.

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

$$\ln E(Y|x) = \alpha_0 + \alpha_1 \ln x + \sum_{j=10,50,90} \alpha_j \, I\big(\ln x > q_j(\ln X)\big)\big[\ln(x) - q_j(\ln X)\big], \qquad [3]$$

where I is the indicator function and the operator $q_j(.)$ is the jth percentile of the variable in its argument (the logarithm of average parental income in this case). The model of Equation [3] assumes that (a) the IGE$_e$ is constant within each of the four regions of parental income defined by the 10th, 50th, and 90th percentiles of this variable, (b) the IGE$_e$ may vary across regions, and (c) the curve relating the log of children's expected income to parental income 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:

$$E(Y|x) = F(x), \qquad [4]$$

where F is an unknown smooth function. For each estimated curve, it is necessary to select a smoothing parameter, which determines the fraction of observations to be included in the computation of each local polynomial regression. In our models, this selection occurs automatically (within the range of [.08, 1]) by finding the global minimizer of 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).[42] We use degree 1 polynomials and a tricube weight function.

We will thus estimate both semiparametric spline models and nonparametric models for our core substantive analyses (unless the samples being analyzed involve multiple imputation).[43] This two-model approach allows us to assess how robust our estimates are to changes in the specification. The models are also complementary: The nonparametric model requires much weaker functional-form assumptions, whereas the spline model yields more efficient estimates and allows us to straightforwardly test the constant-elasticity hypothesis (see below for details).[44]

Although we will mainly present IGE$_e$ estimates, we will also present some estimates of the IGE$_g$. We will provide (a) constant-elasticity estimates produced with the log-log OLS

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[42] For the disposable income models, we reuse the same smoothing parameters selected for the corresponding total income models, as doing so ensures that any difference is due to a change in the income measure (rather than a change in the smoothing parameter).

[43] For analyses that involve multiple imputation, we only estimate nonparametric models. This is partly because, for these models, we do not have any independent evidence to guide our decisions regarding the number and location of knots. We likewise do not estimate spline models for the methodological analyses reported in the subsection titled "Nonfilers and Nonearners."

[44] An alternative way of proceeding, which is substantially more complicated to implement, is to compare the nonparametric and constant-elasticity models using some loss function (see Racine and Parmeter 2014).

estimator, and (b) nonparametric estimates of summary measures of persistence (as introduced in the next section). The latter estimates will be based on the following nonparametric model:

$$\ln GM(Y|x) = E(\ln Y \,|x) = G(\ln x), \qquad\qquad [5]$$

where G is an unknown smooth function.

Summary Measures of Persistence in the Nonlinear Context

Once the spline and nonparametric models are estimated, we then use the estimates of the corresponding parameters or nonparametric curve to estimate the expected value of the IGE$_e$ in regions of the curve defined by intervals of parental income. We obtain estimates of quantities of the form:

$$E_{x_{lb} \leq X \leq x_{ub}}(IGE_e(Y|x)) = E_{x_{lb} \leq X \leq x_{ub}} \left(\frac{d \ln E(Y|x)}{d \ln x} \right), \qquad\qquad [6]$$

where the outer expectation is taken over the population distribution of parental income between the lower bound x_{lb} and the upper bound x_{ub}. We estimate two types of summary measures:

- *Global IGE$_e$:* This is the expected IGE$_e$ over *all* levels of parental income (i.e., x_{lb} and x_{ub} are the minimum and maximum values of X in the sample). The global IGE$_e$, as with the conventionally-estimated constant IGE, provides a convenient single-value summary measure of the degree of persistence found over the full parental distribution (but of course it now does so without assuming that the elasticity is constant).
- *Region-specific IGE$_e$:* We also estimate the expected IGE$_e$ in the four regions defined by the 10th, 50th, and 90th percentiles of parental income. By comparing these region-specific elasticities, we can characterize the shape of the relationship (in log-log space) between parental income and the expected income or earnings of children.

If the constant-elasticity model were used to estimate either a global or region-specific IGE$_e$, the resulting value would of course equal the (single) estimated elasticity from that model. It is appropriate, therefore, to compare a global or region-specific IGE$_e$ estimated with a spline or nonparametric model to the single IGE$_e$ estimated with the corresponding constant-elasticity model.

When the spline model is used, estimates of the region-specific elasticities can be obtained by simply summing estimated parameters.[45] The estimate of the global IGE$_e$ can then be computed as a weighted average of the region-specific estimates (with weights equal to the proportions of children in the four regions). We must use numerical approximations, however, to

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[45] For example, the IGE$_e$ in the 10th-50th percentile region is equal to $\alpha_1 + \alpha_{10}$, while the IGE$_e$ in the 50th-90th percentile region is equal to $\alpha_1 + \alpha_{10} + \alpha_{50}$.

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.[46] The global IGE$_g$ is computed in the same way.

Arc Elasticities

We also estimate standard (i.e., Allen's) arc elasticities to examine the extent to which inequalities between families with very different incomes are preserved into the next generation. Up to now, our discussion has focused on point elasticities, which may be used to characterize the persistence of income differences among children raised in families that are "close together" in the parental income distribution. Under a constant elasticity model, the value of the IGE$_e$ approximates the share of between-family differences (in percent income) that is preserved into the next generation, but the approximation holds only when the differences in question are small.[47] The same is true of global and region-specific measures at the level of expectations.[48] It follows that these measures likewise cannot be used to characterize such shares for families that are far apart in the parental income distribution.

The degree of persistence across long reaches of the parental-income distribution can, however, be measured with standard arc elasticities. In the case at hand, these take the following form:

$$AIGE_e(Y|x,i,j) = \frac{E(Y|q_j(X)) - E(Y|q_i(X))}{q_j(X) - q_i(X)} \frac{q_j(X) + q_i(X)}{E(Y|q_j(X)) + E(Y|q_i(X))} \quad [7]$$

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[46] Our nonparametric estimators of the global and region-specific IGEs ignore 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.

[47] From Equation [2], it follows immediately that $IGE_e = \alpha_1 = [\ln E(Y|x_2) - \ln E(Y|x_1)]/[\ln x_2 - \ln x_1]$, where $x_2 > x_1$ without any loss of generality. It is well known that a difference in logarithms provides a good approximation to a percent difference only if that percent difference is small. The constant-elasticity IGE_e, α_1, thus provides a good approximation to the share of the percent difference between parental incomes x_2 and x_1 that exists between the expected incomes of the corresponding children, but only as long as the percent difference between x_2 and x_1 is small.

[48] Assume a setup in which pairs of families whose incomes differ very little in percent terms are randomly drawn from the parental income distribution. The global IGE$_e$ approximates the expected share of percent differences passed from one generation to the next (across all possible such random draws). The same interpretation can be applied to region-specific elasticities by imposing the proviso that draws are restricted to the relevant region of the parental income distribution.

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 $AIGE_e(Y|x, 10,90)$.[49] We have improved precision by estimating the $AIGE_e$ of children's total income between points "around" the ith and the jth percentiles of parental income. In estimating $AIGE_e(Y|x, 10,90)$, we thus 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.

The virtue of standard arc elasticities, as should be clear from Equation [7], is that they are independent of how proportional differences are computed. The standard arc elasticity is, like all elasticities, a ratio of proportional differences, where these are computed by standardizing differences in their original units. In the case of regular percentages, standardization is achieved by dividing a difference by either the smaller or larger of the values defining that difference. If these two alternative approaches are employed to compute arc elasticities, the results tend to differ substantially (unless the differences involved are very small). This has been long deemed an undesirable property (Allen 1934). In the standard arc elasticity, proportional differences are computed by standardizing differences by the *average* of the two values, thus providing a measure that is independent of how proportional differences are computed.[50] The resulting value of the AIGE_e always falls between (a) the value that would be obtained if standardization were achieved by dividing by the larger of the values defining differences, and (b) the value that would be obtained if standardization were achieved by dividing by the smaller of the values defining differences.

Marital Status and Multiple Imputation

For some of our analyses, we would like to identify children who pool resources with a (life) partner, regardless of whether they are married or not. Although we cannot identify cohabitors with our data, filing status is a proxy for marital status, which in turn is a proxy for resource pooling.[51]

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[49] This means that the arc elasticity is computed with (a) the proportional increase in a child's expected income when her or his parents are at the 90th rather than the 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 mid-point between the two values defining the difference).

[50] This is not the only possible way of defining arc elasticities so that they satisfy Allen's (1934) "symmetry" requirement. See, in particular, Holt and Samuelson (1946) and Vázquez (1998).

[51] Using CPS monthly data, we estimated a cohabitation rate of 9 percent for those who (a) were 35-38 years old in 2010, and (b) were either U.S. natives or did not immigrate into the country after 1987 (as those arriving later are not represented in our SOI-M sample). The CPS, however, excludes institutionalized people (i.e., people living in a correctional institution, mental institution, or an institution for the elderly, handicapped, or poor). If the 2010 American Community Survey (ACS) is restricted to respondents meeting conditions (a) and (b), we find that

For children who file, the category "married" refers to the filing statuses "married filing jointly," and "married filing separately" (with and without spousal exception), while the category "single" pertains to the filing statuses "single," "head of household," and "widower with dependent child." According to tax code, a person who is legally married can file as head of household if, among other conditions, his or her spouse did not live in the same household for the last six months of the tax year.[52] This is an advantageous definition for our purposes given that it allows us to count as "single" those who, despite being legally married, are not likely to be pooling resources.

For nonfilers, we of course lack such filing-status information, which complicates our analysis. In studies matching tax returns to the CPS, it has been reported that 20-25 percent (Cilke 1998:Table 1; Mok, forthcoming) of nonelderly and nondependent nonfilers were married.[53] In our own analysis of *likely* nonfilers (using the CPS-ASEC), we find a similar share of married among the nonelderly, namely 26 percent.[54] The results are very similar when the sample is restricted to correspond to the ages of children in the full SOI-M Panel (26-38 years old) and the late-30s sample (35-38 years old). We find that 24 percent of likely nonfilers are married in the former case and 28 percent are in the latter. If we further exclude those who are separated (as doing so better captures the pooling-of-resources criterion), 16 percent of those aged 26-38 are married and 18 percent of those aged 35-38 are married.[55] In the key "late-30s group," our CPS-ASEC analyses show that only 10 percent of those with earnings or UI income are married, whereas 19 percent of those without such income are married. The vast majority (93 percent) of likely nonfilers in this age group belong to the latter category.

This information informed our imputations for analyses in which marital status plays a role. For nonfilers without earnings or UI income, we carry out multiple imputation (see, e.g., Little and Rubin 2002) with the CPS-ASEC data. We jointly impute marital status (counting "separated" as single), total family income, and spouse's earnings by gender and age. In each round of imputation, we randomly select CPS-ASEC observations of likely nonfilers without earnings or UI income, and we jointly assign their values on those three variables to nonfilers

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approximately 2.2 percent of the population of interest is institutionalized. Assuming a cohabitation rate of zero for this group, our adjusted estimate of the cohabitation rate is 8.8 percent.

[52] For details, see the Form 1040 Instructions, available at www.irs.gov/pub/irs-pdf/i1040.pdf.

[53] We are grateful to Shannon Mok for providing data to us from her unpublished study.

[54] This analysis is based on the same years of the CPS-ASEC as were used to compute values for mean imputation (see above). We define the nonelderly as those who are 19-64 years old.

[55] In the CPS-ASEC, the total income of those who are separated does not include the spouse's income. There is no information on spouse's income available for these cases.

without earnings or UI income in the SOI-M dataset (matching on age and gender).[56] For nonfilers with earnings or UI information, the CPS-ASEC sample is too small to use a similar approach.[57] In this case, we simply assign them the status "married" with a probability that is a function of age and sex, estimated with a logistic regression on the CPS-ASEC data. We complete five imputations of this sort for each variable that we (partially) impute. The values for filers are the same across sets of imputed variables. For nonfilers with other administrative information, the income values are the same across sets of imputed variables, but their marital status (which is imputed) may vary across sets.[58]

How successful are these procedures for assigning a marital status to filers and imputing one for nonfilers? If we compare the share of married children in our (weighted) data with the corresponding share in the American Community Survey (ACS), we find that our share is somewhat lower. In the ACS, we find a marriage rate of 58.6 percent for people who are 35-38 years old in 2010 and U.S. natives (or already living in the U.S. by 1987), and a "separated rate" of 3.5 percent. In the SOI-M Panel, 53 percent of children are classified as married in 2010.[59] This discrepancy is not due to differences between the 2010 filers in the SOI-M data set and in the population of tax returns. The married share among SOI-M filers in 2010 is, to the contrary, virtually the same as that found in a large cross-sectional sample of 2010 returns (i.e., the 2010 Cross Sectional SOI sample).[60]

We have identified two possible sources of this difference between the SOI-M and ACS estimates. First, some people may file as head of household but self-classify as married in the ACS, thus generating a lower marriage rate in the SOI-M relative to the ACS. Second, couples living together or in common-law marriages are allowed to classify themselves as married in the ACS questionnaire, even though they are not legally married.[61]

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[56] We proceed by randomly drawing observations with replacement and with a probability proportional to the CPS-ASEC sampling weight (separately for each gender and age combination).

[57] Even if sample size were not an issue, an identical approach could not be used because the earnings and UI income of these nonfilers should be taken into account.

[58] For nonfilers with other administrative information and with the imputed status of "married," the (potential) income of a spouse is not included or imputed, as we prefer in this case to use the available administrative information rather than imputing total income from the CPS-ASEC. Only 0.4 percent of the observations used in our income models are affected.

[59] This percentage is the average of the point estimates produced with each of the five (partially) imputed marital-status variables.

[60] To compute the share of married in the latter sample, we selected primary filers aged 35-38, and we assumed that the secondary filer was in the same age range.

[61] See the 2010 ACS documentation (Ch. 6) at /www.socialexplorer.com/data/ACS2010/documentation/.

The foregoing suggests that, although our marital-status measure is a reasonable proxy for resource pooling, it is also likely to contain a nontrivial amount of error both as a measure of marital status *stricto sensu* and as a measure of resource pooling. This error should be borne in mind when interpreting the results from our analyses that rely on that measure.

Inference

Although we use standard procedures of statistical inference when we estimate IGEs with constant-elasticity models, we are obliged to adopt less standard inferential approaches when we estimate IGEs with other models. Moreover, when we rely on multiple imputation, further complications with inference arise. We describe here the inferential procedures we employ.

For the constant-elasticity models of Equations [1] and [2], we construct 95 percent Wald confidence intervals and employ robust standard errors, which is mandatory with the PML estimator. The standard errors that we compute also take into account the clustering of children into families (see, e.g., Rogers 1993). Because the constant-elasticity model of Equation [2] is nested within the spline model of Equation [3], we are able to test the constant-elasticity assumption with an F-test of the null hypothesis that all terms of the spline model, save α_0 and α_1, are zero.

In our analyses with the spline and nonparametric models, all inference (save the F-test noted above) is based on the nonparametric bootstrap, using 2,000 bootstrap samples.[62] We generate bootstrap samples via simple random sampling of primary sampling units, with replacement, within strata. To assess the uncertainty of our spline-model estimates, we use the percentile method to compute confidence intervals (Efron and Tibshirani 1986). We can, however, only compute "variability bounds" (Racine 2008) or "confidence bands" (Wasserman 2006) for our nonparametric estimates. 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."[63] 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). For simplicity of terminology, we refer to the resulting intervals as "confidence intervals."[64]

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[62] 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).

[63] 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.

[64] Although Corak and Heinsz (1999) and Hertz (2005) have also used nonparametric models to study economic mobility, neither of these authors provided any information regarding the uncertainty of their nonparametric estimates.

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 will 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.

The analyses in which we use multiple-imputed data all involve nonparametric regressions. We proceed by estimating the nonparametric model five times (once with each set of imputed variables). The reported point estimates are then computed, as is usual with multiple imputed data (e.g., Little and Rubin 2002), as the average of the point estimates produced with each of the five sets of imputed variables. For purposes of inference, we still use the nonparametric bootstrap, but now we nest the multiple-imputation within the bootstrap resampling. With each bootstrap sample, we thus repeat the process of (a) multiple imputation, (b) estimation with each set of imputed variables, and (c) combination of results into one point estimate. This means that we estimate each nonparametric model 10,000 times (i.e., 2,000 bootstrap samples times 5 sets of imputed variables). After averaging results within each bootstrap sample, we have 2,000 point estimates based on the bootstrap samples. The resulting variability reflects not only sampling variability across bootstrap samples but also the additional variability generated by the multiple-imputation that occurs within each bootstrap sample. Thus, by using these 2000 point estimates for purposes of inference, we take into account both sampling uncertainty and the additional uncertainty due to the imputation.

This approach is computer-intensive because it combines nonparametric regression, bootstrap-based inference, and multiple imputation. For this reason, we decided to carry out multiple imputation only for models in which marital status is relevant. The use of mean imputation in the other analyses should lead to a small underestimation of the uncertainty of our estimates (Little and Rubin 2002).

Selection and the "Zeros Problem"

We noted in a preceding section that the conventionally-estimated IGE has been interpreted as the elasticity of the expectation of children's income (or earnings) when in fact it is the elasticity of the geometric mean of children's income (or earnings). The further point that we made was that there is no clear rationale for estimating the elasticity of the geometric mean. Although we have thus emphasized so far the conceptual case for IGE_e, there is also a compelling methodological case for it. We turn to that methodological case here: The purpose of this section is to show that the "zeros problem" looms large when estimating the IGE_g but can be readily solved by instead estimating the IGE_e.

Whereas other commonly used measures of central tendency (e.g., expectation, median, mode) are defined regardless of the support of the random variable, the geometric mean is

undefined, or necessarily zero, when the random variable includes zero in its support.[65] The geometric mean cannot in this situation be employed to characterize the central tendency of the variable's distribution. As children quite often have zero income or earnings, it follows that the geometric mean cannot be employed to characterize the distribution of children's income or earnings conditional on parental income. In our view, this "zeros problem" is a very fundamental one, indeed it alone provides a definitive reason to opt against the IGE_g as a summary measure of economic persistence.

It has not been appreciated, however, that a population regression function like that of Equation [1] is problematic because it implicitly relies on the geometric mean as the measure of central tendency. Instead, when positing population regression functions of this form, researchers understood the "zeros problem" as the wholly practical one that the logarithm of zero is undefined. This practical problem was then addressed by simply dropping children without income or earnings (thus making estimation of the IGE_g possible). We are not aware of any attempt to justify this conventional practice. It has instead been treated as a practical expedient.

It is, however, a very problematic expedient because we care about differences in lifetime income or earnings across *all* children of different origins. The conventional approach of dropping children without income or earnings excludes many children who (a) cannot find work (i.e., unemployed or discouraged workers), or (b) are unable to work (due to mental illness, disability, imprisonment, and so forth). If an analysis is based on administrative data, the conventional expedient further entails excluding children who do not file because their income is too low (i.e., below the filing threshold) or obtained in the informal economy. When these children are excluded, we're in effect estimating an IGE that pertains to the case "when things are going well" (Couch and Lillard 1998; see also Drewianka and Mercan n.d.). Because parental income is inversely related to children's involuntary nonemployment, this approach should thus be expected to introduce a (possibly severe) downward selection bias (e.g., Heckman 1979; 2008).

Can we salvage the IGE_g by retaining the children with zero income or earnings and assigning them arbitrary small positive values? This is an unsatisfactory approach. As we discuss in detail later, small changes in imputed values lead to disturbingly large changes in IGE_g estimates, a result that renders this alternative strategy of replacing zeros with small values deeply problematic, indeed often entirely unfeasible. This problem arises because the IGE_g is highly influenced by small absolute differences in the child's income at the bottom of the children's conditional distributions.[66]

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[65] The arguments that follow hold regardless of whether the geometric mean is considered to be undefined or zero.

[66] From results in Chetty et al. (2014a: Online Appendix, Section C), we can write $IGE_g(Y|x) = \int_0^1 \frac{x}{Q(\tau,Y|x)} \frac{dQ(\tau,Y|x)}{dx} d\tau$, where $Q(\tau, Y|x)$ denotes the τth quantile of $Y|x$. This equation shows that the IGE of the geometric mean can be expressed as a simple average of quantile-specific elasticities and as a weighted average of

The upshot is that those who use the IGE_g must either be willing to (a) accept estimates that are extremely fragile, or (b) subject to (possibly severe) selection bias. Neither of these choices is attractive. Throughout most of this report, we therefore focus on the IGE_e of children's income or earnings with respect to parental income. This approach allows us to retain children with no reported income or earnings and thereby avoid selection bias. Although we also present some estimates in which children with little or no earnings are excluded, we do so mainly with the methodological objective of assessing the effects of selection.

The Endogeneity of Parental Age

When estimating IGEs, it is conventional to include polynomials on children's and parents' age as control variables, each indexing the age at which the income measurements were taken. This practice is understood in the literature as a correction for resorting to measures of income that fall short of capturing true lifetime income. Although we would prefer to measure average income for the full lifetime of both parents and children, unfortunately we only observe their income for some limited period of their lives. This is problematic, so it is argued, because the relationship between lifetime average income and actually-measured income varies with the age at measurement (both for parents and children). To adjust for differences in the ages at which income is measured, polynomials on children's and parental age are then included as controls.

In our own analyses, the variability in children's age is very minor (i.e., 35-38 years old), which means that controlling for age seems unnecessary. The more fundamental point, however, is that the age at which parents have their children is not 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. Insofar as we care about the "gross association" between parental and children's income, controlling for parental age will, therefore, likely bias our estimates. For this reason, our preferred approach is to omit controls for parental age, even though the convention is to include them.

For purposes of comparison, we nonetheless will present estimates with and without such controls. We estimate constant-elasticity and spline models that include (a) cohort dummies

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quantile-specific derivatives. For a lower quantile, the weight, $\frac{x}{Q(\tau, Y|x)}$, takes on a larger value. This differential weighting can be very consequential: The lowest-quantile weights can be expected to be orders of magnitude larger than the highest-quantile weights. It follows that very small absolute differences at the bottom of the distribution can be expected to have a disproportionate influence on the value of the IGE_g. In another paper in preparation, we provide a more detailed discussion of this and related issues, including Chetty et al.'s (2014a: Online Appendix, Section C) representation of the IGE_g as an average of quantile-specific elasticities. The points to which we have alluded in this footnote draw in part on a personal communication with Joao Santos Silva.

(thus simultaneously controlling for cohort and children's age), (b) a third degree polynomial in parental age, and (c) both. The results from these specifications, which are similar to those commonly used in the literature, do not differ much from those that omit the controls. The IGE_e estimates from models that include third-degree polynomials in parental age are typically slightly smaller than those without them (as we will show). The estimates from models that include cohort dummies are essentially the same as without those dummies, while those from models that add both parental-age polynomials and cohort dummies are almost identical to those with controls for parental age only. We will not report results from these two last specifications because they are so similar to those that are reported.

Attenuation and Lifecycle Biases

The history of estimating intergenerational elasticities has in large part been a history of coming to terms with severe attenuation bias. As previously discussed, parental income can be subject to substantial transitory fluctuations and measurement error, with the result that the estimates are downwardly biased when parental income is averaged across a small number of years (Solon 1992, Mazumder 2005, Black and Devereux 2011).[67] We have averaged parental income over 9 years to reduce such bias.

We have also carried out a series of supplementary analyses that document the effect of averaging parental income across 1 to 9 years (see Appendix A). These supplementary analyses allow us to assess the rate of growth of the estimated constant-elasticity coefficients as we increase the number of years of parental information. We find that estimates typically increase with the number of years even as the 9-year maximum is approached. For both the total-income and earnings elasticities, the evidence indicates that (a) our 9-year measures reduce bias substantially, and (b) the benefit to including more than 5 years of information is larger than reported by Chetty et al. (2014a, Online Appendix, Section E). This is not to suggest that 9 years of income is necessarily enough. Even though using 9 years of data clearly reduces attenuation bias, we cannot rule out that some bias remains and that our estimates are still biased downward.

The other main threat to validity is that of lifecycle bias. When income measurements are taken too early or too late in the lifecourse, the observed differences in children's income across levels of parental income may not reflect lifetime income differences very well, and the objective of estimating lifetime IGEs may accordingly be compromised. There is some evidence suggesting that IGEs computed with earnings measured at around age 40 come closest to representing lifetime IGEs (see Haider and Solon 2006; Mazumder 2005).[68] Consistent with this

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[67] The analytic results regarding attenuation bias pertain to the estimation of the IGE_g under a constant-elasticity assumption (and the classical errors-in-variables assumption). These results do not directly extend to the estimation of the IGE_e under constant elasticity, but a heuristic argument similar to that made by Cameron and Trivedi (1998, p. 307) suggests that a downward bias should also be expected in this case.

[68] The evidence on lifecycle bias pertains to the IGE_g (under the constant-elasticity assumption), but it is again reasonable to expect that a similar lifecycle bias should obtain in the case of the IGE_e.

evidence, Appendix A shows that, with the SOI-M data, the IGE$_e$ of men's earnings continues to increase substantially as men move into their late thirties. 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 in Appendix A reveal a marked dip in the IGE$_e$ for male children and pooled children (i.e., males and females together) in 2008 and 2009, a result that is consistent with the hypothesis that the recession led to unusually low income elasticities in those years. Although the recession therefore complicates our interpretations, the results in Appendix A are inconsistent with Chetty et al.'s (2014a, Online Appendix Section C) claim, based on data up to age 32 only, that the IGE$_e$ of income stabilizes around age 30.

For these reasons, our featured analyses are based on data from 2010. We privilege the most recent data because children in the SOI-M Panel were between the ages of 35 and 38 in 2010 and hence come closest to reaching the age at which lifecycle bias is likely to be minimized. By extending out to 2010, our analyses are also less affected by the unusual circumstances of the Great Recession. Because the children in our late-thirties sample still fall short of age 40, it is possible that our estimates understate the true size of lifetime elasticities, a possibility that should be borne in mind in interpreting our results.

Direct and Indirect Transmission of Economic Status

Although our data do not allow for any comprehensive analysis of the mechanisms generating elasticities, we will be able to distinguish between the "direct" and "indirect" transmission of economic status. We proceed by applying an accounting framework that separates these two forms of intergenerational transmission.

There are three channels through which a higher-income upbringing can benefit a child: (a) it can increase her or his own earnings or income; (b) it can increase her or his chances of marrying and staying married (thereby opening up the opportunity of securing income indirectly through a spouse); and (c) it can increase the income of her or his spouse (conditional on being married). The first of these three channels may be considered "direct," while the latter two may be considered spouse-mediated and thus "indirect." The purpose of this section is to introduce and discuss the expressions describing the contributions of each of these three channels to the IGE$_e$.

The first channel has been extensively studied, especially among men, in the literature on economic mobility.[69] By contrast, the second channel is almost entirely unexplored, while the third channel is largely unexplored. We suspect that the second and third channels have received little attention because, as long as the IGE$_g$ is employed, they cannot be examined without running squarely into the zeros problem. If one wished, for example, to estimate the elasticity of

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[69] We have earlier reviewed this literature in the section titled "The Current State of the Evidence."

earnings from spouses (with respect to the childrens' parental income), the IGE$_g$ will be inadequate to the task insofar as the sample includes children who, as adults, are single (at the time when income measurements are taken). This is because singles will, by definition, have zero earnings from their (nonexistent) spouses.

Can this problem be solved by restricting the sample to those who are married (and thus addressing the third channel only)? Insofar as one resorts to the IGE$_g$, the latter approach is again undermined by the zeros problem, as often spouses contribute no income. The relatively few studies that *have* examined the third channel have either (a) excluded offspring whose spouses have no earnings (e.g., Raaum et al. 2007), or (b) retained these offspring by resorting to an indirect method of estimation (Chadwick and Solon 2002) that does not address, and cannot solve, the underlying zeros problem.[70]

These problems can, however, be elegantly solved by turning to the IGE$_e$. Because it can accommodate children with zero earnings or income, it can be used unproblematically (a) to study the first and third channels, and (b) to examine the joint role of the second and third channels in generating family-income elasticities for women and men. The IGE$_e$ also allows us to specify analytically how own-earnings, spouse's earnings, and marriage-probability elasticities combine to generate total-income elasticities. We turn next to the derivation of the corresponding analytic expressions.

In what follows, we assume that the only source of income is earnings, an assumption that's necessary because nonearned income cannot be allocated between spouses in the SOI-M Panel. This assumption allows us to write the following:

$$E(Y|x) = E(Y_o|x) + E(Y_s|x) \qquad\qquad [8]$$

$$= E(Y_o|x) + \Pr(M|x)\, E(Y_s|x, M), \qquad\qquad [9]$$

where Y is total income, Y_o is own earnings, Y_s is spouse's earnings, and M is a dummy variable indicating whether a child is married or not.[71] Any of the earnings variables may be zero. In the case of Y_s, the variable is zero when (a) the child is single, or (b) the child is married but with a nonworking spouse.

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[70] The indirect method of estimation used by Chadwick and Solon (2002) is problematic because they posit a population regression function that is logically inconsistent with the range of admissible values for the dependent variable in that function. Under their method, the population regression function expresses, for example, the logarithm of wife's earnings as a function of the logarithm of her spouse's parental income. It follows that the left-hand side of the function is undefined when the wife has no earnings. The simple fact that it's undefined is of course not altered because Chadwick and Solon can generate an indirect "estimate" of the key parameter (i.e., the IGE$_g$) in this population function. The population regression function is still improper and the estimand being (supposedly) estimated with the indirect method is an improper object.

[71] $\Pr(M|x)$ stands for $\Pr(M = 1|X = x)$, and $E(Y_s|x, M)$ stands for $E(Y_s|X = x, M = 1)$.

From Equations [8] and [9], and using properties of elasticities of functions, the following two expressions follow:

$$IGE_e(Y|x) = S_x \, IGE_e(Y_o|x) + (1 - S_x)IGE_e(Y_s|x) \tag{10}$$

$$IGE_e(Y|x) = S_x \, IGE_e(Y_o|x) + (1 - S_x)[IGE_e(M|x) + IGE_e(Y_s|x, M)], \tag{11}$$

where $S_x = \frac{E(Y_o|x)}{E(Y_o|x)+E(Y_s|x)}$ is the ratio of expected own earnings to expected total income when parental income is x; $IGE_e(Y|x), IGE_e(Y_o|x)$, and $IGE_e(Y_s|x)$ are, respectively, the IGE$_e$ of total income, own earnings, and spouse's earnings when parental income is x; $IGE_e(Y_s|x, M)$ is the IGE$_e$ of spouse's earnings when parental income is x and the child is married; and $IGE_e(M|x)$ is the elasticity of the child's probability of being married when parental income is x (where all elasticities are with respect to parental income).

Equation [10] indicates that the total-income IGE_e is, for each value of x, a share-weighted average of the own-earnings IGE_e and the spouse's-earnings IGE_e (where each is evaluated at x). This equation thus distinguishes the "direct pathway" (via own earnings) and the "indirect pathway" (via marriage) through which inequality in total income is transmitted across generations. It may be noted that Equation [10] is analogous to Chadwick and Solon's (2002) expression for the IGE$_g$ (see their Equation [5]). There are, however, two very important differences: (a) the elasticities in Equation [10] are not assumed to be constant across levels of parental income, and (b) Equation [10] refers to all children, not to married children only.

If the weights in Equation [11] are ignored, the first term again refers to the elasticity of own-earnings, and the second term is now expressed as the sum of the elasticity of the probability of marriage and the elasticity of spouse's earnings for those who are married. It follows that Equation [11] identifies all three channels through which the transmission of economic status takes place. It reflects the payoff to a higher-income origin that, at any given value of parental income, may take the form of (a) a higher own-earnings, (b) a higher marriage probability, and (c) a higher spouse's earnings (when married). The strength of these three contributions to the total-income IGE$_e$ depends on weights determined by the ratio of expected own-earnings to expected total income (again at a given value of parental income).

The next step is to convert Equations [10] and [11], each of which pertains to the elasticity at a given point (in the parental-income distribution), into expressions for the global elasticity. Because the global IGE$_e$ is just the expected IGE$_e$ across all values of parental income, we proceed by taking expectation in Equation [10]:

$$E\big(IGE_e(Y|x)\big) = E(S_x)E\big(IGE_e(Y_o|x)\big) + Cov\big(S_x, IGE_e(Y_o|x)\big) +$$

$$+(1 - E(S_x)\,E\big(IGE_e(Y_s|x)\big) + Cov\big((1 - S_x), IGE_e(Y_s|x)\big).$$

Using properties of the covariance, this expression can be rewritten as:

$$E\big(IGE_e(Y|x)\big) = [E(S_x)E\big(IGE_e(Y_o|x)\big) + (1 - E(S_x)\,E\big(IGE_e(Y_s|x)\big))] +$$

$$+ Cov\,(S_x, IGE_e(Y_o|x) - IGE_e(Y_s|x)\,). \qquad [12]$$

By further using $IGE_e(Y_s|x) = IGE_e(M|x) + IGE_e(Y_s|x, M)$ and substituting into Equation [12], we then obtain:

$$E\big(IGE_e(Y|x)\big) = E(S_x)E\big(IGE_e(Y_o|x)\big) + (1 - E(S_x))\big[E(IGE_e(M|x)) + E\big(IGE_e(Y_s|x, M)\big)\big] +$$

$$+ Cov\,\big(S_x, IGE_e(Y_o|x) - IGE_e(M|x) - IGE_e(Y_s|x, M)\big). \qquad [13]$$

The global income IGE$_e$ is, as indicated by Equation [12], the sum of two quantities. The first quantity is equal to the weighted average of the global IGE$_e$ of own earnings and spouse's earnings. This expression is similar in structure to Equation [10]. The second quantity is the covariance between the own-earnings share and the difference between the two earnings elasticities. If the own-earnings share is positively correlated with the difference between the two elasticities (across values of parental income), the total-income global IGE$_e$ will be larger. The final equation (i.e., Equation [13]) further clarifies how the global elasticities of the marriage probability and of the spouse's earnings (conditional on marriage) contribute to the total-income global elasticity.

The foregoing results allow us to better understand gender differences in the transmission of economic status across generations. If the total-income elasticity is similar for women and men, we can use Equations [12] and [13] to examine whether that similarity nonetheless conceals differences in the way in which it is generated. If the total-income elasticity instead differs by gender, these equations allow us to specify whether that difference is attributable to gender differences in the direct or indirect forms of transmission.

Other Methodological Issues

We conclude this section by covering some of the most important remaining methodological decisions behind our estimates. We discuss (a) the construction of sampling weights and the rationale for using them, (b) the case for estimating IGEs for men and women separately, and (c) the advantages of estimating earnings elasticities with respect to family income.

Sampling weights: Throughout our analyses, sampling weights will be applied. As discussed above, the SOI Family Panel is based on a stratified random sample of 1987 tax returns, with a sampling probability that increases with income. The weights 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.[72] The latter estimates are computed separately for each sampling stratum.

We do not wish to imply that weights should always be used in model estimation (e.g., Winship and Radbill 1994). The decision on weights is partly an empirical one: 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" (Pfeffermann and Sverchkov 2009, p. 455). 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).[73]

Gender-specific models: We will present IGEs separately for men and women throughout our report. This approach is consistent with our objective of measuring "gross association" because, for all practical purposes, gender is exogenous to parental income (in the U.S.). If instead men and women were pooled, we would run the risk of conflating gender-based advantage with parental-income advantage. This possible conflation is especially relevant for earnings IGEs given that employment rates differ by gender and parental income. Moreover, because economic advantage may be transmitted through partly different mechanisms for sons and daughters (as discussed in the preceding section), the size of income IGEs may vary by gender.

Family income and earnings elasticities: In the analyses presented here, we estimate the elasticity of men's and women's earnings with respect to parental disposable income, even though this elasticity is more commonly estimated with respect to fathers' earnings. As argued by Corak (2006:54) and Mazumder (2005:250), there are three reasons why it is preferable to estimate the earnings elasticity with respect to a measure of family income: (a) it incorporates the income of mothers and thus better indexes the full complement of economic resources available to invest in children; (b) it reflects the ability of families to draw on income sources other than earnings in response to transitory earnings shocks; and (c) it avoids any selection bias that may result from omitting children with absent fathers (as they are likely to be comparatively

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[72] The "qualifying returns" include the 1987 returns of the SOI Family Panel and the returns from the refreshment segment of the OTA Panel. When estimating the probabilities in (c), returns from the refreshment segment of the OTA Panel are treated as belonging to a separate sampling stratum.

[73] 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.

disadvantaged).[74] It is hardly novel to use a measure of family income instead of father's earnings. The literature in fact includes several studies that estimate the elasticity of children's earnings with respect to parental income (Behrman and Taubman 1990; Chadwick and Solon 2002; Levine and Mazumder 2002; Mazumder 2005).

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[74] This is one of those rare cases in which virtue and necessity coincide, as we do not have data on father's earnings.

Results

The balance of this report lays out our findings. The main rationale for this study, it may be recalled, is to assess the level of intergenerational persistence once (a) the most important biases are addressed, and (b) estimators of the right estimands are employed. The SOI-M Panel allows us to provide new evidence on intergenerational persistence in the U.S. that incorporates these methodological improvements.

The first set of analyses will provide estimates of the key elasticities in the U.S. after selection, lifecycle, attenuation, and functional-form bias are reduced to the extent possible. We will begin by presenting estimates of the global elasticities pertaining to the full parental income distribution and then examine the effects of nonfiling and the "zeroes problem" on these elasticities. We next examine (a) differences in persistence across regions of the parental income distribution, and (b) the persistence of economic differences among children born into families far apart in the parental income distribution.

The foregoing analyses will make it clear that economic persistence in the U.S. is at the higher end of the current range of estimates. We then ask whether this conclusion, which rests on analyses of total income and earnings, needs to be modified when the tax system is taken into account. We take on this question by examining whether disposable-income IGEs tell a different story about the intergenerational transmission of economic status in the United States. The results will reveal that, after adjusting for federal taxes and transfers, elasticities in the U.S. are almost unchanged.

The third set of analyses examines the role of gender and marriage in generating intergenerational persistence. We describe the structure of gender differences in persistence, the sources of those differences, and the way in which earnings, marriage, and gender interact to generate total-income elasticities. The key conclusion of this section is that, while the global IGE of total income is nearly as large for women as for men, the processes through which it is generated differ markedly across genders. We will show that marriage plays a much more prominent role for women in explaining why children born into higher-income families have higher incomes themselves.

Global Elasticities

We begin, then, by considering summary measures of the extent of intergenerational persistence. As discussed earlier, there is considerable uncertainty on the matter of just how large U.S. intergenerational elasticities are, with some administrative-data estimates of income or earnings elasticities coming in as high as 0.6 (Mazumder 2005) and others coming in as low as 0.34 (Chetty et al. 2014a). This is a wide range of estimates. We seek to narrow it here by offering new estimates that (a) are based on estimators that deliver the correct elasticity, and (b) are affected to the smallest possible extent by selection, lifecycle, and attenuation biases.

We present our first set of global IGE$_e$ estimates in Table 6.[75] The columns of Table 6 provide three sets of estimates based on the models of Equations [2], [3], and [4] introduced in the preceding section. The first column of Table 6 pertains to the constant-elasticity (CE) estimates; the next column pertains to the spline estimates; and the third column pertains to the nonparametric estimates. In addition to reporting point estimates, each column also provides lower and upper bounds for those point estimates.[76] The last column in the table has the p-values pertaining to the test of constant elasticity (CE) that is obtained by comparing our spline specification (see Equation [3]) with a model that constrains the elasticity to be invariant across levels of parental income (see Equation [2]).

Which of the estimates presented in Table 6 best represent the level of earnings and income persistence? In addressing this question, it is useful to first ask whether the constant-elasticity (CE) assumption can be rejected, as the CE estimates are all substantially lower than those that do not assume CE. The results of the CE tests are in fact straightforward: We find that the CE assumption is rejected in all four cases. The estimates based on the CE assumption are downwardly biased by about 10 percent for the men's estimates and by less than 5 percent for the women's total-income estimates.

The estimates from the remaining models that do *not* assume CE are all quite similar in size and imply an IGE at the high end of existing estimates of economic persistence. For men, the spline and nonparametric models show income and (full-sample) 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).

As Table 6 reveals, our estimates are high in part because they are not affected by functional-form bias, which appears to have reduced estimates by as much as 10 percent. Is there reason to believe that selection bias is also downwardly biasing conventional estimates of the intergenerational elasticity? We can assess the role of selection bias for men's earnings by comparing our IGE$_e$ estimates for the full sample with those obtained after low earners and nonearners are excised. After such excising, the elasticity falls from 0.49 to 0.41 with the CE model, from 0.54 to 0.46 with the spline model, and from 0.56 to 0.47 with the nonparametric model. We will further discuss these results subsequently (when Tables 12 and 13 are presented).

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[75] The global 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 (e.g., Chadwick and Solon 2002:335). We present a detailed analysis of this elasticity in a subsequent section.

[76] Throughout this report, confidence intervals and inferential statements are based on a 95 percent significance level.

It suffices for now to conclude that, when low earners and nonearners are dropped, our estimates of the earnings IGE$_e$ decline in size, a result that suggests that selection bias is at work.[77]

We next consider whether our results are affected by our decision to omit the usual controls for parental age. In Table 7, we again present estimates from the constant elasticity and spline models (see Equations [2] and [3] 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.

We argued earlier that, because parental age is endogenous, the estimates of Table 6 are preferred to those of Table 7. At the same time, some readers might counter that our concerns with endogeneity are trumped by the need to include parental-age variables that adjust for errors in measuring lifetime income, an argument that would lead one to privilege the estimates from Table 7. It is reassuring that the position one takes on this issue does not much matter. Because the global IGE$_e$ estimates in Table 7 are only slightly smaller than the corresponding estimates in Table 6, the effect of controlling for parental age would appear to be rather minor. The estimates in Table 7 are also consistent with other key conclusions reached on the basis of the estimates in Table 6. Most importantly, the constant-elasticity assumption is still rejected, and the estimates for women are still lower than the corresponding ones for men.

From the results discussed so far, we can conclude that (a) the key global elasticities are about 0.5 and may well be larger (given that our estimates may still be affected by residual lifecycle and attenuation bias), (b) the constant-elasticity assumption has the effect of reducing estimates of the global IGE$_e$, and (c) the downward selection bias resulting from dropping children with low or no earnings is also substantial. If all forms of bias are addressed, we accordingly yield estimates of economic persistence that are consistent with those at the very high end of existing survey estimates. In the case of men's earnings, they 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.[78] It is especially noteworthy that our

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[77] We do not conduct here a similar exercise for total income. It is difficult to carry out such an exercise because, before we mean-impute the income of nonfilers, the weighted share of our sample without any reported income is significantly larger than its true share in the population (as there are many nonfilers who have positive income). Moreover, after mean-imputing the income of nonfilers, that share is then too low (i.e., close to zero), a direct consequence of our resort to mean imputation. It follows that we cannot legitimately gauge the effects of selection bias by simply comparing estimates from samples in which nonfilers are either dropped or retained. The following section addresses the related topic of selection bias in the estimation of IGEs when nonfilers are dropped from tax data.

[78] Of course, both the studies based on survey data and Mazumder (2005) measure persistence with the IGE$_g$, not the IGE$_e$.

income estimates of about 0.5 are substantially larger than the tax-data estimate of 0.34 (for the IGE_e) that Chetty et al. (2014a) report for men and women pooled.[79]

Why do our estimates differ from those of Chetty et al. (2014a)?[80] There are good reasons to believe that our estimates are higher because of (a) differential attenuation bias (i.e., the effect of measuring parental income with 9 years of information instead of 5 years), (b) differential lifecycle bias (i.e., the effect of measuring children's income when they are 35-38 years old rather than 29-32 years old), and (c) functional-form bias (i.e., the effect of assuming constant elasticity). We can evaluate this argument by estimating a new round of models in which the biases are systematically introduced. If indeed they account for the difference between our estimates, this introduction of them should have the effect of driving the elasticity down to a value similar to that reported by Chetty et al. (2014a).

We carry out this analysis after pooling the male and female samples because doing so allows us to match the pooled estimate available from Chetty et al. (2014a). As shown in Table 8, we begin the reanalysis by reestimating the total-income elasticity with our nonparametric and constant-elasticity models, but still using our late-30s sample and our 9-year measure of parental income. Next, we reestimate it assuming constant elasticity, but now with data for 2004 (when the children in the SOI-M Panel were 29-32 years old) and with a 5-year measure of parental income (with the parental measurements taken when the children were 15-19 years old).

The results of Table 8 show that our nonparametric estimate of the IGE_e is 0.50 when men and women are pooled. As indicated in the second row of Table 8, the estimate shrinks by 8 percent, to 0.46, when constant-elasticity is assumed. When we next switch to the 2004 sample and compute parental income with 5 years of information, the estimate falls to 0.37, which is close to the estimate of 0.34 reported by Chetty et al. (2014a). It follows that, after we introduce attenuation bias, lifecycle bias, and functional-form bias, approximately one fifth of the initial difference remains.[81] This residual difference is most likely due to sampling variability, the discrepancy in periods (2004 versus 2011-2012), the discrepancy in cohorts (1972-1975 versus 1980-1982), and a myriad of small differences pertaining to the way in which variables are computed, the samples are defined, and other methodological decisions.

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[79]This estimate can be found in Section C of Chetty et al.'s (2014a) Online Appendix (a section that was written as a response to an earlier draft of our report). We refer to their estimates of the IGE_g in the next section.

[80] We thank Raj Chetty for stressing to us the importance of examining the source of the difference.

[81] The original difference is 0.50 – 0.34 = 0.16, and the final difference is 0.03, which is 22 percent of the original difference (when the computation is carried out with three significant digits).

Nonfilers and Nonearners

We noted in our review of the literature that IGEs have not been widely estimated in prior analyses of tax data. Why have scholars turned away from what has been the workhorse measure of intergenerational persistence? This decision has been mainly driven by a straightforward methodological problem: The IGE is difficult to estimate with tax data given that (a) a large number of children have missing income data (because income information from earnings reports and other administrative sources is not available for many nonfilers), and (b) the IGE estimates are very sensitive to assumptions about the income of those with missing income data (see Chetty et al. 2014a).

We turn now to a careful examination of this "nonfilers problem" within the context of the SOI-M Panel. This will not be some minor methodological detour. The nonfilers problem is in fact very important because the SOI-M Panel is as much affected by missing income as Chetty et al.'s (2014a) tax data. As reported in Table 4, 7.1 percent of the children in our late-30s sample are missing income data, even after we supplement our tax data with information from other administrative sources.[82]

In the vast majority of cases, those without income information can be assumed to have *some* income, and Chetty et al. (2014a) have shown that IGE_g estimates are very sensitive to assumptions about exactly how much income they have. We proceed here by replicating Chetty et al.'s (2014a) sensitivity analyses for the constant-elasticity IGE_g and then extending them to nonparametric estimates and to a larger set of imputation strategies. Additionally, we will assess whether excluding nonfilers without administrative information is an acceptable expedient, given our concern that it may generate selection bias. We are worried that, because of the strong negative correlation between parental income and missing income data (for children), the true elasticity will be underestimated when nonfilers are excluded.[83] We seek to examine here the extent to which selection bias of this sort is likely in play. Throughout our analyses, we will compare the performance of the IGE_g to that of the IGE_e, with the objective of assessing whether the IGE_e is also affected by the problems affecting the IGE_g.

We start by assessing the sensitivity of our estimates of the IGE_e and IGE_g to different treatments of nonfilers. We address the sensitivity of the total-income elasticity by presenting relevant estimates of the global IGE_e and IGE_g obtained with the constant-elasticity (see Table 9) and nonparametric (see Table 10) models. In both tables, the first two rows of each panel show estimates for samples including children that (a) filed a tax return, or (b) either filed a tax return or had available other administrative information on income. The remaining 10 rows show

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[82] We refer to these other administrative sources in the section titled "The SOI-M Panel."

[83] For evidence on that correlation, see Chetty et al. (2014a, Figure Ib). The same strong correlation is found in the SOI-M data (analysis not presented here).

estimates in which all children can be retained in the sample by virtue of carrying out imputations. The first five rows pertain to estimates based on different constant-income imputations for those without any administrative information. These are imputations of a range of values deemed plausible (rather than mean imputations based on external information).[84] The last five rows pertain to our favored approach in which we carry out mean imputations by gender and age (using CPS data).[85]

The main complication with CPS mean imputations is that approximately one-third of CPS likely nonfilers without earnings or UI income have zero income. When the estimand is the IGE_e, zero incomes are unproblematic, as means can be computed without difficulty. However, when the estimand is the IGE_g, it is necessary to impute the mean of the logarithm of income (rather than mean income), which means that the CPS nonfilers without income pose a problem. The estimates for the first row in this group, where we simply use all values reported in the CPS (including zeroes), are accordingly only available when we estimate the IGE_e. The balance of the estimates come from imputing different amounts (in the $1-$3,000 range) to the CPS nonfilers with zero reported income (under the alternative assumption that their true income is low but not zero).[86]

The volatility of the IGE_g estimates in Tables 9 and 10 is striking. When those without administrative information are retained and assigned a low income amount in the $1-$3,000 range, the global IGE_g estimates for men range from 0.53 to 1.09 (constant-elasticity model) and from 0.59 to 1.13 (nonparametric model), while the corresponding estimates for women range from 0.46 to 1.03 (constant-elasticity model) and from 0.54 to 1.05 (nonparametric model). Even with CPS-based mean imputation, the men's estimates range from 0.51 to 0.73 (constant-elasticity model) and from 0.57 to 0.78 (nonparametric model), while the corresponding estimates for women range from 0.43 to 0.61 (constant-elasticity model) and from 0.52 to 0.67 (nonparametric model). The men's estimates fall to 0.43 (constant-elasticity model) and 0.49 (nonparametric model) when those without any administrative information are excluded, and they fall to 0.40 (constant-elasticity model) and 0.44 (nonparametric model) when all nonfilers are excluded. For women, the estimates fall to 0.36 (constant-elasticity model) and 0.41 (nonparametric model) when those without any information are excluded, and they fall to 0.33 (constant-elasticity model) and 0.38 (nonparametric model) when all nonfilers are excluded.

By contrast, the global IGE_e estimates are very robust, at least when nonfilers without any administrative information are retained. The constant-elasticity estimates range from 0.47 to

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[84] This is the approach taken by Chetty et al. (2014a). 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), 0.62 (recoding $0 to $1), and 0.41 (recoding $0 to $1,000).

[85] For details on our mean imputation, see the section titled "The SOI-M Panel."

[86] The income IGE_e results reported in the previous section correspond to the estimates in the 8th row of each panel.

0.49 for men and from 0.45 to 0.46 for women, and the corresponding nonparametric estimates range from 0.52 to 0.54 for men and from 0.47 to 0.49 for women. Moreover, with the methodologically superior mean-imputation approach, estimates are invariant (up to the second digit) across the various assumptions about the true income of CPS nonfilers with zero income. When we exclude all nonfilers or all those without administrative information, the estimates do fall but by much less than is the case with the IGE_g estimates.

We can therefore conclude that excluding nonfilers or excluding those without any administrative information generates substantial downward biases with both the IGE_e and the IGE_g. We can also conclude that the bias is larger with the latter.[87] If children without any administrative information are retained and imputed a constant income, the global IGE_g estimates are very sensitive to the exact value that is imputed. The IGE_g estimates are less volatile with mean imputation, but they are still sensitive enough to be almost useless. By contrast, estimates of the global IGE_e are very robust across all imputation approaches, and they are especially robust when mean imputation is employed.[88]

What do these results imply for Chetty et al.'s (2014a) estimate of 0.34 for the constant-elasticity IGE_g? The results reported in the preceding section (for the IGE_e) suggest that this estimate may have been driven downward by attenuation bias, lifecycle bias, and functional-form bias. The results from Tables 9 and 10 also suggest that, because the estimate was secured by dropping those without any administrative information, it is likely driven further downward by selection bias as well.[89]

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[87] Given the assumptions of this analysis, the best estimates of bias for the IGE_e are obtained by subtracting, within each panel, the value in the first or second row from the value in the eighth row. For the IGE_g, upper-bound estimates can be obtained by subtracting, within each panel, the value in the first or second row from the value in the ninth row. The corresponding lower-bound estimates rely on the values in the twelfth row. We are of course assuming, by virtue of using mean imputation by gender and age, that the income of those without any administrative information is independent of parental income (within each age-gender group).

[88] These conclusions are also relevant for the estimation of family-income IGEs with survey data. Although there isn't a literal nonfiler problem in the survey context, our conclusions are nevertheless informative because (a) parental income and the probability of institutionalization (e.g., imprisonment) are negatively correlated, and (b) institutionalized persons are typically not covered by surveys. This selection bias should therefore drive estimates downward. Likewise, there are of course many cases with no reported income in survey data, and analysts must therefore decide whether to drop them (thus likely generating selection bias) or assign them an arbitrary income value (thus leading to estimates that are sensitive to the value chosen). The latter problem effectively disappears if the IGE_e is estimated instead of the IGE_g. Among survey analysts, it is also common to rely on samples of household heads and their spouses or partners, practices that can be expected to generate selection bias as well.

[89] This estimate is reported prominently in Chetty et al.'s (2014a) Figure 1 and is used as the main reference point when carrying out comparisons with Mazumder (2005) and Clark (2014). At the same time, Chetty et al. (2014a) point out that (a) estimates are "sensitive to the point of measurement in the income distribution," and (b) "the log-log specification discards observations with zero income," which "overstates the degree of intergenerational mobility" (Chetty et al. 2014a, p. 1573). That is, Chetty et al. (2014a) also refer to one of the four biases we discuss (selection bias), and they further recognize that the assumption of constant-elasticity may be problematic (although they do not explicitly refer to "functional-form bias"). Although Chetty et al. (2014a) thus appreciate the possible effects of two types of bias, they are quite insistent that neither lifecycle nor attenuation bias are in play with their data.

We can provide direct evidence on these hypotheses by reestimating the IGE_g after imposing all four types of bias. We proceed much like we did in the preceding section, but now we address the effects of selection bias as well as the other three biases. In Table 11, we present estimates of the total-income IGE_g, again pooling men and women. The first three rows pertain to estimates with our late-30s sample and our 9-year measure of parental income. The values reported in the first row, 0.55 and 0.74, are lower- and upper-bound nonparametric estimates corresponding to the treatments in the 12th and 9th rows of Table 10. The next two rows show that estimates fall to 0.45 when we drop children without administrative information (i.e., introduce selection bias), and then to 0.39 when we impose constant elasticity (i.e., introduce functional-form bias). Next, we use data for 2004 (when the SOI-M children were 29-32 years old) and a 5-year measure of parental income, thus mimicking Chetty et al.'s (2014a) analysis to the extent possible with our data. We reduce the estimate by approximately 30 percent, to 0.28, when lifecycle and attenuation bias are imposed in this way. The resulting "final estimate," which reflects the effects of all four types of bias, is now somewhat lower than that reported by Chetty et al. (2014a). As with the previous analysis, this difference is likely a result of sampling variability, the discrepancy in periods and cohorts, and a myriad of small methodological differences between the two studies.

We next consider the implications of selection bias for the earnings IGEs. With earnings, there isn't a nonfiler problem, but there is a "zeros problem" when the IGE_g is estimated.[90] This problem is typically addressed by dropping nonearners (and low-earners are conventionally dropped as well). However, because parental income is inversely related to the probability of having zero earnings, this convention may result in downward selection bias. The proportion of men with zero W-2 earnings is 0.33 in the bottom quintile of parental income, 0.20 in the second quintile, 0.15 in each of the next two quintiles, and 0.13 in the top quintile. We turn here to evaluating in more detail whether dropping nonearners (when estimating the IGE_g) indeed leads to a downward selection bias. We also investigate whether the potential alternative approach of assigning them some arbitrary low value results in fragile estimates (as was the case in the analysis of nonfilers).

In the prior subsection, we used the difference between estimates with and without nonearners to weigh in on the selection bias hypothesis, an approach that assumes that those without W-2 reported earnings necessarily have zero earnings. Although this assumption is surely false (mainly because some people without W-2 earnings receive earnings from work in the informal economy), it is likely a good first-order approximation. We now supplement that analysis by assuming that the expected earnings of those with zero W-2 earnings are not zero but are low (i.e., between $0 or $1 and $3,000). This analysis is based on the further assumption that expected earnings are independent of parental income (among those without reported earnings).

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[90] See the subsection titled "Selection and the 'Zeros Problem.'"

The results for the constant-elasticity model are presented in Table 12, and the results for the nonparametric model are presented in Table 13. The rows in these tables refer to different sample inclusion rules, such as excluding children without reported earnings, excluding nonearners and earners below five different earnings thresholds, and retaining nonearners in the sample and imputing mean earnings (for the IGE_e) or mean log earnings (for the IGE_g) for them. Mean earnings are assumed to be in the range \$0-\$3,000.[91]

When nonearners are retained, the estimates for the IGE_g vary substantially with the imputed mean log earnings. The estimates range from 0.44 to 1.07 (constant-elasticity model) and from 0.54 to 1.16 (nonparametric model). Moreover, when nonearners are excluded, the estimates fall to as low as 0.31 (constant-elasticity model) and 0.37 (nonparametric model). Because the estimates vary so much, one would be hard-pressed to specify exactly how much selection bias there is, although the difference between estimates including and excluding nonearners is always large.[92]

The estimates of the global IGE_e, by contrast, are very robust. If nonearners are retained, the estimates range only trivially under the various assumptions about the mean earnings of those without reported earnings. The estimates do fall when nonearners are excluded, but typically less so than with estimates of the IGE_g. Because there is much less variation than with the IGE_g, we can weigh in more confidently about the extent of selection bias, as indeed we did in the preceding section.

The foregoing results thus strongly suggest that excising nonearners produces a downward bias in the estimates for both estimands. This bias is much larger, however, when the estimand is the global IGE_g. Moreover, estimates of the global IGE_g are sensitive to the values imputed to nonearners (when they are retained), indeed so sensitive that those estimates are of very little value. Finally, estimates of the global IGE_e are robust to the values imputed to those with zero W-2 earnings, which 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.[93]

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[91] In the previous section, we reported estimates of the earnings IGE_e with nonearners retained. These estimates can be found in the 6th row of each table.

[92] The implied lower-bound estimates of selection bias in Tables 11 and 12 should be understood as conservative for the IGE_g. This is because $\ln E(Y) \geq E(\ln Y)$, with the equality obtaining only when Y is constant across people. By imputing, for example, a mean value of \$3,000 (for the IGE_e) and a mean log value of $\log 3,000$ (for the IGE_g), we are imputing the largest possible mean log value given our assumption about the mean value in the same row. Because the IGE_g estimates increase when the imputed values decrease, this makes the lower-bound estimates of selection bias for the IGE_g as small as possible.

[93] The results of this analysis are again relevant for research based on survey data. Although the proportion of respondents without reported earnings is smaller in surveys than in administrative sources (as surveys are more likely to include informal-economy earnings), there are nonetheless many such respondents. Moreover, as we pointed out earlier, the institutionalized population is typically excluded from samples, and some survey analyses rely on samples of household heads and their spouses or partners. It follows that implicit low-earners or nonearners are often dropped (because they are not household heads).

This set of results also makes it clear that there is no longer a *practical* rationale for turning away from the IGE. It suffices to simply estimate the IGE_e instead of the IGE_g, not just because the IGE_e is consistent with the long-standing intent of mobility scholars, but also because it is not affected by the zeros problem. We of course have no objection to the rank-rank slope deployed by Chetty et al. (2014a) in his analyses. It is clearly a very useful measure. However, it should be used when the question at hand requires estimating that measure, not because it happens to sidestep the zeros problem.

Region-Specific Elasticities

Although we have showed that the constant-elasticity assumption is consistently rejected, we have not examined the shape of the curves relating children's expected earnings and income to parental income (in log-log space). Do the departures from constant elasticity take on a pattern that is of substantive interest? We address this question next by examining (a) the pattern of predicted values from the nonparametric regressions, and (b) our estimates of region-specific elasticities defined by the 10th, 50th, and 90th percentiles of parental income.

We begin by examining predicted values (i.e., conditional expectations) from the nonparametric regressions for men's earnings, men's income, and women's income (see Figures 3, 4, and 5). The blue dots and grey segments in the figures represent point estimates and confidence intervals corresponding to equidistant quantiles of parental income.[94] The vertical dotted lines correspond to the 10th, 50th, and 90th percentiles of parental income (denoted here as P10, P50, and P90).

In Tables 14 and 15, our estimates based on the spline and nonparametric models are presented, using the four region-specific IGEs defined by these three percentiles of parental income (i.e., below P10, between P10 and P50, between P50 and P90, and above P90). The tables also include p-values from the bootstrap test of the null hypothesis that the expected IGE_e in the P50-P90 region is not larger than in the P10-P50 region. We refer to this as the non-convexity hypothesis.[95]

The figures reveal that conditional expectations are estimated quite imprecisely in the tails of the curves and somewhat more precisely in the interior of the curves (i.e., between P10 and P90). Even so, there are relatively wide confidence intervals for the interior region-specific estimates, as shown in Tables 14 and 15.[96] This imprecision means of course that appearances

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[94] In these and all other similar figures, the dots correspond to the quantiles at the extremes of the segments used in the numerical computation of the global and region-specific IGEs (i.e., quantiles 1, 1.5, 2,…, 98, 98.5, 99).

[95] The intervals reported in Tables 14 and 15 cannot be used to test this hypothesis because estimates across regions are correlated within bootstrap samples.

[96] The imprecision tends to be more prominent for the estimates from the nonparametric model than from the spline model.

(as revealed in the figures) can be deceiving and that a careful analysis of the evidence is especially important.

We start our analysis by examining the nonparametric income curves. For men, the curve in Figure 4 takes on a relatively simple S-shaped form, with the slope of the curve (i.e., the IGE_e) first increasing and then decreasing. On the left side of the curve, the estimated region-specific elasticities are comparatively low, with point estimates of 0.32 for the below-P10 region and 0.43 for the P10-P50 region (see Table 15). 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 estimates are imprecise and therefore must be treated cautiously, it is nonetheless striking that, according to the point estimate, a full two-thirds of the percent differences in parental income within that "middle to upper-middle-class" region persist into the next generation.[97] Moreover, we are confident of our characterization of convexity in the interior of the curve (where the bulk of cases is found), as we can reject the non-convexity hypothesis with a p-value of 0.027. At the top decile of parental income, the curve appears to flatten (relative to that of the preceding region), but here the IGE_e estimate is particularly imprecise (see Table 15).

It is reassuring that the same S-shaped pattern emerges in the results from the spline model. The point estimates are, save for one exception (the below-P10 region), very similar to those obtained with our nonparametric approach. With the spline model, there is also a large gain in precision for the above-P90 estimate, while the hypothesis of non-convexity is again rejected (see Table 14). These results strengthen our confidence in our region-specific estimates above the 10th percentile and suggest that the apparent flattening in the last region is indeed a feature of the population curve.

The seeming discrepancy in the below-P10 region is not necessarily troubling. When we examined the cases in the below-P10 region, we found that the spline model is affected by a small number of outliers in that region. 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.[98]

The shape of the women's nonparametric curve, which is shown in Figure 5, is very similar to that for men. Because the left tail appears less flat, the overall curve is closer to being

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[97] We are of course referring here to the expectation across levels of parental income within that region (a caveat that will be assumed from hereon in). For more methodological details, see the subsection titled "Summary Measures of Persistence in the Nonlinear Context."

[98] There are four very influential outliers at the bottom of the distribution. When we drop them, the region-specific elasticities from the spline model are 0.34 (0.07-0.68), 0.42 (0.29-0.55), 0.70 (0.51-0.91), and 0.37 (0.26-0.47), while those from the nonparametric model are 0.38 (0.23-0.55), 0.44 (0.33-0.55), 0.68 (0.48-0.90), and 0.41 (0.07-0.81).

linear, but otherwise the results are much the same (see Table 15). For this curve, 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 men, the point estimate for the P50-P90 region is especially large (0.63), indeed it implies that almost two-thirds of parental income differences in that region persist into the next generation.

The IGE_e estimates from the spline model for the two key central regions are likewise consistent with those from the nonparametric model (in fact they are identical). In this case, the hypothesis of non-convexity is more definitively rejected, with a p-value of 0.013. For the above-P90 region, the IGE_e estimate is 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.[99]

The nonparametric curve for men's earnings, presented in Figure 3, suggests more prominent convexity than emerged in the income curves. The left tail is flatter, and the slope of the curve continues to increase steadily up to the 97.5th percentile of parental income. However, because the conditional expectations in the upper tail are so imprecisely estimated, the estimate for the above-P90 region is uninformative (see Table 15). The estimate for the P50-P90 region again implies that approximately two-thirds of percent differences in parental income persist into the next generation. In this case, the point estimate for the P10-P50 region is only slightly lower (0.56), and the hypothesis of non-convexity cannot be rejected. There is, however, a gain in precision with the spline model (Table 14). 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 in that region.[100]

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[99] The difference in below-P10 estimates across models again arises because the spline model is especially affected by two influential outliers. When those outliers are dropped, the nonparametric and spline models deliver almost identical estimates, 0.53 and 0.52 respectively, for that region. Without the outliers, the region-specific elasticities from the spline model are 0.52 (0.27-0.80), 0.31 (0.18-0.44), 0.64 (0.49-0.80), and 0.42 (0.33-0.52), while those from the nonparametric model are 0.53 (0.31-0.75), 0.36 (0.20-0.52), 0.63 (0.40-0.87), and 0.25 (-0.27-0.68).

[100] The cross-model difference in below-P10 estimates is reduced somewhat by dropping outliers. After eliminating four very influential outliers, the region-specific elasticities from the nonparametric model are 0.31 (0.03-0.63), 0.56 (0.39-0.73), 0.62 (0.41-0.82), and 0.68 (0.22-1.05), while those from the spline model are 0.19 (-0.10-0.64), 0.48 (0.34-0.63), 0.76 (0.59-0.93), and 0.34 (0.25-0.46).

The men's earnings curve is thus characterized by convexity over the bulk of the parental income distribution and then flattening at the top.[101] Although such convexity has been found in other countries, some scholars have claimed that, in the U.S., the earnings curve is close to linear (in log-log space). The results presented here suggest that this is not the case.[102]

Persistence among "Far-Apart" Families

The discussion up to now has focused on point elasticities. Although these have been the central focus of the literature, they cannot be used to examine the extent to which inequalities between families with large differences in incomes are preserved into the next generation. As discussed above, the IGE_e approximates the share of percent income differences (between families) that is preserved into the next generation, but that approximation holds only when the percent differences in question are small. If one wishes to examine economic persistence for families that are not close together, one way to proceed is by turning to the standard arc elasticities introduced in the preceding section.[103]

The main results of interest are provided in Tables 16 and 17. Here, we present our $AIGE_e$ estimates, computed from the conditional-expectation estimates of the spline and nonparametric models.[104] We estimate the $AIGE_e$ for arcs defined by families with income in the 5th-15th percentiles (P5-P15), 45th to 55th percentiles (P45-P55), and 85th to 95th percentiles (P85-95). These families may be referred to as "poor," "median," and "well-off."

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[101] There are several possible accounts for convexity. A popular one, advanced by Bratsberg et al. (2007) for some Scandinavian countries, suggests that convexity arises because (a) all families are borrowing-constrained due to imperfections in capital markets, (b) inherited ability is correlated with parental income, and (c) educational policies and institutions are such that, at lower levels of inherited ability (and thus lower optimal levels of human capital formation), access to education is much more equal. This popular account has not answered Grawe's (2004b, p. 818) challenge that a convex curve is evidence of borrowing constraints only under the assumption that, in the absence of such constraints, the curve would be a (positive-slope) straight line. It should also be noted that Bratsberg et al. (2007) provide but one of many possible accounts of convexity (see Becker et al. 2015 for an example of a very different account).

[102] Although Bratsberg et al. (2007) found evidence of convexity in the Scandinavian countries, they did not find any such evidence in the U.S. The difference between their results and our own (on this point) may be attributed to a variety of factors, including their use of survey data (which does not adequately cover the upper tail of the children's earnings distribution), their use of father's earnings (rather than family income) to measure parental resources, and their use of the IGE_g to measure persistence.

[103] The standard arc elasticity is not the only arc elasticity that can be used to measure the persistence of economic differences among families that are far apart. Most notably, Allen (1934) discussed several other notions of "arc elasticity," while both Holt and Samuelson (1946) and Vázquez (1998) have argued for an arc elasticity in which absolute differences are standardized by dividing by the logarithmic mean (rather than mean) of the larger and smaller value (see, esp., Vazquez 1998). The data in Appendix B, which underlie all nonparametric results in this subsection, can be used to compute arc elasticities other than the ones reported here.

[104] For the definition of the $AIGE_e$, see the subsection titled "Arc Elasticities."

The first column in Tables 16 and 17 pertains to the persistence of economic differences between children born into families with vastly different economic resources (i.e., poor vs. well-off families). We find that this persistence is very substantial: The tables indicate that about two-thirds of the difference in total income between poor and well-off families is found between their sons' expected incomes.[105] The persistence of advantage is equally high for sons' earnings, but slightly lower for women's total income (0.57 under the spline model and 0.60 under the nonparametric model).

It is also instructive to carry out comparisons with median families. Given the convexity of the curves relating parental and children's income, we should expect a higher persistence of income differences between median and well-off families than between poor and median families. This is indeed what we find. While about half of the income differences between median and poor families persist in the expected incomes and earnings of their sons, about two-thirds persist when median and well-off families are compared. The corresponding total-income results for women are two-fifths (for poor and median families) and three-fifths (for median and well-off families).

These results underscore our earlier conclusion that a large share of income differences is transmitted across generations. Although we're again finding evidence of a very high level of persistence, this conclusion could not have been reached with point elasticities, as they're only legitimately used to characterize inequalities between families close together in the income distribution. It also bears noting that some of our arc elasticities imply even more persistence than the point elasticities might be taken to suggest.[106]

Disposable Income Elasticities

We have been unable to locate *any* studies of disposable-income or after-tax IGEs. Although tax data have many virtues, an important one is that they can provide an approximate measure of disposable income (by subtracting net federal taxes from total income).[107] We care about disposable-income IGEs because they speak directly to the persistence of available economic resources. Because the capacity to consume and invest is more directly determined by

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[105] This estimate is computed, it should be recalled, with data from families that are *near* the 10th and 90th percentiles. However, when we instead rely on families *exactly* at those percentiles, the estimates are much the same (i.e., 0.65 for the nonparametric model and 0.66 for the spline model).

[106] The average point elasticity for men's total income between P10 and P90 is about 0.57 under the spline and nonparametric models (see Tables 14 and 15) and about 0.47 under the constant-elasticity model (see Table 6). The estimate of the arc elasticity between P10 and P90 is, by contrast, about 0.65 (see Tables 16 and 17).

[107] It should be recalled that we measure disposable income by subtracting net federal taxes from total income. 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, reflect the Earned Income Tax Credit (EITC) and other refundable credits.

disposable income than total income, we can better understand how that capacity is transmitted across generations by measuring disposable income. We therefore begin this section with the following simple question: Are elasticities for disposable income different than those for total income?

In Figures 6 and 7, the nonparametric curves for men's and women's total income are reproduced, but they are now joined by the corresponding curves for disposable income. The disposable income curves are, relative to the corresponding total-income curves, displaced in two ways: (a) the curves are displaced downward because the child's disposable income is typically less than his or her total income; and (b) the curves are displaced to the left because the parents' disposable income is typically less than their total income. The figures reveal that the first type of displacement is more prominent than the second (except in the left tail of Figure 7).[108]

We care more, however, about the slopes than the foregoing displacements. As Figures 6 and 7 show, the slopes of the total-income and disposable-income curves are quite similar, with the disposable-income slope registering as just slightly less steep.[109] This result holds across all models and for men and women alike: When averaged across models, the global IGE_e is reduced by about 5 percent for men and by about 3 percent for women (see Table 18).[110] We can conclude that the global total-income IGE_e, while slightly higher than its disposable-income counterpart, does not misrepresent in any fundamental way the persistence in economic status across generations.

The comparison between disposable-income and total-income elasticities also provides some evidence on the effects of recent changes in tax policy. If the tax system becomes more progressive between the time when parental and children's income are measured, such a change would compress the children's distribution and, all else equal, generate disposable-income elasticities that are smaller than their total-income counterparts.[111] The relatively recent rise of

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[108] The reason why the downward shift is less prominent in some parts of the curve will become clear below.

[109] In the analyses for this subsection, we dropped six outlier observations (four men and two women) that affected estimation of region-specific elasticities for the below-P10 region (which, for reasons that will become clear, are of central interest here). The decision to drop those observations has very little effect on the global elasticities or on expected values outside the below-P10 region. When these outliers are not dropped, the point estimates reported in Table 17 are, at most, 0.01 lower (across genders and models).

[110] We have 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 = 0.290). For the spline model, it is rejected for both men (p = 0.000) and women (p = 0.001).

[111] It is well known that the constant-elasticity 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 of the children's and parents' log incomes. Although no similar analytic relationship has been established for the constant-elasticity IGE_e or for any IGE when the elasticity is not constant, heuristic arguments suggest that the global and region-specific IGE_e will increase when the children's income distribution becomes more dispersed, when the parents' income distribution becomes less dispersed, and when the "copula" relating these two variables exhibits stronger association. If the tax system becomes more progressive at a point in time after parental income is measured, the disposable-income IGE_e

refundable tax credits suggests just such a result: That is, the EITC and other tax-credit programs were still in their infancy when our SOI-M children were being raised, but they have since taken off and now provide considerable income supplementation (see Berlin 2009).[112] If an EITC effect of this sort were in play, it would reveal itself at the bottom of the parental-income distribution. This is because such tax credits would have compressed the lower tail of the disposable-income distribution for SOI-M children (relative to the lower tail for SOI-M parents).[113]

The first step in assessing this tax-policy hypothesis is to examine the distribution of the EITC across parental-income quintiles. The key question: To what extent is EITC receipt a function of parental income? As shown in Table 19, about 17 percent of all men and 25 percent of all women received EITC in 2010, but these averages indeed conceal a strong relationship with parental income. Notably, only 10 percent of men and 13 percent of women raised in the top income quintile received the credit, whereas better than one quarter of men and one third of women raised in the bottom quintile received it. The percentages for the other quintiles fall predictably between these two extremes.

In Table 20, we examine the relationship between parental income and EITC receipt (in 2010), with the first panel pertaining to EITC receipt among males and the second to females. The first column shows the mean amount received (including those who did not receive the EITC). We find here that, for men and women alike, there is a clear inverse relationship between mean EITC amounts and parental income. The second column, which recalculates the mean after excluding those who did not receive the EITC, reveals that there is no such clear relationship between parental income and the amount received by men. For women, an inverse relationship more clearly obtains, although it's a nonmonotonic one (as those raised in the third quintile of parental income receive more than those raised in the fourth quintile). The relationship is nonetheless clear: The mean amount of credit for those raised in the bottom quintile is 30 percent larger than for those raised in the top quintile.

If we return to Table 19, we find another important gender disparity. Namely, within each parental income quintile, women are more likely than men to receive an EITC credit. Moreover, Table 20 shows that the mean credit is, within each income category, larger for women than for

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should then be smaller than the total-income IGE$_e$ (unless there are compensating changes in the copula). See Chetty et al. (2014b), Chau (2010), and Kano (2009) for examples of how economic mobility can be studied by decomposing a joint distribution into a copula and the marginal distributions.

[112] For households in the bottom quintile, the average federal tax rate was nearly 10 percent in 1987, which is the first year parental income is measured in our sample. By contrast, it was only 1.5 percent in 2010, which is the year children's income is measured in our sample. This decline is mainly attributable to the expansion of the EITC, the expansion of the child tax credit, and the introduction of the Making Work Pay tax credit (Congressional Budget Office 2013, pp. 16-17).

[113] Put differently, because of the EITC and other credits, the tail of the disposable-income curve should appear displaced more to the left than downward (when compared to the total-income curve).

men. If our hypothesis about the persistence-reducing effects of new tax credits is on the mark, we should accordingly expect to find a larger reduction in the lower-tail IGE_e among women than among men.

We are now in a position to consider the implications of these distributional patterns for the persistence of income differences. The main results are presented in Figures 5 and 6 and Tables 21 and 22. The tables show our IGE_e estimates for the below-P10 region, our estimates of the difference between the IGE_e for total and disposable income, and the p-values from the test of the null hypothesis that this difference is not positive. The results suggest that our hypothesis on persistence-reducing effects is on the mark: The notable finding is that the left tail of the disposable-income curve for women is propped up and its slope flattened out. For women, the difference in the IGE_e is estimated to be in the 0.08-0.10 range across models (i.e., 15-20 percent lower with disposable-income than with total income), and the null hypothesis that the difference is not positive is clearly rejected. For men, we also find positive differences, but they are very small and the null hypothesis of no positive difference cannot be rejected with either model. This pattern of results is largely consistent with our expectations and suggests that changes in tax policy have, at least for women, reduced the persistence of income differences at the bottom of the parental-income distribution.

Gender Differences in Earnings Elasticities

To this point, we have presented both income and earnings elasticities for men, but only income elasticities for women. We have held off reporting earnings elasticities for women because, as noted earlier, women's earnings are not considered a meaningful measure of their overall economic status (e.g., Chadwick and Solon 2002:335). In this subsection and the next two ones, we offer a comparative analysis of men's and women's earnings elasticities, an analysis that focuses on some of the gender-specific forces affecting these elasticities. We also discuss the role that the earnings elasticity plays in generating the total-income elasticity for men and women alike.

We have emphasized throughout this report that there is a troubling "evidence deficit" on economic mobility, but nowhere is that deficit more troubling than when one turns to the literature on the earnings mobility of women. In our earlier review of that literature, we noted that most of the research on earnings elasticities has focused on men, and such studies of women as are available have yielded contradictory results. Indeed, some survey-based studies have suggested that earnings elasticities for men and women are approximately the same, whereas others have suggested that either men's or women's is larger. Even in analyses of SSA data, the results have been inconsistent, with Mazumder (2005) reporting similar estimates for men and women and Dahl and DeLeire (2008) reporting substantially higher values for men.

Although the results available from existing studies are contradictory, there are good reasons to expect that the earnings IGEs for women will prove to be lower than the

corresponding IGEs for men when high-quality administrative data are brought to bear. This expectation arises in part because (a) the earnings distribution for women is less dispersed than that for men, while (b) the parental income distribution is essentially the same for women and men. The reduced dispersion in the earnings distribution for women should, all else equal, result in a lower constant-elasticity IGE_g for women than for men. If the linear correlation between children's log earnings and parental log income was much stronger for women than men, that could of course moderate, eliminate, or even reverse the effects of the gender difference in dispersion.[114] But that possibility is inconsistent with the available data. When Chetty et al. (2014a, Table 1) estimated the rank-rank slope (which is closely related to the linear correlation), they found a weaker correlation for women than for men, a result that only compounds the IGE_g-lowering effect of reduced dispersion in the earnings distribution for women.[115] Moreover, because women's earnings are less dispersed than men's, and because the rank-rank slope for women is smaller than for men, the global IGE_e should likewise be lower for women than for men.[116]

There are various, possibly complementary, mechanisms that might explain both the lower dispersion of women's earnings and women's relatively small rank-rank slope. These mechanisms include the following: (a) the very pronounced occupational sex segregation in the labor force may reduce the likelihood that women from high-income backgrounds capitalize on their advantages (e.g., Charles and Grusky 2004); (b) the disproportionate share of domestic duties taken on by women may induce them to opt for part-time or low-paying jobs that do not fully realize their earnings capacity (Polachek 2012); and (c) the joint effect of assortative mating and the negative income elasticity of labor supply may lead to marriages in which women from relatively affluent backgrounds have higher-income partners and choose to work fewer hours (or not at all) when they have young children (Raaum et al. 2007).

The foregoing mechanisms are often assumed to be quite powerful. It is surprising, then, that the previous literature has not consistently reported lower earnings IGEs for women than for men, especially when using SSA data. We revisit the matter here with administrative data that are better suited to estimating earnings IGEs than those used before. The key advantage of the SOI-M Panel in this context is that it allows us to measure parental disposable income (whereas analyses based exclusively on SSA data cannot).[117] As argued earlier, the elasticity of earnings

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[114] See footnote 111.

[115] As Chetty et al. (2014a, Table 1) report, the rank-rank slope is 0.25 for women and 0.31 for men. See Chetty et al. (2014b, p. 1561) for a discussion of the relationship between (a) the rank-rank slope, and (b) the linear correlation between the logarithms of two variables.

[116] This is because the rank-rank slope is a measure of the strength of the association found in the copula (see footnote 111).

[117] Although Mazumder's core analyses with SSA data are based on measures of fathers' earnings, he also conducts supplementary analyses in which he replaces fathers' earnings with a two-year SIPP-based measure of family

should be computed with respect to parental disposable income not just because parental disposable income is a better measure of origin advantages than father's earnings, but also because this approach avoids the selection bias that would likely result from omitting children with absent fathers.[118]

We present our nonparametric estimates of the global earnings IGE_e in Table 23 (for men and women alike). Because a main focus of the analysis is gender differences, we also present the absolute and proportional differences between the men's and women's estimates. The expected values for men and women are additionally presented in Figure 8. The nonparametric curve for men is the same as that appearing in Figure 3, but now that curve is joined by the corresponding curve for women.

We begin by discussing the estimates in Table 23. The results here indicate that the women's IGE_e is, as we anticipated, substantially lower than the men's IGE_e. The global IGE_e for women, estimated at 0.32, is about 43 percent lower than the corresponding estimate for men, 0.56. The null hypothesis of no positive difference is clearly rejected (p=0.000).

Examining Figure 8 next, we unsurprisingly find that the expected earnings of men are higher than those of women, an earnings gap that obtains at each level of parental income. The more important result is that the difference between men's and women earnings, although always present, is not constant in size across levels of parental income. The male "multiplicative advantage" starts large, grows gradually smaller up to about P10, is relatively stable up to about P25, and then increases steadily thereafter (until nearly the highest levels of parental income).[119] By virtue of this pattern of results, the IGE_e for the below-P10 region is larger for women than men, whereas the IGE_e for the P10-P50, P50-P90, and above-P90 regions is larger for men than women. In the latter three cases, the null of no positive difference is rejected almost without exception.[120]

Because other scholars (e.g., Mazumder 2005) haven't found gender differences of this sort, it's important to establish whether the result reliably shows up in our data. We thus assess whether it's robust to differences in estimand, model, and sample-inclusion rules. The results from these assessments, as shown in Table 24, reveal that the women's IGE is indeed always

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income (see Mazumder 2005, Table 9). In these analyses, the estimate of the earnings IGE_g for women, 0.56, is slightly higher than that for men, 0.48.

[118] See the subsection titled "Other Methodological Issues" for further details.

[119] Because Figure 8 is in logarithms, equal distances entail equal multiplicative factors.

[120] When we test the null hypothesis that the difference between the men's and women's IGE_e is not positive, we obtain the following p-values for the spline model: 0.004 (P10-P50), 0.007 (P50-P90), and 0.000 (above-P90). The corresponding p-values for the nonparametric model are 0.001 (P10-P50), 0.126 (P50-P90), and 0.030 (above-P90). The obverse test for the below-P10 elasticities yields a p-value of 0.173 (spline model) and 0.164 (nonparametric model).

lower than the men's IGE. The percentage difference is in all cases large, ranging from 29.7 for the nonparametric IGE_g (after trimming those with earnings below or equal to $3,000) to 44.9 for the full-sample IGE_e under the constant-elasticity model.

Although a gender difference is therefore uniformly in evidence, it's also clear that there is some variability in the extent of this difference. An important result in this regard is that the difference is consistently larger when nonearners are retained in the sample. For example, the full-sample estimate for the nonparametric IGE_e is 42.9 percent smaller for women than for men, whereas the corresponding reductions for the trimmed samples are 36.2 percent (earnings > $600) and 37.8 percent (earnings > $3,000) smaller. It is of course not possible to estimate the corresponding full-sample elasticities with the IGE_g. Because the field has always used the IGE_g, the gender difference in elasticities has in this sense been made more difficult to detect.

Marital Status, Labor Supply, and Earnings Elasticities

The preceding section established that the earnings elasticity for women is substantially smaller than that for men. Although we have suggested possible accounts for this difference, we have not yet evaluated them. We take on that task here by disaggregating our "late thirties" sample by marital status and then comparing the earnings elasticities for married women, married men, single women, and single men (see Table 23).

The "labor supply hypothesis" arguably provides the most plausible account of the lower earnings elasticity for women.[121] This hypothesis contends that their earnings elasticity is driven down because (a) men and women with similar earnings potentials tend to marry one another, and (b) married women reduce their supply of labor as their spouses' earnings potential increases (Raaum et al. 2007). By contrast, married men are expected to be breadwinners (although this normative presumption is likely weakening), which means that their labor supply is largely unresponsive to their spouses' earnings potential. It follows that the earnings elasticity for married men should be higher than that for married women. Moreover, because single men and women cannot, by definition, reduce their labor supply in response to the high wages of (nonexistent) spouses, their earnings elasticities will likewise be higher than that of married women. We can conclude that, relative to the other groups, married women are especially likely to reduce their labor supply in ways that should drive down their earnings IGE_e.

We do not mean to suggest that married men, single men, and single women will likely have identical elasticities. A second key force at work, in addition to the negative income elasticity of labor supply, is occupational sex segregation. By virtue of socialization, discrimination, and other social and economic forces, women tend to work in a constrained set of

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[121] This hypothesis draws on both assortative mating and labor supply mechanisms, and might therefore be (less compactly) tagged the "assortative mating and labor supply" hypothesis.

occupations, and their earnings potential may not, as a result, be as reliably realized as men's. This suggests that the earnings IGE_e for single women should be lower than that for single men.

The third differentiating force is selection into marriage. Because men with higher earnings are more likely to be married (i.e., their marital status is endogenous to their earnings), and because earnings are also an increasing function of parental income, the IGE_e for married men is likely to be higher than that for single men.[122] In principle, the same selective effect might also obtain among women, but insofar as it does it is likely overwhelmed by the much stronger labor-supply effect.

These three hypotheses, taken together, thus imply a strict ordering of elasticities. We have suggested that married women should have the lowest elasticity (because their labor supply is very responsive to their spouse's earnings), that the earnings elasticity for single women should be lower than that for single men (because of sex segregation), and that the elasticity for single men should in turn be lower than that for married men (because of selection into marriage).

Are these three hypotheses borne out? The results in Table 23 do indeed suggest the distinctiveness of married women: The estimate of the global IGE_e for married women is only half that for married men (with p=0.001 for the null of no positive difference). Likewise, the difference between the estimates for married and single women is very large (p=0.049), as is the difference between the estimates for married women and single men (p=0.006). The foregoing differences, which pertain to the distinction between married women and each of the three other groups, are plausibly generated by the simple labor-supply behavior discussed above.

We additionally suggested that occupational sex segregation would produce a smaller global IGE_e for single women than single men. Although the relevant point estimates are consistent with this prediction, the null hypothesis of no positive difference cannot be rejected (p=0.202). As for the selection hypothesis, the difference is again consistent with the prediction, but it is small and the null hypothesis of no positive difference cannot be rejected (p= 0.248). The main result, then, coming out of Table 23 is that married women have an especially low global elasticity, just as the labor-supply hypothesis would have it.

The labor-supply hypothesis can be more directly tested by examining the relationship between parental income and the probability of employment. For married women, this relationship should reflect not just the hypothesized labor-supply behavior, but also the more general increase in employment that comes with a higher parental income. Indeed, children from higher-income backgrounds enjoy such advantages as better health, lower chances of

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[122] The simple point here is that, as parental income increases, those who are single are progressively more likely to represent lower percentiles of the corresponding conditional earnings distribution. See Raaum et al. (2007, p. 18) for a closely related argument.

institutionalization (especially incarceration), lower chances of single motherhood, lower chances of racial and ethnic discrimination in the labor market, increased human capital, more prestigious educational credentials, and better social networks (i.e., job-relevant "social capital").[123] These are all employment-increasing advantages. At some value of parental income, this increase in the probability of employment may nonetheless reach a ceiling, given that there are limits to (a) how far the probability of incarceration and single motherhood can fall, (b) the number of prestigious educational credentials that might be obtained, and (c) the extent to which sickness can be avoided. Moreover, because parental income is associated with children's wealth, a negative "wealth effect" on employment may at some point emerge.[124] The expected pattern is, then, a monotonic increase in the employment probability (with parental income), with a ceiling effect ultimately emerging. For married women, this pattern is coupled with the hypothesized labor-supply behaviour, which leads us to expect a distinctive *downturn* in their employment probability at the further reaches of the parental income distribution.

These expectations can be assessed with Figures 9 and 10, where we provide nonparametric estimates of the probability of employment, as a function of parental income, for men and women respectively. As Figure 9 shows, the probability of employment increases steadily for men until about $100,000 of parental income, at which point 90 percent of married men and 80 percent of single men are employed. After that, the curves hover at roughly the same level, a result that's suggestive of the ceiling effect described above. The employment rate for single women follows a very similar pattern, although it stops rising at a somewhat lower value of parental income, approximately $85,000. The married-women curve, however, shows a clear inverted-V shape. The employment rate increases steadily up to about $45,000 of parental income, rises much faster between $45,000 and $68,000, and then falls rapidly to a relatively low employment rate of 62 percent at the 99.5[th] percentile of parental income.

These results suggest that assortative mating and the negative income elasticity of labor supply are indeed generating a lower earnings IGE_e for women than for men. It seems especially clear that these factors account for some part of the IGE_e differences observed in the P50-P90 and above-P90 regions. Moreover, it is possible that their contribution is even greater than our

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[123] See Raaum et al. (2007) for a very different account of the relationship between parental income and employment (esp. Equation [3] and p. 33). For Raaum et al. (2007), it suffices to consider how labor supply is affected by earnings potential, either one's own earnings potential or the earnings potential of one's spouse. The role of human capital is, therefore, a very narrow one: It only has an effect on employment by generating a higher earnings potential (which in turn increases labor supply). For men (and probably single women), it is doubtful that human capital has any important effect on employment via labor supply, as the elasticity of men's labor supply to their own wage is close to zero. In our account, a main role of human capital and educational credentials is that of determining the labor markets in which people compete for jobs, with markets varying significantly in terms of their slackness.

[124] We have two wealth effects in mind. It's not just that wealthy people may opt against working (either permanently or for a period of time) but also that they may have a large reservation wage (and hence stay unemployed longer when they lose or quit jobs). The positive association between parental income and children's wealth that is assumed here may arise via intergenerational wealth transfers or as a byproduct of the association between parental income and children's income.

results suggest, given that we have only focused on the extensive margin of employment. If we could measure part-time work in the SOI-M Panel, we might well find yet stronger labor-supply effects in the P50-P90 and above-P90 regions. We might also find that the relatively low P10-P50 earnings IGE_e for married women is due to their tendency to work part-time (rather than full-time). Although we find that married women's employment in the P10-P50 region increases quite steadily with parental income, this is compatible with married women taking a much larger share of part-time work than men in the same region. The latter hypothesis, although plausible, cannot be tested here.

Gender Differences in the Indirect Transmission of Economic Status

We have established to this point that (a) the earnings elasticity for women is substantially lower than that for men, and (b) a main source of this difference is assortative mating and the negative income elasticity of labor supply for married women. There is, however, yet another gender puzzle that we have not yet taken on: Why doesn't the sizable gender difference in the earnings elasticity also show up in the total-income elasticity? As may be recalled, the global elasticity of total income is 0.47 for women and 0.52 for men (for the nonparametric model), a relatively minor difference. The purpose of this section is to examine why the substantial gender difference in earnings elasticities doesn't parlay into a correspondingly substantial gender difference in the total-income elasticities.

The starting point in addressing this question is to recall the three channels through which a higher-income origin can pay off: (a) it can increase the offspring's own earnings or income, (b) it can increase the offspring's chances of marrying and staying married (and thereby pooling resources with a spouse) and (c) it can increase the chances, among those who marry, of marrying a higher-income spouse (thus increasing total income). Because we've established that there's a large gender disparity in the first channel, it follows that there must be a compensating difference in at least one of the other two channels that renders the total-income elasticities similar in size. We examine here whether one or both of these channels is playing this compensating role.

Why haven't the contributions of these channels been sorted out in past research? It is partly because doing so requires including cases with zero earnings or income in the analysis. The zeros problem does not, however, arise with the IGE_e, which means that the IGE_e-based decomposition of Equations [12] – [13] can be used to identify the contribution of each channel (see "Direct and Indirect Transmission of Economic Status"). We present the associated results here.

Because the key results on own-earnings have been presented in the preceding two sections, we begin now with the "second channel" through which the total-income IGE_e is generated. The simple question here: Is the "marriage-probability payoff" to a higher-income background larger for daughters than for sons? Because the research on assortative mating and

intergenerational mobility has focused on married couples, we don't know much about how differential marriage chances themselves are implicated in generating total-income IGEs for women and men. We therefore present here the estimates from a nonparametric model of the probability of being married (as a function of parental disposable income).

The estimated curves are presented in Figure 11 (in log-log space). To facilitate interpretation, we have reexpressed the estimated probabilities as percentages (thus avoiding negative values when taking the logarithm), and we have included a second vertical axis on the right of the figure that shows these percentages. For men and women alike, the curves reveal a very strong relationship between parental income and marriage chances, which implies that the total-income elasticities are large partly because children from advantaged families are much more likely to be married (see Equation 11). This relationship takes an S-shaped form: The elasticity of the probability of being married increases up to about P50 for women and P30 for men, but then falls thereafter. The probabilities reach a ceiling of approximately 0.6 for men and 0.7 for women.

This gender difference in the ceiling and inflection point is, unsurprisingly, associated with a gender difference in elasticities. The global marriage elasticity is larger for women (0.29) than for men (0.24), and each of the region-specific elasticities above the 10th percentile (i.e., P10-P50, P50-P90, above-P90) is likewise larger for women than for men.[125] Moreover, except in the P10-P33 region, women tend to have a higher probability of being married than do men (with the curves touching in just a few places above the 33rd percentile). In the top half of the parental distribution (i.e., above P50), the probability of marriage for women is, on average, 7 percent higher than the probability for men (in the late-30s age group).

The foregoing results imply that the second channel, the "marriage-probability payoff," compensates for the relatively small earnings elasticity for women. We see from Equation [13] that the term $(1 - E(S_x)) \, E(IGE_e(M|x))$ is larger for women than for men because (a) the own-earnings share, S_x, is below 0.5 for women and above 0.5 for men at all levels of parental income (see Figure 8), and (b) the global elasticity of the probability of marriage, $E(IGE_e(M|x))$, is also larger for women than for men (as just discussed).[126] The simple conclusion: A higher "marriage-probability payoff" explains, in part, why the total-income elasticity for women is nearly as large as that for men, even though their earnings elasticity is so much smaller.

What about the third channel? The third way in which an offspring may benefit from a higher-income origin is via increased chances that her or his spouse has a higher income. This channel thus conditions on marriage and assesses the extent to which it brings additional income

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[125] The region-specific elasticities are 0.34, 0.30, 0.19, and 0.07 for men and 0.20, 0.37, 0.26 and 0.13 for women.

[126] The marriage probability has essentially no impact through the covariance in Equation [13]. This covariance can be expressed as a sum of three covariances. Of these, the one involving the elasticity of the probability of marriage, $Cov((1 - S_x), IGE_e(M|x))$, is close to zero for both men and women.

to the family (as parental income increases). Although the third channel has been a central concern within the literature on assortative mating and intergenerational mobility, it is nonetheless important to revisit it because prior analyses have been compromised by the use of the IGE_g as the measure of persistence. Because the IGE_g obliges analysts to drop spouses without earnings, it may produce biased estimates of the true spousal-earnings advantage associated with parental income.[127]

In Figure 12, we present the nonparametric curves pertaining to the spouse's earnings (among those who are married), with zero-earning spouses included. As expected, the married women's curve is above the married men's curve, reflecting both (a) the lower labor force participation rate of married women, and (b) the lower annual earnings of employed women relative to employed men.

We care more, however, that the curves are a mirror image of one another. That is, whereas men from the bottom half of the parental-income distribution receive a larger spousal-earnings payoff to parental income than do women, women from the upper half obtain a larger payoff than do men. This pattern arises partly from the different employment probabilities for married men and women (see Figures 9 and 10). But that is not the only factor behind it. If such differences were the *only* reason for the mirror-image pattern, the slopes of the curves should become identical after eliminating those who are not working. We instead find that, when children whose spouses have no or low earnings (earnings < $600) are dropped, the difference in slopes in the upper half of the parental distribution is reduced but does not disappear. In the bottom half of the parental distribution, the difference in slopes observed in Figure 12 almost disappears, which implies that the role of differential employment is central here (curves not shown). The persistence of a gender difference in the upper half of the parental distribution likely arises from either occupational segregation or a decision by some female spouses to work fewer hours.

The corresponding global IGE_e estimates for men and women are presented in Table 25. The first row of this table pertains to the curves in Figure 12, while the second row pertains to the curves in which spouses with low or no earnings are dropped. If the estimates in these two rows are compared, we can conclude that excluding nonearners and low earners generates a downward selection bias of approximately 18 percent. The full-sample IGE_e estimates, which come in at 0.34 for women and 0.26 for men, are quite large and thus confirm that the spouses' earnings elasticity plays an important role in generating both men's and women's total-income elasticities. But this contribution is clearly more important for women. It is more important because, again from Equation [13], the two components of the term $((1 -$

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[127] As noted earlier, Chadwick and Solon (2002) did include spouses with zero earnings in their analyses, but their indirect method of estimation provides estimates for an "improper parameter" (see footnote 70).

$E(S_x)) \, E\big(IGE_e(Y_s|x, M)\big)$ are each larger for women than for men.[128] It follows that the second and third channels are both playing a compensatory role: The low earnings IGE_e for women is counteracted by both (a) a higher marriage-probability payoff to parental income, and (b) a higher earnings-from-spouse payoff to parental income (among those who are married).

We can examine the joint operation of the last two channels by folding in those who are not married and coding them as having zero "spousal earnings" (as in equations [10] and [12]). The purpose of this final analysis is to measure the intergenerational elasticity of "indirect earnings" with respect to parental income. The resulting curves, shown in Figure 13, are the obverse of those pertaining to own earnings (Figure 8). That is, the women's curve is now located above the men's curve, and the global IGE_e (see Table 25) for the women's curve (0.58) is now larger than that for the men's curve (0.49). It is notable, moreover, that the women's spousal-earnings IGE_e (0.58) is as large as the men's own-earnings IGE_e (0.56). At the same time, the men's spousal-earnings IGE_e (0.49) is larger than the women's own-earnings IGE_e (0.32), a difference that helps explain why the total-income IGE_e is somewhat larger for men than women.

This argument can be formalized via Equation [12]. Under the assumption that earnings are the only source of income, the expected proportion of expected own-earnings to expected total income ($E(S_x)$) is estimated at 0.58 for men and, by implication, 0.42 for women.[129] Because the global own-earnings elasticity is much larger for men (0.56) than for women (0.32), the contribution of the direct pathway is accordingly more important for men ($0.58 \times 0.56 = 0.33$) than for women ($0.42 \times 0.32 = 0.13$). The contribution of the indirect pathway is, by contrast, substantially smaller for men ($0.42 \times 0.49 = 0.20$) than for women ($0.58 \times 0.58 = 0.34$). If we ignore the covariance in Equation [12] (which is very small for both men and women), the foregoing analysis implies total-income elasticities of 0.53 (i.e., $0.33 + 0.20$) for men and 0.47 (i.e., $0.13 + 0.34$) for women. These are virtually identical to the actual nonparametric estimates of these elasticities (using total-income information).[130]

We can conclude that, while the global IGE_e of total income takes on a similar size for women and for men, the processes through which it is generated differ markedly across genders. The direct pathway (via one's own earnings) accounts for the bulk of the IGE_e for men (61

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[128] The second of the three covariances mentioned in footnote 126 is $Cov((1 - S_x), IGE_e(Y_s|x, M))$. Because it is very small for both men and women, we can conclude that the elasticity of spouses' earnings has essentially no impact through the covariance in Equation [13].

[129] We proceeded by computing the proportion of men's expected own earnings to the sum of men's and women's expected own earnings for each of 198 equidistant quantiles of parental disposable income (0.5, 1, 1.5, … 98.5, 99, 99.5). We then computed the average of these 198 proportions.

[130] See Table 6. By Equations [12] and [13], we should be able to reproduce the global elasticity of spousal earnings by summing (a) the global marriage-probability elasticity, and (b) the global elasticity of spousal-earnings (conditional on marriage). For men, the estimates are essentially identical: $0.24 + 0.26 = 0.50$ (compared to 0.49). For women, there is a small difference: $0.29 + 0.34 = 0.63$ (compared to 0.58).

percent), whereas the indirect pathway (via marriage and earnings conditional on marriage) accounts for the bulk of the IGE_e for women (71 percent).

Conclusions

We have provided the first comprehensive set of IGE estimates based on tax and other administrative data. Although there are many useful ways to measure economic mobility, we decided to build our report around the intergenerational elasticity because it is the field's workhorse measure, and because it allows for a simple and concrete assessment of the economic payoff to being raised in a higher-income family.

Do we really need, it might be asked, yet another study of intergenerational persistence? The need for such a study is in fact pressing because, despite the long tradition of research with IGEs, there are fewer definitive conclusions in the literature on intergenerational persistence than one would want. The currently available research embodies quite disparate assessments of the extent of intergenerational persistence, the pattern of gender differences in intergenerational persistence, and the shape of the curve characterizing intergenerational persistence. Moreover, other key questions about the extent of persistence have simply gone unasked, typically for lack of appropriate data to answer them. We thus led off this report with a lengthy review of the literature that exposes many of these ambiguities, inconsistencies, and deficits in our existing knowledge about IGEs.

It might be thought that our knowledge about IGEs would have firmed up as administrative data are increasingly analyzed. This has unfortunately not happened. We have shown that, if anything, uncertainty about the size of key IGEs has increased with the latest wave of research with administrative data. This recent wave has also opened up new questions about the importance of attenuation and lifecycle biases and the types of data that are needed to eliminate or substantially reduce those biases.

There is accordingly an "evidence deficit" on the size and patterning of U.S. IGEs that we have sought to address here. Because this evidence deficit is partly attributable to a data deficit, we have built a new panel, the SOI-M Panel, that makes it possible to reach back to earlier birth cohorts than are available in previous research with tax data (Chetty et al. 2014a; 2014b). By exploiting a sample of 1987 tax returns, we can carry out analyses with children who are older than those available in that previous research, thus allowing us to reduce lifecycle bias substantially. The SOI-M Panel also allows us to estimate, for the first time, IGEs of disposable income.

Although the new SOI-M Panel thus resolves *some* of the problems arising in prior research, the "evidence deficit" on IGEs arises as much from methodological problems as data problems. It is important in this regard to bear in mind why Chetty et al. (2014a) and Dahl and DeLeire (2008) decided to turn away from the IGE. This decision was not based on an argument to the effect that the rank-rank slope was the most appropriate measure given the questions in which they were interested. Rather, the rank-rank slope was treated as a fallback approach, a fallback to which they resorted to address the methodological problems that arose in estimating

the IGE. This turn to the rank-rank slope was motivated, for example, by the extreme sensitivity of IGE estimates to assumptions about the income of nonfilers.

We have shown that these problems can be solved without turning away from the IGE. If we estimate the IGE of the expectation instead of the IGE of the geometric mean, the estimates are very robust, the nonfiler problem can be solved, and the elasticity that the literature long assumed it was estimating can indeed be delivered (Mitnik and Grusky 2015). Although the rank-rank slope is an important and useful measure of mobility, it should be used when the question at hand demands its use, not as a fallback to which one resorts to solve a methodological problem. The IGE speaks concretely to the economic payoff to higher-income origins and thus provides an answer to one of the central questions about mobility in the U.S.

We have also shown that conventional research on IGEs has not always reported estimates that pertain to the real population of interest. We are referring here to the long but unacknowledged tradition of estimating the IGE for a highly selective portion of the full population. When estimating the IGE for men's earnings, it is almost always problematic to exclude prime-age men without earnings, as they're typically unemployed, disabled, incarcerated, discouraged from looking for work, involved in illegal activities, or working in the informal sector. If these men are nonetheless excluded from analysis, as they conventionally are, we're in effect estimating an IGE that pertains to the case "when things are going well" (Couch and Lillard 1998).

The convention of dropping these cases, a consequence of estimating the IGE_g, is accordingly very difficult to justify. So far as we know, no substantive justification has been offered, indeed the convention has been treated as a practical expedient that solves the "logarithm-of-zero problem." If we instead switch to estimating the IGE of the expectation, we not only eliminate the need for this expedient but also return to the elasticity that the field in fact long assumed it was estimating (Mitnik and Grusky 2015).

What have we learned by analyzing the new SOI-M Panel and estimating the IGE of the expectation? Although we will not attempt to review all of our findings here, it is nonetheless useful to highlight some of the most important ones.[131]

- **Approximately half of parental income differences are passed on to children.** The total-income IGE is estimated at 0.52 for men and 0.47 for women. 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 also very large.** The earnings IGE for men, estimated at 0.56, is again at the high end of existing estimates of persistence. It is high in part because the effects

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[131] All estimates reported in the following summary of results are based on the nonparametric models. Unless noted otherwise, they refer to global or region-specific IGEs.

of selection bias have been purged. If low earners and nonearners are instead dropped, the men's earnings IGE drops to 0.47.

- **The persistence of economic differences is especially large among those raised in the middle to upper reaches of the income distribution.** The total-income IGE is estimated at 0.68 for men and 0.63 for women within the region of parental income falling between the 50th and 90th percentiles. This means that approximately two-thirds of parental income differences within this region persist into the next generation.

- **A very large share of the inequality between families at the 10th and 90th income percentiles persists into the next generation.** The total-income arc IGE for families at the 10th and 90th percentiles of parental income is 0.65 for men and 0.60 for women. These estimates underline the extent to which inequality between well-off and poor families persists from one generation to the next.

- **The conventional practice of estimating total-income IGEs (instead of disposable-income IGEs) has not led to any substantial misreading of the amount of intergenerational persistence.** The disposable income IGE, which speaks to the persistence of available economic resources, is only about 5 percent lower than the total-income IGE for men and about 3 percent lower for women. This result dispels concerns that the conventional practice of estimating total-income IGEs has been misleading in any substantial way.

- **The expansion of tax credits, especially the EITC, appears to have reduced the persistence of economic differences among those at the very bottom of the parental-income distribution.** This EITC effect, which takes the form of a flattening-out of the left tail of the disposable-income curve, shows up more clearly for women than for men (presumably because women are the principal beneficiaries of the EITC).

- **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). Although both men and women secure an earnings payoff from being raised in higher-income families, men have a much higher payoff than do women. The payoff is lower for women because, when they are married, they tend to reduce their supply of labor as their spouses' earnings increase (while married men do not).

- **The mechanisms by which the total-income IGE is generated differ for men and women.** While the total-income IGE is nearly as large for women as men, the processes through which that IGE is generated are very different for women and men. The direct pathway (via one's own earnings) accounts for 61 percent of the total-income IGE for men, whereas the indirect pathway (via marriage and spouse's earnings conditional on marriage) accounts for 71 percent of the total-income IGE for women.

There are many important conclusions that follow from these results. We will not, however, attempt to rehearse them here, as we have already done so throughout the preceding section. We will instead conclude by noting that, as revealing as elasticities are, it is of course necessary to supplement them with other measures of mobility. The SOI-M Panel is an important

new resource that can and should be used to examine mobility in the U.S. with a full complement of measures.

Appendix A
Evidence on Attenuation and Lifecycle Biases

This appendix discusses the literature on attenuation and lifecycle biases, the approaches that we have taken to address these biases, and the evidence on behalf of these approaches. We begin with a discussion of attenuation bias and then turn to lifecycle bias.

Attenuation Bias

Since the early 1990s, 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 a 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. (2014a) 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 (2014a, Table 1 and Online Appendix, Section 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 our estimates change as additional years of parental information are incorporated. The key question here is whether the results reported by Chetty et al. (2014a) also hold when the analyses (a) are carried out with the IGE_e instead of the IGE_g, (b) are based on different cohorts and time periods, and (c) entail other more minor differences in methodological procedures. 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).[132]

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.

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

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 recomputed with a different number of years. There is also a special complication that arises in 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).[133]

We start by examining the results obtained with the common sample approach. In Figures A1 and A2, we present the constant-elasticity estimates of the total-income IGE_e and earnings IGE_e for men and women, respectively. We also present corresponding total-income estimates after pooling the samples for men and women (see Figure A3).[134]

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 A4-A6). 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.

Table A1 presents some of the data behind Figures A1-A6. The first three rows in each income-measure panel pertain to the estimates produced with 1-year, 5-year, and 9-year

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[133] 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.

[134] The main reason for producing estimates with pooled data is that the larger sample size makes the trend clearer. Also, because Chetty et al.'s (2014a) estimates use pooled data, doing so makes for a more direct comparison. At the same time, this comparison can only be taken so far, given that our focus is on the elasticity of the expectation, not the geometric mean.

variables. The next three rows show the percent change in the estimates for (a) 5 or 9 years of parental information compared to 1 year, and (b) 9 years compared to 5 years. The percent changes that pertain to moving from the 1-year to 9-year averages confirm, as all figures had already indicated, that attenuation bias is reduced substantially by using 9 years of parental information. The most important results in this table, however, are those pertaining to the percent change in estimates when shifting from 5-year to 9-year measures. These figures indicate that, as Figures A1-A6 also suggested, a 5-year measure generates a nonnegligible amount of additional attenuation bias (as compared to the corresponding 9-year measure). Moreover, the percent changes are larger in the case of earnings (10.2-13.2 percent) than total income (5.2-9.9 percent), which suggests that attenuation bias may decrease more slowly for earnings. The percent changes for total income, when men and women are pooled, are somewhat larger than the change reported by Chetty et al. (2014a). We find a 7.6 percent change when we move from 5 to 9 years (i.e., add four years), whereas Chetty et al. (2014a) report a 6.4 percent change when they move from 5 to 15 years (i.e., add ten years).

The evidence in Figures A1-A6 suggests a plateau by year 9. Although we might conclude that our 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 we use 6 years of parental information in the SOI-M data, we can only include additional years that 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. (2014a) report are likewise affected by the noisiness of the additional years they are incorporating.[135]

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 nonnegligible increase in bias.

Lifecycle Bias

We next consider lifecycle bias. This refers to the bias that arises because children from different parental-income backgrounds have different age-income or age-earnings profiles. If we measure children's income or earnings too early in their lifecycle, we may underestimate the corresponding elasticity. Although there is some evidence, pertaining mainly to men's earnings,

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[135] It should be noted that Chetty et al. (2014a) consider this possibility but reject it on the argument that their Figure IIb (in the same 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, Section 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.

suggesting that a lifetime IGE_g is best approximated by measuring income or earnings at age 40 (Haider and Solon 2006; Mazumder 2005), Chetty et al. (2014a) have argued 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, Section C and Figure Ib).[136] We revisit this issue here with our SOI-M data.

We begin by considering lifecycle bias for earnings elasticities. In Figure A7, we present our estimates of the (constant-elasticity) IGE_e of men's earnings, for our four cohorts, from 2001 to 2010. To facilitate interpretation, we have included a second horizontal axis at the top of the figures, showing the mean age for each cohort in each year. 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.[137]

Is there evidence of an earlier stabilization when we turn to total-income elasticities? In Figures A8 and A9, 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 A10, 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.

The results discussed so far informed our decision to focus on the late thirties sample. Given the evidence in the previous literature and our results, it seems clear that measuring the

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[136] We are not fully persuaded by Chetty et al.'s (2014a) interpretation of their results on the IGE_e. They report that the IGE_e increases at a decreasing rate as children move from ages 22 to 32 and conclude that "the [IGE_e] also stabilizes around age 30: the estimated [IGE_e] is 2.1% higher at age 32 than age 31" (Online Appendix, Section C). Even with a decreasing growth rate, a 2.1 percent increase in one year (of age) is not all that small. If the rate of growth each year is 0.90 of what it was in the previous year (and given their estimate of 0.343 by age 32), we should expect an IGE_e of about 0.38 by age 40, compared to slightly above 0.32 by age 30. (These values, all of which are based on a 5-year measure of parental income, should of course be additionally affected by attenuation bias. We are only focusing here on the "stabilization claim.")

[137] We have not presented estimates of changes in the IGE_e of women's earnings as they become older. This is because, as indicated in the text, women's earnings are not a reliable measure of their economic status (and, correspondingly, there is no literature on lifecycle bias for women). When we nonetheless examine the relevant elasticities, the results show that the IGE_e of women's earnings decreases as they enter their 30s and then stabilizes at this lower value. If nonearners and low earners (women with earnings below $600) are dropped, then the IGE is essentially stable over the entire period/age range.

elasticity of men's earnings when they are relatively young should generate substantial lifecycle bias. Moreover, given that the Great Recession ran through much of 2009, it is prudent to focus on post-recession data. Lastly, 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 the Chetty et al. (2014) estimate of that elasticity at those ages likely involves a substantial amount of lifecycle bias.[138]

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[138] See the subsection titled "Global Elasticities" for our analysis of the joint effects of lifecycle and attenuation biases on Chetty et al.'s estimate.

Appendix B
Children's Expected Income at Selected Parental Percentiles

This appendix presents the data underlying the nonparametric results in the subsection titled "Persistence among 'Far-Apart' Families." These data can be employed to compute standard (i.e., Allen's) arc elasticities, other types of arc elasticities, and other measures that can be used to assess differences in the expected economic outcomes of children raised in families with very different incomes. This appendix also presents disposable-income data similar to the total-income data used to compute arc elasticities in "Persistence among 'Far-Apart' Families."

For the 10th, 50th, and 90th percentiles of parental income, Table B1 provides (a) parental total and disposable income, and (b) the nonparametric estimates of men's expected earnings and of men's and women's expected total and disposable income. The corresponding statistics for families in the 5th-15th, 45th-55th, and 85th-95th percentiles of parental income are presented in Table B2. The values in this table are averages across percentiles of parental income. For example, we calculate parental total income "around" the 10th percentile by averaging parental total income across percentiles 5 to 15, and likewise we calculate parental total income "around" the 50th percentile by averaging parental total income across percentiles 45 to 55.

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Table 1: SOI-M Base Sample and 1987 CPS-ASEC, Weighted Frequencies

Child's Age in 1987	SOI-M Frequency	SOI-M Weighted Frequency	1987 CPS 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 2: Data Sources for the Construction of the SOI-M Data Set

Data	Purpose
Tax returns from SOI Family Panel, 1987-1996	Source of parental income data and parent-child social security links (with claimed children then traced forward)
Tax returns from the refreshment segment of the OTA Panel, 1987-1996	Recover "nonpermanent nonfilers" (i.e., individuals in 1987 non-filing population who appeared in at least one 1988-1996 return); source of parental income data
Population of tax returns, 1997-1998	Source of parental income data
Population of tax returns, 1998-2010	Source of income data for children; source of income data for children's spouses when they file as "married filing separately;" source of children's marital status information
W-2 forms, 1999-2010	Source of gross ("Medicare") and taxable earnings of children, including nonfiler children
1040-SE forms, 1999-2010	Source of self-employment income
Data Master File	Source of demographic information (age of parents, and gender, age, and year of death of children)
1099-G forms, 1999-2010	Source of unemployment income of children
Current Population Survey Annual Economic and Social Supplement (CPS-ASEC), 1999-2003	Source of information for mean imputation of total income (nonfiler children w/o W-2 or UI information), multiple imputation of marital status, total income, and spouse's earnings (nonfiler children w/o W-2 or UI information), and multiple imputation of marital status (nonfiler children with W-2 or UI information)

Table 3: Income Concepts and Their Measurement

Income concept	Measurement
Annual total family income	
Parents	
Return available in SOI Family Panel (1987-1996)	Total income in Form 1040 + nontaxable interest
Return not available in SOI Family Panel (1987-1996)	Value of total income + nontaxable interest, as computed or imputed by OTA Panel
Return available in population of tax returns	Total income in Form 1040 + nontaxable interest
Children	
Filer	Own and spouse's (if married filing separately) total income in Form 1040 + nontaxable interest + nontaxable UI income (2009 only) + nontaxable earnings
Nonfiler	
W-2 or UI information available	W-2 gross ("Medicare") wages + UI income
No W-2 or UI information available	Mean or multiple imputation by gender and age using CPS-ASEC data
Annual family disposable income (i.e., after-federal-tax income), parents and children	Annual total income - net federal taxes paid (including refundable tax credits)
Annual individual earnings, children	W-2 gross ("Medicare") wages + 65 percent of self-employment income
Annual individual earnings, children's spouses	W-2 gross ("Medicare") wages + 65 percent of self-employment income (filers' spouses); multiple imputation by gender and age using CPS-ASEC (spouses of nonfiler children without administrative information)

Table 4: Descriptive Statistics for Late 30s Sample (Unweighted Percentages)

Variables	Models for Children			Earnings-from-Spouse Models	
	Income	Earnings	Earnings (< $600 dropped)	Married children	All children
Child's gender (% fem.)	49.4	49.1	45.7	50.4	49.1
Child's age					
38	27.1	27.1	27.0	27.8	27.1
37	25.1	25.1	25.1	25.3	25.1
36	24.1	24.1	24.2	24.3	24.1
35	23.7	23.7	23.7	22.7	23.7
Child's income information					
Return	88.8	NA	NA	NA	NA
W-2 + UI	4.1	NA	NA	NA	NA
CPS-based imputation	7.1	NA	NA	NA	NA
Number of missing years of parental information					
0	95.2	95.3	95.6	96.3	95.3
1	2.5	2.5	2.3	2.1	2.5
2	1.5	1.5	1.4	1.1	1.5
3	0.8	0.8	0.8	0.6	0.8
Sample size	12,469	12,872	9,972	8,010	12,868

Notes:

All percentages in the earnings-from-spouse models are averages across multiple-imputed variables.

Children with more than 3 missing years of parental information are excluded from all samples.

NA = Not Applicable (variable not relevant in corresponding model).

Table 5: Descriptive Statistics for Late 30s Sample (Weighted Values)

Variables	Models for Children			Earnings-from-Spouse Models	
	Income	Earnings	Earnings (< $600 dropped)	Married children	All children
Child's total income					
Mean	69,329	NA	NA	NA	NA
Standard deviation	107,061	NA	NA	NA	NA
Child's disposable income					
Mean	59,239	NA	NA	NA	NA
Standard deviation	80,890	NA	NA	NA	NA
Child's earnings					
Mean	NA	36,547	47,112	NA	NA
Standard deviation	NA	56,436	60,071	NA	NA
Child's earnings from spouse					
Mean	NA	NA	NA	45,183	24,094
Standard deviation	NA	NA	NA	61,041	49,833
Average parental total income over 9 years					
Mean	74,826	NA	NA	NA	NA
Standard deviation	115,622	NA	NA	NA	NA
Average parental disposable income over 9 years					
Mean	63,530	64,183	65,792	73,305	64,177
Standard deviation	84,744	91,706	88,591	104,589	91,780
Average parental age over 9 years					
Mean	45.3	45.4	45.4	NA	NA
Standard deviation	6.2	6.2	6.0	NA	NA

Notes:

Monetary values in 2010 dollars (adjusted by inflation using CPI-U-RS).

All values in the earnings-from-spouse models are averages across multiple-imputed variables.

NA = Not Applicable (variable not relevant in corresponding model).

Table 6: Global IGE$_e$, Preferred Models

	Constant-Elasticity Model	Spline Model	Nonpara-metric Model	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 earnings, less than $600 dropped	0.41	0.46	0.47	0.000
	(0.36-0.47)	(0.40-0.52)	(0.40-0.52)	
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)	

Table 7: Global IGE_e, Models with Parental-Age Controls

	Constant-Elasticity Model	Spline Model	P-value from CE test
Men's earnings	0.46	0.51	0.000
	(0.40-0.52)	(0.45-0.58)	
Men's earnings, less than $600 dropped	0.40	0.44	0.000
	(0.34-0.46)	(0.38-0.50)	
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)	

Table 8: Global IGE$_e$, Comparison with Chetty et al. (2014a), Men and Women Pooled

Model	Children Ages	Years of Parental Information	Estimates with SOI-M Panel	Estimate from Chetty et al. (2014a)
Nonparametric	35-38	9	0.50	-
Constant-elasticity	35-38	9	0.46	-
Constant-elasticity	29-32	5	0.37	0.34

Table 9: Sensitivity of Constant-Elasticity Estimates of Total-Income IGEs to the Treatment of Nonfilers

Included Children	Imputation for Nonfilers w/o Adm. Inform.	Treatment of CPS $0s in Computing Means	IGE_e	IGE_g
Men				
Tax return filers			0.40	0.40
Filers and nonfilers with adm. inf.			0.42	0.43
All	$0		0.49	N/A
All	$1		0.49	1.09
All	$100		0.49	0.77
All	$1,000		0.48	0.61
All	$3,000		0.48	0.53
All	CPS means	CPS $0 → $0	0.47	N/A
All	CPS means	CPS $0 → $1	0.47	0.73
All	CPS means	CPS $0 → $100	0.47	0.60
All	CPS means	CPS $0 → $1,000	0.47	0.54
All	CPS means	CPS $0 → $3,000	0.47	0.51
Women				
Tax return filers			0.40	0.33
Filers and nonfilers with adm. inf.			0.41	0.36
All	$0		0.46	N/A
All	$1		0.46	1.03
All	$100		0.46	0.70
All	$1,000		0.46	0.54
All	$3,000		0.46	0.46
All	CPS means	CPS $0 → $0	0.45	N/A
All	CPS means	CPS $0 → $1	0.45	0.61
All	CPS means	CPS $0 → $100	0.45	0.51
All	CPS means	CPS $0 → $1,000	0.45	0.46
All	CPS means	CPS $0 → $3,000	0.45	0.43

Table 10: Sensitivity of Nonparametric Estimates of Total-Income Global IGEs to the Treatment of Nonfilers

Included Children	Imputation for Nonfilers w/o Adm. Inform.	Treatment of CPS $0s in Computing Means	IGE$_e$	IGE$_g$
Men				
Tax return filers			0.43	0.44
Filers and nonfilers with adm. inf.			0.45	0.49
All	$0		0.54	N/A
All	$1		0.54	1.13
All	$100		0.54	0.82
All	$1,000		0.53	0.67
All	$3,000		0.53	0.59
All	CPS means	CPS $0 → $0	0.52	N/A
All	CPS means	CPS $0 → $1	0.52	0.78
All	CPS means	CPS $0 → $100	0.52	0.66
All	CPS means	CPS $0 → $1,000	0.52	0.60
All	CPS means	CPS $0 → $3,000	0.52	0.57
Women				
Tax return filers			0.41	0.38
Filers and nonfilers with adm. inf.			0.42	0.41
All	$0		0.49	N/A
All	$1		0.49	1.05
All	$100		0.49	0.76
All	$1,000		0.49	0.61
All	$3,000		0.48	0.54
All	CPS means	CPS $0 → $0	0.47	N/A
All	CPS means	CPS $0 → $1	0.47	0.67
All	CPS means	CPS $0 → $100	0.47	0.58
All	CPS means	CPS $0 → $1,000	0.47	0.54
All	CPS means	CPS $0 → $3,000	0.47	0.52

Table 11: Global IGE$_g$, Comparison with Chetty et al. (2014a), Men and Women Pooled

Model	Included Children	Children Ages	Years of Parental Information	Estimates with SOI-M Panel	Estimate from Chetty et al. (2014a)
Nonparametric	All	35-38	9	0.55 (lb) - 0.74 (ub)	-
Nonparametric	Filers and nonfilers with adm. inf.	35-38	9	0.45	-
Constant elasticity	Filers and nonfilers with adm. inf.	35-38	9	0.39	-
Constant elasticity	Filers and nonfilers with adm. inf.	29-32	5	0.28	0.34

Table 12: Sensitivity of Constant-Elasticity Estimates of the IGEs of Men's Earnings to the Treatment of Low and Nonearners

Included Men	Imputation for Nonearners	IGE$_e$	IGE$_g$
Earnings above $ 3,000		0.40	0.31
Earnings above $ 1,500		0.41	0.32
Earnings above $ 600		0.41	0.32
Earnings above $ 100		0.42	0.35
Earnings above $ 0		0.42	0.35
All	$0	0.49	N/A
All	$1	0.49	1.07
All	$100	0.49	0.71
All	$1,000	0.48	0.52
All	$3,000	0.47	0.44

Table 13: Sensitivity of Nonparametric Estimates of the Global IGEs of Men's Earnings to the Treatment of Low and Nonearners

Included Men	Imputation for Nonearners	IGE_e	IGE_g
Earnings above $ 3,000		0.45	0.37
Earnings above $ 1,500		0.47	0.38
Earnings above $ 600		0.47	0.39
Earnings above $ 100		0.47	0.44
Earnings above $ 0		0.47	0.45
All	$0	0.56	N/A
All	$1	0.56	1.16
All	$100	0.56	0.81
All	$1,000	0.56	0.63
All	$3,000	0.55	0.54

Table 14: Region-Specific IGE_e, Spline Model

	Up to P10	P10-P50	P50-P90	Above P90	P-value from Test of H0: P10-P50 $\geq$ P50-P90
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)	

Table 15: Region-Specific IGE_e, Nonparametric Model

	Up to P10	P10-P50	P50-P90	Above P90	P-value from Test of H0: P10-P50 $\geq$ P50-P90
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 16: Arc IGE$_e$, Spline Model

	Well-off / Poor Families	Well-off / Median Families	Median / Poor Families
Men's earnings	0.68	0.73	0.54
	(0.61-0.75)	(0.58-0.86)	(0.40-0.66)
Men's total income	0.64	0.67	0.49
	(0.57-0.71)	(0.51-0.85)	(0.38-0.60)
Women's total income	0.57	0.62	0.40
	(0.49-0.64)	(0.49-0.76)	(0.27-0.52)

Note:

"Well-off," "median," and "poor" families pertain to the 5th-15th, 45th-55th, and 85th-95th percentiles, respectively.

Table 17: Arc IGE_e, Nonparametric Model

	Well-off / Poor Families	Well-off / Median Families	Median / Poor families
Men's earnings	0.64	0.61	0.54
	(0.56-0.71)	(0.45-0.78)	(0.42-0.66)
Men's total income	0.65	0.67	0.49
	(0.57-0.73)	(0.49-0.86)	(0.40-0.58)
Women's total income	0.60	0.65	0.42
	(0.52-0.67)	(0.50-0.81)	(0.30-0.54)

Note:

"Well-off," "median," and "poor" families pertain to the 5th-15th, 45th-55th, and 85th-95th percentiles, respectively.

Table 18: Total and Disposable Income Global IGE$_e$

	Constant-Elasticity Model	Spline Model	Nonparametric Model
Total income			
Men	0.48	0.52	0.53
	(0.44-0.52)	(0.46-0.59)	(0.46-0.59)
Women	0.46	0.47	0.47
	(0.42-0.50)	(0.42-0.53)	(0.42-0.53)
Disposable income			
Men	0.46	0.49	0.50
	(0.42-0.51)	(0.43-0.55)	(0.44-0.56)
Women	0.44	0.46	0.46
	(0.40-0.48)	(0.40-0.51)	(0.41-0.53)

Table 19: Share of Children Receiving EITC, by Gender and Parental Income Quintile

	Men	Women
Total	17.0	24.8
By parental total income quintile		
First quintile	10.2	13.3
Second quintile	13.2	18.2
Third quintile	14.5	24.3
Fourth quintile	21.7	32.7
Fifth quintile	25.4	35.3

Table 20: Mean EITC Amount Received by Population Group, Gender, and Parental Income (2010 Dollars)

	All children	Children Receiving EITC
Men		
Total	348	2,042
By parental total income quintile		
First quintile	198	1,937
Second quintile	235	1,779
Third quintile	360	2,478
Fourth quintile	424	1,956
Fifth quintile	525	2,066
Women		
Total	674	2,718
By parental total income quintile		
First quintile	308	2,309
Second quintile	449	2,460
Third quintile	682	2,804
Fourth quintile	868	2,652
Fifth quintile	1,061	3,008

Table 21: Total-Income and Disposable-Income IGE_e for the Below-P10 Region, Spline Model

	Total Income	Disposable Income	Difference	P-value
Men	0.34	0.31	0.03	0.108
Women	0.52	0.41	0.10	0.000

Notes:

Difference is (total-income IGE_e) - (disposable income IGE_e).

P-value corresponds to the test of the null that the difference is not positive.

Table 22: Total-Income and Disposable-Income IGE_e for the Below-P10 Region, Nonparametric Model

	Total Income	Disposable Income	Difference	P-value
Men	0.38	0.38	0.01	0.342
Women	0.53	0.45	0.08	0.007

Notes:

Difference is (total-income IGE_e) - (disposable income IGE_e).

P-value corresponds to the test of the null that the difference is not positive.

Table 23: Global Earnings IGE_e by Gender and Marital Status, Nonparametric Model

	Men	Women	Difference	Proportional Difference (%)
All	0.56	0.32	0.24	42.9
	(0.49-0.62)	(0.27-0.38)		
Married	0.49	0.24	0.25	51.0
	(0.39-0.59)	(0.14-0.36)		
Single	0.44	0.39	0.05	11.4
	(0.35-0.52)	(0.29-0.46)		

Notes:

Difference is (men's IGE_e) - (women's IGE_e).

Proportional difference is the difference in IGE_es as a percent of men's IGE_e.

Table 24: Global Earnings IGEs by Gender, Model, and Sample-Inclusion Rule

	Men	Women	Difference	Proportional Difference (%)
IGE$_e$				
Nonparametric model				
All	0.56	0.32	0.24	42.9
Earnings > $600	0.47	0.30	0.17	36.2
Earnings > $3,000	0.45	0.28	0.17	37.8
Constant-elasticity model				
All	0.49	0.27	0.22	44.9
Earnings > $600	0.41	0.25	0.16	39.0
Earnings > $3,000	0.4	0.24	0.16	40.0
IGE$_g$				
Nonparametric model				
All	N/A	N/A	N/A	N/A
Earnings > $600	0.39	0.26	0.13	33.3
Earnings > $3,000	0.37	0.26	0.11	29.7
Constant-elasticity model				
All	N/A	N/A	N/A	N/A
Earnings > $600	0.32	0.2	0.12	37.5
Earnings > $3,000	0.31	0.19	0.12	38.7

Notes:

Difference is (men's IGE) - (women's IGE).

Proportional difference is the difference in IGEs as a percent of men's IGE.

Table 25: Earnings from Spouse IGE_e, Nonparametric Model

	Men	Women
Married children		
All	0.26	0.34
	(0.14-0.38)	(0.27-0.43)
Spouse earnings ≥ $600	0.21	0.28
	(0.13-0.30)	(0.21-0.35)
All children	0.49	0.58
	(0.39-0.59)	(0.50-0.67)

Table A1: Global IGE$_e$ Estimated with 1, 5, and 9 Years of Parental Information

Income Measure	Common Sample Approach			Common Rules Approach		
	Men	Women	All	Men	Women	All
Total income						
Estimates						
1 year of par. inf.	0.27	0.31	0.29	0.40	0.37	0.38
5 years of par. inf.	0.43	0.43	0.43	0.44	0.42	0.43
9 years of par. inf.	0.47	0.45	0.46	0.47	0.45	0.46
Change (%)						
From 1 to 5 years	60.6	38.3	49.2	8.6	13.9	11.4
From 5 to 9 years	9.9	5.2	7.5	8.4	7.1	7.7
From 1 to 9 years	76.4	45.5	60.4	17.7	22.0	20.0
Earnings						
Estimates						
1 year of par. inf.	0.29	0.16	NA	0.39	0.21	NA
5 years of par. inf.	0.44	0.24	NA	0.44	0.25	NA
9 years of par. inf.	0.49	0.27	NA	0.49	0.27	NA
Change (%)						
From 1 to 5 years	50.0	48.4	NA	11.8	16.2	NA
From 5 to 9 years	11.8	13.2	NA	10.5	10.2	NA
From 1 to 9 years	67.8	68.1	NA	23.6	28.0	NA

Table B1: Parental Income and Children's Expected Income and Earnings at Selected Parental Percentiles (2010 dollars)

	Parental percentile		
	10th	50th	90th
Men's earnings			
Parental disposable income	14,912	51,418	109,594
Children's expected earnings	21,998	41,190	64,971
Men's total income			
Parental total income	15,749	57,061	128,356
Children's expected total income	33,930	60,507	104,679
Women's total income			
Parental total income	15,841	57,137	128,654
Children's expected total income	40,516	66,192	118,255
Men's disposable income			
Parental disposable income	14,788	51,354	108,773
Children's expected disposable income	31,308	53,556	86,274
Women's disposable income			
Parental disposable income	15,631	51,144	107,498
Children's expected disposable income	37,026	55,839	100,364

Note:

Estimates of expected income and earnings are based on the nonparametric models.

Table B2: Parental Income and Children's Expected Income and Earnings around Selected Parental Percentiles (2010 dollars)

	Parental percentile		
	10th	50th	90th
Men's earnings			
Parental disposable income	14,791	51,504	112,302
Children's expected earnings	22,097	41,021	65,011
Men's total income			
Parental total income	15,362	57,470	132,460
Children's expected total income	33,628	60,331	104,111
Women's total income			
Parental total income	16,179	57,289	132,601
Children's expected total income	40,772	65,926	111,868
Men's disposable income			
Parental disposable income	14,762	51,353	111,557
Children's expected disposable income	31,375	53,205	85,557
Women's disposable income			
Parental disposable income	15,494	51,331	111,141
Children's expected disposable income	36,748	56,302	92,331

Notes:

Estimates of expected income and earnings are based on the nonparametric models.

The expected values around percentiles 10, 50, and 90 are averaged across the 5th-15th, 45th-55th, and 85-95 percentiles, respectively.

Figure 1: Parental Income in SOI-M Base Sample and CPS-ASEC
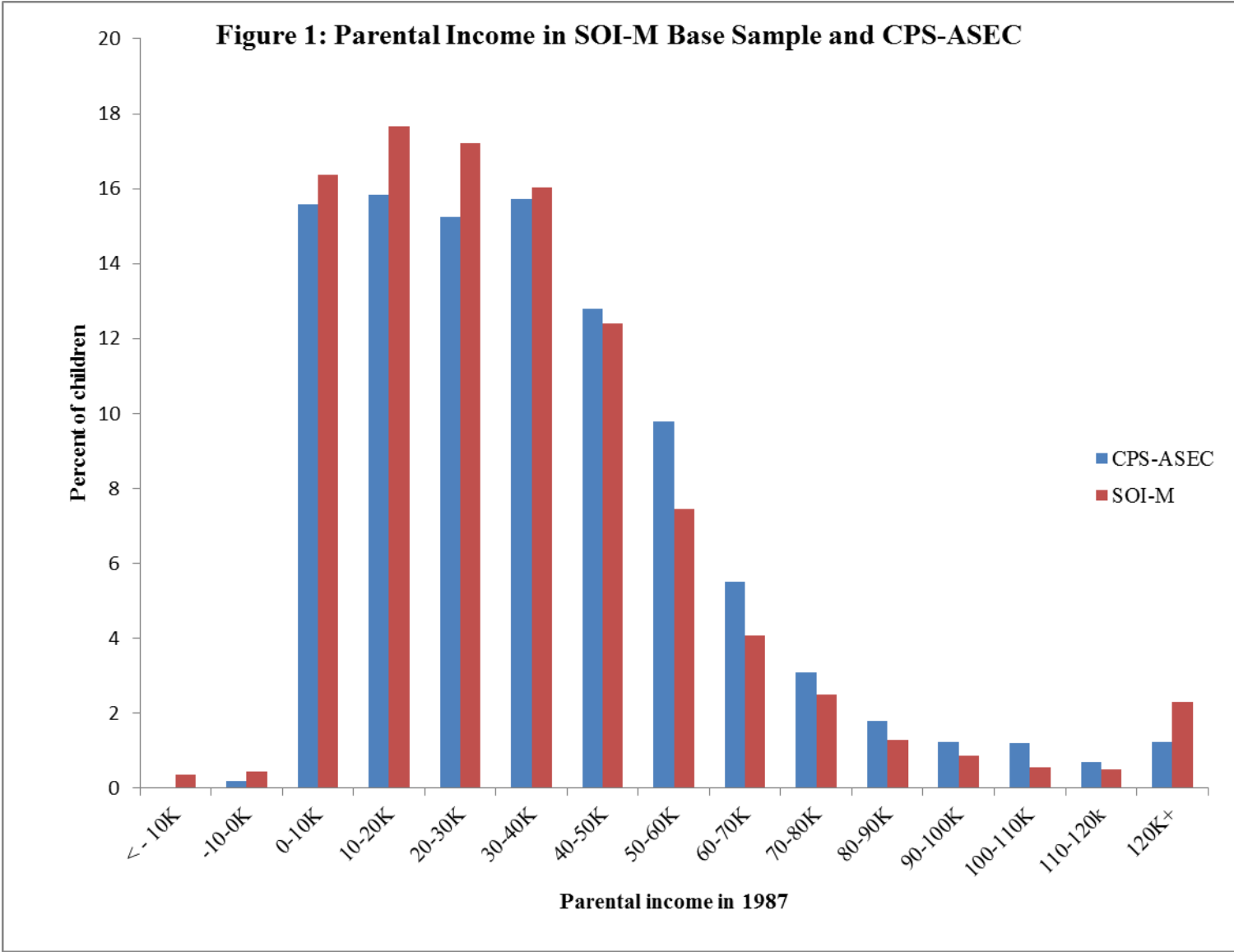
Figure 2: Structure of SOI-M Panel

Cohort		1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
1975		12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35
1974	Children	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36
1973	Age	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37
1972		15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38

Year

Base sample

Late 30s sample

Children's ages and years at which parental income is measured

Children's ages and years at which children's income is measured (earnings starting in 1999)

Figure 3: Log of men's expected earnings as a function of log parental disposable income
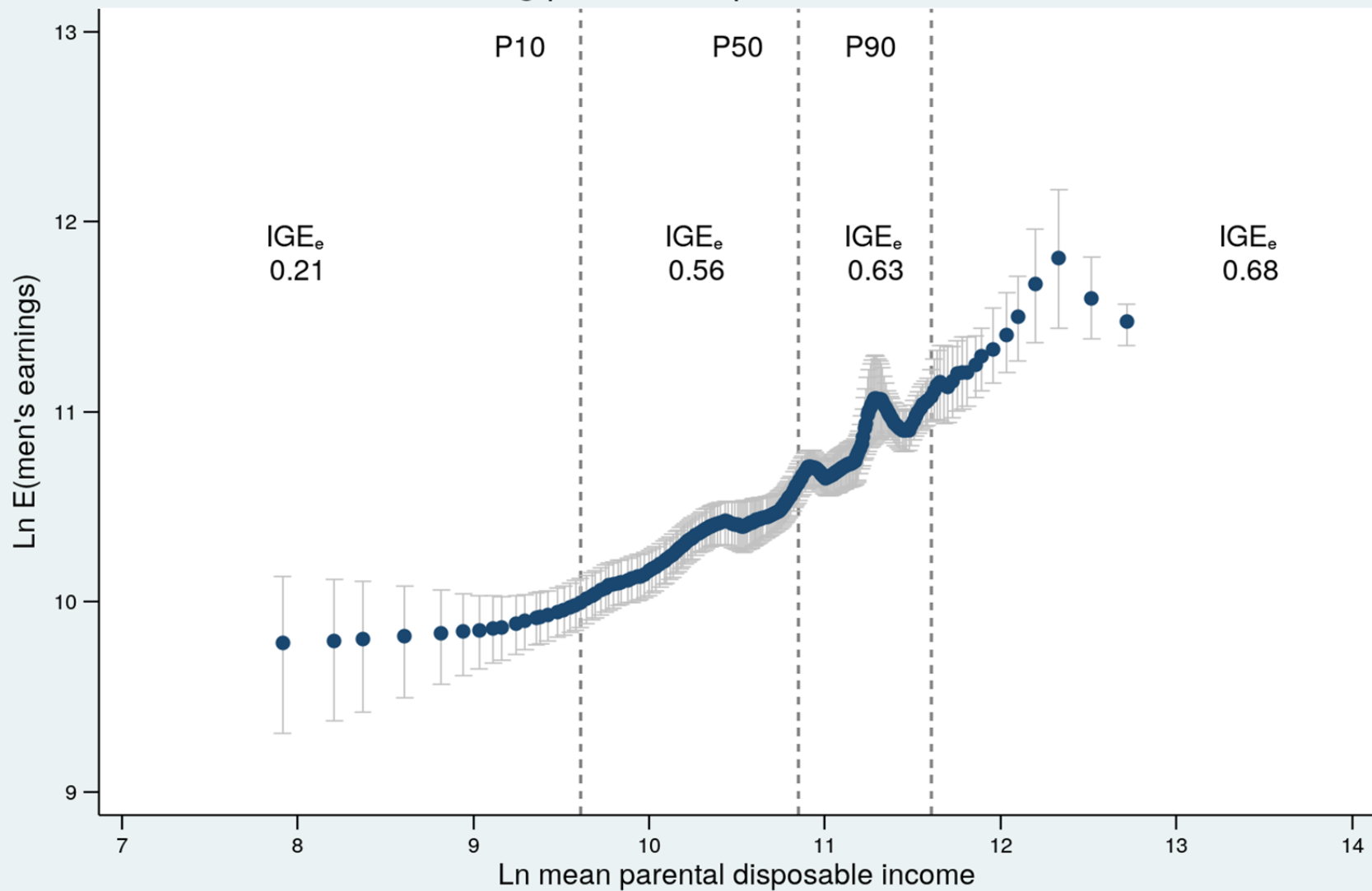
Figure 4: Log of men's expected total income as a function of log parental total income
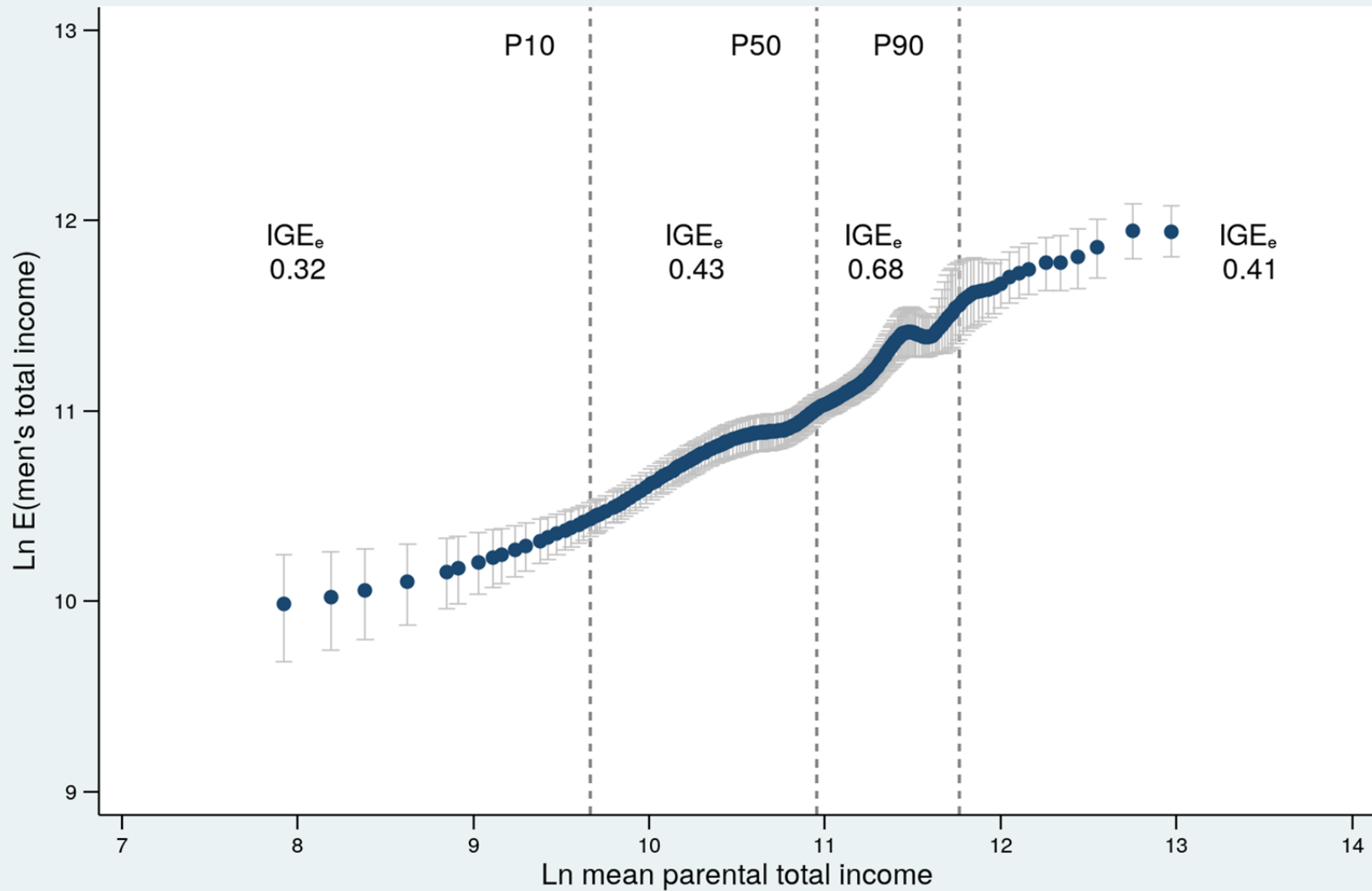
Figure 5: Log of women's expected total income as a function of log parental total income
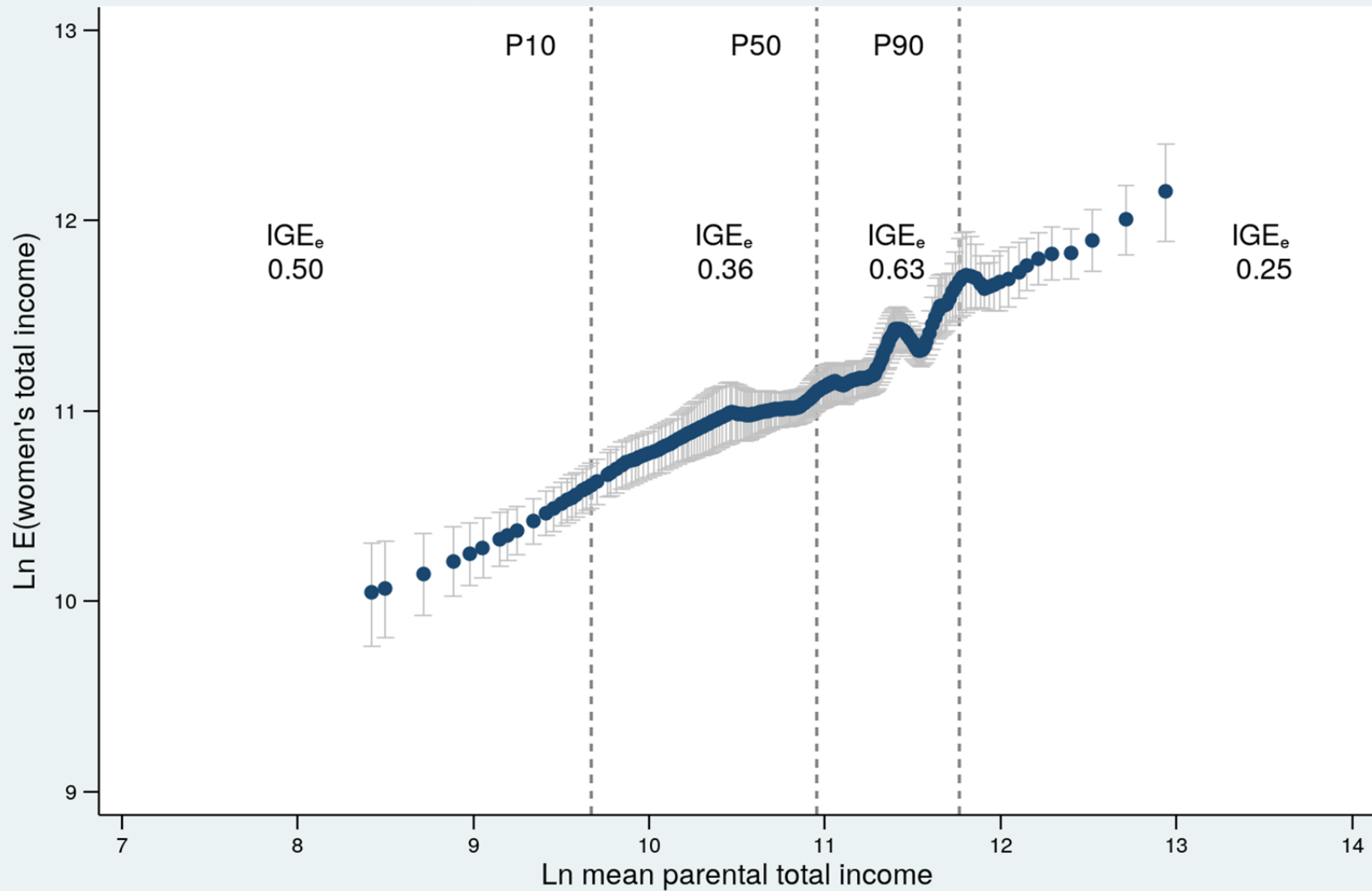
Figure 6: Log of men's expected total and disposable income as a function of log parental income
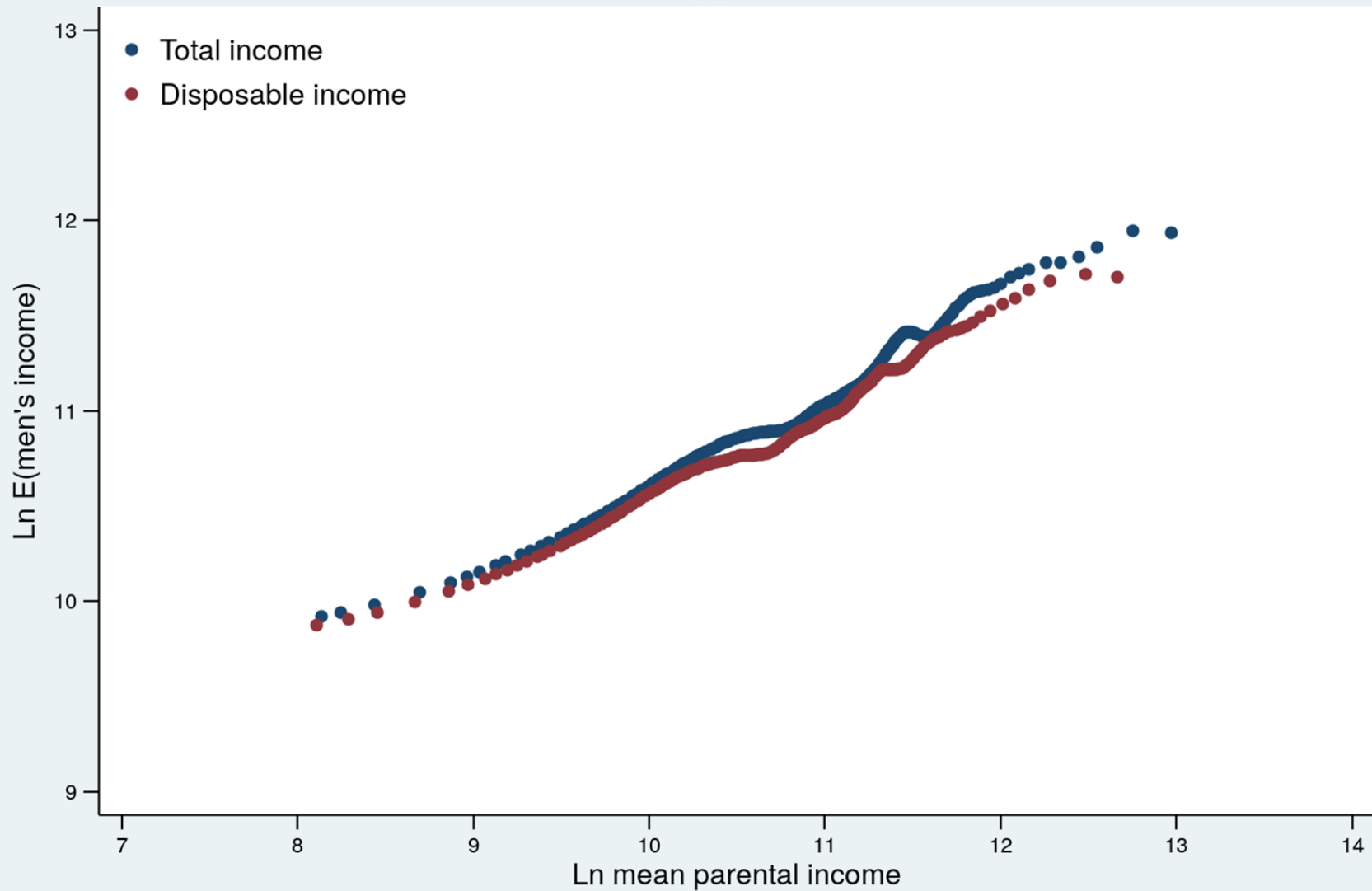
Figure 7: Log of women's expected total and disposable income as a function of log parental income
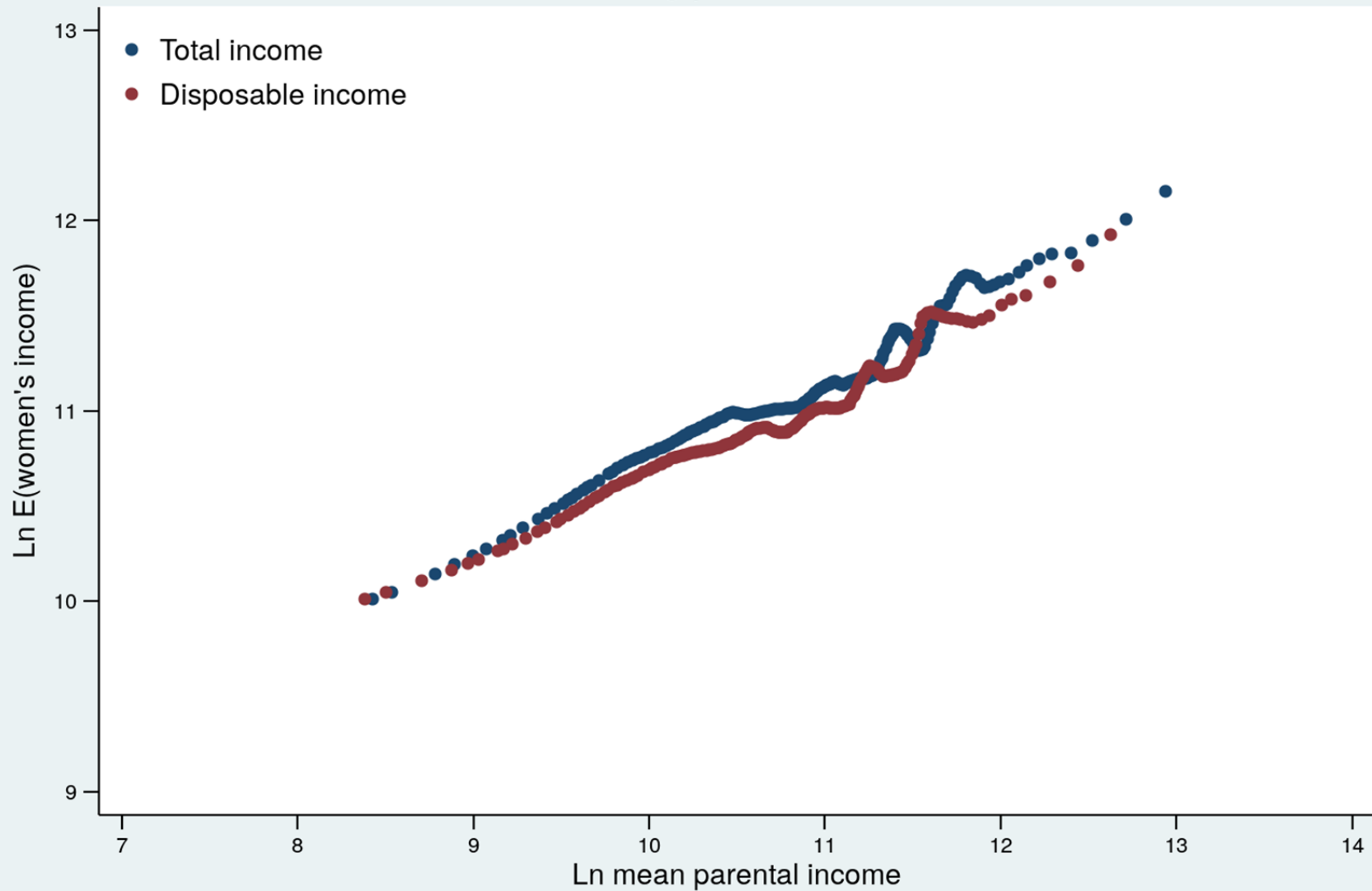
Figure 8: Log of expected earnings as a function of log parental disposable income
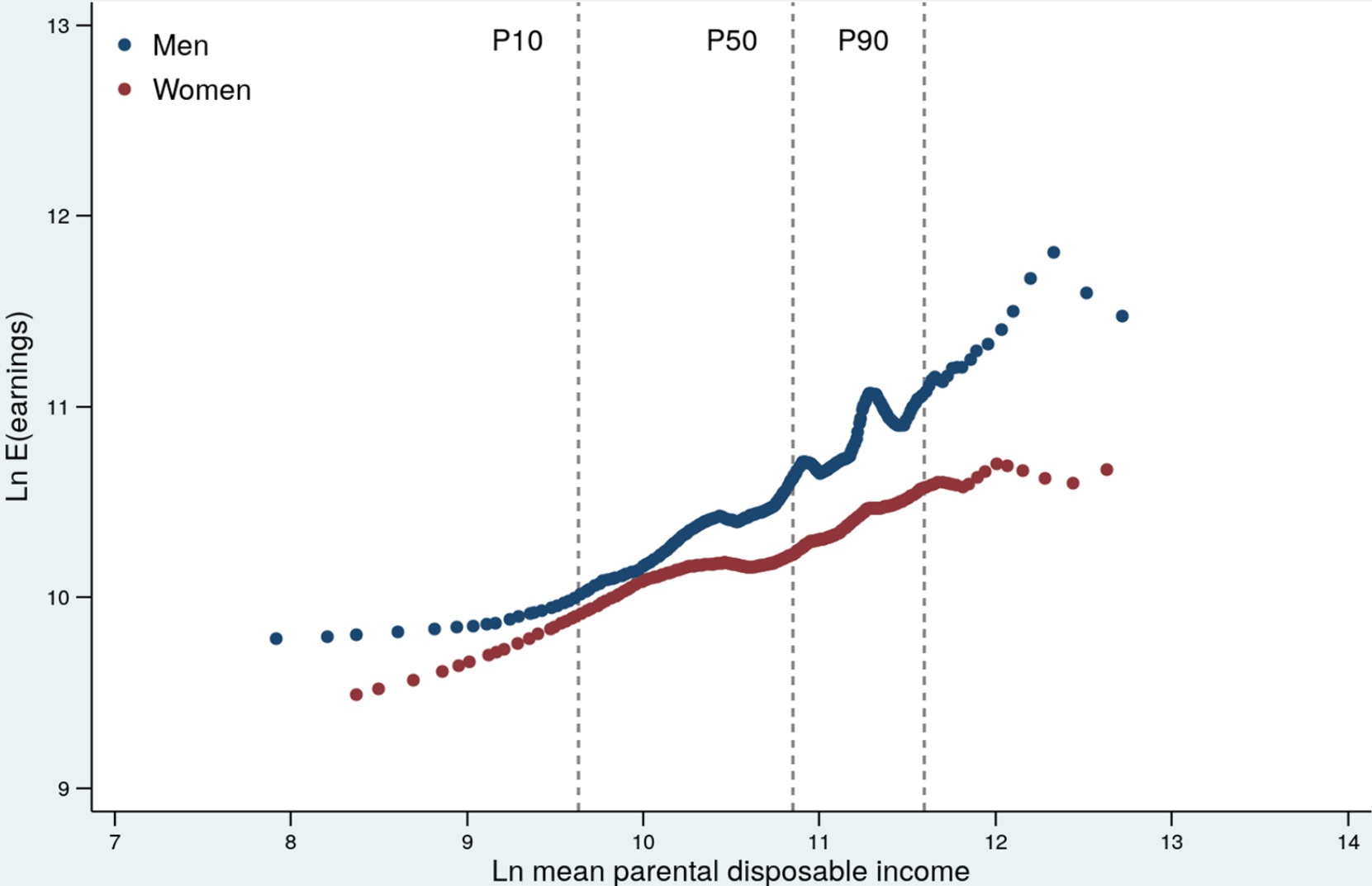
Figure 9: Probability of employment in 2010 as a function of log parental disposable income, men
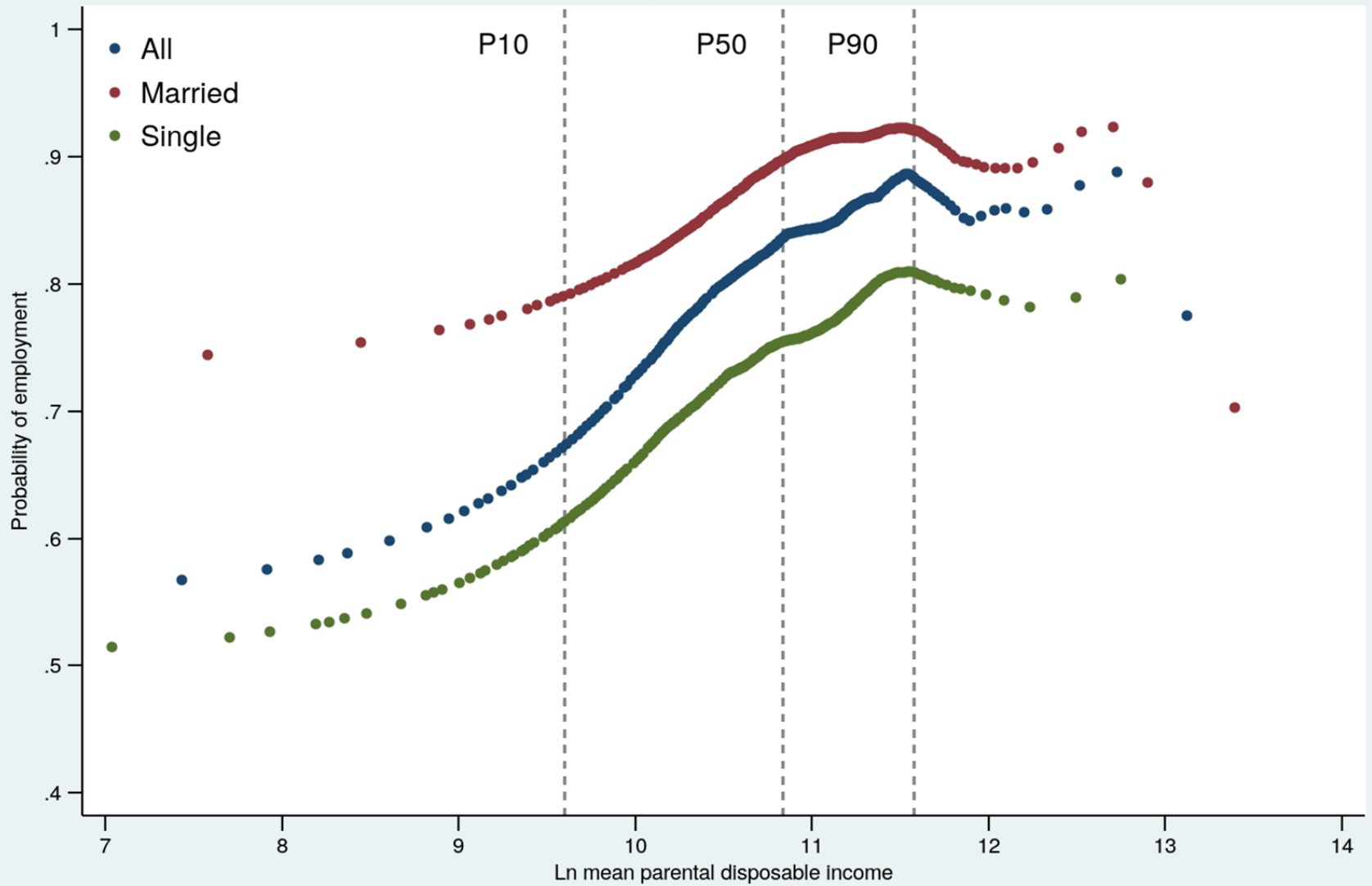
Figure 10: Probability of employment in 2010 as a function of log parental disposable income, women
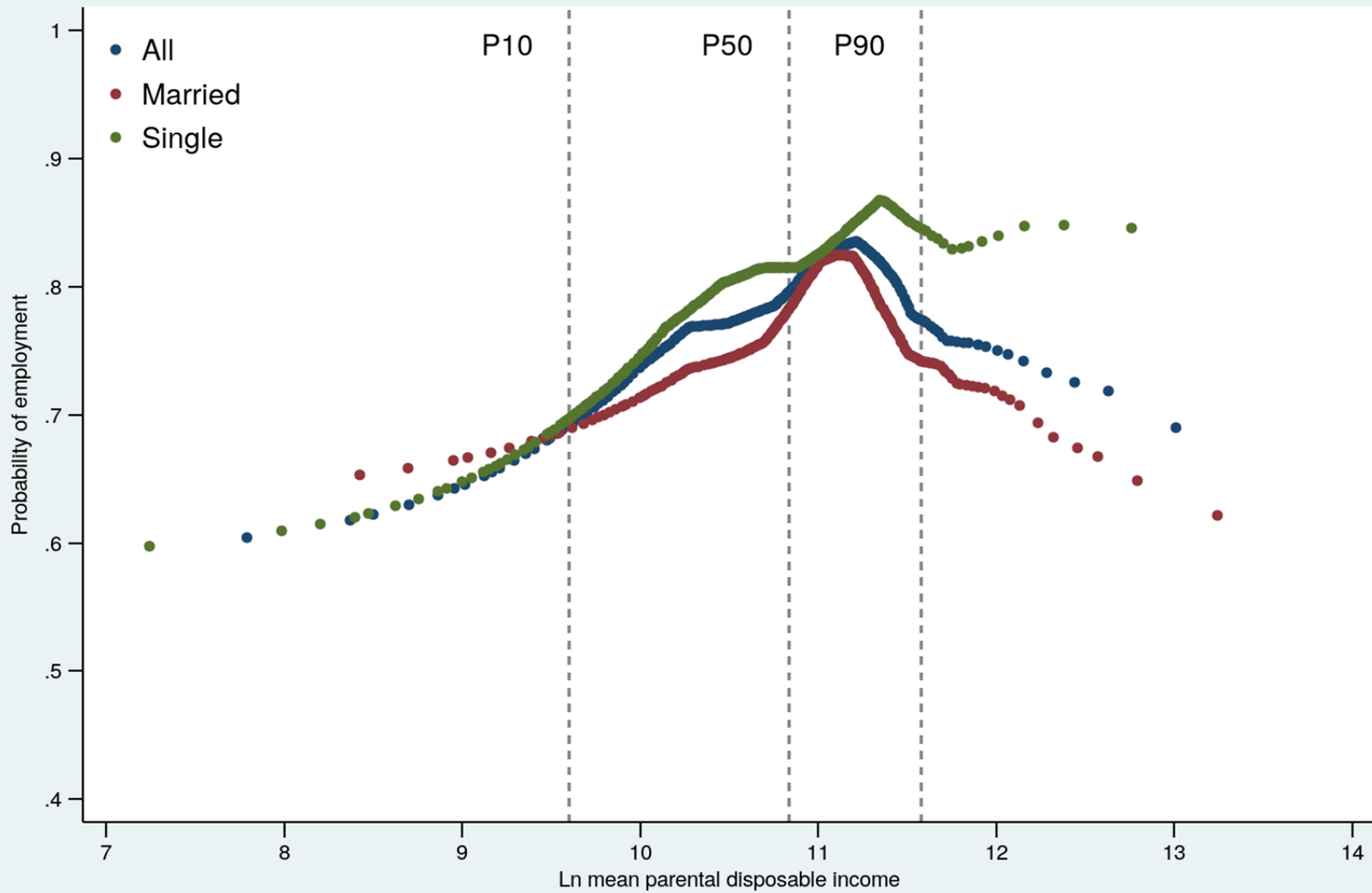
Figure 11: Log of percent married as a function of log parental disposable income, by gender
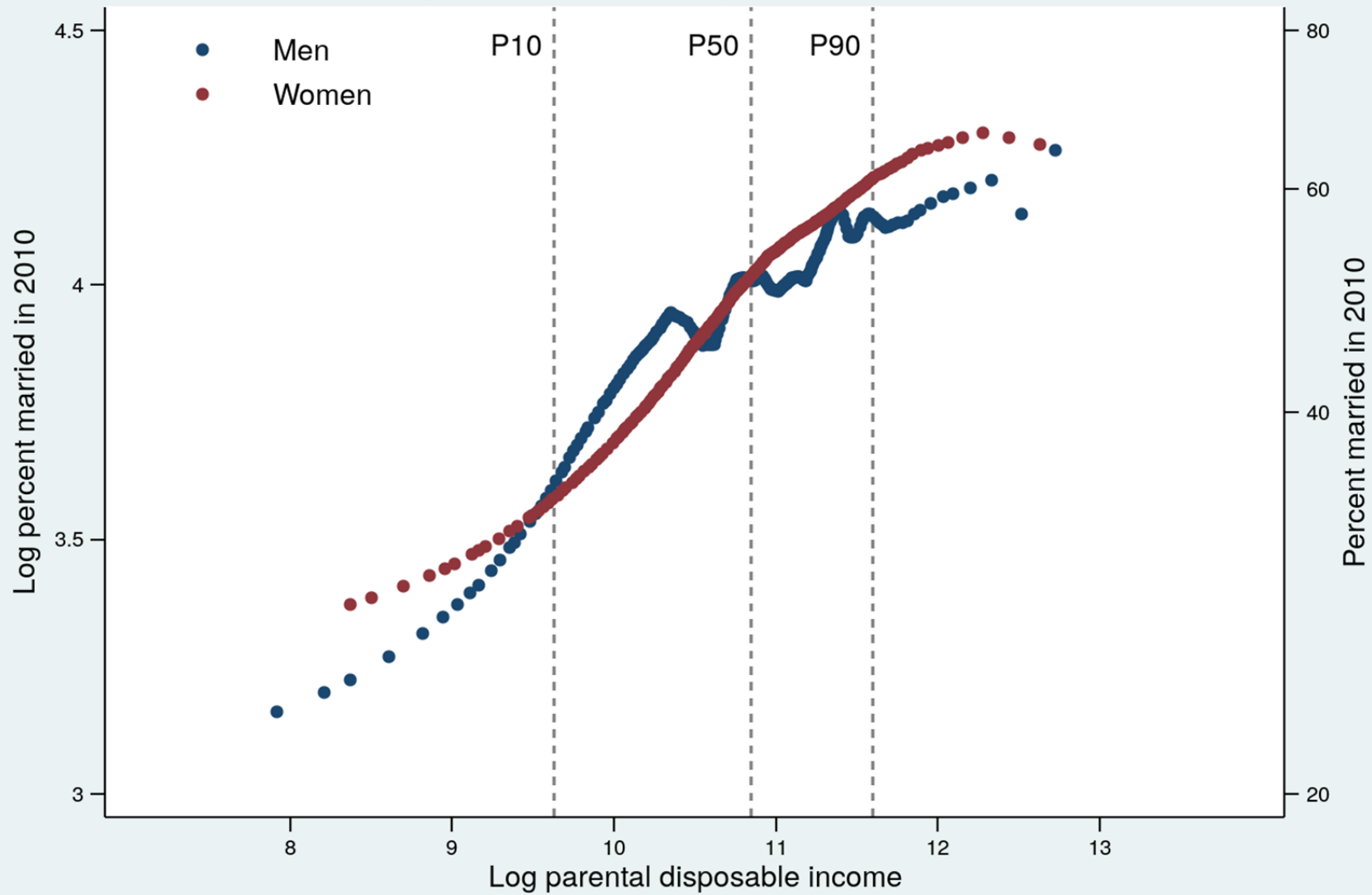
Figure 12: Log of expected earnings from spouse as a function of
log parental disposable income, married children
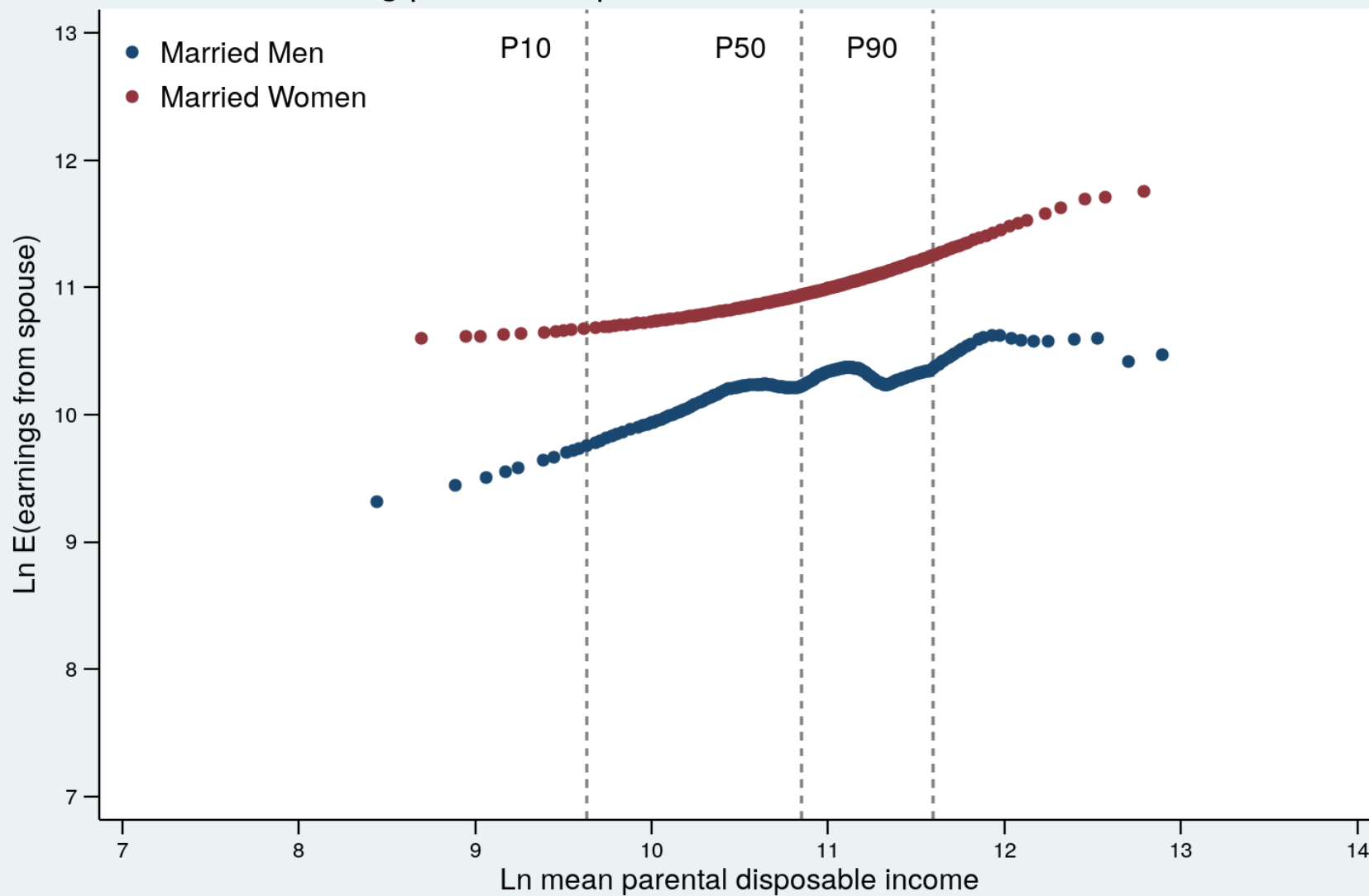
Figure 13: Log of expected earnings from spouse as a function of log parental disposable income, all children
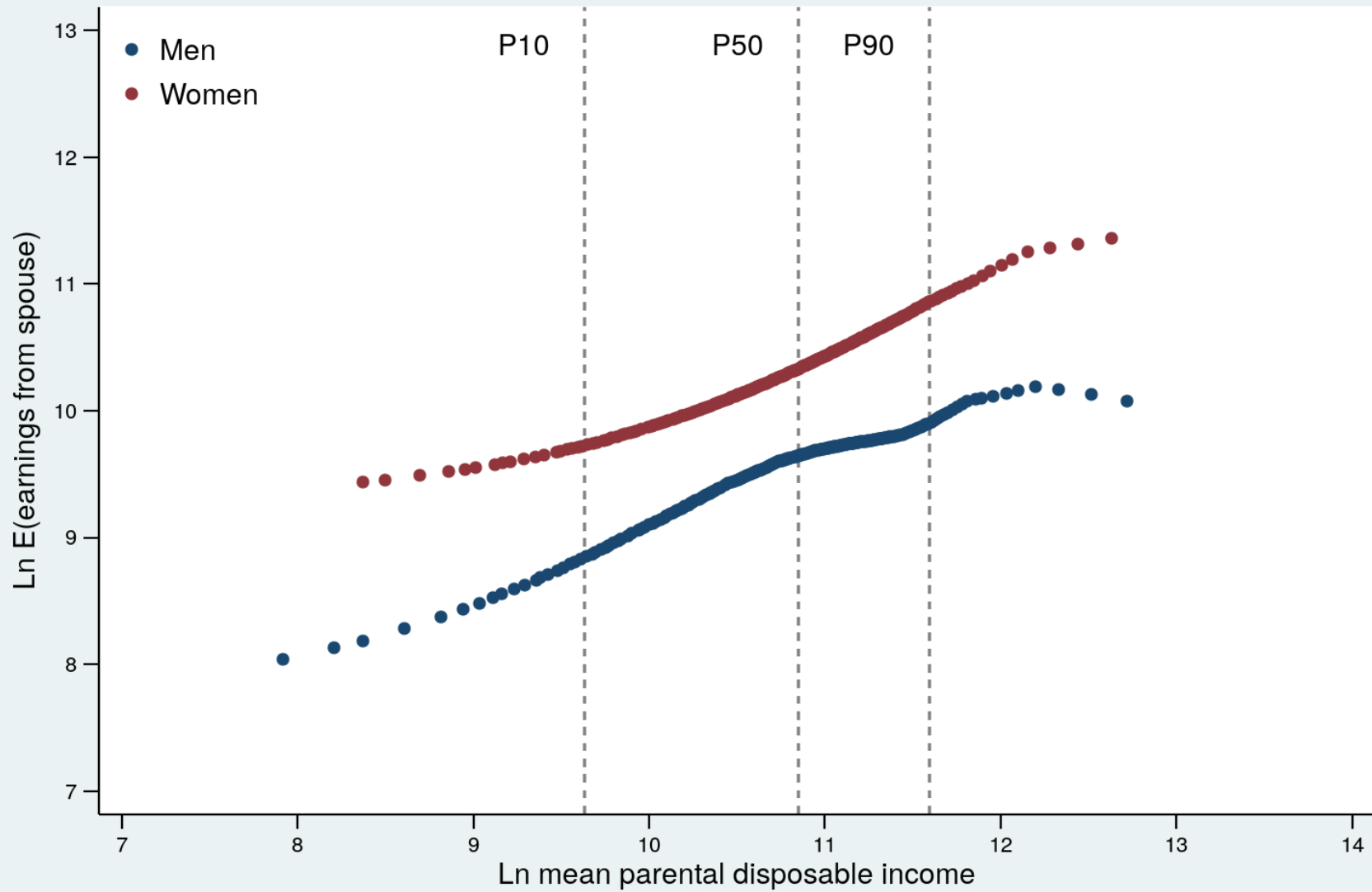
Figure A1: IGE$_e$ in late 30s as a function of years of parental information, men (common-sample approach)
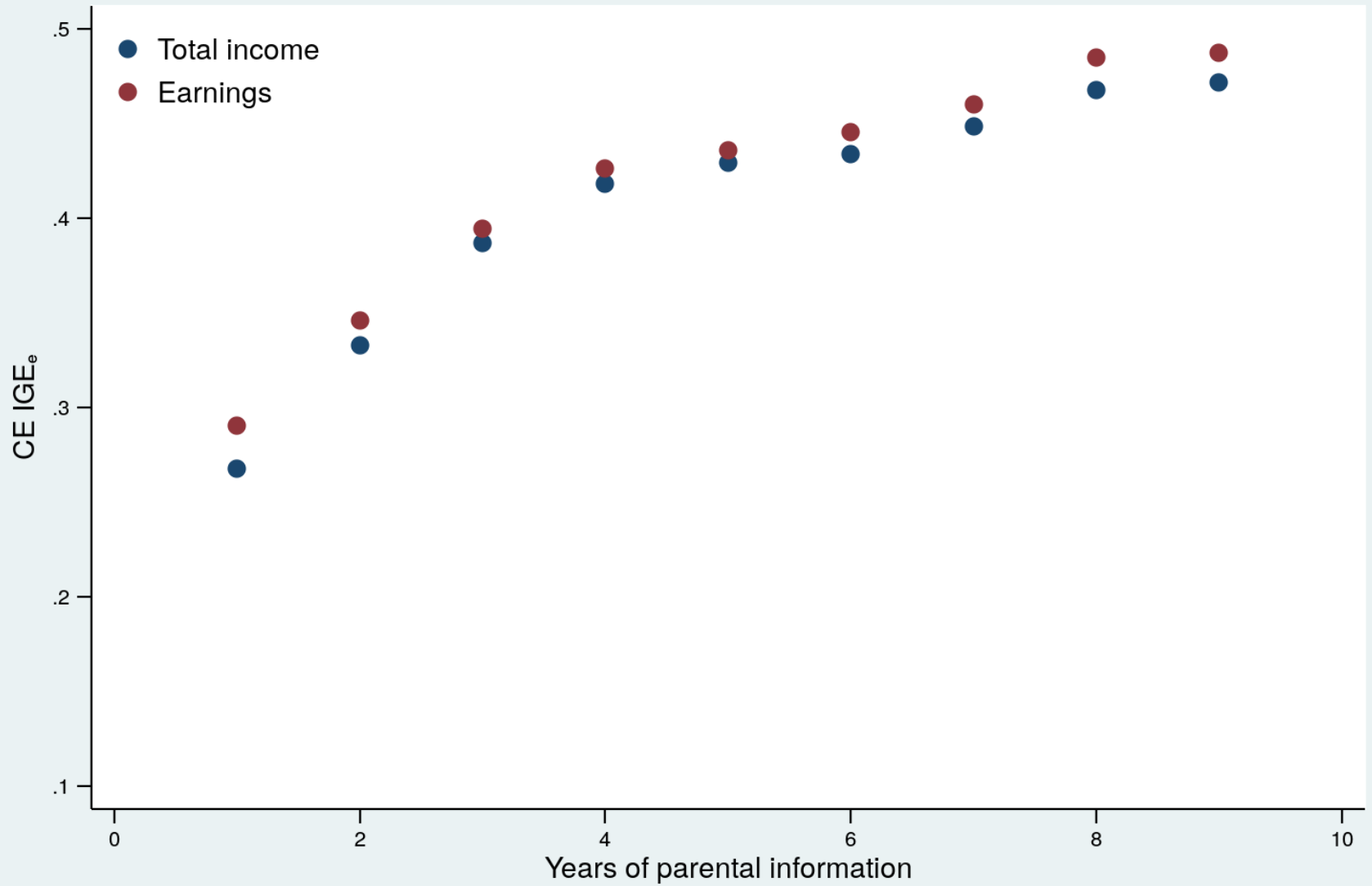
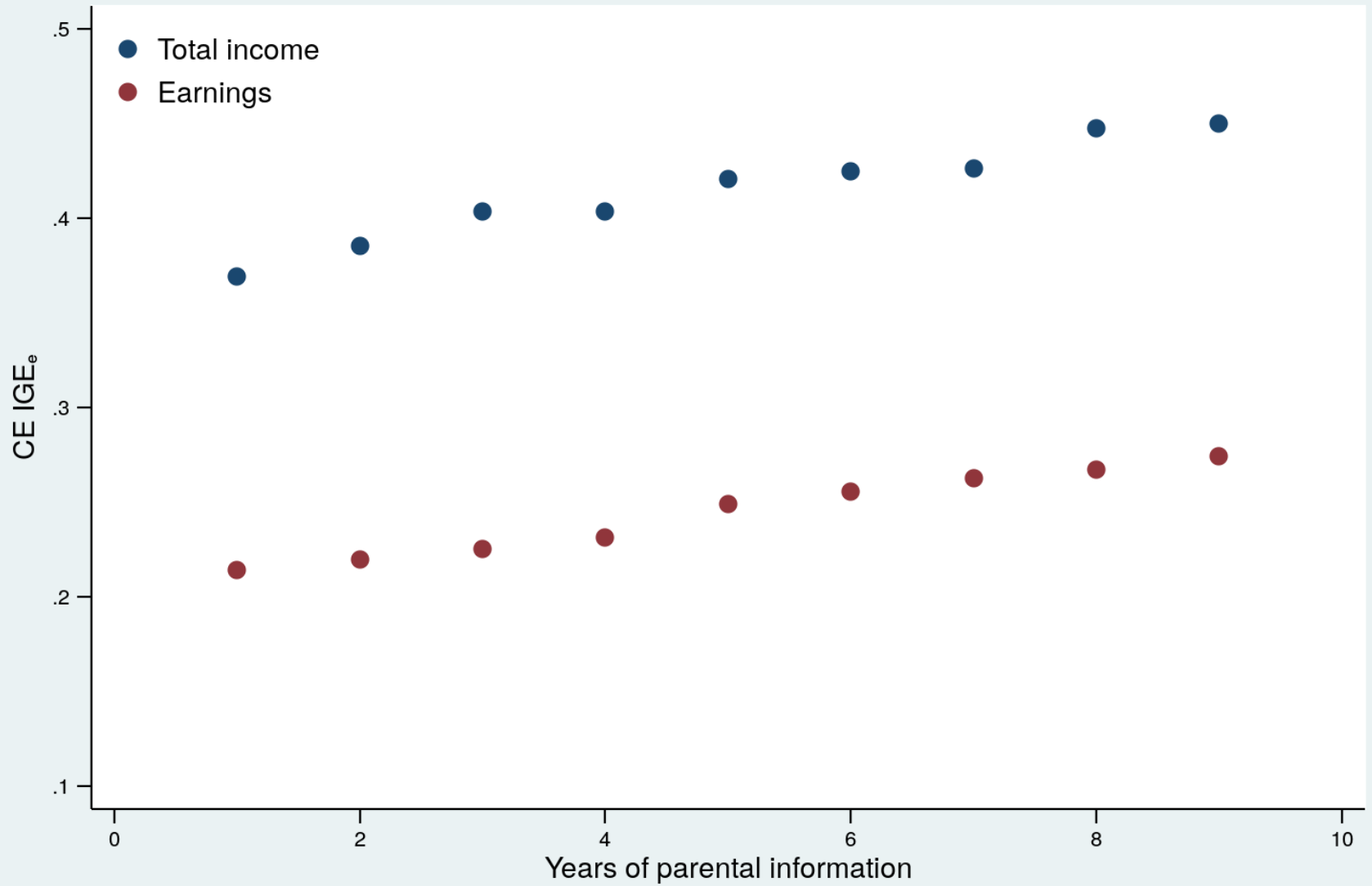Figure A2: IGE$_e$ in late 30s as a function of years of parental information, women (common-sample approach)
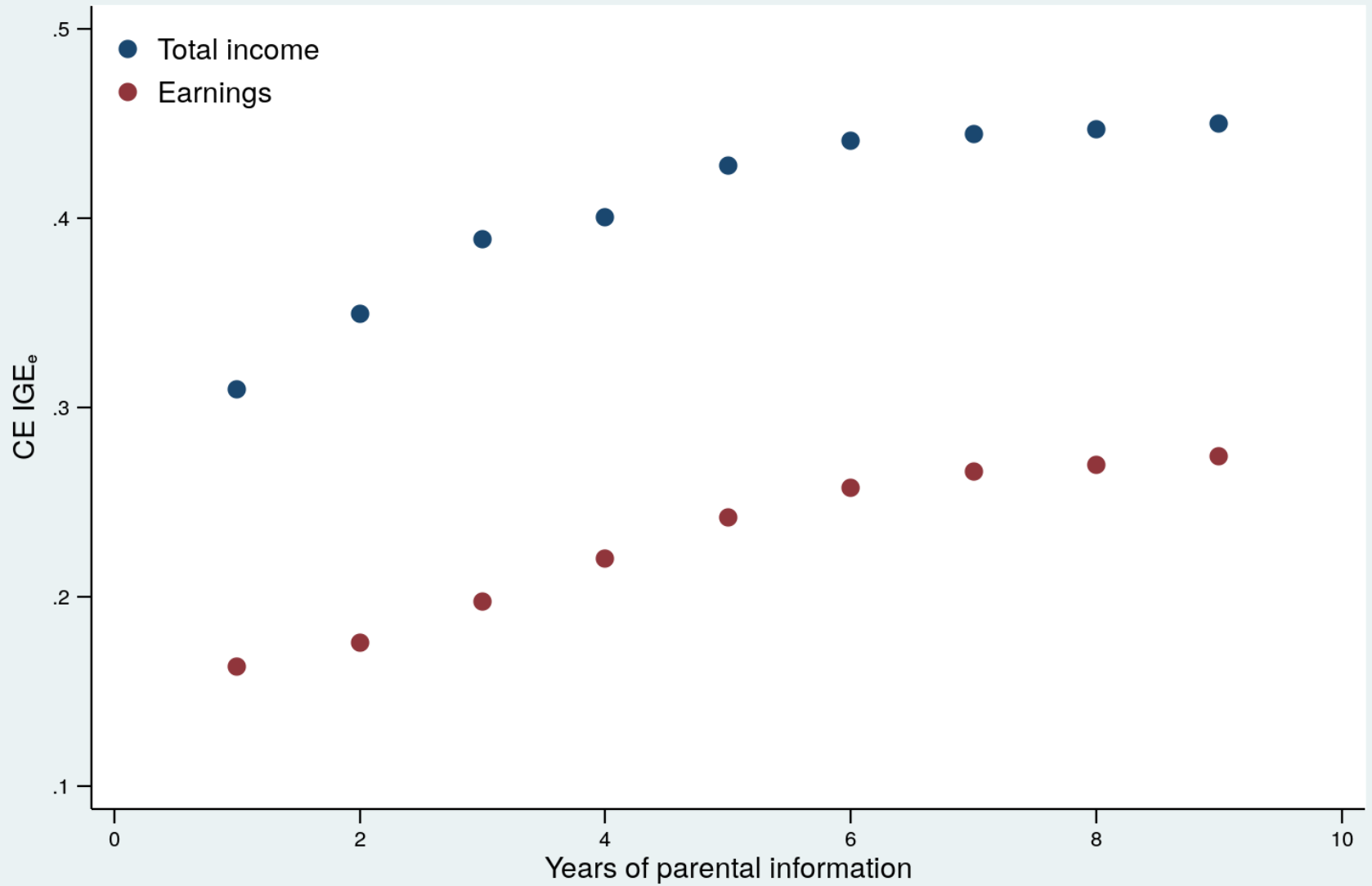
Figure A3: Total-income IGE_e in late 30s as a function of years of parental information, all children (common-sample approach)
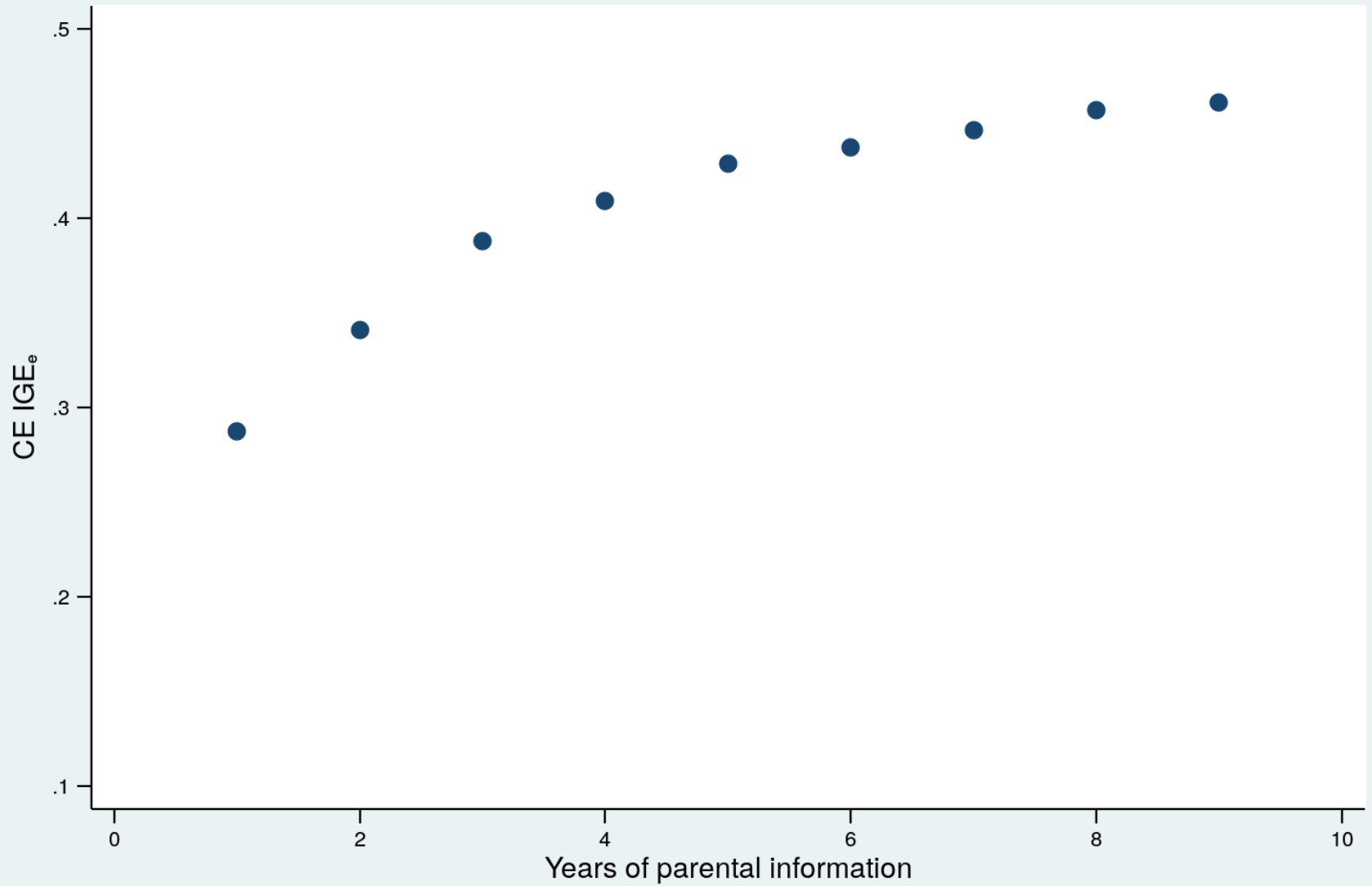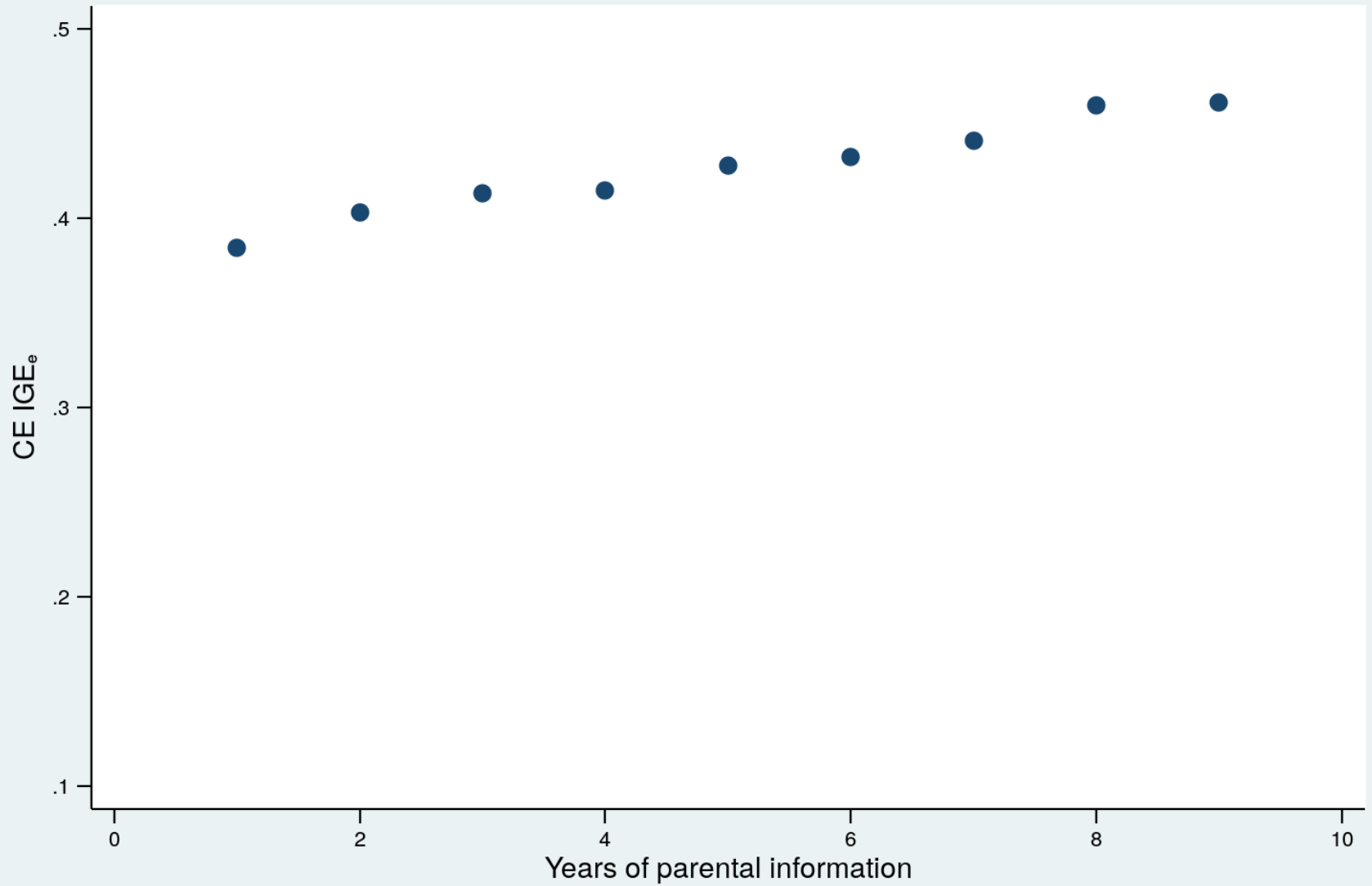
Figure A4: IGE$_e$ in late 30s as a function of years of parental information, men (common-rules approach)
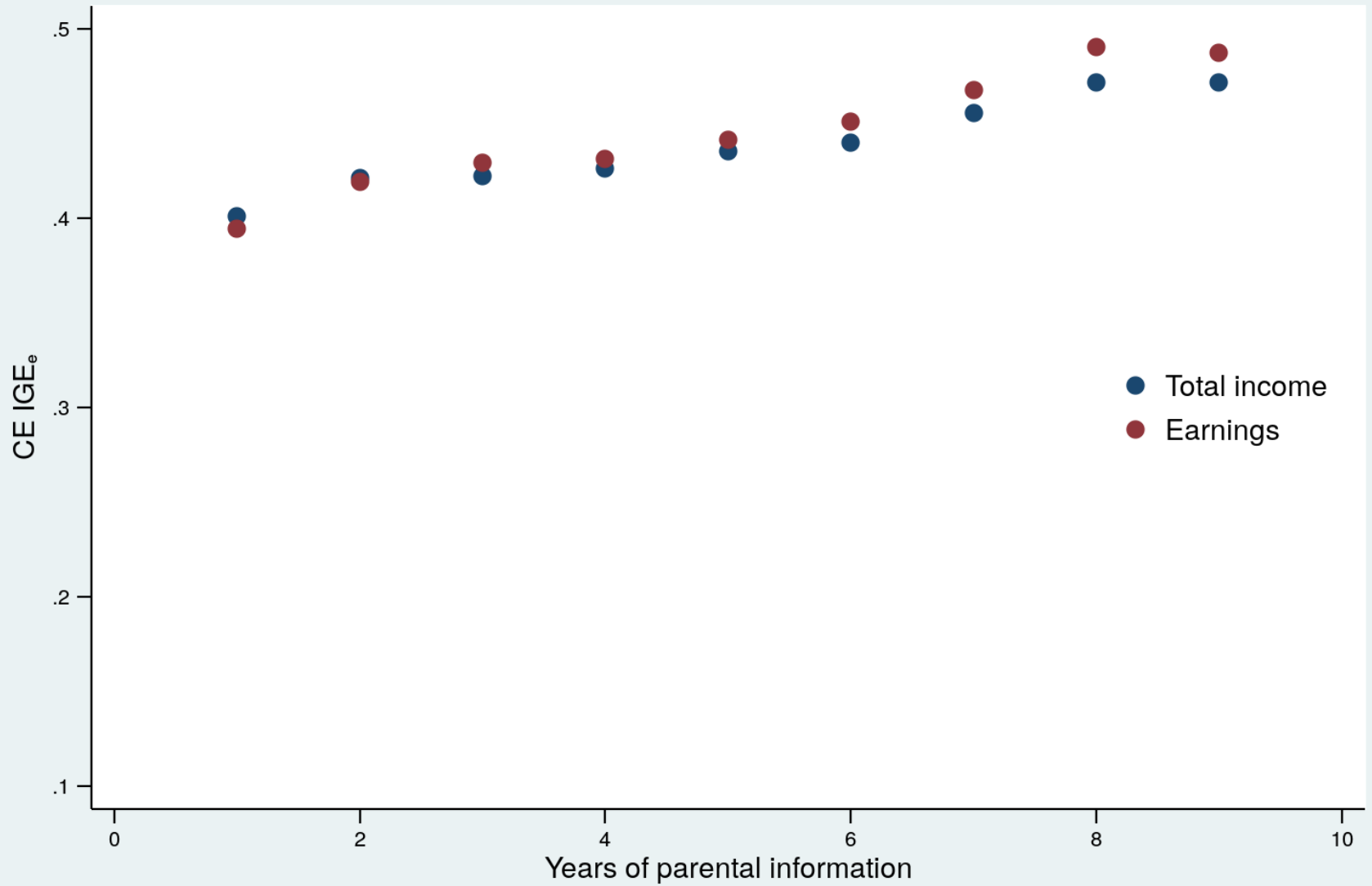
Figure A5: IGE$_e$ in late 30s as a function of years of parental information, women (common-rules approach)

Figure A6: Total-income IGE_e in late 30s as a function of years of parental information, all children (common-rules approach)

Figure A7: IGE$_e$ of men's earnings, 2001-2010
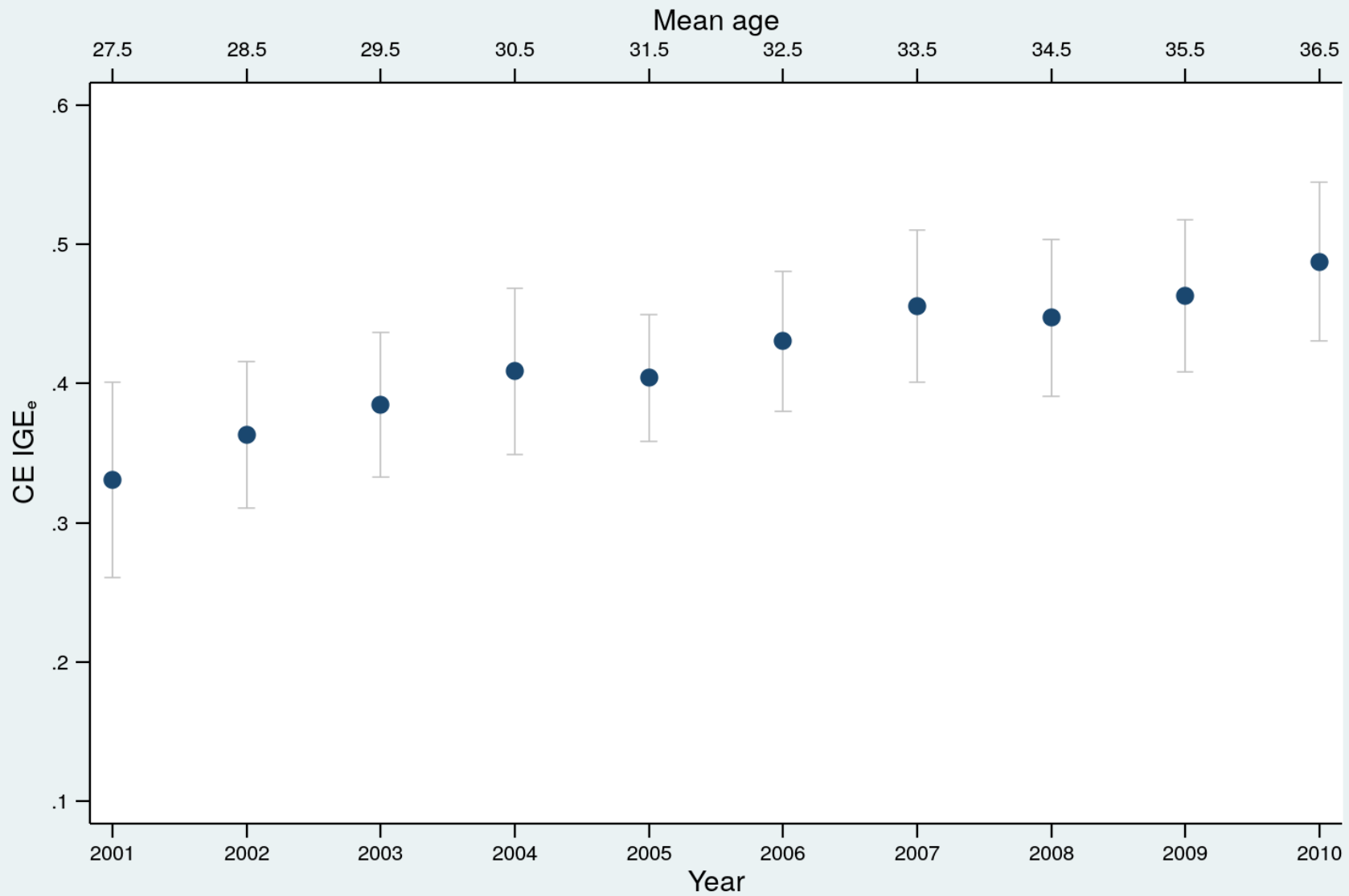
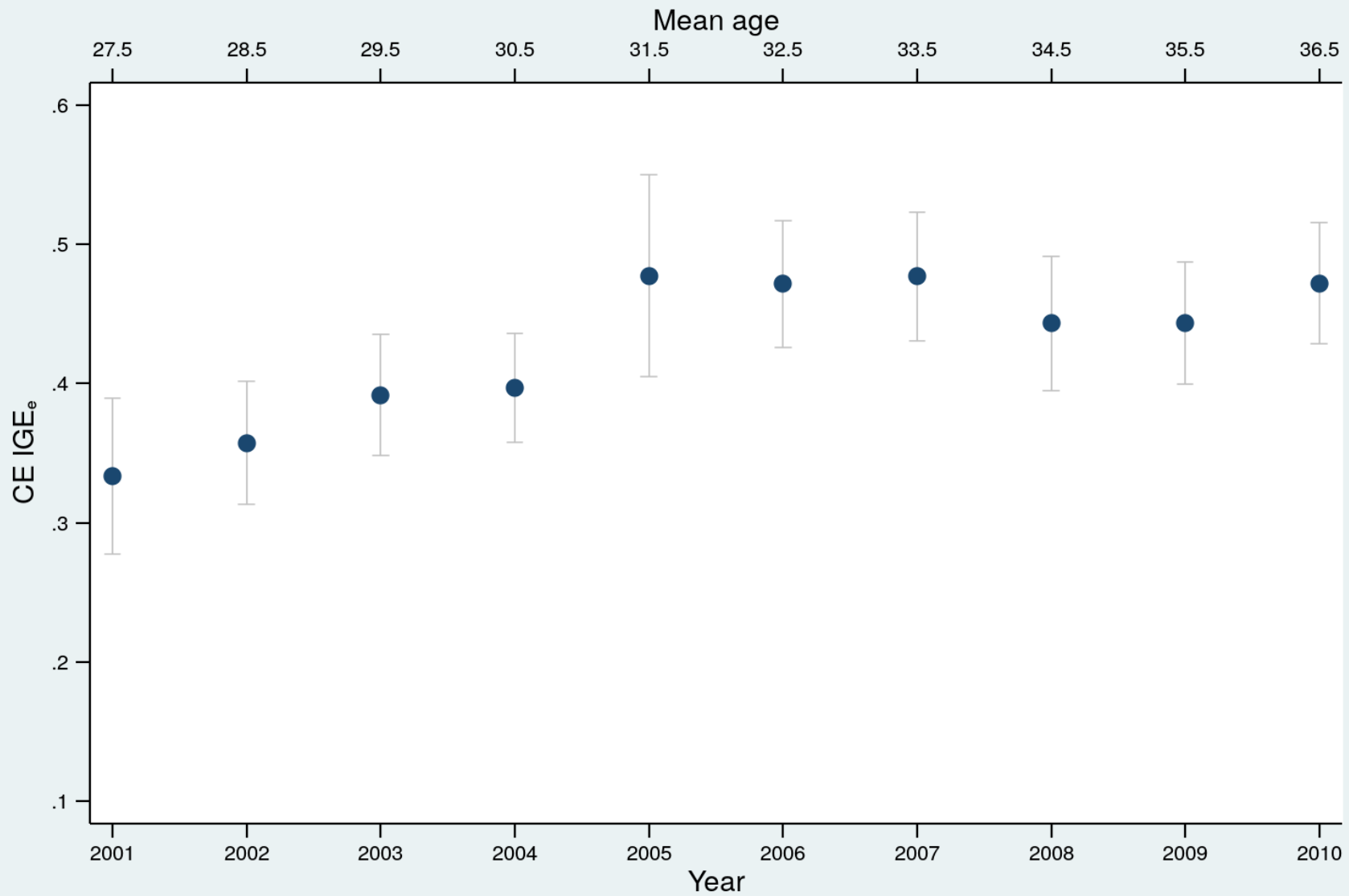Figure A8: IGE$_e$ of men's total income, 2001-2010
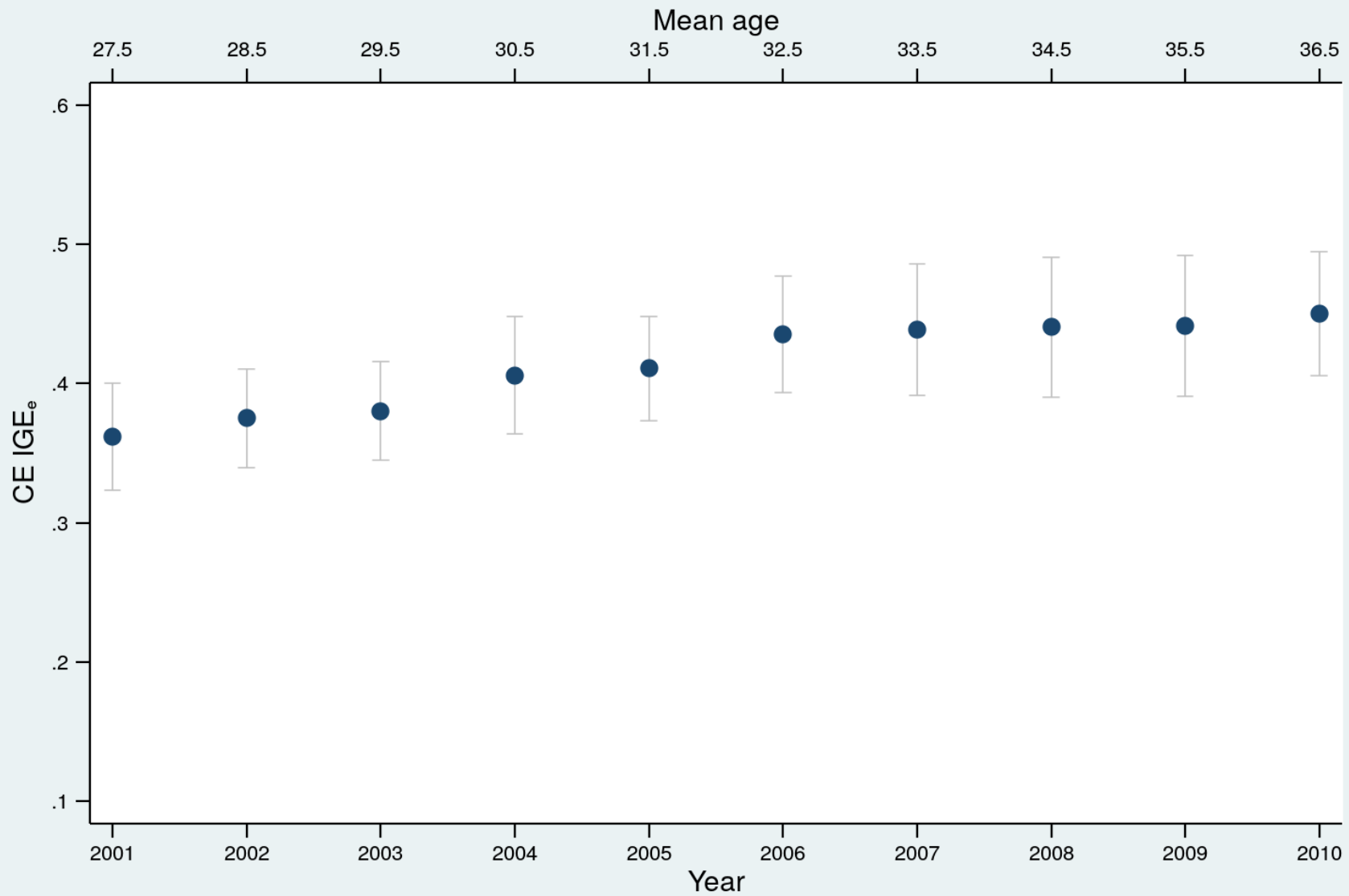
Figure A9: IGE$_e$ of women's total income, 2001-2010

Figure A10: IGE$_e$ of children's total income, 2001-2010