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Estimating the Intergenerational Elasticity and Rank Association in the US: Overcoming the Current Limitations of Tax Data

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Estimating the Intergenerational Elasticity and Rank Association in the US:

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Abstract:

Ideal estimates of the intergenerational elasticity (IGE) in income require a large panel of income data covering the entire working lifetimes for two generations. Previous studies have demonstrated that using short panels and covering only certain portions of the lifecycle can lead to considerable bias. I address these biases by using the PSID and constructing long time averages centered at age 40 in both generations. I find that the IGE in family income in the U.S. is likely greater than 0.6 suggesting a relatively low rate of intergenerational mobility in the U.S. I find similar sized estimates for the IGE in labor income. These estimates support the prior findings of Mazumder (2005a, b) and are also similar to comparable estimates reported by Mitnik et al (2015). In contrast, a recent influential study by Chetty et al (2014) using tax data that begins in 1996, estimates the IGE in family income for the U.S. to be just 0.344 implying a much higher rate of intergenerational mobility. I demonstrate that despite the seeming advantages of extremely large samples of administrative tax data, the age structure, and limited panel dimension of the data used by Chetty et al leads to considerable downward bias in estimating the IGE. I further demonstrate that the sensitivity checks in Chetty et al regarding the age at which children's income is measured, and the length of the time average of parent income used to estimate the IGE, are also flawed due to these data limitations. There are also concerns that tax data, unlike survey data, may not adequately reflect all sources of family income. Estimates of the rank-rank slope, Chetty et al's preferred estimator, are more robust to the limitations of the tax data but are also downward biased and modestly overstate mobility. However, Chetty et al's main findings of sizable geographic differences within the US in rank mobility, are unlikely to be affected by these biases. I conclude that researchers should continue to use both the IGE and rank based measures depending on their preferred concept of mobility. It also important for researchers to have adequate coverage of key portions of the lifecycle and to consider the possible drawbacks of using administrative data.

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I. Introduction

Inequality of opportunity has become a tremendously salient issue for policy makers across many countries in recent years. The sharp rise in inequality has given rise to fears that economic disparities will persist into future generations. This has led to a heightened focus on the literature on intergenerational economic mobility. This body of research, which is now several decades old, seeks to understand the degree to which economic status is transmitted across generations. A critical first step in understanding this literature and correctly interpreting its findings is having a sound understanding of the measures that are being used and what they do and do not measure. This paper focuses on two prominent measures of intergenerational mobility, the intergenerational elasticity (IGE), and the rank-rank slope, and discusses several key conceptual and measurement issues related to these estimators.

The IGE has a fairly long history of use in economics dating back to papers from the 1980s. It is generally viewed as a useful and transparent summary statistic capturing the rate of "regression to the mean". It can, for example, tell us how many generations (on average) it would take the descendants of a low income family to rise to the mean level of log income. In recent years many notable advances have been made in terms of measurement and issues concerning life-cycle bias (e.g. Jenkins, 1987; Solon, 1992; Mazumder, 2005a; Grawe, 2006; Böhlmark and Lindquist, 2006; Haider and Solon, 2006).[1] As a result of these contributions, most recent US estimates of the IGE in family income are generally around 0.5 or higher.[2]

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[1] Reviews of this literature can be found in Solon (1999) and Black and Devereaux (2011).

[2] Solon's (1992) estimate is 0.483. Hertz (2005) reports an IGE of 0.538. Hertz (2006) finds the IGE to be 0.58. Bratsberg et al (2007) estimate the IGE of family income on earnings to be 0.54. Jäntti et al's (2006) estimate of the same measure is 0.517. Mitnik et al's (2015) estimate of the standard IGE is between 0.55 and 0.74. Note that all of these studies (like Chetty et al, 2014) report some variant of the IGE with respect to *family income*. Of course, many other studies have used a different income concept such as labor market earnings.

Thus far, no study of intergenerational mobility in the US has yet been conducted that has used very long time averages of family income of parents and has also utilized averages of family income in both generations centered at age 40, where lifecycle bias is minimized.[3] This paper fills this void in the literature by using PSID data that meet these requirements. Using up to 15 year averages of income in the parent generation yields estimates of the IGE with respect to family income of sons that are greater than 0.6. I also find that the IGE with respect to the labor income of male household heads is greater than 0.6 and very similar to the estimate found by Mazumder (2005a) using social security earnings data.[4]

These results stand in stark contrast with the results in a recent highly influential study by Chetty et al (2014), who use large samples drawn from IRS tax records and produce estimates of the IGE in family income of just 0.344 suggesting significantly greater intergenerational mobility. Furthermore, Chetty et al argue that none of the previous biases identified in the literature on IGE estimation apply to their data. Given the importance of the IGE as one of the key conceptual measures of intergenerational mobility, it is worth revisiting the measurement issues in the context of their sample. This exercise is not only useful for revisiting the specific results of Chetty et al, but also holds more general lessons for other research seeking to exploit administrative data to measure intergenerational mobility.

The IRS-based intergenerational sample used by Chetty et al is fundamentally limited in a few key respects that ultimately stems from the fact that the data only begins in 1996. First, children's income is only measured in 2011 and 2012. This is at a relatively early point in the

[3] The closest is Mitnik et al (2015) who use 9-years of parent income and children between the ages of 35 and 38.
[4] Chetty et al (2014) suggest that the high estimates in Mazumder (2005a) are solely due to data imputations of fathers' SSA earnings that are topcoded in some years and are not the result of using longer-time averages of father earnings. Below, I reiterate arguments against that claim that were originally discussed in Mazumder (2005a) but subsequently ignored by Chetty et al 2014 in their Appendix E discussion. I also point to other studies in the literature that are supportive of the findings in Mazumder (2005a). It is notable that this study yields similar estimates to Mazumder (2005a) while requiring no imputations of income.

life cycle for cohorts born between 1980 and 1982 (ages 29 to 32) and during a period when unemployment was quite high in the US. This age range is one in which we would expect substantial life cycle bias in producing IGE estimates (Haider and Solon, 2006). Moreover, relative to a more ideal data structure, where cohorts of children could be chosen such that they were observed over the 31 years spanning the ages of 25 to 55, Chetty et al are limited to using only 6 percent of the lifecycle. Second, parents' income is also measured for only a short period (5 years) covering just 16 percent of the lifecycle and at a relatively late period in life. Roughly 25% of observations of fathers' income in their sample are measured at age 50 or higher. The literature has shown that starting around the age of 50 a substantial share of the variance in income is due to transitory fluctuations. This leads to substantial attenuation of the IGE relative to what would be found if one used lifetime income for the parents (Haider and Solon, 2006; Mazumder, 2005a). Third, recent research has established that administrative data can sometimes lead to worse measurement error than survey data, particularly at the bottom end of the income distribution (e.g. Abowd and Stinson, 2013 and Hokayem et al., 2012, 2015).

It is important to make it very clear that the main focus of Chetty et al is not their national estimates of the IGE. Instead, the authors make an important contribution to the literature by producing the first estimates of a different measure of mobility, rank mobility, at a very detailed level of U.S. geography. Notably, they provide evidence of substantial heterogeneity across the U.S. As I discuss below, the biases that affect their national estimates of the IGE likely have little effect on their main conclusions regarding geographic differences.

The limitations of the tax data for intergenerational analysis can be sharply contrasted with the PSID sample used in this paper. In the PSID sample, family income is observed in both generations over a vastly larger portion of the lifecycle and the time averages are *centered* over the prime working years in both generations. I estimate the IGE using this closer to "ideal" sample and then show how the estimates change if I impose the same kinds of data limitations that exist in the IRS data. The results show that the data limitations lead to IGE estimates that are roughly half the size of the estimates with the complete data and similar in magnitude to the estimates of Chetty et al. A very similar pattern of results is also found by Mitnik et al (2015).[5]

Chetty et al also find the IGE to be very sensitive to how they choose to impute the income of children who report no family income during 2011 and 2012. However, it is the limited panel dimension of their data and their reliance on administrative data which makes their analysis susceptible to this problem. Had they been able to observe the income of children during later periods of the lifecycle and other sources of income, then such imputation becomes unnecessary.[6] This is important because it is their concern about the robustness of the IGE that led Chetty et al to using rank-based estimators.[7] This contrasts with other studies that have also used rank-based measures to study intergenerational mobility but for conceptual reasons.[8]

Given the recent shift in the literature to using rank-based measures, it is useful to distinguish the measurement concerns with the IGE from the conceptual differences between the two estimators. In short, both measures can provide useful insights about different mobility concepts. Since certain questions are best answered by the IGE, researchers should continue to use that estimator as at least one tool in their arsenal. Nevertheless, rank-based estimators are also valuable. In addition to providing information on a different concept of mobility, positional

[5] Mitnik et al (2015) use IRS data that begins in 1987 enabling children to be observed into their late 30s and for parent income to be measured over 9 years. Not only do Mitnik et al also produce similar sized estimates to the PSID results when using a comparable methodology (0.55 to 0.74), but they also show that they can match the Chetty et al estimates if they restrict their analysis to 29-32 year olds and use five year averages of parent income.
[6] Mitnik et al (2015) introduce a new approach to estimating the IGE that enables them to overcome this sensitivity to years of missing income when using a small window to measure child income. I discuss this in section 3.
[7] Dahl and Deliere (2008) also shift to rank based measures based on concerns regarding the robustness of the IGE but their concerns revolve around a different measurement issue than Chetty et al which I discuss in section 3.
[8] See Bhattacharya and Mazumder (2011), Corak et al (2014), Mazumder (2014), Davis and Mazumder (2015) and Bratberg et al, 2015.

mobility, rank-based measures are also useful for distinguishing upward versus downward movements, making subgroup comparisons, and for identifying nonlinearities. I would argue that even if Chetty et al had found the IGE to be perfectly robust in their tax data, it would still be preferable to use rank-mobility measures to understand geographic differences. This is because an IGE estimated in, say, Charlotte, North Carolina would only be informative about the rate of regression to the mean income i*n Charlotte*. If ranks are fixed to the national distribution, then rank mobility measures enable a more meaningful comparison across cities.

Finally, I use the PSID to estimate the rank-rank slope. The estimates (0.4 or higher) are only moderately larger than what is found with the IRS data (0.341) or what is found with the PSID data when imposing the tax data limitations. Although the rank-rank slope may be more robust to the data limitations of the IRS sample than the IGE, it is still not perfect and suggests that the rate of intergenerational mobility even by rank-based measures may be overstated by the tax data. This is broadly in line with findings for Sweden (Nybom and Stuhler, 2015). In the future as the panel length of US tax data increases, these biases will recede in importance. However, it is uncertain whether researchers will be able to obtain tax data in future decades.

I conclude that researchers should continue to use the IGE if that is the conceptual parameter of interest. Even when the ideal data is not available, researchers can still attempt to assess the extent of the bias based on prior research. The rest of the paper proceeds as follows. Section 2 describes conceptual differences between the IGE and the rank-rank slope. Section 3 discusses measurement issues with the IGE and outlines an "ideal" dataset. It then compares this ideal dataset with Chetty et al's IRS-based sample and samples that can be constructed with publicly available PSID data. Section 4 describes the PSID data. Section 5 presents the main results and Section 6 concludes.

II. Conceptual Issues

The concept of regression to the mean over generations has a long and notable tradition going back to the Victorian era social scientist Sir Francis Galton who studied, among other things, the rate of regression to the mean in height between parents and children. Modern social scientists have continued to find this concept insightful as a way of describing the rate of intergenerational persistence in a particular outcome and to infer the rate of mobility as the flip side of persistence. In particular, economists have focused on the intergenerational elasticity (IGE). The IGE is the estimate of β obtained from the following regression:

$$y_{1i} = \alpha + \beta y_{0i} + \varepsilon_i \tag{1}$$

where y_{1i} is the log income of the child's generation and y_{0i} is the log of income in the parents' generation.[9] The estimate of β provides a measure of intergenerational persistence and 1 - β can be used as a measure of mobility. For simplicity, if we assume that the intergenerational relationship actually follows a simple autoregressive process then one can use β to extrapolate how long it would take for gaps in log income between families to recede.[10] For example, consider a family whose log annual income is around 9.8 ($18,000). We might be interested in knowing roughly how many generations it would take (on average) for the descendants of this family's log income to be within 0.05 of the national average log income of 11.2 ($73,000). If for example, the IGE is around 0.60 as claimed by Mazumder (2005a) then it would take 7 generations (175 years). On the other hand if the IGE is around 0.34 as claimed by Chetty et al

[9] Often the regression will include age controls but few other covariates since β is not given a causal interpretation but rather reflects all factors correlated with parent income

[10] Recent research has cast doubt on the simple AR(1) model arguing that there may be independent effects emanating from prior generations such as grandparents and great-grandparents (Lindahl et al; 2014). Nevertheless, the AR(1) assumption provides a useful first approximation and conveys the general point about why the magnitude of the estimates might matter.

(2014) then it would take just 4 generations. Clearly, the two estimates have profoundly different implications on the rate of intergenerational mobility by this metric. If the rate of regression to the mean is, in fact, what we are interested in knowing, then the IGE is what we ought to estimate. For example, some papers find that the IGE is particularly useful for calibrating structural models of interest (e.g. DeNardi and Yang, 2015, Lee and Seshadri, 2005) The concept of regression to the mean is also widely used in other aspects of economics such as the macroeconomic literature on differences in per-capita income across countries (e.g. Barro and Sala-i-Martin, 1992).

The rank–rank slope on the other hand is about a different concept of mobility, namely *positional* mobility. For example, a rank-rank slope of 0.4 suggests that the expected difference in ranks between the adult children of two different families would be about 4 percentiles if the difference in ranks among their parents was 10 percentiles. How are the two measures related? Chetty et al (2014) point out that that the rank-rank slope is very closely related to the intergenerational correlation (IGC) in log income. They and many others have also shown that the IGE is equal to the IGC times the ratio of the standard deviation of log income in the child's generation to the standard deviation of log income in the parents' generation:

$$IGE = IGC\,\frac{\sigma_{y1}}{\sigma_{y0}} \qquad (2)$$

This relationship is sometimes taken to imply that a rise in inequality would lead the IGE to rise but not affect the IGC and that therefore, the IGC may be a preferred measure that avoids a "mechanical" effect of inequality. By extension one might also prefer the rank-rank slope if one accepts this argument. Several comments are worth making here. First, in reality the parameters are all jointly determined by various economic forces. In the absence of a structural model one cannot meaningfully talk about holding "inequality" fixed. For example, a change in

β might cause inequality to rise, rather than the reverse, or both might be altered by some third force such as rising returns to skill. The mathematical relationship shown in (2) does not substitute for a behavioral relationship and so we cannot truly isolate forces driving inequality from the IGE. Second, even if it was the case that the IGC or rank-rank slope was a measure that was "independent of inequality", that doesn't mean that society shouldn't continue to be interested in the rate of regression to the mean. It may well be the case that it is precisely because of the rise in inequality that societies are increasingly concerned about intergenerational persistence and so incorporating the effects of inequality may actually be critical to understanding the rates of mobility that policy makers want to address. Mitnik et al (2015) for example, argue in favor of the IGE precisely because it incorporates distributional changes.

In addition to providing useful information about positional mobility, the rank-rank slope has other attractive features. Perhaps its' most useful advantage over the IGE is that it can be used to measure mobility differences across subgroups of the population with respect to the national distribution. This is because the IGE estimated within groups is only informative about persistence or mobility with respect to the *group specific mean* whereas the rank-rank slope can be estimated based on ranks calculated based on the national distribution. Chetty et al (2014) were able to use this to characterize mobility for the first time at an incredibly fine geographic level. Mazumder (2014) used other "directional" rank mobility measures to compare differences in intergenerational mobility between blacks and whites in the U.S. However, for characterizing intergenerational mobility at the *national level* both the IGE and the rank-rank slope are suitable depending on which concept of mobility a researcher is interested in studying.

III. Measurement Issues and the Ideal Intergenerational Sample

Measurement Issues

The literature on intergenerational mobility has highlighted two key measurement concerns that I briefly review. The first issue is attenuation bias that arises from measurement error or transitory fluctuations in parent income. In an ideal setting the measures of y_1 and y_0 in equation (1) would be measures of lifetime or permanent income, but in most datasets we only have short snapshots of income that can contain noise and attenuate estimates of the IGE. Solon (1992) showed that using a single year of income as a proxy for lifetime income of fathers can lead to considerable bias relative to using a 5 year average of income. Using the PSID, Solon concluded that the IGE in annual labor market earnings was 0.4 "or higher". Mazumder (2005a) used the SIPP matched to social security earnings records and showed that using even a 5-year average can lead to considerable bias and estimated the IGE in labor market earnings to be around 0.6 when using longer time averages of fathers earnings (up to 16 years). Mazumder argues that the key reason that a 5-year average is insufficient is that the transitory variance in earnings tends to be highly persistent and appeals to the findings of U.S. studies of earnings dynamics that support this point. Using simulations based on parameters from these other studies, Mazumder shows that the attenuation bias from using a 5-year average in the data is close to what one would expect to find based on the simulations. In a separate paper that is less well known, Mazumder (2005b) showed that if one uses short term averages in the PSID and uses a Hetereoscedastic Errors in Variables (HEIV) estimator that adjusts for the amount of measurement error or transitory variance contained in each observation, then that the PSID adjusted estimate of the IGE is also around 0.6.

This latter paper is a useful complement because unlike the social security earnings data used by Mazumder (2005a) the PSID data is not topcoded and doesn't require imputations. Chetty et al (2014) has contended that the larger estimates of the IGE in Mazumder (2005a) were due to the nature of the imputation process rather than due to larger time averages of fathers' earnings. Specifically, in cases where earnings were above the social security taxable maximum they were imputed by using the mean earnings level by race and education level from other data sources. Mazumder acknowledges that this moves a step in the direction towards "instrumenting" for fathers earnings based on demographic characteristics but argues that it is not obvious that this imparts an upward bias and may well lead to a downward bias.[11] Mazumder also shows that when using up to 7 year averages and dropping fathers who are ever topcoded, which is about half of the sample, that the resulting IGE of 0.439 (N=1144) is not very different from the IGE of 0.472 (N=2240) for the full sample. Mazumder further argues that this robustness check of dropping fathers who are ever topcoded, may impart a downward bias due to a potential selection effect of eliminating father son-pairs whose IGE may be higher because they are selected from the top of the income distribution.[12] In any event, a number of other studies in addition to Mazumder (2005b), that also do not require imputed data, and in some cases use administrative tax data, demonstrate that longer time averages lead to substantially higher IGE estimates. These studies include: Nilsen et al (2012); Gregg et al (2013); Mazumder and Acosta, 2014; and Mitnick et al, 2015.

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[11] See footnote 13 in Mazumder (2005a). That footnote explains why in the presence of lifecycle bias, an IV estimate of the IGE for sons who are younger than 40 and fathers who are older than 40 leads to downward bias. The mean age of sons in Mazumder (2005a) is 32 and the mean age of fathers in 1984 is 47. Chetty et al (2014) ignore this point when they discuss Mazumder (2005a) in their Appendix E.

[12] Mitnik et al (2015), for example, find that the IGE is higher at the upper half of the income distribution. Chetty et al (2014) in their Appendix E do not address this selection argument in their discussion of Mazumder's Table 6 and imply that the results of the robustness check are explained by an upward bias due to IV. Mazumder (2005a) points out that if he uses longer time averages than 7 years and also drops fathers who are ever topcoded that this results in dramatically smaller samples that are likely to be highly selected and are likely to be uninformative about the effects of topcoding. One possible way to gauge the potential upward or downward bias of using imputing topcoded values would be to run simulations with fake data where one can use a range of parameter values to assess the magnitude of the bias.

The second critical measurement concern in the literature concerns lifecycle bias best encapsulated by Haider and Solon (2006). One aspect of this critique concerns the effects of measuring children's income when they are too young. Children who end up having high lifetime income often have steeper income trajectories than children who have lower lifetime income. Therefore if income is measured at too young an age it can lead to an attenuated estimate of the IGE in lifetime income. Haider and Solon show that this bias can be considerable and is minimized when income is measured at around age 40. A related issue is that transitory fluctuations are not constant over the lifecycle but instead follow a u-shaped pattern over the lifecycle (Baker and Solon, 2003; Mazumder, 2005a). This implies that measuring parents' income when they are either too young or (especially) when they are too old can also attenuate estimates of the IGE. While there are econometric approaches one can use to correct for lifecycle bias, one simple approach is to simply center the time averages of both children's and parents' income around the age of 40. Using this approach with the PSID, Mazumder and Acosta estimate the IGE to be around 0.6. Mitnik et al (2015) also use this approach with IRS data covering older cohorts who are observed as late as age 38 and find that both sources of bias are quantitatively important. Further, Nilsen et al (2012), Gregg et al (2014) and Nybom and Stuhler, (2015) using data from other countries, show that both time averaging and life-cycle bias play a role in attenuating IGE coefficients. Importantly, these studies find that these biases matter even when using administrative data.[13]

Comparisons of Intergenerational Samples

To better understand the limitations with currently available intergenerational samples in the US with respect to these measurement issues, it is useful to think about what an ideal sample

[13] Chetty et al speculate that perhaps they find that time averaging and life cycle bias don't matter because of their use of administrative data which they suspect to be less error prone than survey data.

would look like. In an ideal setting we would want to construct an intergenerational sample where income is measured for both generations throughout the entire working life cycle, say between the ages of 25 and 55.[14] For example, suppose our data ends in 2012 (as in Chetty et al); then for full lifecycle coverage for the children's generation we would want cohorts of children who were born in 1957 or earlier. For the 1957 cohort we would measure their income between 1982 and 2012. For the 1956 cohort we would measure income between 1981 and 2011 and so on. Suppose that for the parents' generation, the mean age at the time the child is born is 25. Then for the 1957 cohort we would collect income data from 1957 to 1987, from 1956 to 1986 for the parents of the 1956 birth cohort and so on. With such a dataset in hand we would be confident that we would have measures of lifetime income that are largely error-free and would also be free of lifecycle bias.

Unfortunately, for most countries, including the US, it is difficult to construct an intergenerational dataset with income data going back to the 1950s.[15] Still, we can come somewhat close to this ideal sample with publicly available survey data in the Panel Study of Income Dynamics (PSID).[16] The PSID began in 1968 and started collecting income data beginning in 1967 for a nationally representative sample of about 5000 families. The 1957 cohort would have been 11 years old at the time the PSID began so this cohort along with those born as early as 1951 would have been under the age of 18 at the beginning of the survey. The approach I take in this paper is to construct time averages of both parent and child income

[14] The precise end points are debatable but for measurement purposes one might want to ensure that most sample members have finished schooling and that most sample members have not yet retired. In theory, however, it may be better to consider earnings even at young ages when adolescents may have chosen to forego earnings for human capital accumulation that pays off later in life. In any case, the main point of the argument in this section would still hold if one used a much broader age range.

[15] The SIPP-SER data used by Mazumder (2005) and Dahl and Deliere (2008) meets some but not all of these requirements.

[16] The code used to construct the main estimates in this paper will be made available to researchers either through the author's website or through personal communication.

centered around the age of 40 in order to minimize life-cycle bias. For parents, these time averages include income obtained between the ages of 25 and 55 and for children these averages include income obtained between the ages of 35 and 45.

Relative to the ideal sample, the PSID sample is close in several regards. Since it covers the 1967 to 2010 period it is able to utilize large windows of the lifecycle for both generations. For example, for the 16 cohorts born between 1951 and 1965, in principle, income can be measured in all years that cover the age range between 35 and 45. For the cohorts born between 1967 and 1975, their parents' income can also be measured through the ages of 25 and 55. Of course, attrition from the survey diminishes the size of the actual samples with observations in all of these years but at least the potential for such coverage is there.[17]

Now let us contrast this with the limitations faced by Chetty et al (2014) in their analysis of currently available IRS data. First the tax data is currently only digitized going back to 1996, which is far from what the ideal dataset would require (1957), or even what is available in the PSID (1967). Therefore, there is no birth cohort for whom the income of parents can be measured for the entire 31 year time span between the ages of 25 and 55. Furthermore, the authors chose to limit the analysis to just a 5-year average between 1996 and 2000. A possible explanation for this choice is that lengthening their time averages further would have necessitated measuring income when parents were at an older than ideal age. I will return to this point later when I explain why their sensitivity analysis is flawed. The mean age of fathers in their sample in 1996 is reported to be 43.5 with a standard deviation of 6.3 years. This implies that over the 5 years from 1996 through 2000, roughly 24 percent of the father-year observations

[17] As discussed later, I use survey weights to address concerns about attrition.

used in constructing the average would be when fathers are over the age of 50.[18] This is an age at which the transitory variance in income is quite high (Mazumder, 2005a). They also report that prior to 1999 they record the income of non-filers to be zero. Therefore for about 3 percent of observations in three of the five years used in their average they impute zeroes to the missing observations.[19]

For the children in the sample, the data limitations are even more severe. Chetty et al use cohorts born between 1980 and 1982 and measure their income in 2011 and 2012 when they are between the ages of 29 and 32. For this age range, simulations from Haider and Solon (2006) suggest that there would be around a 20 percent bias in the estimated IGE compared to having the full lifecycle. A further complication is that their measures are taken in 2011 and 2012 when unemployment was relatively high and labor force participation quite low. They report that they drop about 17 percent of observations from the poorest families due to their having zero income *over those 2 years*. If their sample had covered 29 to 32 year olds in other time periods spanning other periods of the business cycle, then using such a short window would have been somewhat less of a concern.

Finally, there is a concern about whether administrative income data adequately captures true income, particularly at the low and the high ends of the income distribution. For example, at the lower end of the distribution, tax data could miss forms of income that go unreported to the IRS. At the higher end, tax avoidance behavior could lead to an under-reporting of income. Hokayem et al (2015) find that administrative tax data can do a worse job than survey data in

[18] This example assumes the data is normally distributed. In 2000, more than a third of the observations would be when fathers are over the age of 50.

[19] See footnote 14 of Chetty et al. (2014). They show that measuring income over 1999 to 2003 has no effect on their rank mobility estimates but they do not show how the IGE estimates change. Measuring income from 1999 to 2003 potentially worsens the attenuation bias in the IGE resulting from measuring fathers at late ages.

measuring poverty. Abowd and Stinson (2013) argue that it is preferable to treat both survey data and administrative data as containing error. I also discuss below how a preferred concept of family income that includes all resources available for consumption, including transfers and income of other family members, would render tax data inadequate.

It is useful to visualize just how different the data structure of the Chetty et al sample is from an ideal intergenerational sample. This is shown in Figure 1. For each of three samples there are two columns of 31 cells representing the ages from 25 to 55 in each generation and we assume that just one parent's income can be measured. The degree of coverage over the life course is represented by the extent to which the cells are colored. Panel A shows that if we measured income in both generations using data spanning the entire life course for two generations then all the cells in both generations would be colored in. Panel B contrasts this with a typical parent-child observation in the Chetty et al sample.[20] This makes it clear just how small a portion of the ideal lifecycle is covered. Just 6 percent of the child's lifecycle and just 16 percent of the parent's lifecycle would be covered. Panel C contrasts this with an example of a result that will be produced with the PSID in the current study. There are many cohorts for whom both child and parent income can be measured over several years centered around the age of 40 when lifecycle bias is minimized. The figure presents an example of a 7-year average of child income and a 15-year average of parent income. Such a sample would cover 23 percent of the child's lifecycle and 48 percent of the parent's lifecycle.

To their credit, Chetty et al (2014) attempt to conduct some sensitivity checks to assess these issues but their data, which only begins in 1996, are not well suited to doing effective

[20] This example takes a child born in 1981 whose income is observed at age 30 and 31 during the years 2011 and 2012. I assume that the father was 29 years old when the child was born so that the father's income is measured between the ages of 44 and 48 during the years 1996 to 2000. This example closely tracks the mean ages of the sample as reported by Chetty et al (2014).

robustness checks for the IGE measure. Below I will replicate their sensitivity checks with the PSID data and show how the current IRS data limitations lead them to reach incorrect conclusions regarding the sensitivity of their IGE estimates to these measurement problems.

Estimating the IGE when children have zero income

Chetty et al (2014) also argue that the IGE estimator is not robust to imputing years of zero family income observed for individuals in the child generation.[21] They obtain an estimate of 0.344 when they restrict the sample to those children with positive income in 2011 and 2012. If they impute $1000 of income to these individuals then their IGE estimate rises to 0.413. If they assign $1 then their IGE estimate rises to 0.618. There are three points worth making here.

First, the issue of having to deal with missing values is largely a consequence of the poor lifecycle coverage of their sample. To see why this is the case, imagine a hypothetical researcher in the year 2035 that attempts an intergenerational analysis for the 1980 birth cohort using the tax data. In 2035 one would have complete information on family income throughout the ages of 25 to 55 and would not have to worry that some of these individuals reported no income in 2 of the 31 years of the lifecycle, during a period when unemployment was relatively high. There would be as many as 29 other years of income data available to calculate lifetime income. In fact, based on the prior literature, a researcher could probably obtain a fairly unbiased estimate of the IGE for the 1980 birth cohort by the early 2020s if they could obtain even a few years of income around the age of 40. In the PSID one can track cohorts born as far back as the 1950s who may be observed over many years, at many ages, and at different stages of the business cycle.

Second, recent work by Mitnik et al (2015) point to an alternative approach for estimating the IGE that is not sensitive to situations in which researchers may have only a short

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span of data on children's income and encounter cases of zero income. Specifically, they estimate the elasticity of the expected income of children rather than the elasticity of the geometric mean of income, which the literature has traditionally focused on. They argue that this is the estimand that researchers should actually be interested in estimating.[22] They present striking evidence that unlike the traditional IGE estimator, their alternative estimator of the IGE is relatively immune to the treatment of missing income of children when income is measured over only a short window of the lifecycle. However, it is unclear, and ultimately an empirical question as to whether the Mitnik et al approach to estimating the IGE would yield substantially different results from the traditional approach if one had access to the entire lifetime income stream of children. In such a situation there would likely be very few cases of zeroes. This would be a fruitful avenue for future research to explore.

A third remark relates to the *concept* of family income one wants to use. Economists (e.g. Mulligan, 1997) have sometimes argued that an ideal measure of intergenerational mobility would seek to measure lifetime consumption in both generations since consumption is perhaps the measure closest to utility which is what economists like to focus on. In this case ideally we would like to measure *total family resources* which includes income obtained from transfers and from other family members. This is an example where survey data that has access to transfer income would be preferable to tax data that may not. Including transfers may not only be a preferred measure but may also help alleviate the problem of observing zero earnings or zero income as is common in administrative data. It is also not obvious why the preferred measure of family income would be one that only includes labor market earnings, transfers and capital

[22] Chetty et al (2014) argue that the preferred estimator of Mitnik et al (2015) can be interpreted as a "dollar-weighted" estimator of the IGE and the traditional IGE can be viewed as a "person-weighted" estimator and suggest that each answers a different question.

income that happen to be reported on tax forms. This may help explain why Chetty et al estimate an IGE of 0.452 when they limit their sample to individuals between the 10th and 90th percentiles. The lack of coverage of all forms of transfer income may be less problematic for this range since it excludes the bottom of the income distribution.

Estimating the IGE when parents have zero income

It is worth pointing out that the prior discussion is in many ways very distinct from the problem of having a measure of zero income for *parents*. Chetty et al (2014) and Mitnik et al both cite Dahl and Deliere (2008) in their discussions of the robustness of the IGE but Dahl and Deliere actually confront an entirely different issue. Dahl and Deliere utilize social security earnings data. For the years 1951 through 1983, they cannot distinguish between years of zero earnings due to non-coverage in the SSA sector from "true" zeroes due to non-employment. When they construct measures of parent average earnings over the ages of 20 to 55 and include all years of earnings they obtain estimates of the intergenerational elasticity of only around 0.3 for men. However, their estimates may be including many years when actual earnings are positive but are erroneously treated as zero because fathers were working in the non-covered sector. Since this measurement error is on the right hand side it can severely attenuate the estimate of the IGE.

They attempt to correct for this in some specifications by restricting the sample to parents who were not in the armed forces or self-employed and who therefore would likely be in the covered sector. But, importantly, the class of worker variable is only observed in one year, 1984, which is at a relatively late point in the lifecycle for most of their sample of fathers. Therefore, their long-term averages still include many years of zero earnings for workers who were actually in the non-covered sector in the 1950s, 1960s or 1970s but who had shifted to the covered sector by 1984. Not surprisingly, using the class of worker status observed in 1984 to restrict the sample still yields very low estimates of the IGE. However, when they restrict the number of years of zero earnings in other very sensible ways to more directly address the issue, they obtain estimates of around 0.5 to 0.6. For example, when they use the log of average earnings beginning with the first 5 consecutive years of positive earnings up to age 55 they obtain an estimate of 0.498.

A clear advantage of the IRS tax data compared to the SSA data is that there is no requirement of working in sectors covered by SSA. However, there may be concerns related to whether individuals file their taxes and whether the IRS samples contain those who don't file. As mentioned earlier, Chetty et al assign zero income to parents who are in their sample but did not file taxes in years prior to 1999. This can also lead to attenuation bias in estimating the IGE.

IV. PSID Data

I restrict the analysis to father-son pairs as identified by the PSID's Family Identification Mapping System (FIMS) and use all years of available family income between the ages of 25 and 55 between the years of 1967 and 2010.[23] For the main analysis I consider a measure of family income that excludes transfers and excludes income from household members that are not the head of household or the spouse. This provides a measure of family income that is probably most comparable to the concept used by Chetty et al (2014). In addition, I also constructed a measure of family income that also includes transfers received by the household head or spouse, but these results are not presented.[24] Finally, I construct a measure that uses only the labor income of the father and son to be more comparable to papers that emphasize the IGE in labor

[23] The focus on sons contrasts with Chetty et al (2014) who pool sons and daughters and Mitnik et al (2015) who mainly produce separate estimates by gender.
[24] These results were broadly similar to the baseline findings using the narrower measure of family income.

market income (e.g. Solon, 1992; Mazumder, 2005). Labor income is not simply earnings from an employer but also incorporates self-employment. Observations marked as being generated by a 'major' imputation are set to missing. Yearly income observations are deflated to real terms using the CPI. In the PSID the household head is recorded as having zero labor income if their income was actually zero or if their labor income is missing, so one cannot cleanly distinguish true zeroes with labor income. All of the main analysis only uses years of non-zero income when constructing time averages of income. When using family income, instances of reports of zero income are relatively rare so the results are virtually immune to the inclusion of zeroes. Therefore the concerns about the sensitivity of results around how to handle years of zero income is effectively a non-issue when using family income.

The main analysis only uses the nationally representative portion of the PSID and includes survey weights to account for attrition. All of the analysis was also done including the SEO oversample of poorer households and includes survey weights. While the samples with the SEO are larger and offer more precise estimates, there is some concern about the sampling methodology (Lee and Solon, 2009). Finally all estimates are clustered on fathers.

The approach to estimation in this study is slightly different than in most previous PSID studies of intergenerational mobility. Rather than relying on any one fixed length time average for each generation and relying on parametric assumptions to deal with lifecycle bias (e.g. Lee and Solon, 2009), instead I estimate an entire matrix of IGE's for many combinations of lengths of time averages that are all centered around age 40. I will present the full matrix of estimates along with weighted averages across entire rows and columns representing the effects of a particular length of the time average for a given generation. For example, rather than simply comparing the IGE from using a ten-year average of fathers' income to using a five year average of fathers' income *for one particular time average of sons' income,* I can show how the estimates are affected for every time average of sons' income.

V. Results

IGE Estimates

Table 1 shows the estimates of the IGE in family income that is conceptually similar to that used by Chetty et al (2014). The first entry of the table at the upper left shows the estimate if we use just one year of family income in the parent generation and one year of family income for the sons when they are closest to age 40 and also are within the age-range constraints described earlier. This estimate of the IGE is 0.414 with a standard error of 0.075 and utilizes a sample of 1358. One point immediately worth noting is that this estimate which uses just a single year of family income around the age 40 is higher than the 0.344 found by Chetty et al (2014). Moving across the row, the estimates gradually include more years of income between the ages of 35 and 45 for the sons. At the same time the sample size gradually diminishes as an increasingly fewer number of sons have will income available for a higher length of required years. For the most part the estimates don't change much and most are in the range of 0.35 and 0.42. At the end of the row I display the weighted average across the columns, where the estimates are weighted by the sample size. For the first row the weighted average is 0.381.

Moving down the rows for a given column, the estimates gradually increase the time average used to measure family income in the parent generation and as a consequence also reduces the sample size. For example, if we move down the first column and continue to just use the sons' income in one year measured closet to age 40 and now increase the time average of parent income to 2 years, the estimate rises to 0.439 as the sample falls to 1317. Using a five year average raises the estimate to 0.530 (N=1175). Increasing the time average to 10 years increases the estimate to 0.580 (N=895). Using a 15 year average raises the estimate further to 0.680 (N=533). The weighted average for each row is displayed in the last column and the weighted average for each column is displayed in the bottom row.

A few points are worth making. Since expanding the time average in either dimension reduces the sample size it risks making the sample less representative. The implications on the estimates, however, are quite different for whether we increase the time average for the sons' generation or for the fathers'. For the parent generation, increasing the time average tends to raise estimates. This is consistent with a story in which larger time averages reduce attenuation bias stemming from mis-measurement of parent income (Solon, 1992; Mazumder, 2005). This also accords with standard econometric theory concerning mis-measurement of the right hand side variable. On the other hand, econometric theory posits that mis-measurement in the dependent variable typically should not cause attenuation bias. Indeed, increasing the time average of sons' family income has little effect. But crucially, this is because we have *centered the time average* of family income in each generation so that the lifecycle bias which induces "non-classical" measurement error in the dependent variable (Haider and Solon, 2006) may already be accounted for.

By this reasoning one might consider the estimates in the first column to be the most useful since they allow one to see how a reduction in measurement error in parent income affects the estimates while simultaneously minimizing life cycle bias and keeping the sample as large as possible. A more conservative view would be to use the weighted average in the final column that takes into account the possible effects of incorporating more years of data on sons' income while also giving greater weight to estimates with larger samples. Figure 2 shows the pattern of estimates from the two approaches as I gradually use longer time averages. With either approach, time averages of 10 to 15 years yield estimates of the IGE in family income that are consistently greater than 0.6. Appendix Table 1 and Appendix Figure 1 show the analogous set of estimates using larger samples that include the SEO oversample.

The key idea of the study is to see how these IGE estimates would compare to what one would obtain by imposing the current data limitations of the IRS sample. To do this, one can use the second column and fifth row of Table 1 as a baseline estimate. That estimate of 0.493 uses a two year average of family income of sons centered around age 40 and a five year average of parent income centered around age 40. If I now impose a sample restriction such that I use a two year average of sons taken over the ages of 29-32 and use a five year average of parent income centered around the age of 46 then the estimate I obtain is 0.282 (s.e. = 0.099). This is only 57 percent of the value when using similar time averages centered at age 40. Furthermore, if the true IGE is actually 0.7, then it is only 40 percent of the true parameter. If I include the SEO subsample then the estimate rises a bit to 0.325 (s.e. = 0.081). For that sample, the data limitations yield estimates that are 62 percent of the comparable estimates when using time averages centered at age 40. Neither of the two estimates are statistically different from the Chetty et al estimate of 0.344. This suggests that it is the data limitations in the tax data that lead Chetty et al to produce estimates that are vastly lower than what has been reported in most of the previous literature.

Interestingly, Mitnik et al (2015) report a strikingly similar pattern of results. Their baseline estimates use children’s income measured between the ages of 35 and 38 and 9 year averages of parent income. Using a non-parametric approach on a sample that includes all children, their estimates of the traditional IGE range from 0.55 to 0.74 depending on how they impute the income of children who report no income.[25] When they move from this baseline sample to one that mimics the sample used by Chetty et al (children between the ages of 29 and 32 and using a 5 year average of parent income), their estimate falls to 0.28. If they instead use their preferred IGE estimator, then their main estimate is 0.50 and their estimate when mimicking the sample used by Chetty et al is 0.37.

Table 2 shows a set of IGE estimates that only use the labor income of fathers and sons. On the whole, the estimates in Table 2 are fairly similar to those in Table 1 as is shown in Figure 3 which plots the weighted average across the columns. For example, when using a 12-year average of fathers' income, the IGE when using labor income is 0.611 and when using family income the estimate is 0.612.

These estimates are broadly similar and slightly higher than those found by Mazumder (2005a) who used the labor market earnings of fathers and sons from social security earnings data. Mazumder (2005a) relied on several data imputation approaches to deal with issues related to social security coverage and topcoding. However, with the PSID, none of these kinds of imputations are necessary. These findings, along with similar results in Mazumder (2005b), Mazumder and Acosta (2014) and Mitnik et al (2015) which also do not require imputations, suggest that the results of Mazumder (2005a) are likely not due to the use of imputations as argued by Chetty et al (2014) but instead are due to the longer time averages available in the SSA data and the PSID. This also suggests that Mazumder (2005a) may have been correct in arguing that the use of imputations may not have imparted an upward bias.[26]

Robustness Checks

[25] See their Table 11.
[26] Sees section 3 for a more detailed discussion of this issue.

A drawback of the PSID data is that there can be substantial attrition. One may be concerned that the samples that use longer time averages of parent income could be very different from the ones that use shorter time averages. Perhaps, it is the case that the higher estimates that I attribute to using longer time averages in Table 1 are instead due to a change in the composition of families.

To address this I conduct two robustness exercises. First, I use a set of *fixed* samples to show how IGE estimates change as I increase the time average of parent income while holding the composition of families constant. To narrow the focus of the exercise, I consider the case of using 1 available year of income for sons when they are closest to the age of 40.[27] I then consider, for example, the 1063 families where I have 7 years of available family income of fathers and see how the estimates as I gradually increase the time average of parent income from 1 to 7 years. This is shown in column 3 of Table 3. If I use 1 year of parent income the IGE is estimated to be 0.358. If I use a 3-year average the estimate rises to 0.446. If I use a 5-year average the IGE rises further to 0.504 and rises to 0.529 when averaging all 7 years. In column 4, I show how the estimates change for the 895 families with 10 years of income and in column 5 I present the pattern of estimates for the 533 families with 15 years of income. Columns 1 and 2 consider the effects of time averaging for smaller time averages of 3 and 5 years where the samples are even larger. In nearly all cases, the estimates rise monotonically as more years are used to increase the time average. Mitnik et al (2015) present a similar set of exercises using their IRS samples in their Appendix and show a similar pattern when they increase time averages of parent income up to 9 years. This is comforting because one clear advantage of administrative tax data is that attrition from a survey is not a concern.

[27] These are the samples that are in column 1 of Table 1.

A second exercise directly examines the characteristics of families in which longer time averages of parent income are available. I consider 7 characteristics of fathers: income, age; education; percent black, percent white; percent married and percent ever divorced.[28] As before I consider how these characteristics differ for the samples presented in column 1 of Table 1. The results are shown in Appendix Table 2. The table shows for example, that the mean education level of fathers in the sample of 533 families with 15 years of parent income is 13.0. This compares to a mean of 12.9 years for fathers with 5 years of income. The lower panel of the table shows that the difference is not statistically significant (*p*-value = 0.50). While the families with longer time averages may be slightly more educated they are also more likely to be black and more likely to be divorced, suggesting some evidence of negative selection. Overall, there is no clear pattern of selection with respect to socioeconomic status.

If one compares the samples with 10 year averages to those with 1 or 5 years, there are no statistically significant or economically meaningful differences in father characteristics. The fact that the estimates of the IGE are already well above 0.5 even when using 10-year averages suggests that the main points of the paper likely hold. In summary, there are no especially striking patterns that suggest that the longer time averages are due to changing characteristics of parents.

Despite these checks I would still be somewhat cautious in arguing that the considerably smaller samples, with say 11 to 15 years of parent income, do not suffer from *any* concerns related to selection. One concern is that if the attenuation bias with using short-term averages is truly due to measurement error and serial correlation in transitory fluctuations as argued by Mazumder (2005a), then one would not expect the IGE to increase nearly linearly with the length

[28] I also consider mother's education.

of the time average as Figure 1 shows, but instead, would exhibit a more concave pattern as the simulation results in Mazumder (2005a) depict. This is one argument in favor of using administrative data where attrition is typically not an issue. Although the currently available US tax data does not yet have a long enough panel length to resolve this issue, future studies using administrative data in the US and other countries should continue to shed light on the nature of the earnings process and what it might imply for IGE estimates using longer-time averages.

Sensitivity Checks in Chetty et al (2014)

Chetty et al (2014) argue that their national estimates of the IGE are unaffected by the age at which children's income is measured. They also argue that their estimates are unaffected by the length of the time average used to measure parent income. They perform sensitivity checks to demonstrate this empirically and present the results visually in figures. In this section I describe why those sensitivity checks are flawed and show how one can demonstrate this using the PSID. In short, their sensitivity checks introduce new attenuation bias from using parent income at older ages. This bias appears to fully offset the reductions in attenuation bias that would otherwise have been apparent when using older children or when extending the time average of parent income.

They first discuss the sensitivity of the IGE to the age at which child income is measured, Chetty et al claim that while there is some lifecycle bias early in the career that this stabilizes once children have reached the age of around 30. They conduct an empirical exercise that is shown in their Appendix Figure IIA. They implement this sensitivity check by using an additional tax dataset that includes much smaller intergenerational samples from the Statistics of Income (SOI). With the SOI data they can examine the IGE between parents and children for earlier birth cohorts going back to 1971. However, they continue to use family income in 2011 and 2012 for children and in 1996 to 2000 for the parents. This implies, for example, that when they examine the 1971 cohort to measure the IGE for 41 year olds they are actually using parent income that is measured when the child was between the ages of 25 to 29 and unlikely to be living at home. Perhaps more importantly, this also requires that they use the income of fathers when they are likely to be especially old. For example, the income of a father who was 28 when his child was born in 1971 would be 53 to 57 years old when his income was measured in 1996 to 2000. Using parent income at such late ages when transitory fluctuations are a substantial part of earnings variation can lead to substantial attenuation bias that could offset the reduction in lifecycle bias from measuring child income at age 40 (Mazumder 2005a). Overall it could make it appear as though there is no lifecycle bias when in fact it may actually be substantial.

With a long-running panel dataset like the PSID one can replicate this sensitivity check but can also show how the results differ if one *simultaneously keeps the age at which father's income is measured, constant*. To implement this exercise, I first replicate the findings in Chetty et al by gradually increasing the age at which sons' income is measured from 22 to 41 while simultaneously increasing the parent age range at which the five year average of parent income is measured to match the analogous age range implied by the tax data.[29] I then fix this problem by using a 5 year time average of parent income that is always centered at the age of 40 while simultaneously raising the age of sons when their income is measured from 22 to 41.

Figure 4 shows the results of this exercise. The red line replicates the flawed sensitivity check. Lifecycle bias appears to level off around the age of 30 and may even appear to decline slightly in the late 30s. The green line demonstrates that this sensitivity check is flawed once

[29] To fix ideas, for those sons who are aged 32, one would use the income of fathers when the child is between the ages of 15 and 19 as in Chetty et al. For those who are 33 one would use the income of fathers when the child is between 16 and 20 and so on.

you hold parent age constant around the age of 40. While both lines track each other reasonably well before the age of 30, they start to diverge after the age of 32. This is precisely around the time when the red line utilizes data on parents when the child is no longer in the home, when the parents are entering their 50s and when their income becomes noisy. With the green line, however, we continue to use centered time averages of parents around the age of 40 to eliminate this downward bias. The bottom line is that there is in fact substantial lifecycle bias that cannot be uncovered by the sensitivity checks in the Chetty et al version of the tax data because of inherent data limitations.

There is also a second pertinent sensitivity analysis that Chetty et al present in their Figure 3B. Here they consider how their results change when they increase the time average of parent income. They do this by adding additional years beyond the 1996-2000 time frame and showing that their rank-rank slope estimates do not increase, though they never show the results of this exercise for the IGE. The key problem with this approach is that can only extend the length of the time averages *forward* in time. This necessarily results in increasing the attenuation bias from using later ages in the lifecycle of parents. This can again have an offsetting effect due to attenuation bias. For example, the mean age of fathers in their sample in 2003 exceeds 50 so once they start lengthening time averages to include data in 2003 and beyond, they are actually including income observations containing a large transitory component. As before, this also implies that they are actually utilizing many years of income when the child is likely no longer living at home. With the PSID, one can avoid this pitfall. Specifically, one can increase the length of the time average while still holding constant the mean age of fathers by using centered time averages.

As before, I first use the PSID to replicate the results of the sensitivity check in Chetty at al and then show that time averaging does in fact reduce the attenuation bias once one removes the mechanical effect of increasing parent age.[30] The results are shown in Figure 5. First, I am able to replicate the spirit of the finding in Chetty et al's Figure 3B. The red line shows that as I extend the time average of fathers' income by using years when the fathers are getting older, I find that the time averages appear to have no effect on increasing estimates of the IGE. The IGE stays flat at first and then actually starts to decline when the time averages get very large. However, when I use a centered time average of fathers' income around the age of 40, a lengthening of the time average generally leads to greater IGE estimates suggesting that larger time averages of parent income do tend to reduce attenuation bias. It is worth noting here that Mazumder (2005a) was able to use SSA data to extend his time averages of fathers earnings backwards in time and also use centered time averages. Finally, Mitnik et al (2015) also show that in their tax data that longer time averages of parent income and measuring child income at later ages lead to higher IGE estimates.

Rank-Rank Slope Estimates

In this section, I present an analogous set of results for the rank-rank slope. For this analysis I use the same measure of family income that is used to generate Table 1. The results are shown in Table 4. With the rank-rank slope some new patterns emerge. First, it appears that increasing the length of the time averages centered at the age of 40 for sons does appear to increase the slope estimates. For example, looking over the first 10 rows, it appears that in

[30] Specifically, I use just a 2 year average of sons' income over the ages of 29 to 32 and then start with a single year of fathers' income that is measured when the son is 15 and then gradually add years of fathers' income from subsequent years. For a five year average, this uses the income of fathers when the son is between the ages of 15 and 19. This mimics the 1981 birth cohort in Chetty et al whose parent income is measured between 1996 and 2000. A ten year average then utilizes the income of fathers when the son is between the ages of 15 and 24.

nearly every case the slope estimates are higher when sons' income is averaged over 8, 9 or 10 years rather than just 1 or 2 years. This was not the case with the IGE. In Table 1 it was typically the reverse pattern. It is not obvious why this is the case but perhaps there is some aspect of lifecycle bias that is more pronounced when using ranks than when using the IGE. This may be a fruitful issue for future research to investigate.

Second, the effect of using longer time averages of parent income is much more muted with the rank-rank slope than with the IGE. In table 1, the weighted average of the IGE across all the rows goes from around 0.38 when using a single year of family income to about 0.66 when using 15 year averages of family income –a 72 percent increase. The analogous increase in the rank-rank slope is a rise from 0.31 to 0.40 or just a 29 percent increase. A takeaway from Table 3 is that the rank-rank slope may be around 0.4 or higher rather than the 0.34 reported by Chetty et al. If we do the same exercise of imposing the limitations of the tax data on our PSID sample, the estimate drops from 0.33 when using centered time averages (two years of sons and five years for fathers) to 0.28 when using sons between the ages of 29 and 32 and fathers between the ages of 44 and 48. Again, these results suggest that even the rank-rank slope estimates using the tax data are likely attenuated, albeit to a lesser degree than the substantial attenuation with the IGE estimates. These results are also very similar if one includes the SEO oversample of poorer households or just uses labor income of fathers and sons (results available upon request).

In Figures 6 and 7 I return to sensitivity analysis exercises from Chetty et al (2014) in the context of the rank-rank slope and replicate those exercises with the PSID, first allowing father's age to shift higher mechanically but then correcting for this by holding father's age constant using centered time averages around the age of 40. Figure 6 doesn't point to a very clean story.

In this case the red line is often larger than the green line suggesting that estimates are often slightly lower when using the centered time averages. On the other hand both lines, but especially the green one, appear to trend higher over the course of the 30s suggesting that perhaps life-cycle bias does not taper off around age 30. Figure 7 is also interesting. The red line is flat to declining and very similar to what Chetty et al find but has the problem of conflating two different biases. The green line, which fixes the mechanical increase in fathers' age when taking longer time averages does show evidence of larger estimates but only when the time averages are very long. Overall, estimates of the rank-rank slope are also likely biased down due the limitations of the tax data but to a much lesser extent than the IGE.

VI. Conclusion

The literature on intergenerational mobility over the past few decades has shown how attenuation bias and lifecycle bias can substantially affect estimates of the intergenerational elasticity (IGE). Most previous estimates of the IGE in family income in the U.S. are around 0.5. Utilizing PSID data I generate the first estimates of the IGE in the US using long time averages of parent income and using income centered at age 40 in both generations. I find that the IGE for sons is likely greater than 0.6 with respect to both family income and labor market earnings suggesting less mobility than most previous estimates and similar to estimates in Mazumder (2005a).

In contrast, using very large samples of tax records that begin in 1996, Chetty et al (2014) estimate that the IGE is actually much lower at 0.344. Further they claim that these estimates are not subject to attenuation bias or lifecycle bias. If accurate, this finding is important because it implies that income gaps between families in America will dissipate relatively quickly over time. It is important to understand whether the evidence of greater mobility from their tax data sample is accurate or spurious. Revisiting the results from this study may also hold more general lessons for researchers who use administrative data to estimate intergenerational mobility.

I first describe the fundamental data limitations of using intergenerational samples based on US tax data that only begin in 1996. The key point is that the panel length is currently too short to do a good job overcoming the issues concerning attenuation bias and lifecycle bias. I demonstrate that a long-lived survey panel such as the PSID that may only have a few thousand families is actually more useful for estimating the national IGE than having millions of tax records if the data are limited in their ability to cover long stretches of the life course. Specifically, I show that when I use the PSID but impose the same age structure and use the shorter time averages of parent income to mimic Chetty et al, that I obtain similar IGE estimates of around 0.3. I also demonstrate that the sensitivity checks used by Chetty et al to address concerns about 1) the age at which sons' income is measured, and 2) the length of time averages of parent income, are flawed because they impose an offsetting attenuation bias by increasing the age at which parent income is measured. Correcting for this confounding, I show that the lifecycle bias and attenuation bias almost surely exist in the tax data when estimating the IGE.

Further confirmation of this is provided by Mitnik et al (2015) who find evidence of these biases in a different IRS sample that extends further back in time and enables an analysis of older children and the use of longer time averages of parent income. The fact that several other papers that also use administrative data in other countries (Nilsen et al, 2012; Gregg et al, 2013 and Nybom and Stuhler, 2015) also show that these biases matter, suggest that Chetty et al's findings are more the exception than the rule.

On the other hand, the results with the PSID with respect to the rank-rank slope suggest that these biases are much smaller and that the rank-rank slope is relatively more robust (though not entirely immune) from these measurement concerns. It is important, however, to remember that the IGE is conceptually different from the rank-rank slope and may continue to be of substantial value to researchers and policy-makers especially in an era of rising inequality when income gaps in society may be expanding. In that context, focusing only on positional mobility solely because measurement is easier, may not be appropriate.

Another point worth emphasizing is that survey data may be advantageous for measuring certain sources of income that simply may not be tracked in tax records. These sources of income may provide better measures of the true resources available to families, especially for those at the low end of the income distribution. Given the growing use of administrative data and the excitement over new sources of such data, this characteristic of survey data may start to become overlooked. This is an argument for continuing to produce estimates of intergenerational mobility using survey data despite their smaller sample sizes, at least as a complement to using administrative data.

Finally, it is important to make clear that Chetty et al (2014) makes a notable contribution to the literature by demonstrating that there may be large geographic differences in intergenerational mobility across the U.S. It is likely that these large geographic differences will remain even after correcting for the biases in the tax data. Nevertheless, it may be useful for future research to more directly examine this issue and verify that the central findings in their paper are robust to these biases.

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Table 1: Estimates of the father-son IGE in family income

	Time Average of Sons' Income (years)										
Time Avg. Fath. Inc.	1	2	3	4	5	6	7	8	9	10	Wgt. Avg.
1	0.414	0.372	0.405	0.375	0.397	0.361	0.317	0.315	0.354	0.415	0.381
	(0.075)	(0.067)	(0.069)	(0.068)	(0.064)	(0.070)	(0.063)	(0.068)	(0.080)	(0.091)	
	1358	1184	1050	932	786	595	440	351	267	183	
2	0.439	0.420	0.434	0.402	0.429	0.443	0.391	0.379	0.419	0.453	0.423
	(0.066)	(0.059)	(0.062)	(0.062)	(0.068)	(0.088)	(0.067)	(0.069)	(0.082)	(0.089)	
	1317	1145	1015	901	758	572	419	331	251	170	
3	0.478	0.445	0.450	0.414	0.440	0.440	0.401	0.380	0.416	0.449	0.441
	(0.067)	(0.060)	(0.064)	(0.064)	(0.071)	(0.088)	(0.062)	(0.066)	(0.078)	(0.088)	
	1268	1099	970	862	719	537	389	306	230	154	
4	0.478	0.455	0.467	0.435	0.453	0.463	0.419	0.388	0.431	0.422	0.453
	(0.068)	(0.061)	(0.069)	(0.069)	(0.079)	(0.105)	(0.063)	(0.067)	(0.085)	(0.091)	
	1216	1051	926	819	678	497	354	273	203	133	
5	0.530	0.493	0.500	0.468	0.479	0.477	0.428	0.398	0.441	0.454	0.485
	(0.071)	(0.065)	(0.075)	(0.076)	(0.088)	(0.113)	(0.065)	(0.069)	(0.090)	(0.098)	
	1175	1015	892	788	649	471	332	255	188	123	
6	0.517	0.482	0.492	0.458	0.473	0.476	0.420	0.389	0.434	0.452	0.477
	(0.071)	(0.066)	(0.077)	(0.078)	(0.091)	(0.120)	(0.064)	(0.067)	(0.091)	(0.092)	
	1120	966	843	741	606	431	299	228	165	105	
7	0.529	0.485	0.492	0.459	0.464	0.462	0.379	0.369	0.399	0.402	0.474
	(0.077)	(0.073)	(0.086)	(0.089)	(0.105)	(0.144)	(0.065)	(0.078)	(0.109)	(0.104)	
	1063	915	795	696	564	396	271	202	143	87	
8	0.552	0.518	0.546	0.521	0.545	0.595	0.368	0.345	0.430	0.468	0.523
	(0.086)	(0.082)	(0.091)	(0.096)	(0.110)	(0.166)	(0.092)	(0.114)	(0.166)	(0.156)	
	1005	863	747	648	520	354	232	168	114	67	
9	0.573	0.537	0.558	0.536	0.560	0.629	0.435	0.391	0.494	0.624	0.548
	(0.090)	(0.087)	(0.096)	(0.101)	(0.115)	(0.179)	(0.090)	(0.117)	(0.183)	(0.159)	
	956	818	710	614	488	326	208	147	97	54	
10	0.580	0.529	0.545	0.521	0.550	0.633	0.421	0.388	0.502	0.698	0.544
	(0.095)	(0.092)	(0.101)	(0.106)	(0.124)	(0.197)	(0.092)	(0.124)	(0.201)	(0.192)	
	895	766	660	569	449	298	185	129	83	45	
11	0.630	0.567	0.590	0.576	0.602	0.691	0.460	0.380	0.461	0.650	0.588
	(0.099)	(0.099)	(0.107)	(0.113)	(0.134)	(0.220)	(0.093)	(0.140)	(0.234)	(0.245)	
	818	696	595	510	399	255	149	98	59	31	
12	0.648	0.592	0.623	0.589	0.624	0.747	0.474	0.386	0.400	0.604	0.612
	(0.109)	(0.108)	(0.117)	(0.123)	(0.151)	(0.258)	(0.119)	(0.164)	(0.247)	(0.271)	
	743	633	541	465	358	224	121	78	46	24	
13	0.667	0.612	0.649	0.625	0.605	0.533	0.462	0.287	0.395	0.363	0.612
	(0.122)	(0.107)	(0.110)	(0.113)	(0.114)	(0.117)	(0.155)	(0.215)	(0.301)	(0.164)	
	656	554	470	399	307	184	96	57	31	13	
14	0.714	0.692	0.714	0.681	0.659	0.629	0.511	0.457	0.986	0.761	0.685
	(0.129)	(0.104)	(0.116)	(0.120)	(0.122)	(0.115)	(0.182)	(0.311)	(0.411)	(0.368)	
	590	495	415	349	263	146	70	36	15	7	
15	0.680	0.664	0.662	0.616	0.651	0.597	0.532	0.576	1.527	0.954	0.656
	(0.134)	(0.099)	(0.109)	(0.108)	(0.123)	(0.129)	(0.216)	(0.393)	(0.258)	(0.700)	
	533	448	374	309	228	120	54	24	11	6	
wgt avg.	0.539	0.501	0.517	0.485	0.501	0.510	0.405	0.374	0.432	0.469	

Table 2: Estimates of the father-son IGE in labor income

Time Avg. Fath. Inc.	Time Average of Sons' Income (years) 1	2	3	4	5	6	7	8	9	10	Wgt. Avg.
1	0.299	0.308	0.308	0.335	0.358	0.333	0.373	0.395	0.384	0.359	0.359
	(0.072)	(0.069)	(0.063)	(0.064)	(0.065)	(0.066)	(0.069)	(0.070)	(0.085)	(0.084)	
	955	824	696	581	466	360	264	202	156	104	
2	0.412	0.412	0.407	0.405	0.407	0.369	0.439	0.422	0.427	0.412	0.383
	(0.063)	(0.061)	(0.061)	(0.065)	(0.074)	(0.075)	(0.059)	(0.059)	(0.077)	(0.081)	
	928	799	674	562	450	345	250	191	147	96	
3	0.436	0.422	0.401	0.395	0.393	0.368	0.440	0.430	0.441	0.421	0.473
	(0.061)	(0.061)	(0.059)	(0.064)	(0.073)	(0.076)	(0.056)	(0.055)	(0.072)	(0.080)	
	900	773	649	539	431	329	236	180	137	88	
4	0.420	0.412	0.408	0.392	0.387	0.359	0.435	0.404	0.409	0.395	0.491
	(0.064)	(0.063)	(0.062)	(0.067)	(0.076)	(0.077)	(0.058)	(0.058)	(0.073)	(0.082)	
	864	741	622	514	411	310	218	163	123	80	
5	0.472	0.462	0.440	0.416	0.397	0.367	0.445	0.416	0.437	0.437	0.516
	(0.069)	(0.066)	(0.067)	(0.071)	(0.080)	(0.082)	(0.059)	(0.059)	(0.077)	(0.091)	
	841	720	609	502	401	300	209	155	116	76	
6	0.485	0.473	0.450	0.432	0.402	0.360	0.455	0.434	0.471	0.488	0.490
	(0.071)	(0.068)	(0.068)	(0.074)	(0.083)	(0.082)	(0.059)	(0.061)	(0.084)	(0.102)	
	797	683	574	469	371	274	190	139	101	63	
7	0.486	0.468	0.440	0.420	0.385	0.332	0.441	0.401	0.432	0.430	0.497
	(0.077)	(0.074)	(0.075)	(0.082)	(0.091)	(0.087)	(0.073)	(0.080)	(0.104)	(0.106)	
	760	652	546	445	349	255	174	123	88	52	
8	0.510	0.487	0.473	0.459	0.451	0.407	0.414	0.352	0.382	0.409	0.559
	(0.085)	(0.081)	(0.079)	(0.087)	(0.094)	(0.080)	(0.092)	(0.108)	(0.135)	(0.117)	
	725	622	518	419	327	235	158	110	80	45	
9	0.511	0.476	0.461	0.445	0.452	0.408	0.427	0.356	0.392	0.406	0.583
	(0.086)	(0.081)	(0.079)	(0.088)	(0.094)	(0.080)	(0.092)	(0.109)	(0.136)	(0.122)	
	699	597	500	404	314	225	149	103	73	41	
10	0.513	0.474	0.468	0.461	0.475	0.426	0.446	0.413	0.443	0.516	0.593
	(0.088)	(0.084)	(0.081)	(0.092)	(0.102)	(0.085)	(0.101)	(0.122)	(0.161)	(0.144)	
	657	561	470	377	290	203	130	89	62	34	
11	0.578	0.503	0.491	0.494	0.528	0.429	0.461	0.437	0.483	0.526	0.616
	(0.093)	(0.091)	(0.087)	(0.096)	(0.106)	(0.092)	(0.116)	(0.143)	(0.203)	(0.186)	
	597	512	425	340	255	176	107	69	48	24	
12	0.596	0.506	0.526	0.496	0.565	0.413	0.427	0.376	0.346	0.391	0.611
	(0.102)	(0.108)	(0.105)	(0.113)	(0.131)	(0.118)	(0.158)	(0.158)	(0.233)	(0.157)	
	539	461	380	302	226	151	85	53	34	15	
13	0.690	0.589	0.628	0.636	0.701	0.551	0.494	0.556	0.790	0.323	0.615
	(0.111)	(0.114)	(0.112)	(0.127)	(0.153)	(0.143)	(0.176)	(0.208)	(0.316)	(0.323)	
	477	401	327	254	185	118	66	38	22	7	
14	0.744	0.651	0.676	0.715	0.793	0.699	0.638	0.695	1.526	5.971	0.702
	(0.116)	(0.122)	(0.119)	(0.138)	(0.161)	(0.158)	(0.216)	(0.266)	(0.259)	(0.916)	
	427	356	285	218	161	96	48	25	10	4	
15	0.751	0.623	0.649	0.652	0.719	0.641	0.547	0.659	1.335	4.240	0.713
	(0.122)	(0.127)	(0.125)	(0.138)	(0.163)	(0.179)	(0.266)	(0.282)	(0.253)	(0.000)	
	386	319	251	189	135	78	35	18	7	3	
wgt avg.	0.574	0.517	0.458	0.469	0.504	0.471	0.519	0.535	0.564	0.592	

Table 3: Estimates of the father-son IGE in family income using fixed samples

Time Avg. Father Inc.	Samples use sons with 1 year of income and fathers with the following available years of income: 3	5	7	10	15
1	0.397	0.393	0.358	0.388	0.291
	(0.070)	(0.074)	(0.078)	(0.100)	(0.131)
2	0.438	0.430	0.401	0.429	0.366
	(0.068)	(0.071)	(0.076)	(0.097)	(0.122)
3	0.478	0.473	0.446	0.497	0.453
	(0.067)	(0.070)	(0.075)	(0.095)	(0.124)
4		0.484	0.460	0.522	0.479
		(0.069)	(0.072)	(0.091)	(0.114)
5		0.530	0.504	0.579	0.568
		(0.071)	(0.075)	(0.094)	(0.121)
6			0.515	0.581	0.580
			(0.075)	(0.092)	(0.118)
7			0.529	0.595	0.622
			(0.077)	(0.094)	(0.120)
8				0.583	0.619
				(0.094)	(0.119)
9				0.584	0.635
				(0.095)	(0.123)
10				0.580	0.643
				(0.095)	(0.122)
13					0.662
					(0.132)
15					0.680
					(0.134)
N	*1268*	*1175*	*1063*	*895*	*533*

Table 4: Estimates of the father-son rank-rank slope in family income

	Time Average of Sons' Income (years)										
Time Avg. Fath. Inc.	1	2	3	4	5	6	7	8	9	10	Wgt. Avg.
1	0.282	0.304	0.326	0.309	0.333	0.322	0.295	0.290	0.307	0.423	0.310
	(0.032)	(0.036)	(0.039)	(0.040)	(0.043)	(0.050)	(0.059)	(0.065)	(0.071)	(0.080)	
	1358	1184	1050	932	786	595	440	351	267	183	
2	0.290	0.312	0.341	0.329	0.362	0.376	0.352	0.339	0.356	0.448	0.334
	(0.032)	(0.035)	(0.038)	(0.040)	(0.043)	(0.050)	(0.059)	(0.065)	(0.073)	(0.083)	
	1317	1145	1015	901	758	572	419	331	251	170	
3	0.296	0.317	0.341	0.328	0.362	0.379	0.373	0.352	0.375	0.451	0.338
	(0.032)	(0.035)	(0.039)	(0.041)	(0.043)	(0.049)	(0.055)	(0.065)	(0.072)	(0.086)	
	1268	1099	970	862	719	537	389	306	230	154	
4	0.296	0.320	0.347	0.333	0.366	0.391	0.396	0.363	0.389	0.435	0.343
	(0.032)	(0.036)	(0.039)	(0.041)	(0.044)	(0.050)	(0.055)	(0.067)	(0.078)	(0.096)	
	1216	1051	926	819	678	497	354	273	203	133	
5	0.309	0.333	0.362	0.348	0.378	0.399	0.413	0.374	0.402	0.482	0.357
	(0.032)	(0.036)	(0.040)	(0.042)	(0.045)	(0.052)	(0.058)	(0.069)	(0.083)	(0.097)	
	1175	1015	892	788	649	471	332	255	188	123	
6	0.299	0.319	0.348	0.333	0.362	0.385	0.404	0.365	0.399	0.504	0.344
	(0.034)	(0.037)	(0.042)	(0.044)	(0.047)	(0.054)	(0.063)	(0.072)	(0.088)	(0.095)	
	1120	966	843	741	606	431	299	228	165	105	
7	0.283	0.302	0.328	0.311	0.336	0.350	0.370	0.316	0.339	0.436	0.318
	(0.035)	(0.039)	(0.044)	(0.046)	(0.049)	(0.057)	(0.067)	(0.076)	(0.095)	(0.104)	
	1063	915	795	696	564	396	271	202	143	87	
8	0.282	0.303	0.336	0.321	0.348	0.362	0.332	0.279	0.280	0.396	0.317
	(0.037)	(0.042)	(0.046)	(0.049)	(0.052)	(0.062)	(0.075)	(0.088)	(0.112)	(0.123)	
	1005	863	747	648	520	354	232	168	114	67	
9	0.292	0.309	0.342	0.333	0.365	0.394	0.403	0.327	0.318	0.493	0.334
	(0.038)	(0.043)	(0.048)	(0.050)	(0.053)	(0.062)	(0.071)	(0.091)	(0.125)	(0.113)	
	956	818	710	614	488	326	208	147	97	54	
10	0.287	0.304	0.330	0.319	0.352	0.379	0.397	0.330	0.314	0.539	0.325
	(0.039)	(0.044)	(0.049)	(0.051)	(0.054)	(0.064)	(0.074)	(0.098)	(0.138)	(0.134)	
	895	766	660	569	449	298	185	129	83	45	
11	0.299	0.315	0.345	0.338	0.364	0.394	0.413	0.315	0.267	0.518	0.336
	(0.040)	(0.046)	(0.051)	(0.054)	(0.057)	(0.069)	(0.080)	(0.112)	(0.162)	(0.169)	
	818	696	595	510	399	255	149	98	59	31	
12	0.310	0.326	0.354	0.336	0.363	0.386	0.390	0.303	0.236	0.587	0.339
	(0.042)	(0.049)	(0.054)	(0.057)	(0.062)	(0.077)	(0.093)	(0.125)	(0.179)	(0.196)	
	743	633	541	465	358	224	121	78	46	24	
13	0.335	0.355	0.392	0.384	0.385	0.357	0.311	0.133	0.076	0.264	0.354
	(0.046)	(0.051)	(0.055)	(0.057)	(0.065)	(0.088)	(0.118)	(0.162)	(0.232)	(0.295)	
	656	554	470	399	307	184	96	57	31	13	
14	0.356	0.391	0.433	0.428	0.432	0.445	0.400	0.322	0.446	0.618	0.403
	(0.048)	(0.050)	(0.055)	(0.057)	(0.066)	(0.088)	(0.127)	(0.191)	(0.285)	(0.244)	
	590	495	415	349	263	146	70	36	15	7	
15	0.350	0.382	0.428	0.426	0.447	0.436	0.418	0.399	0.635	0.423	0.401
	(0.050)	(0.051)	(0.057)	(0.057)	(0.068)	(0.095)	(0.137)	(0.242)	(0.231)	(0.382)	
	533	448	374	309	228	120	54	24	11	6	
wgt avg.	0.300	0.321	0.350	0.337	0.364	0.377	0.371	0.329	0.346	0.457	

Figure 1: Comparison of life cycle coverage across intergenerational Samples

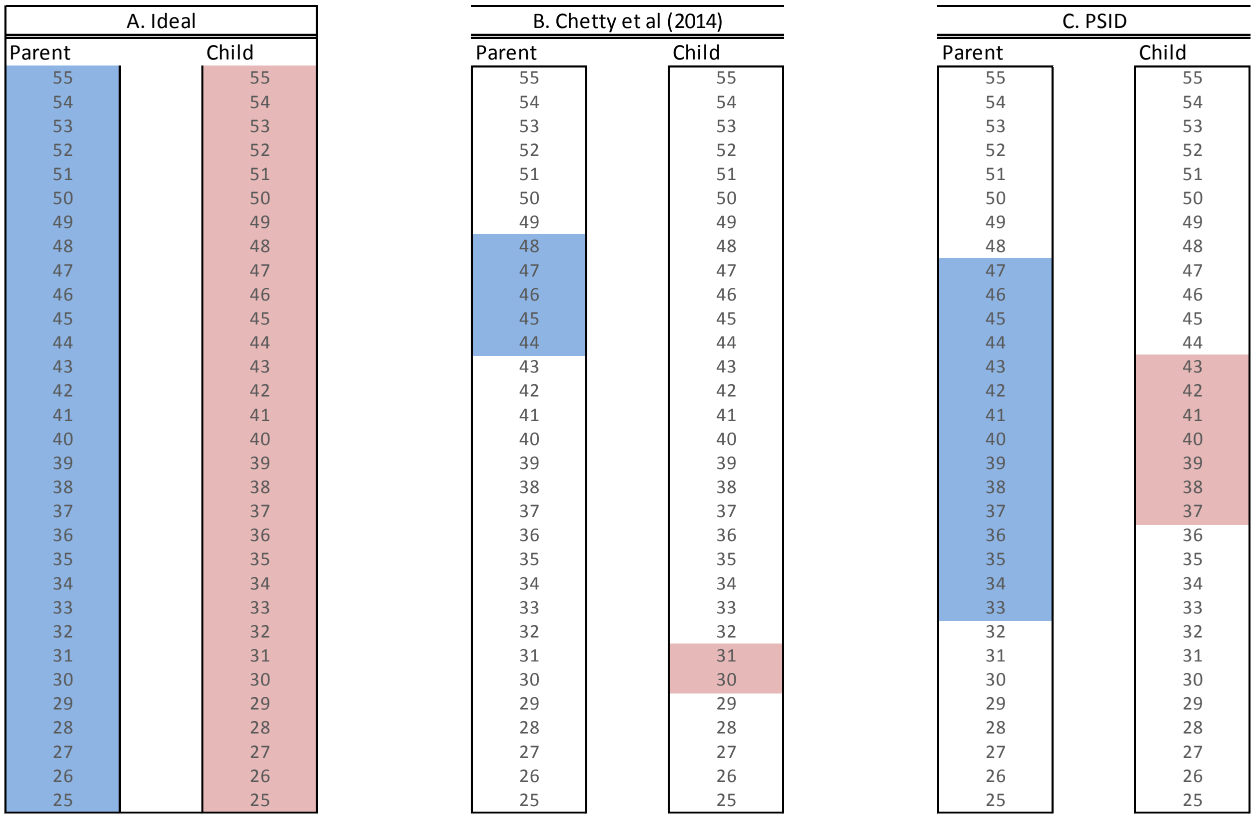


Figure 2: Effects of time averaging on father-son IGE in family income

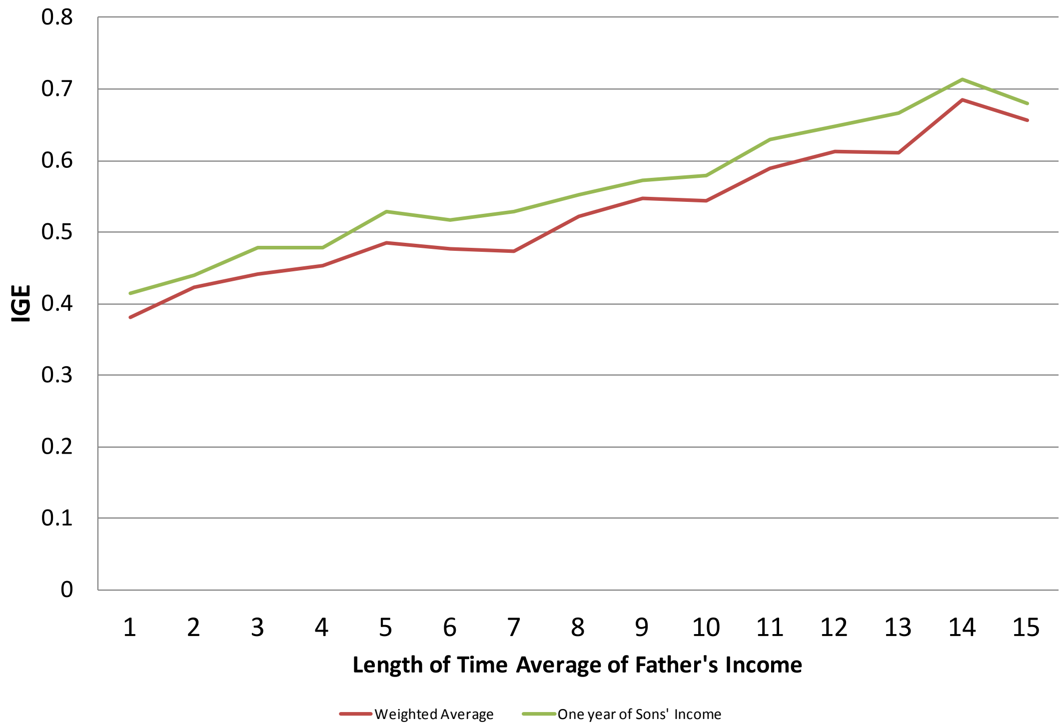


Figure 3: Father-son IGE in labor income vs family income

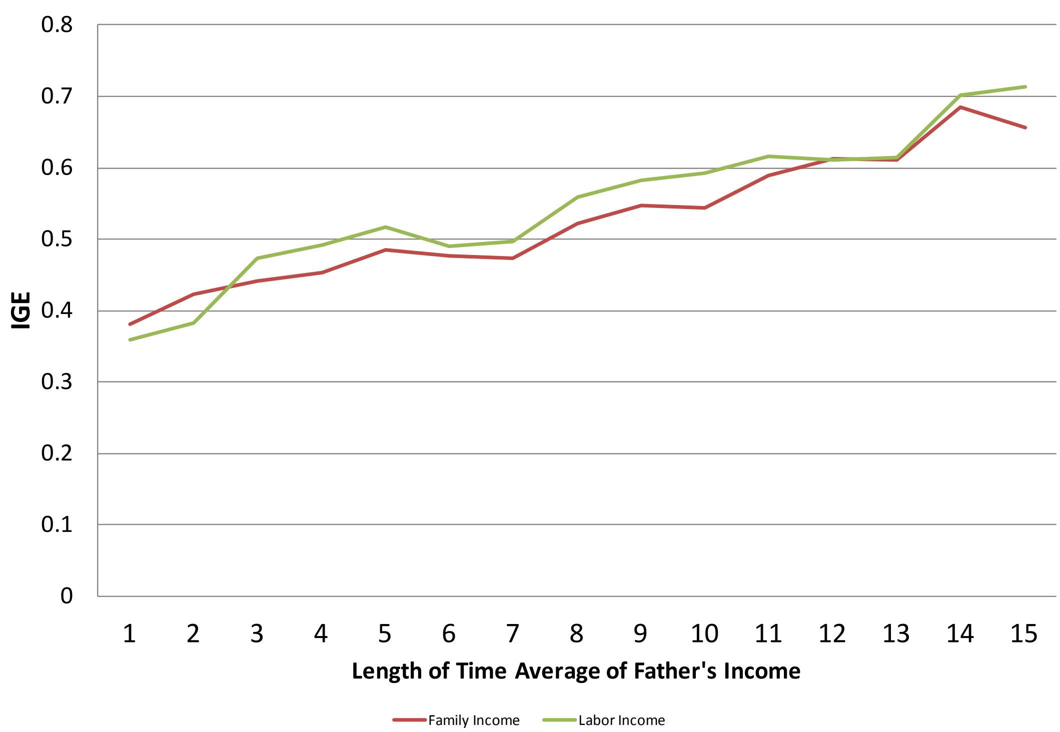


Figure 4: Re-examining the Sensitivity of the IGE to Son's Age

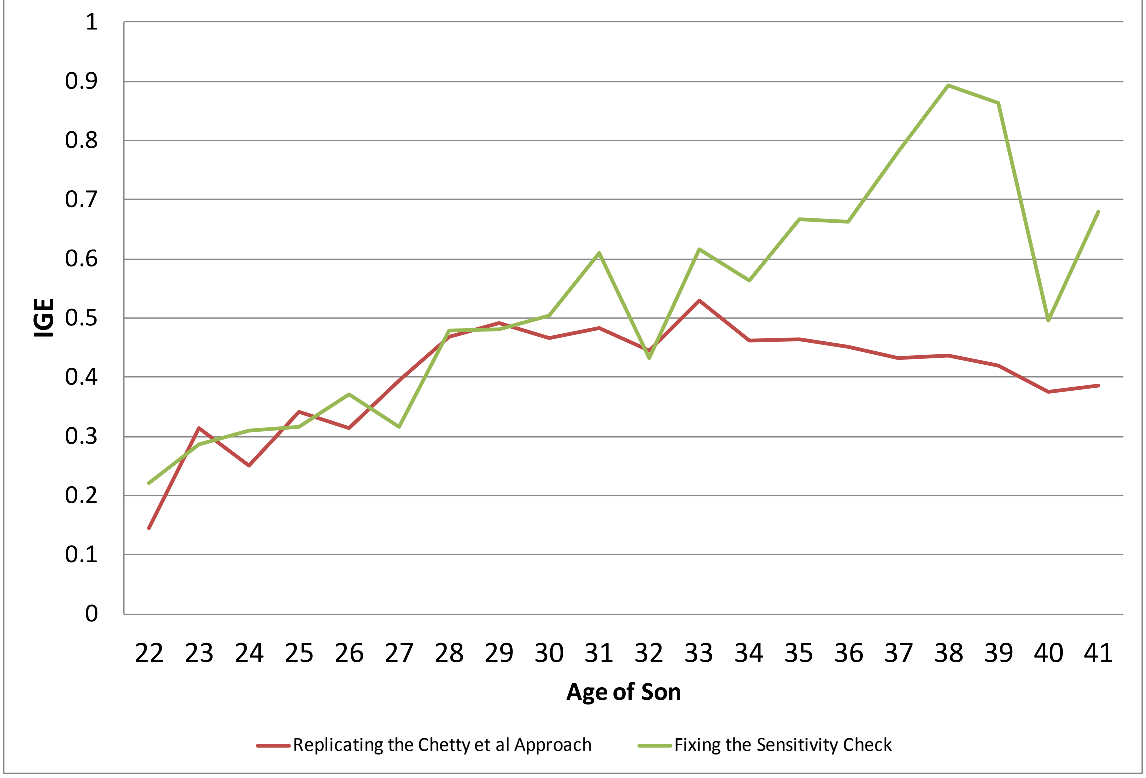


Figure 5: Re-examining the Sensitivity of the IGE to longer time averages of parent income

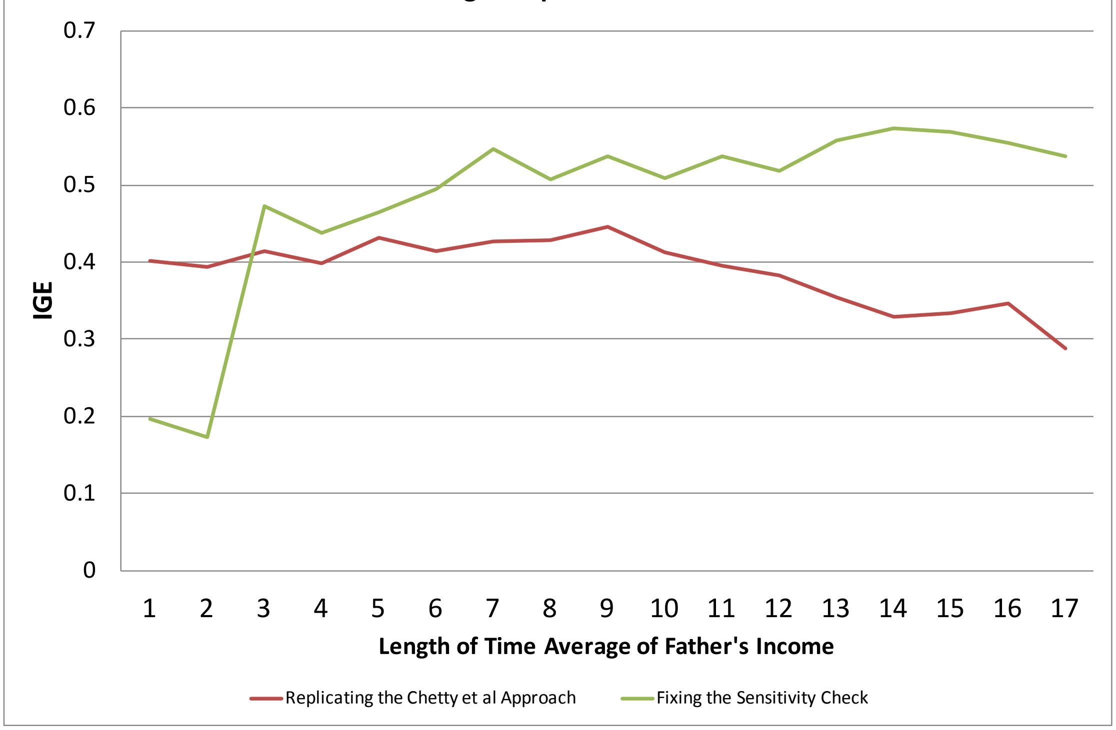


Figure 6: Re-examining the Sensitivity of the Rank-rank Slope to Son's Age

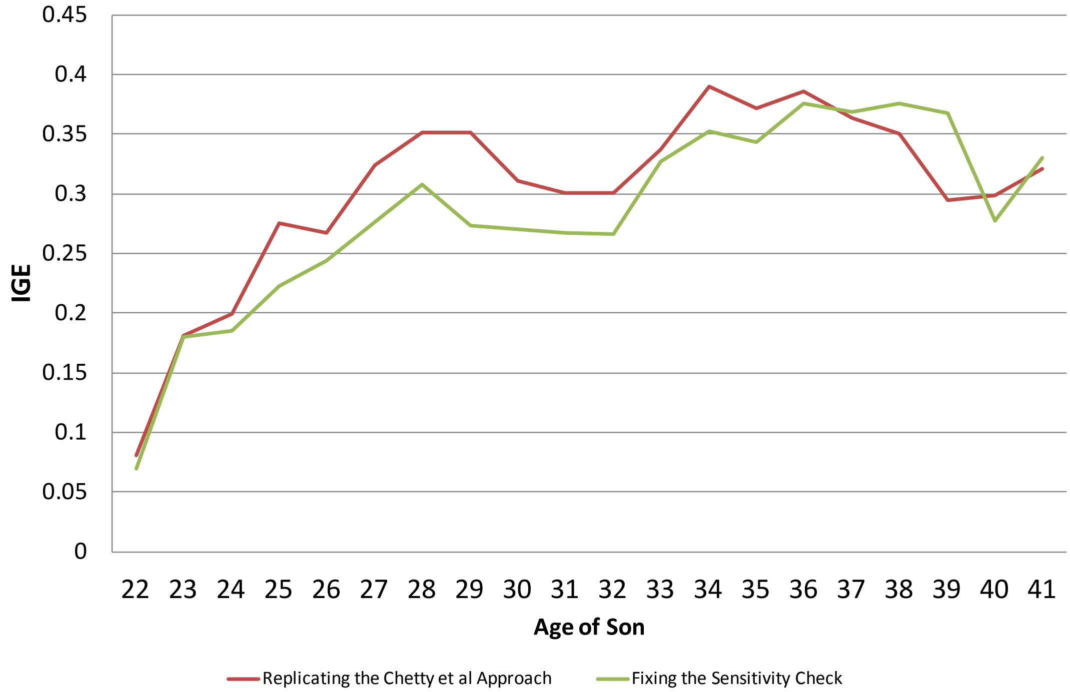
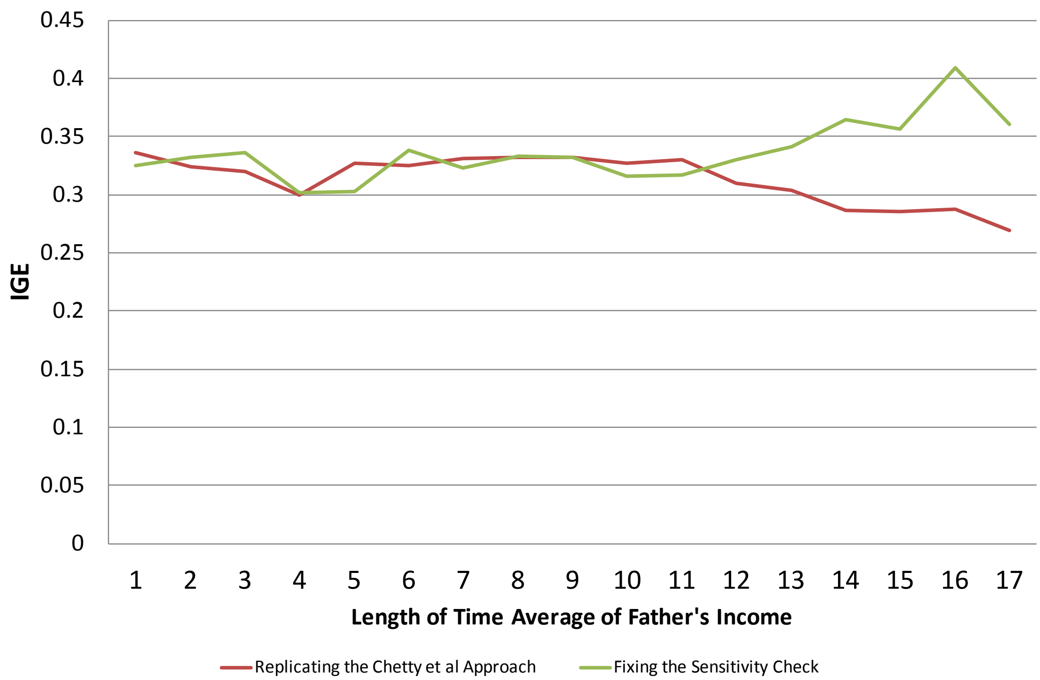


Figure 7: Re-examining the Sensitivity of the rank-rank Slope to longer time averages of parent income

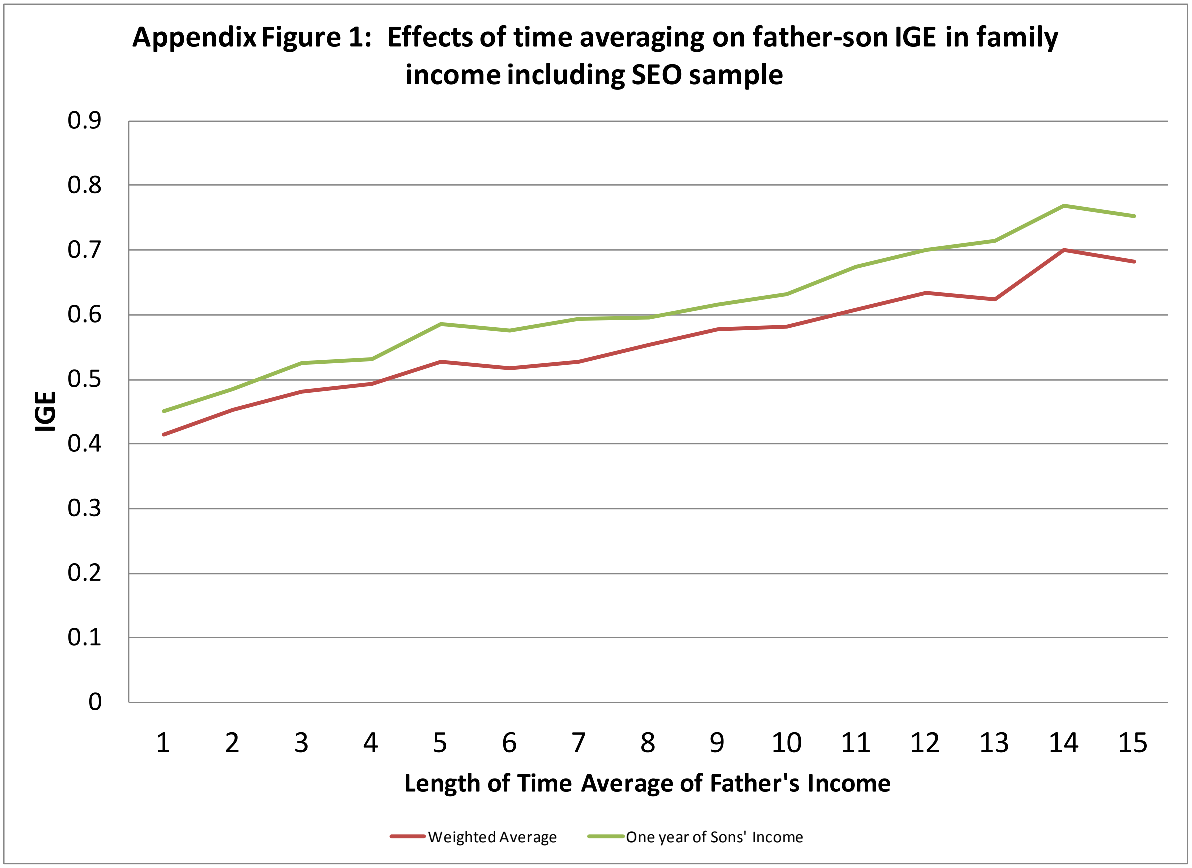


Appendix Table 1: Estimates of the father-son IGE in family income using SEO subsample

	Time Average of Sons' Income (years)										
Time Avg. Fath. Inc.	1	2	3	4	5	6	7	8	9	10	Wgt. Avg.
1	0.451	0.403	0.434	0.414	0.426	0.393	0.357	0.363	0.383	0.401	0.415
	(0.054)	(0.048)	(0.049)	(0.051)	(0.047)	(0.050)	(0.047)	(0.054)	(0.066)	(0.077)	
	2133	1842	1611	1400	1158	867	623	490	364	251	
2	0.485	0.450	0.468	0.438	0.457	0.451	0.406	0.402	0.418	0.430	0.453
	(0.053)	(0.045)	(0.047)	(0.049)	(0.052)	(0.061)	(0.048)	(0.053)	(0.063)	(0.069)	
	2062	1774	1550	1345	1109	828	590	460	340	234	
3	0.526	0.476	0.491	0.457	0.476	0.473	0.439	0.428	0.446	0.461	0.480
	(0.056)	(0.049)	(0.054)	(0.053)	(0.059)	(0.068)	(0.051)	(0.054)	(0.065)	(0.074)	
	1989	1706	1483	1287	1054	780	548	427	313	213	
4	0.531	0.487	0.508	0.476	0.487	0.493	0.449	0.435	0.464	0.443	0.492
	(0.058)	(0.051)	(0.058)	(0.059)	(0.066)	(0.080)	(0.052)	(0.056)	(0.072)	(0.079)	
	1865	1594	1389	1200	975	709	491	379	278	186	
5	0.586	0.526	0.544	0.510	0.514	0.508	0.457	0.446	0.482	0.483	0.527
	(0.062)	(0.056)	(0.066)	(0.067)	(0.076)	(0.092)	(0.058)	(0.062)	(0.082)	(0.093)	
	1780	1519	1320	1137	917	661	452	346	253	167	
6	0.575	0.515	0.535	0.499	0.506	0.503	0.437	0.425	0.476	0.481	0.518
	(0.063)	(0.056)	(0.068)	(0.069)	(0.079)	(0.097)	(0.057)	(0.062)	(0.086)	(0.094)	
	1677	1429	1235	1057	848	597	398	300	215	137	
7	0.593	0.528	0.548	0.514	0.516	0.513	0.420	0.418	0.446	0.428	0.528
	(0.067)	(0.063)	(0.076)	(0.079)	(0.093)	(0.119)	(0.060)	(0.071)	(0.099)	(0.102)	
	1579	1343	1155	984	783	543	357	263	186	115	
8	0.595	0.544	0.576	0.546	0.554	0.587	0.422	0.411	0.480	0.514	0.553
	(0.073)	(0.069)	(0.077)	(0.081)	(0.090)	(0.125)	(0.079)	(0.097)	(0.137)	(0.136)	
	1490	1266	1087	918	724	490	313	225	154	95	
9	0.616	0.560	0.588	0.560	0.571	0.622	0.479	0.461	0.546	0.660	0.577
	(0.077)	(0.073)	(0.081)	(0.085)	(0.094)	(0.135)	(0.077)	(0.102)	(0.153)	(0.140)	
	1405	1190	1025	862	674	447	276	195	129	74	
10	0.633	0.558	0.587	0.555	0.575	0.645	0.476	0.451	0.547	0.691	0.582
	(0.082)	(0.079)	(0.087)	(0.092)	(0.105)	(0.157)	(0.086)	(0.109)	(0.170)	(0.155)	
	1321	1123	961	803	623	410	245	172	112	63	
11	0.674	0.584	0.618	0.587	0.599	0.669	0.471	0.385	0.470	0.606	0.608
	(0.087)	(0.086)	(0.093)	(0.099)	(0.114)	(0.179)	(0.084)	(0.119)	(0.195)	(0.183)	
	1182	1003	849	703	540	342	194	130	81	44	
12	0.700	0.610	0.650	0.601	0.620	0.720	0.482	0.381	0.415	0.617	0.633
	(0.097)	(0.095)	(0.103)	(0.110)	(0.131)	(0.214)	(0.108)	(0.143)	(0.215)	(0.226)	
	1068	903	764	632	478	297	158	103	63	34	
13	0.714	0.619	0.656	0.621	0.598	0.526	0.454	0.291	0.392	0.389	0.625
	(0.111)	(0.097)	(0.099)	(0.104)	(0.104)	(0.106)	(0.144)	(0.187)	(0.251)	(0.161)	
	942	794	667	546	414	248	126	78	45	21	
14	0.769	0.698	0.721	0.678	0.651	0.616	0.507	0.436	0.909	0.859	0.700
	(0.118)	(0.094)	(0.106)	(0.112)	(0.112)	(0.104)	(0.170)	(0.274)	(0.354)	(0.353)	
	831	694	581	471	350	193	88	48	21	8	
15	0.752	0.675	0.677	0.626	0.653	0.598	0.546	0.569	1.318	1.094	0.682
	(0.123)	(0.090)	(0.099)	(0.100)	(0.112)	(0.114)	(0.200)	(0.335)	(0.334)	(0.642)	
	745	624	521	419	304	163	70	34	16	7	
wgt avg.	0.587	0.526	0.549	0.515	0.522	0.524	0.435	0.415	0.461	0.479	

Appendix Table 2: Summary statistics of fathers by available years of income

	N	Income ($)	Age	Father Education	Mother Education	Black	White	Married	Ever Divorced
1	1358		41.6	12.9	12.2	5.1%	94.1%	97.2%	24.0%
			(0.1)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.01)
2	1317		41.9	12.9	12.2	5.1%	94.1%	97.2%	24.5%
			(0.1)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.01)
3	1268	79233	41.5	12.9	12.2	5.1%	94.2%	97.2%	25.1%
		(1601)	(0.1)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.01)
4	1216	79805	41.7	12.9	12.3	5.3%	94.0%	97.2%	25.9%
		(1565)	(0.1)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.01)
5	1175	79477	41.4	12.9	12.3	5.2%	94.0%	97.2%	26.3%
		(1520)	(0.1)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.01)
6	1120	80877	41.6	12.9	12.3	5.3%	94.0%	97.2%	26.5%
		(1630)	(0.1)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.01)
7	1063	80962	41.3	12.9	12.3	5.2%	94.0%	97.2%	26.9%
		(1659)	(0.1)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.01)
8	1005	81612	41.5	12.9	12.3	5.5%	93.7%	97.3%	27.5%
		(1675)	(0.1)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.01)
9	956	81175	41.2	12.9	12.3	5.4%	93.8%	97.4%	27.5%
		(1682)	(0.1)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.01)
10	895	82609	41.4	12.9	12.4	5.1%	94.0%	97.3%	27.9%
		(1744)	(0.1)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.02)
11	818	82650	40.9	12.9	12.5	5.3%	93.7%	97.1%	29.1%
		(1780)	(0.1)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.02)
12	743	82005	41.0	13.0	12.5	5.2%	93.8%	96.8%	29.0%
		(1823)	(0.1)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.02)
13	656	81503	40.6	13.0	12.5	5.2%	93.7%	96.4%	30.0%
		(1909)	(0.1)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.02)
14	590	81444	40.7	13.0	12.5	5.7%	93.1%	96.1%	31.4%
		(2012)	(0.0)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.02)
15	533	82272	40.3	13.0	12.5	5.9%	92.7%	95.9%	32.5%
		(2131)	(0.0)	(0.1)	(0.1)	(0.01)	(0.01)	(0.01)	(0.02)
Two-Sample Test in Difference of Means									
5 v. 10	T-Stat	-1.35	0.38	-0.32	-0.45	0.10	0.03	-0.12	-0.80
	P-Val	0.18	0.71	0.75	0.65	0.92	0.98	0.91	0.42
5 v. 15	T-Stat	-1.07	13.49	-0.68	-1.64	-0.56	0.98	1.34	-2.57
	P-Val	0.29	0.00	0.50	0.10	0.58	0.33	0.18	0.01


Appendix Figure 1: Effects of time averaging on father-son IGE in family income including SEO sample
IGE
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
Length of Time Average of Father's Income
Weighted Average
One year of Sons' Income

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