Estimating the Intergenerational Elasticity and Rank Association in the US:

Overcoming the Current Limitations of Tax Data

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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. A recent influential study by Chetty et al (2014) using tax data, estimates the IGE in family income for the entire U.S. to be 0.344, considerably lower than most previous estimates. Despite the seeming advantages of extremely large samples of administrative tax data, I demonstrate that the age structure and limited panel dimension of the data used by Chetty et al leads to considerable downward bias in estimating the IGE. Specifically I use PSID samples that overcome the data limitations in the tax data to estimate the IGE when using long time averages centered around age 40 in both generations. I demonstrate how imposing the data limitations in Chetty et al (2014) lead to considerable downward bias relative to these preferred estimates. 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. The lack of robustness of the IGE to the treatment of years of zero earnings among children found by Chetty et al is also largely due to data limitations . Estimates of the rank-rank slope on the other hand are much less downward biased and tend to be more robust to the limitations of the tax data. Nevertheless, researchers should continue to use both the IGE and rank based measures depending on which concept of mobility they wish to address. Substantively, I find that the IGE in family income in the U.S. is likely greater than 0.6 and that the rank-rank slope is 0.4 or higher.

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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 resulted in 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. Of course, 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 will focus on two prominent measures of intergenerational mobility: the intergenerational elasticity (IGE) and the rank-rank slope and discuss several key conceptual and measurement issues related to these estimators in the context of the U.S.

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 family to rise to the mean level of 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] In a recent highly influential study

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

[2] Solon (1992) estimates the IGE in family income to be 0.483 (Table 4). Hertz (2005) reports an elasticity in age adjusted family income of 0.538. Hertz (2006) estimates the IGE in family income to be 0.58 and the IGE in family income per person to be 0.52. Bratsberg et al (2007) produce an estimate of the elasticity of family income on earnings of 0.54. Jantti et al (2006) produce an estimate of the same elasticity of 0.517. In making comparisons

Chetty et al (2014) use large samples drawn from IRS tax records produce estimates of the IGE in family income of just 0.344. As I show below, such an estimate paints a dramatically different view of the rate at which a family can expect to escape poverty. The main focus of Chetty et al (2014) is not their national IGE estimates. Instead Chetty et al are the first to produce estimates of mobility at a very detailed level of U.S. geography and to provide evidence of substantial heterogeneity across the U.S. Nevertheless, Chetty et al make strong claims about their tax their IGE estimates, arguing that none of the previous biases in the literature 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 issues concerning measurement and life-cycle bias in the context of their sample.

A key point of this paper is to demonstrate that despite having extremely large sample sizes, the IRS-based intergenerational sample used by Chetty et al is fundamentally limited in a few key respects stemming from the fact that the data is only available going back to 1996. First, children's income is only measured in 2011 and 2012. This is at a relatively early point in the life cycle for cohorts born between 1980 and 1982 and at a time in which unemployment was quite high in the US. Even setting aside business cycle effects, this is an age at which we would expect substantial life cycle bias based on the estimates from the prior literature (Haider and Solon, 2006). Moreover, relative to an 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. I estimate that about 25% of observations of fathers' income in their sample are

with Chetty et al (2014) it is important to distinguish these estimates from those that use a different income concept such as labor market earnings in both generations.

measured at age 50 or higher. The prior literature has shown that the transitory fluctuations in income comprise a substantial share of the variance in parent income around the age of 50 also attenuating the estimate of the IGE relative to what would be found if one used lifetime income (Haider and Solon, 2006; Mazumder, 2005a). Third, recent research has established that administrative data can 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).

I use the PSID which covers income going back to 1967 to demonstrate the implications of these data limitations, empirically. First, I construct an intergenerational sample where both kids and parents family income is observed over a vastly larger portion of the lifecycle than the IRS records and where crucially, 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 of this exercise show that the data limitations lead to IGE estimates that are about 60 percent of the size of the estimates with the complete data and similar in magnitude to the estimates of Chetty et al.

Chetty et al. also find that with their tax data the IGE estimates are very sensitive to how they choose to impute the income of children who report no family income during 2011 and 2012. This apparent sensitivity of the IGE estimates is also due in large part to the limitations of the tax data that is currently available rather than an intrinsic feature of the estimator. The critical issue is that it is the limited panel dimension of the tax data that makes the analysis especially sensitive to this problem. This is also important because it is this concern about robustness of the IGE with their particular sample that is a primary reason that Chetty et al turn to rank-based estimators.[3] This can be contrasted with several other studies (Bhattacharya and Mazumder, 2011; Corak et al., 2014; Mazumder, 2014; Davis and Mazumder, 2015; and Bratberg et al., 2015), that have also used rank-based measures to study intergenerational mobility but for conceptual reasons.

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, I argue that conceptually, both measures can provide useful insights about different aspects of mobility. I argue that there are clearly certain questions that are best answered by the IGE and for that reason researchers should continue to use the IGE as at least one tool for measuring intergenerational mobility. Nevertheless, rank-based estimators are also valuable because in addition to providing information on a different concept of mobility, positional mobility, rank-based measures are also useful for distinguishing upward versus downward movements, making subgroup comparisons and for identifying nonlinearities in intergenerational mobility. In fact, I would argue that even if Chetty et al found the IGE to be perfectly robust in their tax data, that it would still be preferable to use rank-mobilty 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 in Charlotte whereas rank estimators can use ranks that are fixed to the national distribution which may make for a more meaningful comparison across cities.

Perhaps the most significant contribution of the paper is to show that the magnitude of the IGE estimates when using many years of income data centered over the prime working years in both generations are higher than almost all previous estimates in the literature. For example,

[3] Dahl and Deliere (2009) 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. I discuss the problems with Dahl and Deliere's analysis in footnote 14 in section 3.

the estimates of the IGE with respect to family income are greater than 0.6. Turning to a different measure of income, the labor income of male heads of household, I also find that the IGE is greater than 0.6 and roughly similar to the estimate found by Mazumder (2005a) using social security earnings data. Chetty et al suggest that the high estimates in Mazumder (2005a) are due to data imputations of fathers earnings that are topcoded in some years. The analysis here suggests that those earlier findings can easily be replicated using publicly available survey data that requires no imputations of topcoded earnings whatsoever. Instead, obtaining such high estimates of the IGE simply requires using samples with the appropriate ages and long time spans of available income data centered around the prime working ages in both generations. I also point to other studies in the literature that yield findings consistent with Mazumder (2005a) that do not require imputations (e.g. Mazumder, 2005b; Nilsen et al (2012); and Mazumder and Acosta, 2014).

A final exercise uses the PSID data to estimate the rank-rank slope which is one of the two main measures used by Chetty et al (2014). In this case the estimates are only moderately larger with the PSID (0.4 or higher) than what one obtains when using the IRS data (0.341) or the PSID data when imposing the age structure and short panels found in the tax data. This suggests that while 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. These pattern of results are broadly in line with similar findings for Sweden (Nybom and Stuhler, 2015).

One clear conclusion to be drawn from this paper is that researchers should continue to use the IGE if that is the conceptual parameter of interest and when their intergenerational samples have the appropriate panel lengths and age structure. Even when the ideal data is not available, researchers can still attempt to assess the extent of the bias based on prior papers in the literature that propose methodological fixes. Substantively, the results of this paper show that intergenerational mobility in the US is substantially lower than what one would think based on using the currently available IRS data especially if one is interested in the rate at which income gaps between families are closed. If one is more interested in purely positional mobility then the bias due to the data limitations in the tax data is less severe. Over the next few decades, as the panel length of the tax data increases, these biases will recede in importance. However, one cannot know with certainty whether researchers will be able to obtain such tax data in future decades.

The rest of the paper proceeds as follows. Section 2 describes the conceptual differences between the estimators. Section 3 describes measurement issues with IGE estimation and describes the structure of an "ideal" dataset. It then compares this ideal dataset with the IRS-based intergenerational sample used by Chetty et al (2014) and a close to ideal sample that can be constructed with publicly available PSID survey data. Section 4 outlines the PSID data used for the analysis. Section 5 presents the main results when using the PSID and demonstrates the effects of imposing the limitations with the tax data. 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:

$$(1) \qquad y_{1i} = \alpha + \beta y_{0i} + \varepsilon_i$$

where y_{1i} is the log income of the child generation and y_{0i} is the log of income in the parent generation.[4] The estimate of β provides a measure of intergenerational persistence and 1 - β can be used as a measure of mobility. One way to interpret the elasticity in practical terms is to consider what it implies about how many generations it would take for a family living in poverty to attain close to the national average household income level. If for example, the IGE is around 0.60 as claimed by Mazumder (2005a) then it would take 6 generations (150 years). On the other hand if the IGE is around 0.34 as claimed by Chetty et al (2014) then it would take just 3 generations.[5] Clearly, the two estimates have profoundly different implications on the rate of intergenerational mobility and if the rate of regression to the mean is what we are interested in knowing then the IGE is what we ought to estimate. 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. One can imagine that depending on the shape of the income distribution the descendants of a family in poverty might experience

[4] 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

[5] This exercise is based on Mazumder (2005) and considers a family of four with two children under the age of 18 whose income is $18,000 (under the poverty threshold in 2013 of around $24,000). Mean household income in 2013 was approximately $73,000. The calculation specifically examines how many generations before the expected income of the descendants of this family would be within 5% of the national average.

relatively rapid positional mobility but slower regression to the mean.[6] 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 generation to the standard deviation of log income in the parent 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 it does not make sense to pretend that we can separately isolate 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. I would argue 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.

[6] In the example above, one can imagine that it may take significantly longer for the family with income of $18,000 to attain the mean income level of $73,000 than the median income level of $52,000.

In addition to providing useful information about positional mobility the rank-rank slope has other attractive features. Perhaps it's 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 racial 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 the researcher is interested in studying.

III. Measurement Issues and the Ideal Intergenerational Sample

Measurement Issues

The prior literature on intergenerational mobility has highlighted two key measurement concerns that I will briefly review here. 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. I will return to this point below and show that the evidence from the PSID suggests that their contention is incorrect.

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 incomes. Therefore if income is measured at too young an age it can lead to an attenuated estimate of the IGE. 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 parent 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 would be 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. Further, Nilson et al (2012), using Norweigian data show that both time averaging and life-cycle bias play a role in attenuating IGE coefficients. It should be noted that Nilsen et al find that these biases matters despite using administrative data like Chetty et al.[7]

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 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.[8] 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 parent 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 and from 1956 to 1986 for the parents of the earlier birth cohorts, and so on. With such a dataset in hand we would be

[7] Chetty et al speculate that perhaps time averaging and life cycle bias don't matter because of their use of administrative data.

[8] The precise end points are debatable but one might want to ensure that most sample members have finished schooling and that most sample members have not yet retired.

confident that we would have measures of lifetime income that are 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.[9] Still, we can come somewhat close to this ideal sample with publicly available survey data in the Panel Study of Income Dynamics (PSID). The PSID began in 1968 and started collecting income data beginning with 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 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 cover 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 parent's income can also be measured through the ages of 25 and 55. Of course, attrition from the survey can diminish the size of samples with observations in all

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

of these years but unlike with the currently available tax data, the potential for such coverage is there.[10]

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 nowhere near as far back as 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. Although they do not explain why they limited their time average to just 5 years, a likely explanation is that if they had extended their time averages further, they would have been forced to measure income when parents were at an older than ideal age. This will become important 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 used in constructing the average would be when fathers are over the age of 50.[11] 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.[12]

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

[11] 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.

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

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 also included 29 to 32 year olds over several decades which also included many boom years then this would be 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. Hoyakem et al (2012, 2015) find that administrative tax data does a worse job than survey data in 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.[13] 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 these issues, but for the same reasons discussed above, their data are not well suited to doing effective robustness checks for the IGE measure. Below I will replicate their sensitivity checks with the PSID data and show how the current data limitations with the IRS data lead them to erroneous conclusions regarding the sensitivity of their IGE estimates to these measurement problems.

Is the IGE Robust to Zeroes?

Chetty et al (2014) also argue that the IGE estimator is not robust to how they impute years of zero family income observed for individuals in the child generation observed in 2011 and 2012.[14] They obtain an estimate of 0.344 when they restrict the sample to those children

[13] 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 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).

[14] It is worth pointing out that this is very different from the robustness issue discussed by Dahl and Deliere (2008) who are worried about years of zero earnings for *parents*, not kids. 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-

with positive income in both years. 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 two points worth making here. First, this seeming lack of robustness again reflects, at least in part, 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 contrast, datasets such as the PSID covering cohorts born as far back as the 1950s can be observed over many years, at many ages, and at different stages of the business cycle.

A second point 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 focus on. In this case ideally we would

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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 zero earnings they obtain estimates of the intergenerational elasticity of around 0.3 for men. Their estimates may be including many years when actual earnings are actually positive but are erroneously treated as zero because fathers were working in the non-covered sector. They attempt to correct for this by restricting the sample to parents who were not in the armed forces, or self-employed in some specifications. But, importantly this is only observed *in 1984*.and their long-term averages may 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. When they restrict the number of years of zero earnings in other very sensible ways to 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.

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 income that happen to be reported on tax forms.

Overall, there are strong reasons to think that the seeming lack of robustness of the IGE in Chetty et al is more a problem of limitations with their tax data rather than with the estimator itself. In fact, the results using broader concepts of family income with the PSID shown below are virtually identical regardless of whether one includes or doesn't include years of zero income since there so few zeroes.

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. 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 might be the most comparable to the concept used by Chetty et al (2014). In addition, I also construct a measure of family income that also includes transfers received by the household head or spouse but these results are not shown in this paper. 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 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 the 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.

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 also 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 income 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 fathers' 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 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 a 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. Using either approach suggests that the IGE in family income is greater than 0.6. Appendix Table 1 and

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.

Table 2 shows a set of IGE estimates that only use the labor income of fathers and sons. Appendix Table 2 presents similar estimates that also include the SEO samples. 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 0.610 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) and Mazumder and Acosta (2014) 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.

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. In this section I describe why those sensitivity checks are flawed and show how one can demonstrate this using the PSID. First, with respect to the sensitivity of the IGE to the age at which child income is measured, Chetty et al (2014) 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 that go back to 1971. They continue to use family income in 2011 and 2012 for children and the period 1996 to 2000 for the parents. This implies 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. Importantly, this also implies that they are using 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. Although Chetty et al do not highlight these points, they have important implications. 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 type of erroneous sensitivity check but then also show the results differ if one allows the age at which children's income is measured to rise *while simultaneously keeping the age at which father's income is measured, constant*. To implement this exercise, I first replicate the findings in Chetty by gradually increasing the age at which sons' income is measured from 22 to 41 but simultaneously increase the age range at which the five year average of father's income is measured to match the analogous age range implied by the tax data.[15] In addition, one can also fix this problem by using a 5 year time average that is always centered around the age of 40 while simultaneously raising the age of sons income from 22 to 41.

Figure 4 shows the results of this exercise. The red line replicates the basic pattern of Chetty et al. 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.

[15] 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.

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 fathers when the child is no longer in the home and when the fathers are entering their 50s and when their income becomes noisy. With the green line, however, we continue to use centered time averages of fathers 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 current version of the tax data because of inherent data limitations.

There is also another pertinent sensitivity analysis around lengthening the time average of parent income that Chetty et al present in their Figure 3B, Chetty et al do this by adding additional years beyond the 1996-2000 time frame and show that their rank-rank slope estimates do not increase, though they never show the results of this exercise for the IGE.[16] The key problem with this approach is that once again they must necessarily increase the attenuation bias from using later ages in the lifecycle of parents as they extend the length of the time averages. 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 with lots of noise. And once again, when they extend the time averages in this manner they are actually utilizing many years of income when they 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

[16] See Chetty et al (2014, Figure 3B).

the mechanical effect of increasing parent age.[17] 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 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.

Rank-Rank Slope Estimates

In this section, I present an analogous set of results for the rank-rank slope. I begin by showing the rank-rank slope estimate when using the main measure of family income that is most similar to what is measured in the tax data and what was used to generate Table 1. The results are shown in Table 3. With the rank-rank slope some new patterns emerge. First, it appears that increasing the length of the time averages centered around 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 nearly every case that the slope estimates are higher when sons' income is averaged over 8, 9 or 10 years 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.

[17] Specifically, 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 in 1996 and 2000. A ten year average then utilizes the income of fathers when the son is between the ages of 15 and 24.

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 just 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. The more muddled view from Figures 6 and 7 serve to reinforce the more direct evidence from Table 3 and the results when imposing the tax data limitations. Namely, 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. Using very large samples of tax records that are digitized going back only to 1996, Chetty et al (2014) estimate that the IGE is actually much lower at 0.344. Further they make strong claims that these estimates are not subject to attenuation bias or lifecycle bias. If accurate, this finding is important because the IGE is an important gauge concerning the extent to which gaps between families in America will dissipate over time and so it is important to understand whether the evidence of greater mobility from the tax data is accurate or spurious.

I shed some light on this by describing the fundamental data limitations of the currently available tax data. 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 show 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. Using longer and centered time averages around the age of 40 I show that the IGE in family income is close to 0.7. I also show that when

I impose the same limitations as the tax data, that I obtain similar IGE estimates of around 0.3. Further I 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. Correcting for the fact that their sensitivity checks are confounded by the rising age at which parent income is measured, I show that the lifecycle bias and attenuation bias almost surely exists in the tax data when estimating the IGE.

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 gaps in society may be expanding. In that context focusing only on positional mobility because measurement is easier, may not be appropriate.

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 he central findings in their paper are robust 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 Average of Sons' Income (years)										
Time Avg. Fath. Inc.	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 rank-rank slope in family income

Time Avg. Fath. Inc.	Time Average of Sons' Income (years)										Wgt. Avg.
	1	2	3	4	5	6	7	8	9	10	
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

A. Ideal		B. Chetty et al (2014)		C. PSID	
Parent	Child	Parent	Child	Parent	Child
55	55	55	55	55	55
54	54	54	54	54	54
53	53	53	53	53	53
52	52	52	52	52	52
51	51	51	51	51	51
50	50	50	50	50	50
49	49	49	49	49	49
48	48	48	48	48	48
47	47	47	47	47	47
46	46	46	46	46	46
45	45	45	45	45	45
44	44	44	44	44	44
43	43	43	43	43	43
42	42	42	42	42	42
41	41	41	41	41	41
40	40	40	40	40	40
39	39	39	39	39	39
38	38	38	38	38	38
37	37	37	37	37	37
36	36	36	36	36	36
35	35	35	35	35	35
34	34	34	34	34	34
33	33	33	33	33	33
32	32	32	32	32	32
31	31	31	31	31	31
30	30	30	30	30	30
29	29	29	29	29	29
28	28	28	28	28	28
27	27	27	27	27	27
26	26	26	26	26	26
25	25	25	25	25	25

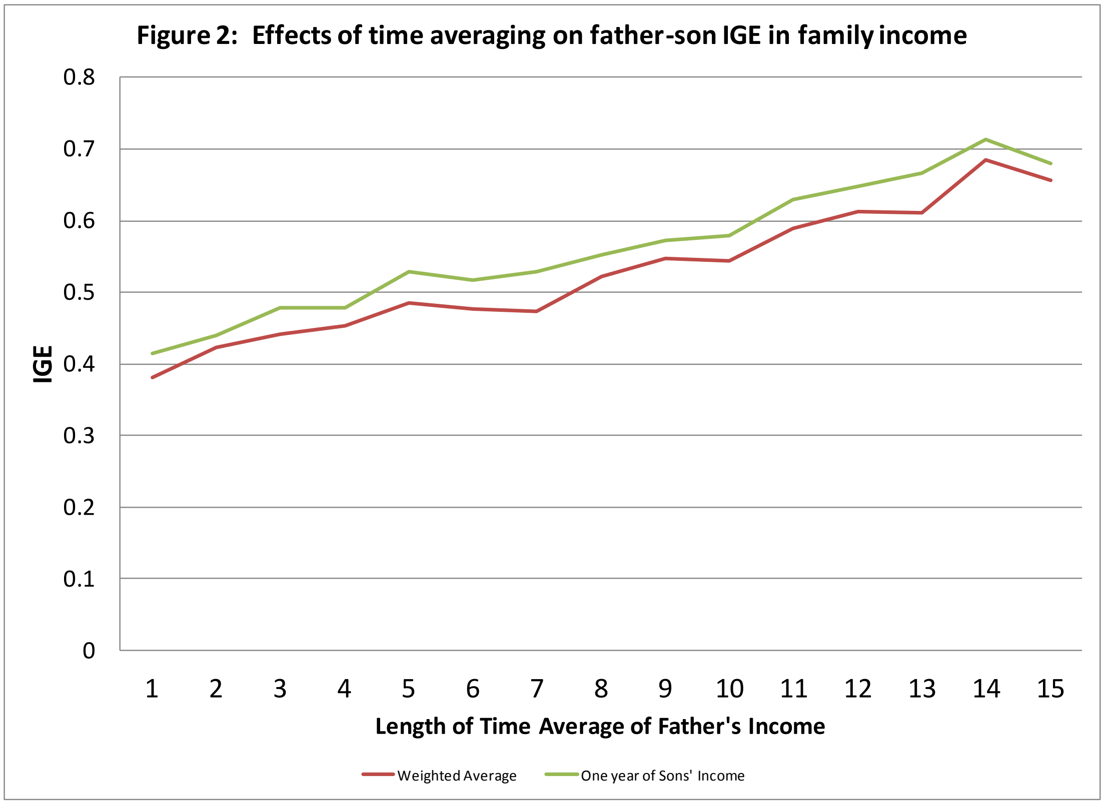


Figure 2: Effects of time averaging on father-son IGE in family income
IGE
0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
Length of Time Average of Father's Income
Weighted Average
One year of Sons' Income

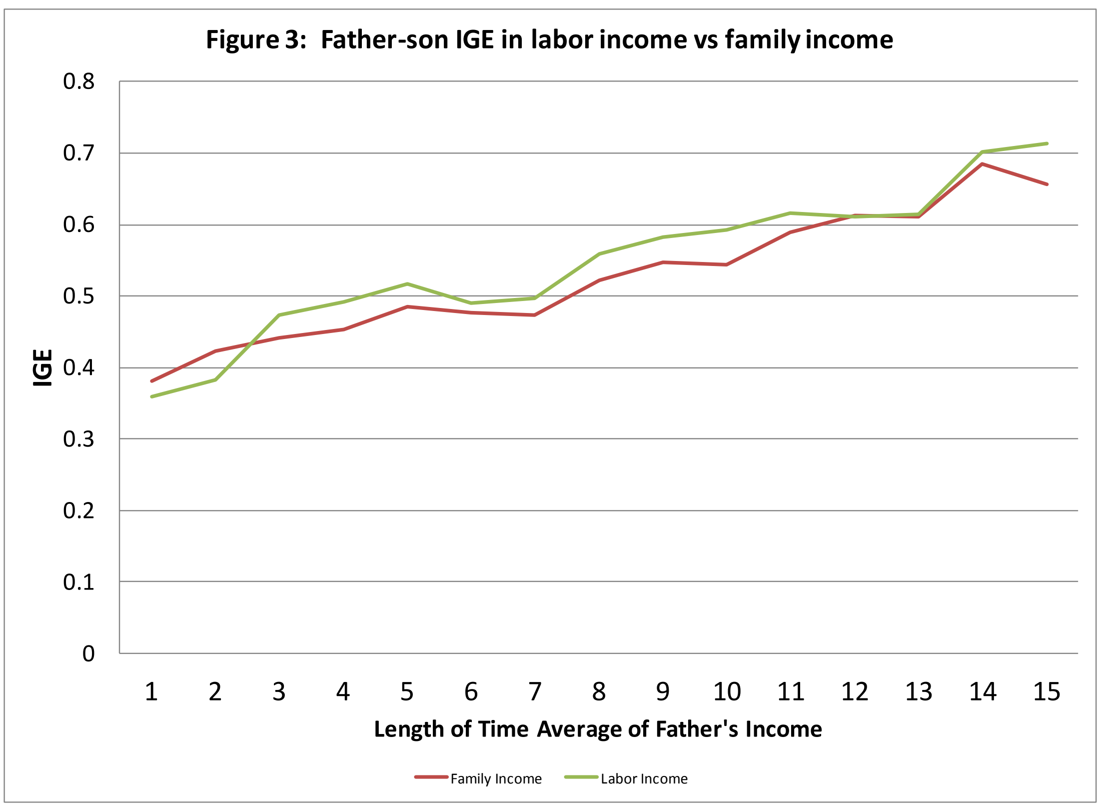


Figure 3: Father-son IGE in labor income vs family income
IGE
0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
Length of Time Average of Father's Income
Family Income
Labor Income

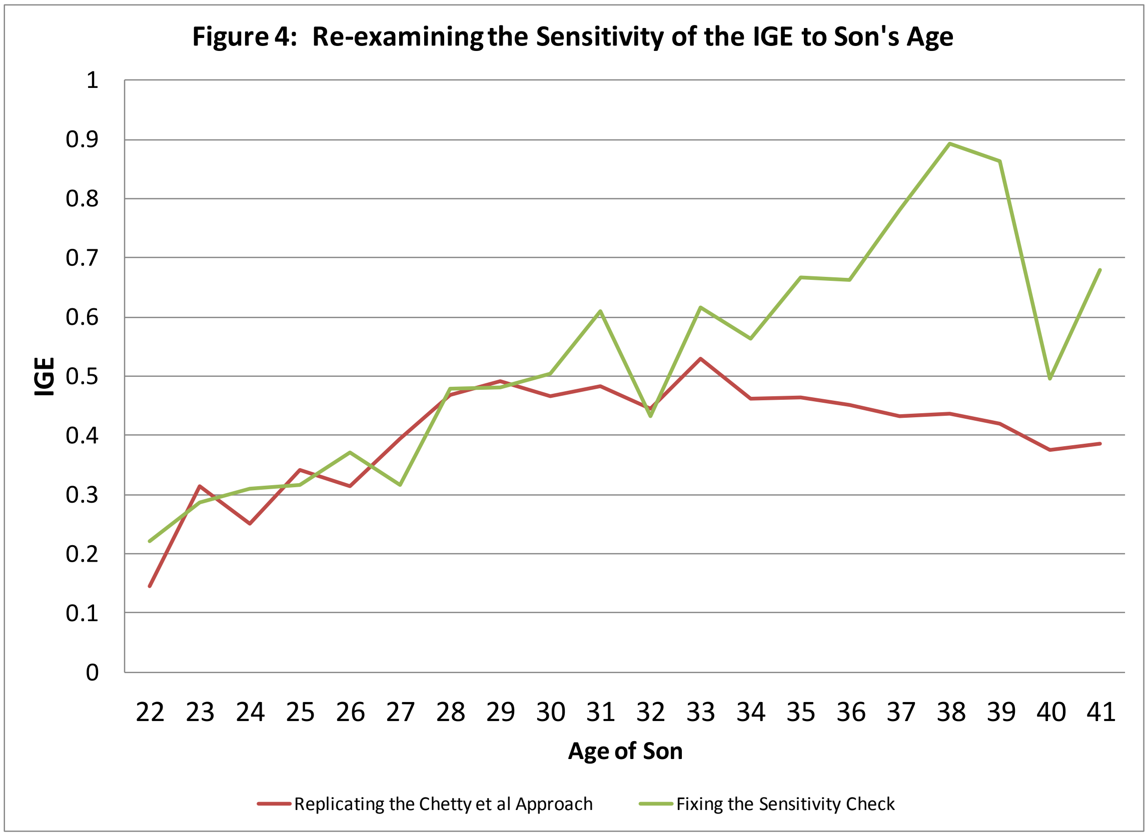


Figure 4: Re-examining the Sensitivity of the IGE to Son's Age
IGE
1
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
22 23 24 25 26 27 28 29 30 31 32 33 34 35 36 37 38 39 40 41
Age of Son
Replicating the Chetty et al Approach
Fixing the Sensitivity Check

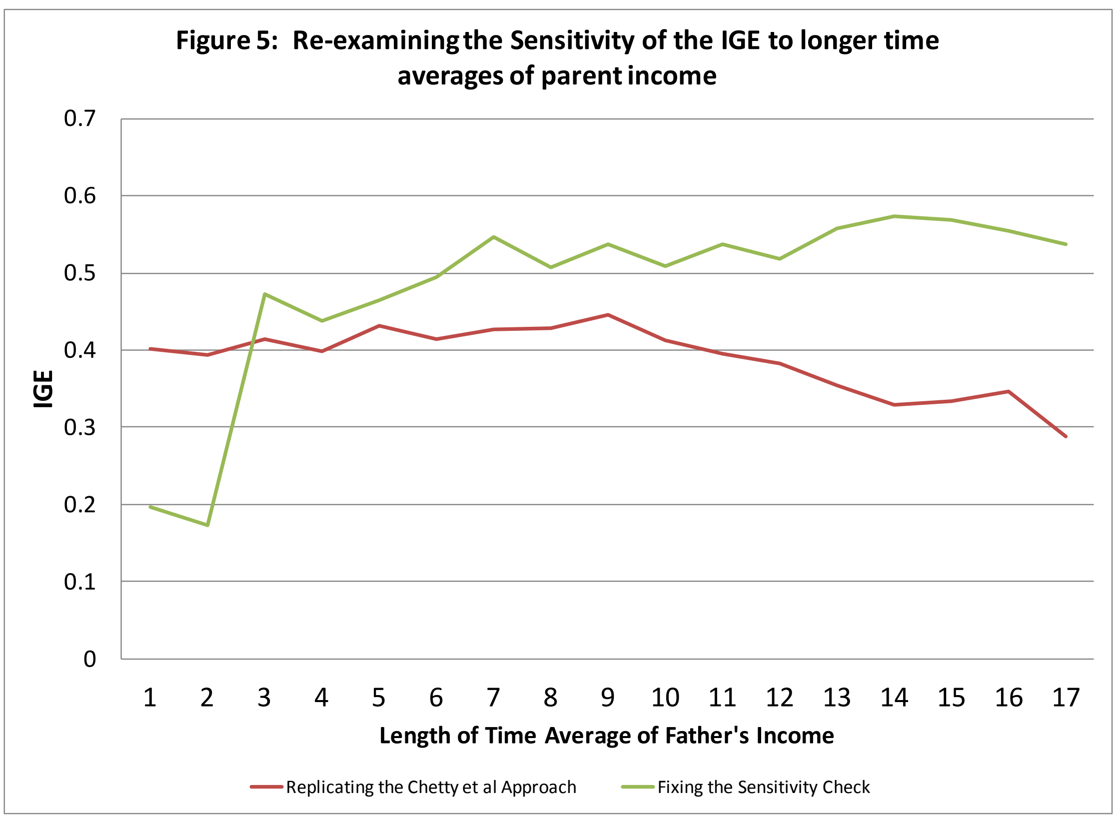


Figure 5: Re-examining the Sensitivity of the IGE to longer time averages of parent income
IGE
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17
Length of Time Average of Father's Income
Replicating the Chetty et al Approach
Fixing the Sensitivity Check

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

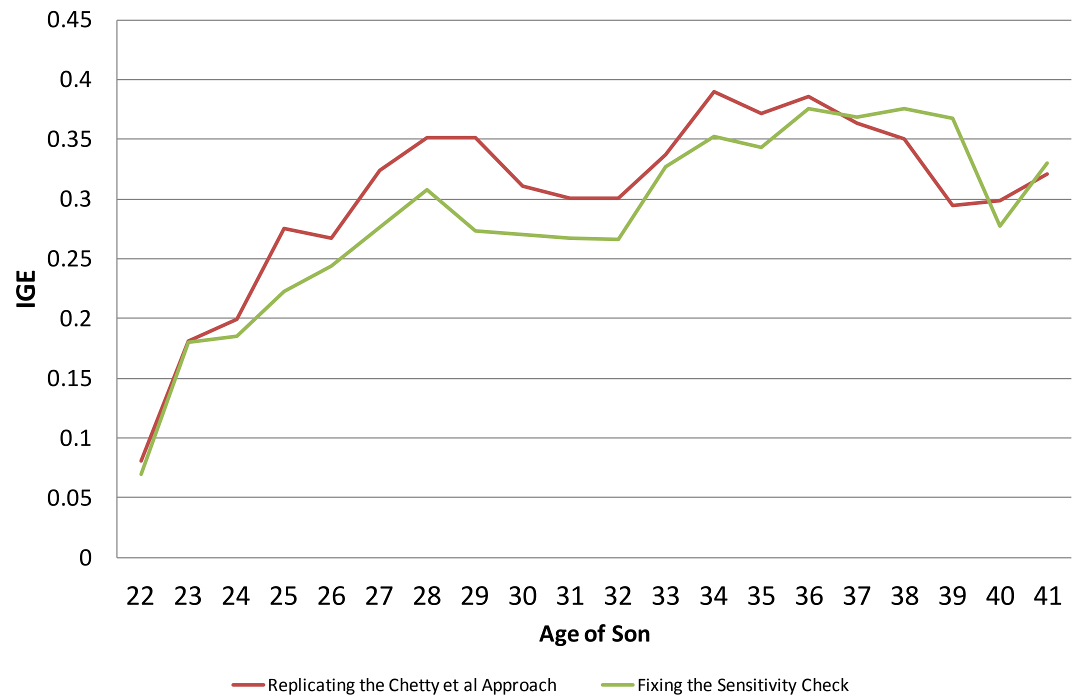
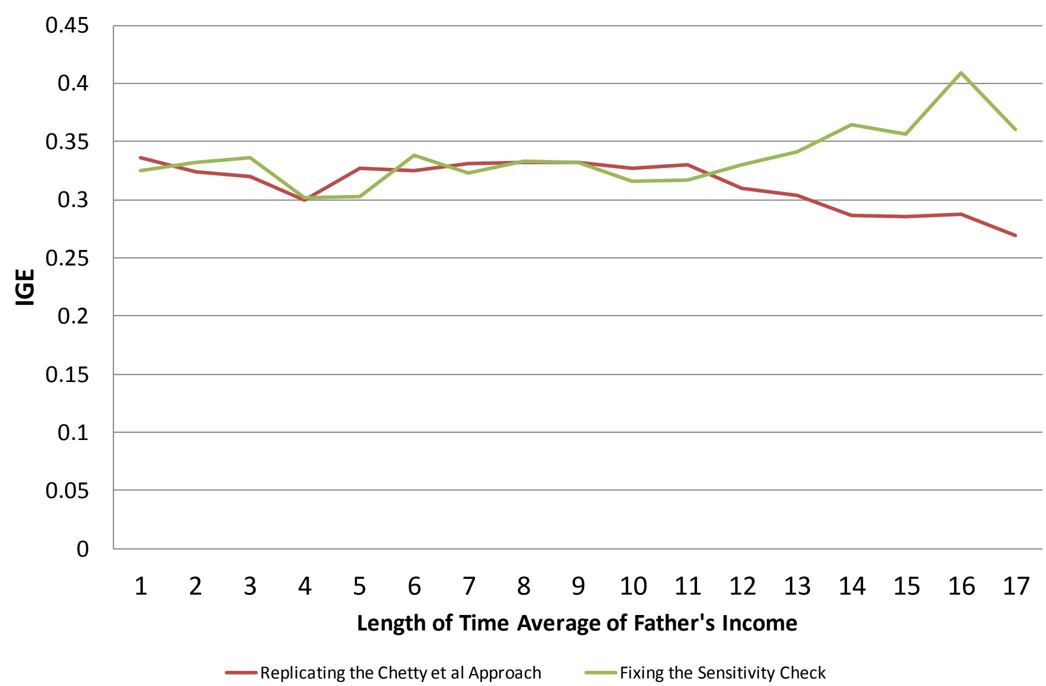
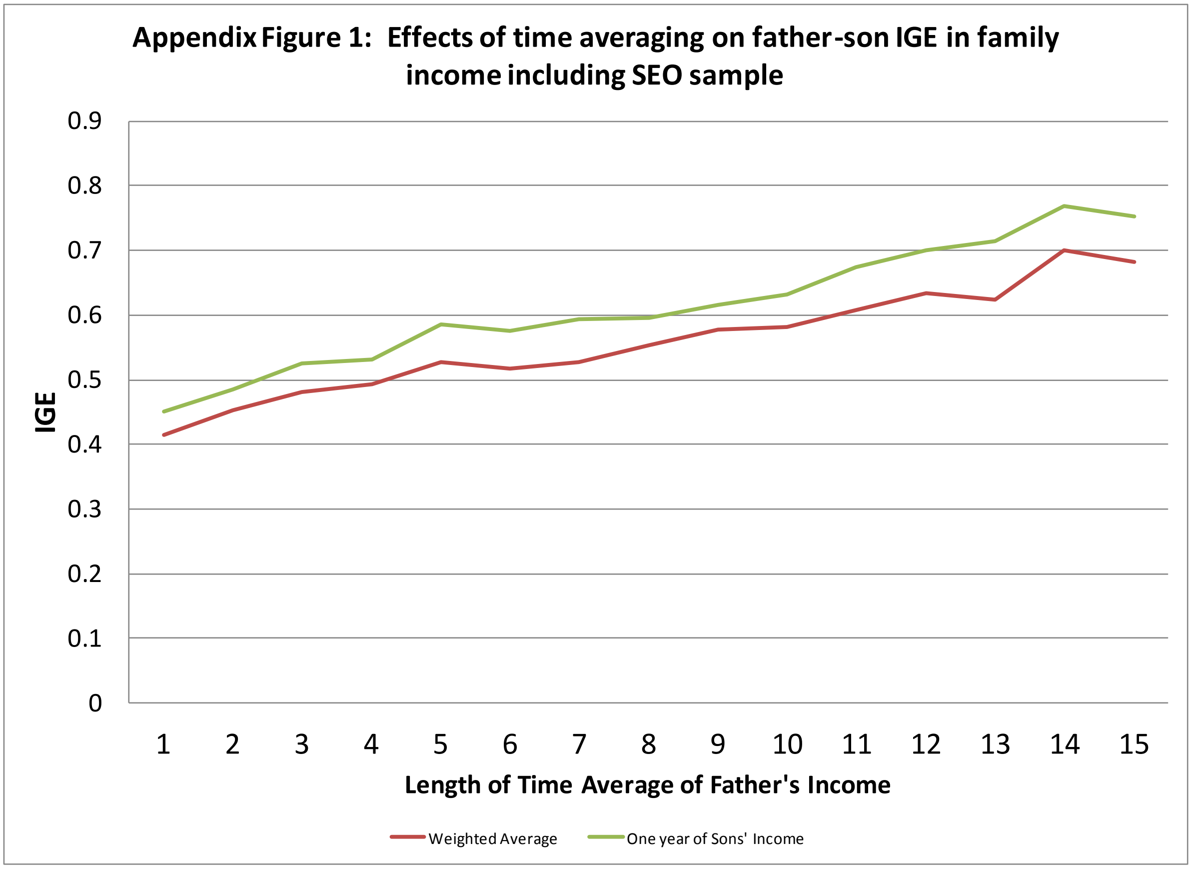


Figure 7: Re-examining the Sensitivity of the rank-rank Slope to longer time averages of parent income



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 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