Begging the Question:
Permanent Income and Social Mobility

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Working Paper Number 75

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September, 2007

[1] I wish to thank Martin Wittenberg, my supervisor, for helpful comments and suggestions, and to acknowledge the inputs of participants at the African Econometric Society Conference (Cape Town, 2007) and an anonymous referee. The work was conducted with financial support from the National Research Foundation, Public Policy Partnership (SA) and the University of Cape Town, Rondebosch, Cape Town, 7700. Data was obtained from the Panel Study of Income Dynamics: "The Panel Study of Income Dynamics is primarily sponsored by the National Science Foundation, the National Institute of Aging, and the National Institute of Child Health and Human Development and is conducted by the University of Michigan". The usual caveats apply.

1 Introduction

The extent of social mobility is one of three key considerations - along with living standards and the level of inequality - in any assessment of a society's economic merits. The intergenerational correlation (IGC) between the incomes of parents and their children provides a summary statistic of income mobility, where low income correlations reflect high mobility, whilst high correlations reflect low mobility. As such it has become the primary measure of this variable.[1] The main alternative measure, in the form of mobility matrices, is unable to provide a single measure of mobility but has the advantage of capturing variation in mobility across different parts of the income distribution.[2]

The extant literature on the calculation of the IGC has been premised on the notion that 'permanent income' is the key variable in the estimated equation. In other words, to the extent that the factors relating parents' income status to their children's can be captured using income correlations, this can be achieved solely through *permanent* income.[3] This appears to have been the premise since the earliest calculations of this measure were made - see for instance (Bowles, 1972)[4] - but has been especially prominent since the realisation that many of the previous estimates of income correlations (in the United States) appear to have been biased downward by the attenuation bias that is the outcome of classical measurement error (CEV). [5] The estimates affected by this attenuation bias had led various authors - most notably Becker (1988) - to conclude that the United States was a society characterised by a high level of social mobility.

Subsequently, a number of studies have shown that using multi-year averages of parental income dramatically increases the calculated regression coefficients and hence the correlation coefficient. In addition, it appears that this effect increases with the number of years used, to the extent that with up to sixteen years the correlation approaches 0.6 (Mazumder, 2005b) as compared to about 0.4 with four years (Solon, 1992; Zimmerman, 1992), and 0.2 with one year (Behrman and Taubman, 1985).[6]

More recently the literature has moved-on in two directions: assessing the dynamics of mobility based on the IGC; and attempting to break the IGC into its constituent

[1] Although note that correlations amongst other attributes - occupational status, education level, etc. - might be used in place of income. The former is particularly popular in the sociology literature - see for instance the survey by Birkelund (2006), and the work by Erikson and Goldthorpe (1993).

[2] Behrman (1999) is an accessible discussion on the merits and limitations of mobility matrices.

[3] Notice that there is presently no use of causal notions here, or suggestion that the relevant factors *are* wholly captured by income (permanent or otherwise). See the caveats later in this Introduction and the discussion in section 4.

[4] Although Bowles was primarily interested in the effect of education rather than income on children's outcomes, his emphasis in considering the latter was very much on permanent income (Bowles, 1972: Table 2: 230)

[5] We prefer the word 'realisation' to 'discovery' in this context because in fact the issue of measurement error appears to have been appreciated from the outset. Becker and Tomes (1986) note that a number of authors had by that time used multi-year averages to address this concern. That their estimates were still very low is perhaps indicative of the additional attenuation bias due to relatively homogenous samples (see the brief discussions in (Solon, 1989, 1992)). In fact, Bowles (1972), referred to previously, appears to have been the first to recognise this problem as well as the salience of the correlation in the transitory component of income - see section 4.

[6] Solon and Zimmerman use different datasets but reach similar estimates: the former uses the University of Michigan's Panel Study of Income Dynamics (PSID) dataset, whilst the latter uses the National Longitudinal Survey (NLS).

parts.[7] In other words, asking how mobility has changed over time, and what factors underlie the intergenerational persistence in income/earnings.

Whilst these are important issues - the latter being particularly critical for policy purposes - in this paper we initially return to the original problem of estimating the magnitude of the intergenerational correlation coefficient. Our fundamental proposition is that using a regression based on a notion of 'permanent income' to estimate the IGC may result in unobserved heterogeneity if 'transitory' parental income also affects children's income. In addition, we suggest that transitory income may matter to different degrees depending on the age of the child in the corresponding year. In econometric terms, the first implication of these propositions is that the variables being used to proxy for permanent income should be independent regressors in a full model of children's income. Furthermore, assuming these to be of equal importance - either as proxies, or even as independent covariates - may itself introduce a bias into the calculation of the IGC.

Neither of these possibilities appears to have been formally accounted for in the literature to date. This is especially problematic because in decomposing the IGC a number of researchers have asked the, seemingly counter-intuitive, question: "Does (parents') money matter?". But as we will argue in subsequent sections, the permanent income model of intergenerational mobility that is used in the literature to some degree begs this very question.

Some Caveats in the Interpretation of the IGE

Before proceeding, we should state some important caveats regarding the interpretation of the intergenerational correlation in income.

IGE versus IGC First, we shall, for the rest of this paper, deal primarily with the IGE (intergenerational *elasticity*) rather than the IGC that we have referred to until now. Most of the more recent literature (for instance Solon (1992) and Mazumder (2005b)) assumes these to be equivalent. In fact, this is only the case if the variance in the (explanatory) parental income variable is equivalent to that in the (dependent) children's income variable as shown below. If children's log income is represented by y_c, parents' log income by y_p, and IGE = β_y, IGC = ρ_y, then we have:

$$\rho_y = \beta_y \frac{\sigma_{y_p}}{\sigma_{y_c}}$$

So as (Bowles and Gintis, 2002: 6) note, "In effect, the intergenerational correlation coefficient ρ is affected by changes in the distribution of income while the intergenerational elasticity is not". Most authors follow Solon (1992) in assuming that approximate equivalence between the two is an empirically reasonable assumption - which it appears to be.

[7] See d'Addio (2007) for perhaps the most comprehensive survey to date of the literature on intergenerational mobility.

However, the original theoretical contribution of this paper - put forward in section 4 - leads us to propose a conception of the IGE which cannot be equated to a correlation, but which we argue is superior to the approach currently used in the literature. For the rest of the paper we shall therefore focus on the IGE rather than the IGC.

Causality A second caveat is that one should not necessarily infer the extent of a causal relation from the value of the IGE. Whilst this paper challenges the assertion that there is *no* causal relation between parental income and children's economic outcomes, it is inevitable that the true IGE overstates any causal relation that does exist because of the correlation between parental income and other explanatory variables in the structural equation for children's economic success. Thus whilst the IGE remains a valid and relevant measure of (im)mobility, the details of what determines that (im)mobility need to be the subject of further enquiry.

Optimality Given that a low IGE implies high social mobility - and therefore potentially lower dynastic inequality - it might seem an obvious step to suggest that the lowest possible IGE is best by some egalitarian standard. However, as a number of authors have pointed out, higher mobility is not necessarily desirable.[8] The idea that immobility is bad is largely a function of the belief in the importance of equal opportunity: it seems unfair that some children should earn more than others simply because they were lucky enough to be born to rich parents. And yet, there are few people today who would argue that the inheritance of attractiveness or intergenerational transmission of traits associated with hard work are unfair and should be done away with. Thus the inferences we make about the desirability of a given society based on its IGE are fundamentally reliant on the factors underlying that correlation - which is why unearthing these factors is such a major concern of the recent literature.

Life-Cycle Bias A final caveat relates to a possible bias in the calculation of the IGE. This may result from the fact that income for parents and their children are often taken at different stages of the life-cycle; something reinforced by the nature of panel datasets in which children's income is often from the beginning of their careers, whereas parents' income is observed somewhat later. The standard way of dealing with this problem in empirical work is to first regress the variables on a quadratic or quartic in age, and use the residuals from these regressions in calculating the intergenerational correlation - though Jenkins (1987) casts doubt on the merits of this approach. From the perspective taken in this paper we will not assume that our variables have been residualised in this way unless stated otherwise. Since age variation in income can in some sense be characterised as 'transitory', and the fundamental hypothesis of this paper is that transitory income may have independent explanatory power, it seems inappropriate to residualise on age *ex ante*. (Though we do in fact pursue this approach in the empirical work of section 6, but for somewhat different reasons.)

The remainder of the paper proceeds as follows. In Section 2 we discuss the conceptual basis for the proposal that transitory income is of independent importance for

[8] In fact there are a number of reasons why one would not want to reach the extreme of complete mobility represented by an IGE of zero. For instance, Harding, Jencks, Lopoo and Mayer (2005) suggest that there are at least three reasons - genes, assortative mating, and 'cultural ideals and preferences' - why we would still calculate a non-zero IGE even if full equality of opportunity prevailed. For more detailed discussions of the philosophical issues around mobility and equal opportunity, see Roemer (2000) and Swift (2005).

children's outcomes. In Section 3 we outline the standard econometric approach to calculating the IGE that has been used in the literature to date, including the estimation issues that have arisen within the bounds of that model. In Section 4, which is the theoretical core of this paper, we propose an alternative model that includes parental transitory income as an explanatory factor in the equation for children's permanent income. The consequences for the interpretation of past IGE estimates are then demonstrated, and we briefly discuss some possible solutions to the problems with past approaches. Section 5 assesses various estimation options using Monte Carlo simulations, and gives an indication of the extent of the bias in the IGE for different values of the parameters in the alternative model. Section 6 presents the findings from an empirical assessment of the proposals of the previous two sections using the Panel Study of Income Dynamics (PSID). Section 7 discusses the ouputs in Section 6, and allays concerns about our estimation procedures. The results suggest that transitory parental income *does* matter for children's future income, explaining about 20% of the intergenerational elasticity. Section 8 goes on to consider some important extensions of the foregoing analysis and arguments, and examines their implications for public policy based on intergenerational mobility studies. Section 9 concludes.

2 The Problem with *Ex Ante* Black-Boxing of Transmission Mechanisms

The neglect of the above-mentioned issues in the literature appears to stem from the view that permanent income is the variable with which we should be exclusively concerned when assessing income persistence. This need not be a problematic assertion *per se*, but the concept itself is typically not interrogated to any significant degree. In the context of the transmission of material well-being, one needs to ask what the envisioned role of parental income is. Most studies black-box this issue on the grounds that the authors are not interested in the details of the channels of transmission but rather the gross effect.[9] The problem, however, is that if we give insignificant attention to possible causal channels the implicit model upon which we base our estimation of the gross effect may be flawed.

To see how this approach can result in incorrect econometric logic consider two hypothetical extreme cases. In the first, we have a society in which all children are to be sent to state boarding schools - established in all major towns - from the age of 6 until at least the age of 15. The state covers all costs, but parents can choose to have their children return home (i.e. leave school) after the child has completed nine years of schooling. In the second, the society in question is a very poor one, with malnutrition and disease being common problems - particularly amongst infants and young children.

In the first example it seems perfectly reasonable to expect that parental income around the period at which the child finishes their ninth year of schooling (and possibly around the time at which the child turns six years old - depending on the extent

[9] Sometimes this is expressed explicitly when explaining the reason for not including explanatory variables other than income. (Mazumder, 2005b: 237) for instance states that: "Other covariates are generally not included (in the regression equation), because the goal is to obtain a summary measure of all factors related to income that are transmitted over generations. Therefore ρ should not be given a causal interpretation."

of enforcement) will exert a greater effect on the child's future income, because of the returns to further education, than that of other periods. This admittedly relies on the non-existence of perfect credit markets, but not on existing credit markets being especially imperfect.[10] Retaining this assumption, in the second example it is again plausible to expect that parental income in the first five years of childhood will have a greater impact on the child's future income than income in other years. This could work(negatively) through children's ability (proxied by IQ) and health, as the most obvious channels.

These examples demonstrate our two key points, that:

1. Transitory income could well be important for child outcomes and hence also their future incomes

2. The *extent* of this importance may vary by the stage of childhood.

Note that in the above we are assuming the interest in analysing income correlations to be a general one i.e. not confined to particular societal contexts or levels of development. The broader intuition can nevertheless be translated into more familiar contexts: replace the decision to stay in school with the decision to enter tertiary education; and replace early childhood malnourishment and disease with non-participation in preschool, and the relevance of the argument to estimates based on developed country datasets becomes more apparent. In the case of education, Cameron and Heckman (2001) in fact suggest that - based on their analysis of US data - family income matters primarily for education decisions prior to college enrolment.[11]

Permanent Income

Since the analysis that follows hinges on the above arguments, it is worthwhile considering the notion of permanent income in a little more detail. Essentially, the proposition, made by Friedman (1957), is that investment and savings behaviour will be premised on long-term income or expectations thereof rather than simply year-by-year variations. Consumption and expenditure are also expected to be smoothed over a multi-year period, possibly an entire lifetime. Some authors assume that the only real-world factor which prevents the realisation of this model's predictions is the absence of perfect credit or capital markets (and possibly also the absence of perfect foresight). In addition, from an empirical perspective, there is the possibility of inaccuracies in the reported income captured in surveys.

[10] The assumption of non-perfect credit markets should not be controversial given that it is in some sense implicit in claims that 'measurement error' is a serious problem. Later in this paper we make use of the empirical output of longitudinal studies that decompose the variance in income into a permanent component, and transitory white noise and serially correlated components. Although some of the the transitory variance may be attributable to measurement error from data capture it seems unlikely to all be of this sort. Mazumder (2001, 2003) attributes the variance from the white noise component to this, but does not provide his basis for doing so.

[11] Cameron and Heckman study the effect of family income on children's education and find that "(the) evidence suggests that it is family income at earlier ages and not later ones that matters in explaining college attendance" (Cameron and Heckman, 2001: 488).

If these assumptions of perfect markets and perfect measurement do not hold, then using one year's income will imperfectly measure 'permanent' income - provided annual (realised) income varies to some degree over an individual's lifetime - and hence the associated investment, savings and consumption decisions. To the extent that parental permanent income affects children's income via any of these channels, and in the absence of controlling for factors such as parental ability that it proxies for, using one year of parental income to calculate intergenerational income correlations will result in estimates subject to attenuation bias. This is the rationale for the approaches taken in most estimates of intergenerational relations in the economics literature over recent decades.

The model used in the intergenerational mobility literature is typically more crude than that envisioned in theory. In part this is because the notion of permanent income in its original form is notoriously difficult to pin-down empirically: How are we to define income expectations? On what basis and over what period are they formed? And how, for instance, do we incorporate these into estimations of intergenerational elasticities? One way around these complications is to suggest - as Mazumder (2003) and others do - that permanent income and average lifetime income are equivalents, allowing us to use the latter as our 'ideal'/preferred explanatory variable. There are two reasons to be sceptical of such a formulation. First, actual income is merely a realisation of some underlying, possibly transmissible, earning potential. Since it is this potential that we in fact want to capture, it is more plausible to view actual income as imperfectly proxying for this fundamental, but inherently unobservable, variable. Second, individuals' income expectations are likely to be over limited horizons. So in fact permanent income is not likely to be constant either. [12]

There is a large literature spread across the sub-disciplines of decision theory, behavioural economics and experimental economics which suggests that individuals may consume in ways quite different from such idealised, lifetime smoothing even without credit constraints. As Thaler (1990) points out, "a consensus seems to be emerging among economists that consumption is too sensitive to current income to be consistent with a lifetime conception of permanent income". That being the case, there is good reason to believe that estimates of intergenerational income relations based on the simplistic standard model, as well as similarly-founded attempts at determining a causal role for such income, are likely to be flawed.

One should also note that under the permanent income hypothesis different goods are likely to be associated with different conceptions of permanent income. The 'income horizon' that influences consumption of perishables is likely shorter than that which influences the consumption of durables: "there seems no reason why the horizon should be the same for all individual categories of consumption and some reasons

[12]Friedman himself actually makes both these points. On the first: "It is tempting to interpret the permanent components as corresponding to average lifetime values and the transitory components as the difference between such lifetime values and the measured values in a specific time period. It would, however, be a serious mistake to accept such an interpretation for two reasons…the experience of one unit is itself but a small sample from a more extensive hypothetical universe, so there is no reason to assume that transitory components average out to zero over the unit's lifetime…more important, it seems neither necessary nor desirable to decide in advance the precise meaning to be attached to 'permanent'. The distinction between permanent and transitory is intended to interpret actual behaviour." (Friedman, 1957: 23).

why it should differ systematically. For example, it seems highly plausible that housing expenditures are planned in terms of a longer horizon,and so a different concept of permanent income, than expenditures on, say, food." (Friedman, 1957: 207-208).

This point is particularly important for the proposals in this paper. Analyses of the time-series properties of individuals' income only identify one 'permanent component' which one might view as in fact being the permanent component associated with the longest horizon length. In a developed country like the United States where virtually the whole population is above a basic level of nutrition - whether through their incomes or state support via food vouchers - we would not expect transitory income to affect children's development to a *great* degree via any differences it might make to food consumption. However, in a poor country this may be a primary channel of transmission. We shall return to this issue, but note that in general one might expect that consideration of transitory incomes in estimating persistence will increase the IGE by more in poor countries than richer ones. This is because fluctuations will influence the consumption of goods with shorter horizons at a level at which - in a developing country - they continue to have impact on children's outcomes, and this effect will be further reinforced by the relative absence of credit markets.[13]

In response to these concerns two alternatives present themselves:

- Develop a more nuanced model of permanent income which incorporates income expectations and their horizons, and use this to estimate the IGE in income;
- Or extend what we shall call the 'standard model' for estimating the IGE to incorporate a role for 'transitory' parental income in influencing children's future income.[14]

In the rest of this paper we will pursue the second of these options. This has a number of advantages. It allows us to clearly link the model we develop to the preferred model in the literature. At the same time it requires no *ex ante* assumptions about the precise structure of income expectations and the associated consumption behaviour. Finally, it means that we are able to use the results from decompositions of variation in longitudinal income - e.g. Baker and Solon (2003) - as existing estimates of some of our model parameters. At the end of the day, as Friedman notes, the fundamental objective is the explanation of actual behaviour; provided the conceptualisation achieves this it has served its purpose. Section 4 outlines a model which we suggest covers a sufficiently broad range of possibilities to satisfy that criterion.

3 The Standard Model

Before developing our alternative model it may be useful to formally review the econometric structure of the standard model discussed above, as well as the technical devel-

[13] Although there has been important progress in the provision of credit to the poor (particularly the rural poor) in some countries, recognised for instance by the awarding of the 2006 Nobel Peace Prize to Muhammad Yunus the founder of Grameen Bank, this remains the exception rather than the norm.

[14] The scare quotes around 'transitory' are intended to indicate that this is not necessarily transitory income as envisioned by Friedman (1957) but rather transitory income in the sense of variation not explained by a permanent *lifetime* component of income variation (which is assumed in the literature to be equivalent to actual permanent income as envisioned by Friedman).

opments that have taken place within the boundaries of this structure.

The primary model used in the IGE literature to date is one which bases calculation of this parameter on a regression of children's permanent income on parents' permanent income and some random noise.[15] The *annual* income of both parents and children is assumed to be a function of their respective permanent incomes and other, transitory, factors. These assumptions are represented in the following equations.

$$y_{0is} = y_{0i} + w_{0is} + v_{0is} \quad (1)$$
$$y_{1it} = y_{1i} + w_{1it} + v_{1it} \quad (2)$$
$$y_{1i} = \rho y_{0i} + \varepsilon \quad (3)$$

Where y_{0is} represents parental income in year s and y_{1it} represents the child's income in year t. Following Mazumder (2003) these are each expressed as functions of a permanent component (y_{0i} and y_{1i}), transitory component (w_0is and w_{1it}) and a white-noise component (v_{0is} and v_{1it}) respectively.[16]

Within the bounds of this model, two problems were identified (see Solon, 1989): bias due to homogenous samples, and that due to measurement error. The former has been neglected in the more recent literature because of the increased availability of nationally representative datasets, but it is important in as much as it serves to explain why earlier estimates that corrected for the problem of measurement error by using averaged parental income still calculated correlations much lower than subsequent estimates. We will not, however, discuss this bias any further in this paper.

It is the problem created by the transitory factors in the latter two equations that have been the concern of more recent attempts at estimating the true IGE. In particular, it is well known that in this type of case one can show that a calculation based on one measure (year) of the explanatory variable (parental income) will be subject to attenuation bias and hence be lower than the true value.

$$plim\ \hat{\rho} = \rho \lambda_T \quad (4)$$

Where

$$\lambda_T = \frac{\sigma^2_{Y_0}}{\sigma^2_{Y_0} + (1/T)\alpha\sigma^2_{W_0} + (1/T)\sigma^2_{V_0}}$$

$\sigma_{W_0}, \sigma_{V_0}$ and σ_{Y_0} are the variances of the transitory, white noise and permanent components respectively.

[15]Previous studies have typically focused on *fathers'* income rather than parental income as a whole, due in large part to the empirical complications of using mothers' income. Our empirical analysis in Section 6 makes this assumption for similar reasons, but since family income is arguably a more relevant measure - particularly if one believes that there is a causal influence of income on children's outcomes - we assume for this and the next section that the independent variable represents family income.

[16]In fact, Mazumder discards the white-noise component in a later paper (Mazumder, 2005b) as it doesn't add much to his main results. It may however be rather important for the concerns of this paper, and hence we retain it.

α represents the effect of serial correlation in the transitory component. We show below that if there is no serial correlation - as assumed above - then $\alpha = 1$. So in (4) λ_T is the attenuation factor, with $0 < \lambda_T < 1$. The attenuation problem can be mitigated by averaging over T such measures (years), as indicated by the terms in the denominator.

A more plausible assumption - following Bowles (1972), Solon (1992), Zimmerman (1992) and Mazumder (2005b) - is that there is serial correlation in the transitory component of income, in which case we have:

$$w_{0is} = \delta w_{0is-1} + \xi_{0is} \tag{5}$$

If this is the case, then α in our attenuation factor λ can now be expressed as:

$$\alpha = \left\{ 1 + 2\delta \left(\frac{T - \delta T - 1 + \delta^T}{T(1-\delta)^2} \right) \right\}$$

Whilst it is true in both cases that increasing the number of years in the average will decrease the bias, in the latter case the correlation between the transitory components serves to mitigate this. This can be easily seen by noting that $\alpha > 1, \forall \delta > 0$. Mazumder's innovation is to note the implications of this result for the extent of attenuation remaining after taking four- or five-year averages (as Solon (1992) and Zimmerman (1992) did). By taking averages over much longer periods (Mazumder, 2005b), and using a new technique to account for this possibility (Mazumder, 2003), he argues that the correlation coefficient in the United States is around 0.6, rather than the previous estimates of 0.4.

From the above it should be clear that in the literature to date on calculating IGEs in income, transitory components have been seen as mere nuisance terms - obscuring the true relation between the incomes of children and that of their parents. Hence the efforts to remove their influence through averaging. In the next section, following our discussion in Section 2, we examine the implications of children's income being a function of the permanent *and* transitory components of their parents' income.

4 Implications of an Alternative View

As noted in the previous discussion of permanent income, the idea of transitory income affecting child outcomes - and hence their incomes - is quite plausible. What it does require is an acceptance of a causal link between parental resources and child outcomes. The model above implicitly assumes no correlation between the transitory and permanent components. The latter will capture the impact of such constant factors as parental education, traits, genetic attributes and any causal role of the permanent component of income itself. Hence if transitory income is to have any effect it must be via a direct, causal effect on children's outcomes. Those unfamiliar with this particular literature might be surprised to discover that the possibility of income of *any* sort having a causal effect is not widely accepted. Shea (2000) for instance, in his article entitled "Does parents' money matter?", finds that parental income has a *negative* relation to their children's incomes once other factors are controlled for. And Mayer (1998) concludes her investigation into what parents' money can buy for their children's futures by stating that "although children's opportunities are unequal, income inequality is not the primary reason"(Mayer, 1998: 156).[17]

There are at least two authors (see Mazumder, 2005b; Hertz, 2005) who have discussed the possibility that income matters. (Hertz, 2005: 10-11) states that he prefers "the residency criterion to the biological criterion", and therefore uses averages of parental income earned during childhood (or at least when the child was resident in the household) rather than from other periods. Mazumder does not explicitly do this in his estimations, although given the breadth of his averaging he probably includes most childhood income. However, he does have a fairly detailed discussion (2005b: 251-253) on the possible import of borrowing constraints. The salient point here is that such constraints imply an inability to fully smooth the usage of income over the lifecycle, and hence can be captured as a *deviation* from permanent income.

The problem with both these papers is that their discussions are strictly speaking disallowed by the implicit structure in their models; characterised by equations (1) - (3), which only allow for a relation between children's permanent income and their parents' *permanent* income. The result is a lacuna between work of the kind done by Cameron and Heckman (2001) mentioned previously, and that of Mazumder, Solon and others. Even worse, as we will argue in Section 6, it may result in potentially serious flaws in the conclusions drawn from disaggregating the intergenerational correlation.[18] To discuss the salience of transitory income in a rigorous fashion requires that we first allow for the possibility in our estimated models.

4.1 An Alternative Model

The necessary modification to the basic model above is shown in (6). The second term allows children's permanent income (y_{1i}) to be a function of deviations from parents' permanent income (z_{0iq}).

$$y_{1i} = \rho y_{0i} + \sum_q \beta_q z_{0iq} + \varepsilon \tag{6}$$

$$\begin{array}{ll} \text{Where} & z_{0iq} = y_{0iq} - y_{0i} = (w_{0iq} + v_{0iq}) \\ \text{and} & q \in Q, s \in S, Q \subset S \end{array}$$

Two things are noteworthy here. First, q indexes years of *childhood*. We will assume henceforth that there are twenty-one such years, including the year preceding the child's birth.[19] s indexes all years of parental income (see equation (1)). Second, the coefficient on these years - represented by β - is allowed to vary by year of childhood. In other words, transitory income may have a different impact depending on what stage

[17] A remarkable aspect of Shea's paper is that he does not seek to justify the highly counterintuitive finding of negative, rather than merely insignificant or very small, coefficients on parental income. However, his instrumentation strategy is - as has been noted by Solon (1999) - not wholly convincing.

[18] We refer to the IGC rather than IGE here because it has tended to be the former which is dealt with in these decompositions (see Bowles and Gintis, 2002).

[19] The issue of which years are salient is in fact an empirical question. One might think that the lower bound cannot be extended further - though see the results in section 6 - but there may be good reasons for the upper bound to be extended; the possible importance of parental income in assisting job search, or keeping children in tertiary education, amongst others.

of childhood it is experienced in. (This is something we will return to in subsequent sections.)

This model can be classified as having a ‘mechanical’ rather than ‘economic’ structure, in as much as it does not explicate the the details of the relational mechanims (see discussion in 7.3). Under it we continue to have the problem of correctly estimating the impact of unobservable permanent income, but in addition we now need to estimate the independent impact of the transitory components as well. It seems appropriate to first investigate the implications of our model for the estimations carried-out in previous studies, and on the basis of this we will then suggest some alternative approaches.

The True IGE

Before we can do this there is one fundamental question that needs to be answered: what is the *true* IGE under this alternative model? Initially one might think that it is $\rho + \sum \beta_q$ where the betas are summed over all T years of childhood. However, this is incorrect. Given that permanent income is - by definition - present in every year, it would be a mistake to add the betas without adding rho for *each* year. Thus we have :

$$\rho^* = \left(\frac{\rho T + \sum \beta_q}{T} \right) = \rho + \frac{\sum \beta_q}{T}$$

Henceforth, when we refer to the ‘true IGE’ we are referring to ρ^*.

4.2 Estimation of IGE Using One Year of Parental Income

As noted in the introductory discussion, many earlier estimates of the IGE were based on regressions using a single year of parental income. One might wonder what the probability limits on these coefficients would be under our alternative model. In particular, we are concerned with the extent to which these estimates would pick-up the impact of transitory income if it exists.

An important point to make at this stage is that for any estimations conducted in the context of the alternative model proposed above it is necessary to be explicit about whether the parental income used is from *within* childhood, or *outside* it; where we have assumed ‘childhood’ to include the year in which the child is *in utero* until they are twenty years of age (though see footnote 19).

4.2.1 Income earned outside of childhood

If the single year of income is taken from a period outside of childhood we can characterise the probability limit of the coefficient as follows:

$$plim \hat{B}^* = \frac{\rho \sigma^2_{Y_0} + \sigma^2_{W_0} \sum_q \beta_q \delta^{|q-k|}}{\sigma^2_{Y_0} + \sigma^2_{W_0} + \sigma^2_{V_0}} \tag{7}$$

Note that we use k to index the year(s) used in the estimation.

The denominator in the expression above is the same as that in (4) - with the appropriate expression for α (here, $\alpha = 1$) - but the numerator now has a second term. It represents the sum of the coefficients on transitory income for each year of childhood, each weighted by a value ($\delta^{|q-k|}$) which is a function of the correlation in the transitory component w_0 (i.e. δ), and the proximity of that particular year to the year used in the estimation ($|q - k|$). Notice that this is dependent on the assumption that $\delta \neq 0$. If there was no serial correlation in transitory income the plim would be identical to (4) with $\alpha = 1$, so using a year outside childhood would pick-up *none* of the impact of transitory income.[20] The result is that in this model higher autocorrelation can actually play a positive role of sorts with respect to capturing the importance of the transitory component.[21]

4.2.2 Income earned inside childhood

Assuming that we do have access to childhood income, using any one year of it in our regression will produce an estimate with the probability limit given below (the derivation of this result, and subsequent ones, is provided in the Appendix).

$$plim\hat{B}^* = \frac{\rho\sigma^2_{Y_0} + \sigma^2_{W_0}\sum_q \beta_q\delta^{|q-k|} + \beta_k\sigma^2_{V_0}}{\sigma^2_{Y_0} + \sigma^2_{W_0} + \sigma^2_{V_0}} \tag{8}$$

Using childhood income means that the impact of the white noise term (v_0) *for the particular year used* becomes part of the calculated coefficient. Note in addition that the 'k' in this case will be different from the 'k' in (7) - it will be closer to childhood income and (based on our assumption of equal importance for those years) the second term in (8) will be larger than the equivalent term in (7). To give an idea of the relative importance of the various components of income, Mazumder (2003) suggests the following values (based on a number of studies of longitudinal income data): $\frac{\sigma^2_{W_0}}{\sigma^2_{Y_{0t}}} = 0.3$, $\frac{\sigma^2_{V_0}}{\sigma^2_{Y_{0t}}} = 0.2$ and $\frac{\sigma^2_{Y_0}}{\sigma^2_{Y_{0t}}} = 0.5$.

The difference between (7) and (8) implies a testable prediction of our alternative model: estimates of the intergenerational correlation based on within-childhood income should be *higher* than those using non-childhood income. At least one paper in the literature has made such a comparison (Behrman and Taubman, 1990), ostensibly to test for credit market constraints, and found that mobility was generally lower (that is, the IGE was higher) when estimated using childhood income - as predicted by our model. (We test this implication more thoroughly in section 6).

Two recommendations follow from this for empirical work using single-year estimation procedures:

1. Ideally use income earned during childhood,
2. If this is not feasible at least try to use a year that is as close to childhood as possible. The extent to which the calculated coefficient picks-up the impact of

[20]Note however that even if δ is, for instance, 0.5, if the year in question is more than ten years away from childhood $\delta^{|q-k|} \approxeq 0$. We make use of this fact in section 6.

[21]Though once one takes averages an additional attenuation effect due to the serial correlation - represented by α - enters this model as it did in the standard one; see section 4.3, equation (9).

transitory income will depend on its distance in years from the childhood ones, and the size of δ.

4.3 Estimation of IGE Using Multi-Year Averages of Parental Income

As a result of the increased awareness of the attenuation bias problem few authors now use only a single year of parental income to estimate the IGE. Rather, the preferred method is to use an average of parental income over as many years as is feasible.[22]. In this section, we characterise the probability limit of the coefficient in the seemingly ideal case where we have data on all years of childhood income and we use an average of these as our explanatory variable. In actual fact, the plim for the coefficient on multi-year averages varies depending on the nature of the overlap between the years used and the years of childhood. However, in the same vein as the results above for single year measures, since we expect the coefficient to be highest where we use the maximum amount of childhood income data we restrict ourselves to this case for the purpose of exposition.[23]

If we estimate the IGE using an average of all childhood income, the probability limit of the coefficient can be expressed as follows:

$$plim\hat{B}^{*} = \left(\frac{\rho\sigma^2_{Y_0} + (1/T)\sigma^2_{W_0}\sum_q \beta_q\Lambda_q + (1/T)\sum_q \beta_q\sigma^2_{V_0}}{\sigma^2_{Y_0} + (1/T)\alpha\sigma^2_{W_0} + (1/T)\sigma^2_{V_0}}\right) \tag{9}$$

Where

$$\Lambda_q = \left(\frac{1 + \delta - \delta^{|q-r|+1} - \delta^{|q-m|+1}}{1-\delta}\right)$$

and as before,

$$\alpha = \left\{1 + 2\delta\left(\frac{T - \delta T - 1 + \delta^T}{T(1-\delta)^2}\right)\right\}$$

r is the earliest year used in the regression and m the latest - in the particular case analysed here these correspond to the first and last years of childhood respectively (see Appendix for full details).

It is not easy to see what this expression implies for the estimated intergenerational correlation coefficient relative to its true value. If the true IGE were represented by $\rho + \sum\beta_q$ then using an average of all childhood income would understimate the coefficient of interest. However, if as we suggest the true IGE is equal to $\rho + \dfrac{\sum\beta_q}{q}$, then using an average of childhood income as the independent variable will likely *overestimate* the true coefficient. In the next section we use Monte Carlo simulations to show this for certain parameter values. This can however be seen directly from (9), by noting

[22]Note that in many cases it may not be desirable to use the maximum number of years available because of the reduction this causes in sample size

[23]Let K and Q represent the sets indexing the years used in the average, and the years of childhood respectively. Then there are seven possible types of overlap that are relevant for the calculations. The first three are where $K \subset Q, K \supset Q$, or $K = Q$. The other four are for the case where $K \cap Q = 0$, and $K \cap Q \neq 0$. The expression in (9) in fact cover cases 2 *and* 3.

that for the parameter values cited in 4.2.2, and $\delta = 0.5$, we will have $plim\hat{B}^* = \rho c_1 + \sum_q \beta_q c_2$; where $c_1 \approxeq 0.95$ and $1.518 \lessapprox c_2 \lessapprox 2.087$. So that the overall IGE may be substantially overestimated (depending on the magnitude of the β_q).

4.4 The Lubotsky-Wittenberg (LW) Estimator

Another method for estimating the IGE is that proposed by Lubotsky and Wittenberg (2006). They propose a new method of utilising multiple proxy variables, which involves weighting the coefficients of the various proxies derived from a simple OLS regression in which they are the explanatory variables for the dependent variable of interest. The optimality of this approach is demonstrated under particular conditions, one of which is that the proxies do not belong in the structural equation.[24]

Of the two applications of this technique in their paper, one involves the calculation of the effect of family income on children's reading comprehension scores. The objective of using the LW estimator in that instance is to reduce the noise in the independent variable - which in the structural model is permanent family income - in such a way that accounts for the variation in this noise (i.e. relative magnitude of the variance due to transitory factors) in earnings over the life-cycle, and which is therefore optimal.[25] The parameter is estimated using a three-step procedure:

1. Regress test scores separately on each individual year of family income, and calculate a set of weights for each measure (proxy) by dividing each estimated coefficient by the largest such value estimated.

2. Run a multiple regression of test scores on all the proxies entered seperately.

3. Weight the multiple regression coefficient for each proxy by the weight calculated for it in stage 1, sum over all proxies and divide by the average of the weights.

Using this method the authors find a substantially higher coefficient: "Using family income when the mother is 22 to 39, the effect from using the optimally weighted coefficients is 2.2, compared to only 1.6 when income is averaged prior to the regression, an increase of 31%." (Lubotsky and Wittenberg, 2006: 558). Since the measurement error problem here is similar to the one we would face if we were trying to calculate the IGE (in which case the dependent variable would just be a measure of children's income), the method used should, *ceteris paribus*, give a more accurate measure of this coefficient too. [26]

The key point the authors make is that the LW estimator is a better estimator than that which averages income prior to the regression (which, as we have seen, is the norm), or that which utilises a multiple regression and takes a simple average of the estimated coefficients. Were we using the standard model of Section 3, and there was life-cycle noise in the data, the LW estimator would certainly be better. But notice that

[24]See Lubotsky and Wittenberg (2006) for the full technical details.

[25]The premise being that the earnings in earlier years are a more noisy measure of permanent income than later ones; an assumption widely accepted within the literature.

[26]We say 'similar' rather than 'identical' since there are arguably other children's outcomes important for their future income which will be affected differently by income as a causal factor, as well as being related to the permanent factors proxied by the permanent component of income to a different degree.

our alternative model implies that one of the assumptions of the procedure is violated - namely the assumption, mentioned above, that the proxies do not have a direct effect on the dependent variable.

Nevertheless, we might still wonder what the nature of the LW estimate would be in this case and whether it would be an improvement on the other approaches. As it happens we can use the results above to give an idea. In particular, the weight ('p') applied to an individual year can be represented by:

$$\hat{p_j} = \frac{\hat{\beta}_j}{\hat{\beta}_i}$$

Where $\hat{\beta}_j$ comes from estimating $y_{1it} = \beta_j y_{0ij} + e$, and similarly for $\hat{\beta}_i$ which is here assumed to be the largest estimated coefficient so that we set $\hat{p_i} = 1$.

Using our previous result from (8) we can show that

$$plim(\hat{p_j}) = \frac{plim(\hat{\beta}_j)}{plim(\hat{\beta}_1)} = \frac{\rho\sigma^2_{Y_0} + \sigma^2_{W_0}\sum_q \beta_q \delta^{|q-j|} + \beta_j \sigma^2_{V_0}}{\rho\sigma^2_{Y_0} + \sigma^2_{W_0}\sum_q \beta_q \delta^{|q-i|} + \beta_i \sigma^2_{V_0}} \tag{10}$$

In words: On the assumption that equations (1), (2), (5) and (6) are valid, the LW estimator will weight the multiple regression coefficients on the individual years of childhood income by their relative *importance* (β_j in the numerator versus β_i in the denominator), *and* their relative *proximity* to years of relatively greater importance ($\sum_q \beta_q \delta^{|q-j|}$ versus $\sum_q \beta_q \delta^{|q-i|}$). On the basis of this one might therefore expect that if transitory income matters the LW estimator will be closer to the true IGE, and therefore higher than estimates based on the typical approaches outlined above. We confirm the latter intuition in our Monte Carlo simulations in Section 5. However, given our discussion in 4.1 regarding the true IGE under our alternative model, closer inspection suggests that the WL estimator is likely to *over*estimate the true IGE since this is comprised of the *average* of each year's β - not a weighted average. Section 5 confirms this intuition as well.

4.5 Better Estimation Methods?

The implications - outlined above - of using different approaches to estimating the IGE suggest that, if transitory income matters, past estimates may be flawed.[27] Given this, there are two things we wish to do:

- Characterise the nature of the problem (under- or over-estimation) and the extent of it. In this way we may make some inference about the validity of past estimates.

- Determine a more accurate alternative to the approaches used to date.

[27]Note that here we are concerned only with estimating the correct *magnitude* of the IGE. The equally important issue of the IGE's true composition in terms of permanent and transitory income, which is the other concern of this paper, is deferred for the moment.

Leaving issues of data availability aside, one might think that using an average of all years of parental income would yield the true intergenerational elasticity ($\rho + \frac{\sum_q \beta_q}{T}$). Indeed, in the context of the standard model of Section 3, accepting the notion that permanent income can be equated to average lifetime income (as assumed for instance by Mazumder (2003, 2005a)) implies that using such an average as the explanatory variable *will* capture the true IGE. Our redefinition of the coefficient of interest as being ρ^* rather than ρ, as well as the result in (9) suggests however that this intuition may be flawed. An alternative would be to explicitly exploit the fact that transitory income outside childhood plays no role in the structural equation for children's permanent income. For instance, one could run a regression on all years of childhood income entered as individual regressors, along with an average of all non-childhood income. We expect that that the coefficient on the latter in a simple regression will be akin to that in (9), but with the second term in the numerator being relatively small and the third being equal to zero.[28] This suggests that entering it into the multiple regression would provide an effective control for ρ, thereby allowing us to estimate $\sum_q \beta_q$ (and therefore, by simple division, the average of this sum).

It is perhaps worth mentioning at this point that in this paper we ignore attempts to *instrument* for permanent income (see for instance Solon, 1992; Zimmerman, 1992). We do so because it should be fairly clear that the use of instrumental variables will not resolve the problem that results from the exclusion of transitory income from the estimated model. [29]

In the next section we examine the merits of the speculative solutions presented above, using Monte Carlo simulations, and confirm, for various parameter values, the earlier assertions in this section regarding the bias that may result from using the conventional estimation approaches.

5 Simulation Methodology and Results

For the purposes of this paper we fix all parameters in our simulations except for the relative importance of permanent and transitory factors, and the relative magnitude of the betas. As noted in section 4.1.2 we will follow Mazumder (2003) and assume that: $\frac{\sigma^2_{W0}}{\sigma^2_{Y0t}} = 0.3$, $\frac{\sigma^2_{V0}}{\sigma^2_{Y0t}} = 0.2$, $\frac{\sigma^2_{Y0}}{\sigma^2_{Y0t}} = 0.5$ and $\delta = 0.5$.[30] In addition, we will assume that the true IGE is fixed at 0.5. That is, $\rho + \frac{\sum_q \beta_q}{T} = 0.5$.

The simulations work as follows: First, we generate lifetime incomes (over 45years) for 10,000 hypothetical parents. The first year is generated by taking draws for the permanent, serially correlated and white noise components from normal distributions in such a way that the variance in annual income explained by the components corresponds to the parameter values above. The subsequent years of income are generated

[28]The reason for this is apparent in the derivation of that result - see the Appendix.

[29]The very premise of instrumenting in this context is that there is one underlying latent variable - permanent income - which is the sole variable of interest. This presumption is precisely what we dispute in this paper.

[30]Solon (1999) makes similar assumptions.

based on this one, incorporating the fact that w_0 is correlated across years. We then generate one year of children's income (for 10,000 children) as a function of the permanent component of parental income (y_0) and the deviations from this in individual years of parental income from childhood (z_{0q}). The former is weighted by our assumed value for ρ and the latter by our assumed values for the β_q's, which for the first of these simulations are assumed to be equal so that if $\sum_q \beta_q = X$, then $\beta_q = \frac{X}{21}, \forall q$ (we relax this assumption somewhat in two subsequent simulations). Note that in all simulations we assume that childhood lasts 21years.

This provides us with a set of data satisying the assumptions of our alternative model. Having done this we then run a set of regressions on the generated data. The first set of these are aimed at giving a numerical characterisation of the bias that results from what were until recently the most popular approaches to estimating the IGE. The second set explore approaches based on subsequent developments (Mazumder, 2005b; Lubotsky and Wittenberg, 2006), as well as possible alternatives discussed in the previous section. Each is iterated 10,000 times.[31].

The results of our estimations are shown in Table 1. This reports the results of regressions of children's income on: 1. A single year of parental income from far (twenty years) outside childhood, 2. A five-year average of income outside of childhood, 3. A single year of income earned in the middle of childhood and, 4. A five-year average from mid-childhood.[32] Recall that in all cases the true coefficient is 0.5.

The results confirm the well-known fact that the coefficient on the permanent component is heavily attenuated when using single-year measures of parental income, and as Mazumder (2005b) has demonstrated, even averages (of non-childhood income at least) using five years of income substantially understimate the true coefficient. In the context of the model proposed in this paper it is particularly notable that there is a striking difference between the coefficients estimated using a single year of childhood, and non-childhood, income; with the latter being lower as expected. (If the magnitude of the former coefficient appears implausible it may be worth noting that Solon estimated coefficients on single years of income as high as 0.4 (Solon, 1992: Table 3), as did Zimmerman (Zimmerman, 1992: Table 3: 418)).

For the first parameter set, using one year of non-childhood income underestimates the true IGE by about 65%, whereas using a year within childhood underestimates it by slightly more than 20%. The results for the five-year averages are similar, and the estimated coefficients for the second parameter set indicate that the extent of the difference depends inversely on the magnitude of the betas. Whilst this demonstrates the dangers of using non-childhood income, even utilising income from childhood in this way does not necessarily yield a good approximation of the true IGE. The results

[31] The Stata .do files are available from the author on request

[32] In the second regression the average begins ten years outside childhood (s=31) and finishes four years hence (s=35). The reason for noting this detail is that since $0 < \delta < 1$, we can assume that the second term in the numerator of (7) will effectively equal zero if $|q - k| > 10, \forall k$. This serves to accentuate the difference between using childhood and non-childhood income since the latter picks-up none of the salience of transitory income.

for the last two parameter sets indicate that the *relative* importance of different periods in childhood is almost as important as whether the income is from childhood at all.[33]

For this reason we turn to the second set of estimation approaches. These regress the child's income on: 5. An average of all childhood income, 6. An average of all non-childhood parental income, 7. An average of *all* parental income. 8. Calculates the Lubotsky-Wittenberg estimator discussed in Section 4.4, and 9. and 10. contain the outputs from regressing the child's income on all years of childhood income entered separately whilst controlling for an average of all non-childhood income. (9. contains the summed coefficients of the individual years whilst 10. gives the coefficient on the control).

The results in row 5 merely confirm our counter-intuitive, analytically-derived conclusion of Section 4 that using an average of all childhood income may *over*estimate the true IGE for certain parameter values. By contrast, using a full (twenty-year) average of non-childhood income underestimates the IGE by 25-40% depending on the importance of transitory income. Confirming our concerns about the approaches advocated by Mazumder (2005b) and Lubotsky and Wittenberg (2006) respectively, row 7. shows that using an average of *all* parental income may *under*estimate the IGE. On the other hand, 8. shows that the Lubotsky-Wittenberg estimator may provide spuriously inflated estimates of the IGE if transitory parental income does matter for children's permanent income.[34] Finally, the results in row 9. and 10. are not particularly promising. The sum of the coefficients on the individual years of childhood income is clearly capturing the sum of the betas (equal to 4.2, or 2.1 for the second parameter set) but only to a relatively small degree, whilst the coefficient on the control overestimates ρ and understimates the true IGE.

These simulations do help us to characterise the nature of the bias for various approaches, but no obvious solution emerges. At best we can say that - for plausible parameter values - the true IGE is bounded below by estimates based on averages over all years of parental income, and above by those over only parental income earned during childhood. In an empirical setting this fact provides another test of the alternative model: if adding a number of years of non-childhood income to an average initially only over childhood *decreases* the coefficient, this supports the claim that transitory income matters.[35] We discuss this possibility further in the next section.

The fundamental point that emerges is that if transitory income matters all existing estimates of the intergenerational correlation in income are biased in ways dependent on:

- The extent to which they have utilised income from childhood
- The relative importance of transitory income (i.e. the magnitude of the betas)

[33]As noted in the table header, the β_i for the thrid parameter set start at 0.4 and then decline to 0, whereas for the fourth set they start at 0 and then increase to 0.4. The years used in the average are the first five years of childhood - hence the large difference in the estimated coefficients.

[34]This is not to suggest that the results in Lubotsky and Wittenberg (2006) are necessarily spurious, but it does indicate that there may be another factor at play other than the life-cycle noise with which they are concerned.

[35]Notice that this holds most strongly for the addition of *many* more years of non-childhood income.

6 Empirical Evidence

6.1 Data

In this section we examine the implications of the alternative model empirically. Our data source is the University of Michigan's Panel Study of Income Dynamics (PSID) dataset (Panel Study of Income Dynamics, 2007a), which has been one of the primary sources of intergenerational mobility data in the literature.[36] The PSID is an ongoing, nationally representative, longitudinal survey of households in the United States that has been conducted annually since 1968 and bi-annually since 1997. It began with 4,800 families and has subsequently conducted follow-up surveys with these families and individuals who moved out to form their own households. Because of the addition of newly-formed households to the sample, by 2001 it had grown to 7,000 families.[37] In this way it provides excellent data on intergenerational income mobility which is unavailable from cross-sectional surveys, as we have access to the income data of both children and their parents over a substantial period.

With such a survey, sample attrition is inevitably a concern. For the purposes of this study we take the data at face value, and make no pre-analysis adjustments in this or any other respect (though we do use the relevant weights provided with the PSID data). Fitzgerald, Gottschalk and Moffitt (1998a,b) have conducted a thorough analysis of attrition in the PSID. Although it had lost almost 50% of its original sample through attrition by 1989, Fitzgerald et al. (1998a) find that despite this it has retained "continued cross-sectional representativeness" (1998a: 296) for the first generation of respondents. The same authors' analysis of the affect of attrition on the second generation of respondents Fitzgerald et al. (1998b) has more measured conclusions as to whether this attrition is likely to bias the estimates of intergenerational relationships. This is in part due to the limitations placed on their analysis by having to estimate attrition effects *within* the sample itself (since there is no comparable survey against which to check these figures), which leads them to state that: "All that we can conclude is that our analysis has not uncovered evidence of statistically significant attrition bias in estimates of the intergenerational relationship between fathers' and sons' earnings" (1998b: 336). The finding therefore cannot be taken as categorically demonstrating that attrition bias is not likely to be a problem. However, for the purposes of *this* study we suggest that it should not be a problem for two reasons:

- We wish to contrast our results with those in the literature, most of which have been derived from similarly raw data
- Our primary interest is not the magnitude of the IGE per se but the composition of this figure and the trends within it, which - given their nature - we suggest are less likely to be distorted by non-random attrition (see discussion in 6.3).

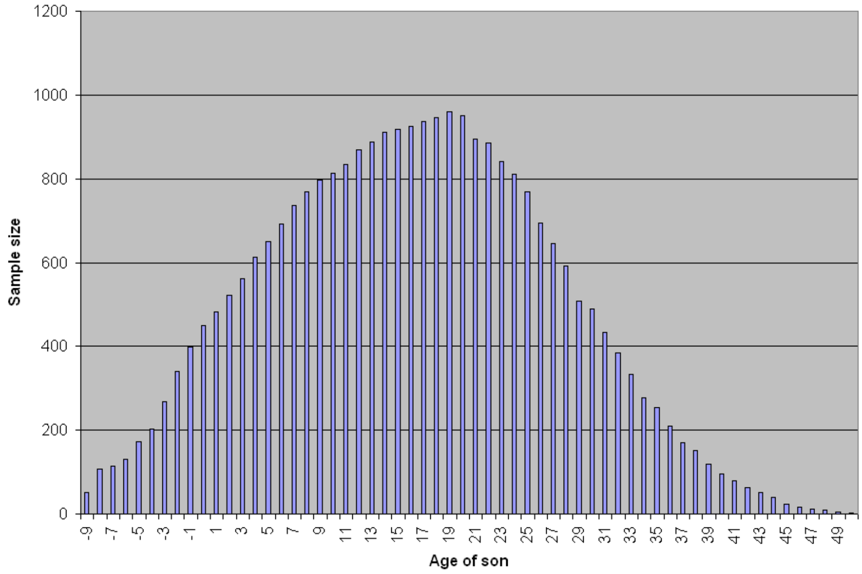
We therefore attempt no corrections for attrition other than the use of the PSID-provided weights.

[36] It is such a popular source for intergenerational mobility analyses that the PSID Datcentre recently released a full tutorial on their website which guides the reader through the - somewhat arduous - process of compiling an interegenerational dataset (Chiteji, Gouskova and Stafford, 2007).

[37] In fact, it had grown to more than 8,500 families in 1996 but the sample was subsequently reduced (see Panel Study of Income Dynamics, 2007b).

We do nevertheless restrict our sample of parent-child pairs in two ways. First, we use only father-son pairs. Second, the pairs are limited to those in which the child was younger than 20 in 1968 (including those not yet born), and older than 25 in the year in which we require their income data. The first restriction is not ideal - since if income plays a causal role our primary variable of interest should be *all* household income - but we make it in order to avoid complications relating to women's changing role in society and the details of marriage markets. The second is so that we have *children's* income measured during a period outside any reasonable conception of childhood (which also means less noise in that income data), whilst also ensuring that we have some *parental* income data that is certainly *within* childhood. All income data is converted to 2003 dollars using the consumer price index (CPI) provided by the United States Bureau of Labor Statistics.

FIGURE 1. SAMPLE SIZES OF PARENTAL INCOME BY AGE OF SON



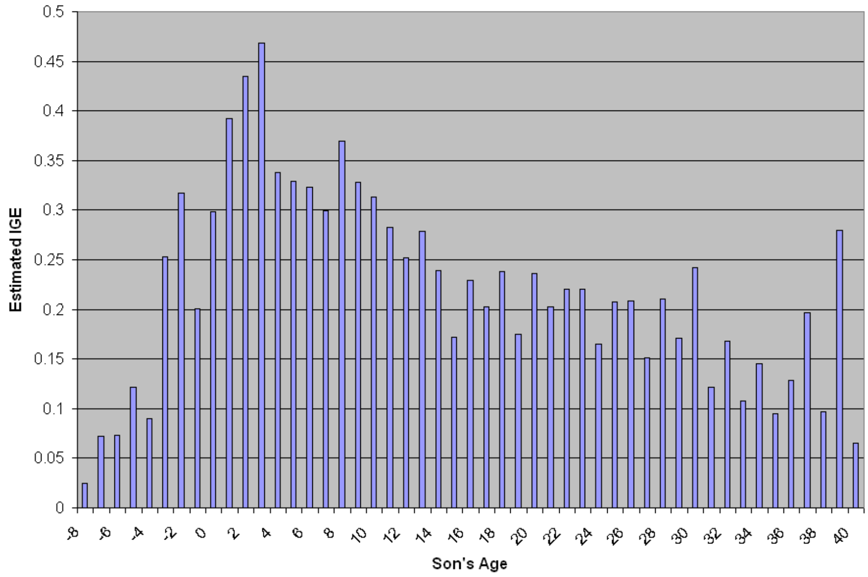
The samples sizes - by the age of the sons when father's income was earned - are provided in Figure 1. Note that the nature of the sample attrition is due both to the inherent limitations of the PSID sample as well as the above restrictions. For instance, if children are 30 in 1997 then they were only born in 1967 and we will not have *any* pre-childhood information on them (since the PSID started in 1968). More generally, if we use 1997 income then we will only have more than five years of pre-childhood parental income for those children who were newborns in or after the 1972 PSID and whose fathers reported their income. But since these children must be at least 25 years old in 1997 they have to have been born before 1972. Thus, if we use 1997 data we will not have more than five years of pre-childhood income data on *any* individuals. We have much more data on late childhood, and post-childhood, income for analogous reasons.

In order to maximise the sample available we utilise 2001 data, which yields the sample sizes shown in Figure 1. We did however run the estimations below using the 1997 data as a sensitivity test. The primary differences between the two sets of results are that the coefficients on childhood years appear to be somewhat higher, and the downward trend in later years is more pronounced, in the 1997 regressions.[38] There is a possibility that this could be due to the inclusion of more individuals in their mid-twenties in the sample who were initially below the age cut-off in 1997, though the quartics in age we use should account for such life-cycle biases. Nevertheless, the results below do not appear importantly sensitive to our choice of year.

6.2 Results

Recall that our main objective in this section is to investigate the role of transitory parental income matters for children's future permanent income. Based on our derivations and discussion in 4.2 we do this by estimating the elasticity of son's income in 2001 with respect to father's income earned at different ages of the son. The output is presented in Figure 2.

FIGURE 2. SINGLE-YEAR IGE ESTIMATES



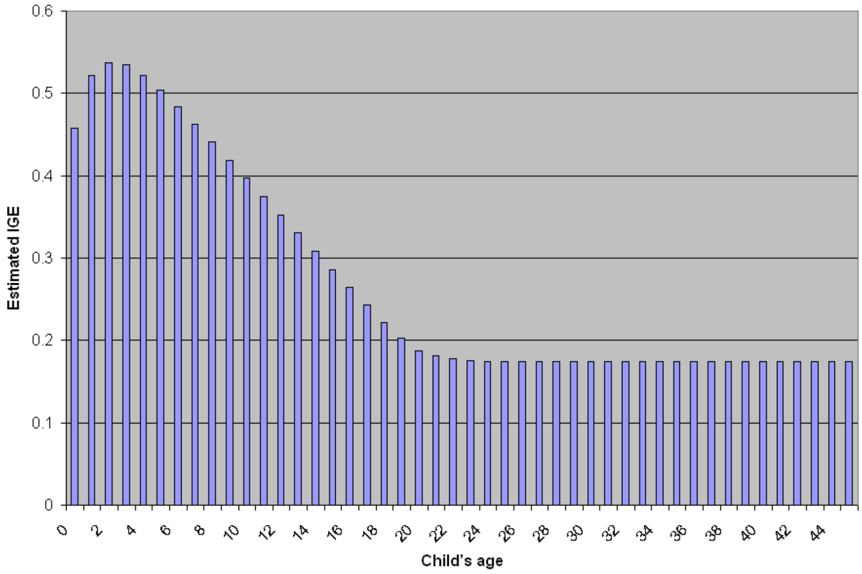
The smallest sample size we use is 96 (when the sons are 40) and the largest is 960 (when they are 19). The results appear to strongly support a causal role for income. The single-year IGEs using father's income more than two years before birth and when the son is in his mid- and late-30s are of a similar magnitude to those found in some of the earliest calculations of this variable (Behrman and Taubman, 1985), whilst they are highest - as much as 0.47 - when using income from early childhood. The magnitude and pattern of these differences cohere with the assertion that whilst IGEs based on income outside of childhood capture the (attenuated) import of the permanent component of income, they fail to capture the *causal* importance of income (transitory or permanent).

Recall that one of our parameter sets in the previous sections included a case where the betas where highest (0.4) at birth and decreasing linearly thereafter. It is interesting

[38] The 1997 results are available from the author.

to compare the graph in Figure 3 of the single-year IGEs for those 10,000 replications to our empirical estimates here.[39]

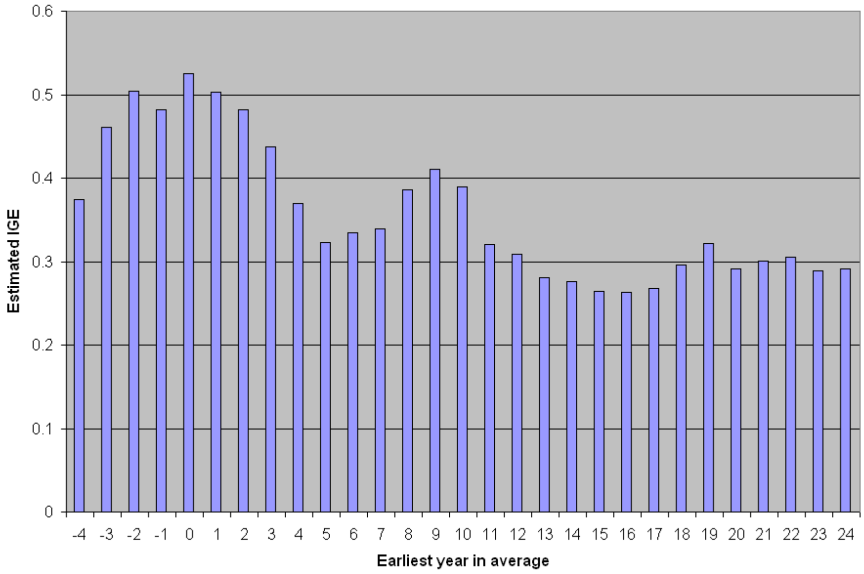
FIGURE 3. IGE ESTIMATES FROM SIMULATIONS WITH A DOWNWARD LINEAR TREND IN BETAS



The few authors in the literature to attempt an assessment of the causal role of income (discussed in 8.2) have typically used comparisons of IGEs based on multi-year averages rather than single years of parental income. Consequently, in Figure 4 we provide IGE estimates based on five-year averages, ordered by the earliest year used in the average.

[39] An unexpected result is that the first year of childhood income in the simulations is *not* the one with the highest coefficient, despite the fact that in the data generating process it carried the highest coefficient value of 0.4. The reason for this is because as we noted in Section 4, it is both the importance of a given year as well as its *proximity* to years of importance that will determine the magnitude of the estimated coefficient. The simulation result may be partly due to our artifical censoring of that distribution, however it does suggest that the lesser magnitude of the coefficient on years of, and immediately after, birth in our empirical analysis may be slightly misleading. Coefficients on years at either end of the causal range for parental income are likely to be downward-biased relative to other years in that range.

FIGURE 4. IGE ESTIMATES USING FIVE-YEAR AVERAGES



Although these results also appear to demonstrate that income in early childhood is most important, the magnitudes of the differences are substantially reduced.[40] The alternative model of Section 4 would suggest that this is due on the one hand to the decreased attenuation of the permanent component (ρ), combined with a smoothing of the coefficient on transitory income (β_q in year q) by virtue of averaging the betas over multiple years. Quite clearly, this approach is more likely to refute the notion that: a. Income matters more in some periods than others; b. On the basis of this, that transitory income has any causal role whatsoever.

6.3 Estimation Concerns

One might be concerned that our results in Figure 2 are driven by the sample attrition shown in Figure 1. If this attrition is systematic it could bias the individual estimates, and therefore also any comparisons of these, by life-cycle trends in income over the age of the child. Indeed this may be a valid concern; since fathering a child is more likely at some ages than others and fathers' income is known to be prone to such life-cycle effects, we would expect something of a correlation. By this theory it is not surprising to find that income falls as one measures it well before childhood or long after childhood. This is one reason to control for a quartic in the age of fathers, which we do in our estimations. One might also be concerned that there may be selection effects amongst children since only children born in certain years will have particular years of income (e.g. two years before birth) available. To control for this also we include a quartic in children's age.[41]

However, this does not fully deal with the problem. It may be that the coefficients are declining as they do because our samples are getting progressively homogenous,

[40] The shorter range of these estimates is due to the reduction in sample size that results from requiring a continuous five years of parental income data.

[41] This is equivalent to controlling for the child's year of birth.

so that our estimations are suffering from the attenuation bias noted by Solon (1989).[42] To allay this fear we might like to make a detailed comparison of the sub-samples by variables such as race, gender, education level, etc. Another way of making such an assessment though is to make comparisons within 5-year intervals of notable changes in the IGE for a *fixed* sample. (The PSID sample does not allow us to fix a sample of sufficient size to make this comparison over much longer periods). We conduct analysis on such subsets and find broadly similar results (not shown) in terms of the trends, although the magnitude of the coefficients from the subsets differs at times from that in the full-sample estimations - as one might expect given that the factors influencing the IGE can vary over time (see the discussion in 8.1).

Perhaps the best way of allaying concerns that our results are due to biases resulting from sample selection, is to consider the pattern in Figure 1. If the results in Figure 2 were driven by increasing sample homogeneity due to age-based 'attrition' in our sample then we would expect the highest IGEs to be associated with the largest sample sizes. However, whilst the sample sizes peak at age 19, the single-year IGEs peak at the age of 3. On the basis of all these arguments we reject the possibility that our results are driven by biases; either relating to life-cycle effects or homogenous sub-samples.

7 Discussion

The results above are consistent with a number of plausible hypotheses:

- That parents who plan their children are likely to have children who are better off (reflected by the importance of parental income up to three years before birth), over and above the fact that this is an attribute typically associated with higher income brackets.
- That parental income during childhood generally matters more than income outside of childhood.
- And that, consistent with the early childhood development literature, income appears to matter more in the early stages of childhood.[43]

7.1 The Direction of Averaging

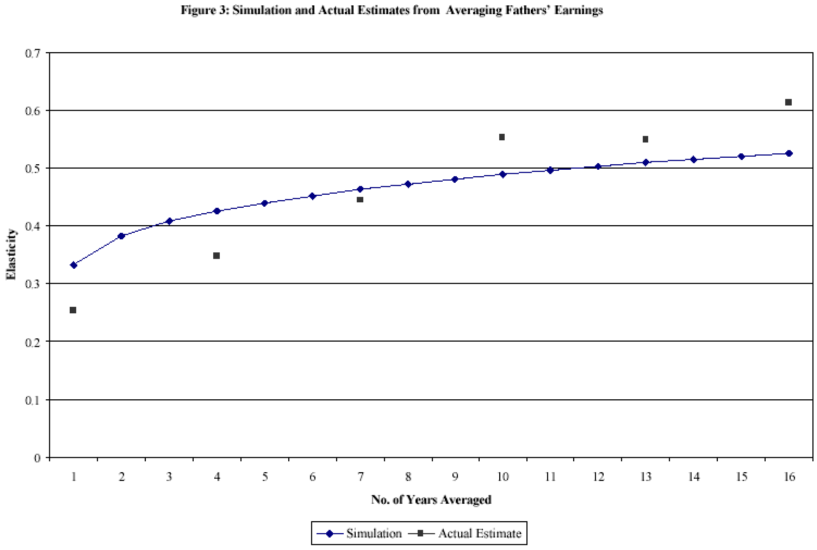
There is one important prediction of the model presented in section 4, and discussed in previous sections, that we are unable to test here because of our small sample sizes. By virtue of the fact that childhood income matters more than non-childhood income, the trend in the IGE when the independent variable is averaged over longer periods of time should depend on the direction of the averaging. Consider Figure 5 reproduced from Mazumder (2001).[44]

[42] As discussed previously, this results from using unrepresentatively homogenous samples to calculate the IGE.

[43] As we have noted previously, these hypotheses are likely to be different for a country with a lower level of development. In that case one might expect that income around the period of birth would be most important, and perhaps also income at the age of first school attendance.

[44] (Mazumder, 2001: Figure 3)

FIGURE 5. DIRECTION OF MAZUMDER'S AVERAGING



We would expect that the shape of the plotted line will depend on the first year used as well as the order of the subsequent years. For instance, if one were to begin with a year of parental income after childhood and then include more years from earlier in life we would expect the slope to be steeper than if the first year was from within childhood and some of the subsequent years were not. Notice that under the crude permanent income approach it should not matter which years are included, or in what order, since the increase in the coefficient should be solely due to decreased attenaution bias.[45]

One reason this might not emerge from an analysis such as Mazumder's is that - as we have noted - authors rarely pay attention to the age of the child when income was earned, but tend rather to focus on the age of the adult. Although there is a demographic relation between the two, it's not clear *ex ante* what form this will take. There are some cases where one can infer the age of children for the particular years being used. This is true of both Solon(1992) and Zimmerman(1992). The former's choice of cohort and years of parental income means that for the first year his sample is aged 8-16years, becoming 12-20years by the last year used. One cannot be as specific about Zimmerman's sample, except to say that the average age in the first year used is 18 (Zimmerman, 1992: 416) becoming 23 in the last. In both authors' analyses of the intergenerational relation between fathers' and sons' wages, the magnitude of the individual-year coefficients decreases as later years of parental income are used.[46] Because of their approach, this coincides with these years moving (for some of the sample at least) outside of childhood - which is precisely the result our model would predict.

The broader point is that to test whether the direction of averaging matters, years must be ordered by the age of observation; that is, ordered by the age of the child when

[45]We are implicitly assuming here that any biases due to other factors - such as life-cycle effects - have been satisfactorily dealt with.

[46](Solon, 1992: 401) and (Zimmerman, 1992: Table 3, 418). One should add the caveat that the decrease is not wholly uniform, and the pattern in Zimmerman's results using *other* measures such as log hourly earnings is quite different.

the parental income was earned. And to do this comprehensively for a long period will likely require access to social security and income tax datasets of the kind used by Mazumder (2005b), and Corak and Heisz (1999).

7.2 Determining the Magnitude of the Betas

In addition to demonstrating that transitory income matters, we would also like to ascertain just how much it matters. In particular, what proportion of the true IGE is due to transitory income and what proportion is due to permanent income? To answer this we may turn to the derivations in section 4. In particular, note that if we use a year of income *from well outside childhood* the resulting coefficient will not pick up any of the impact of transitory income. However, it should, *ceteris paribus* pick-up as much of the effect of *permanent* income as a year in childhood. Hence if we subtract the former coefficient from the latter (equation (8) minus equation (4) with T=1), we get only the attenuated effect of transitory income as shown below.[47]

$$plim\hat{B}^{*}_{childhood} - plim\hat{B}^{*}_{non-childhood} \approxeq \frac{\sigma^2_{W_0}\sum_q \beta_q \delta^{|q-k|} + \beta_k \sigma^2_{V_0}}{\sigma^2_{Y_0} + \sigma^2_{W_0} + \sigma^2_{V_0}} \qquad (11)$$

Plugging-in our earlier parameter values and assuming the simplest scenario in which all the betas are of the same individual magnitude, the the term on the right-hand side approximately equals the true beta of the childhood year in question. In the event that some years are more important than others - as the data suggests - this approach will *under*estimate the magnitude of the largest betas and *over*estimate it for the smaller ones. For our estimates we may therefore put the range of the betas conservatively at somewhere between 0.05 (in the late teens and early 20s) and 0.25 (in early childhood). The contribution to the total IGE is equal to the average of the betas and appears to be in the order of about 0.11. Thus about 20% of the intergenerational elasticity in the United States over this period is due to transitory income.[48]

7.3 Policy Considerations

The implications of these findings go beyond transitory income. It should be clear from all the preceding analysis that if transitory income matters for children's outcomes and later income, we may use variation in single-year income elasticities (the IGEs) to ascertain whether income is more important at some ages than others. If only permanent income mattered this would be impossible since - in the crude version of this hypothesis - permanent income by definition is constant over the lifecycle. Notice that even though we could not *test* this using income, that would not preclude the empirical possibility that differences in permanent income could matter more at some ages than others.[49]

[47]As emphasised already, this does rely importantly on the assumption that the non-childhood years are well outside childhood. The closer they are to childhood the more likely it is that the correlation in the transitory component will mask differences between years.

[48]This assumes a true IGE of 0.5. As noted earlier, the accuracy of IGEs calculated by the likes of Mazumder will depend on a number of factors and could be either over- or under-estimates.

[49]If that were the case, testing this would require analysing the importance of certain childhood outcomes - likely to be affected by differences in permanent income - for their future permanent income. Which to some degree is what Cameron and Heckman (2001) do in their analysis of children's educational outcomes.

What transitory implies about permanent

It is possible, however, to make inferences about the varying importance of permanent income from the varying importance of transitory income. If transitory income matters more at a particular age then it is very likely that, to the extent that permanent income has a direct causal role, it too will be more important at such ages. In fact, one would expect that the consumption of goods which importantly influence children's outcomes will be higher from permanent income than it is from transitory income. Therefore one expects that the former will be at least as important as the latter.

We should therefore clarify a point about our two examples in Section 2 relating to education and malnutrition. If parents only consume from the permanent component of their income, and the purchase of education and nutrition affects children's outcomes, then it will be *true* that differences in income matter more at the respective periods of childhood but *not* that transitory income matters. Those examples are intended to illustrate some extreme situations in which income might matter in a causal way at a particular period. If the notion of permanent income in the mobility literature was conceptualised in a more sophisticated way - à la Friedman, as discussed in Section 2 - then the argument for the salience of transitory income might be less persuasive. Since they are not, and the literature on the components of longitudinal income takes a similar approach, we argue that transitory income conceptualised in this way *is* important.

From a policy perspective, the implications may go beyond income itself.[50] Since it appears that the causal effects of income are greater at some ages rather than others, policy interventions need not take the form of income transfers but can instead be targeted at important developmental factors in those years (such as attendance at pre-school). Indeed, direct interventions of this sort may be more efficacious (not to mention being easier to sell to sceptical taxpayers).

There is an important caveat to these comments. Implicitly we are assuming that the relationship between transitory income and the consumption of goods that influence children's outcomes is fixed. Although not an unreasonable assumption, if wrong it could affect any policy conclusions and we should therefore be a little circumspect. For instance: If the goods that influence children's development in early childhood are more responsive to transitory income than those in later childhood, then it could seem that income is not causally important in later childhood; whereas in fact it is only transitory income that this is true of, and there may still be a case for policy interventions. As an example, there could be thresholds that determine certain important developmental experiences such as college attendance. If transitory income is insufficient in magnitude to take families over *some* such thresholds, then it may appear - and will indeed be the case - that transitory income does not matter in these periods, even though the decisions taken in those periods may have great impact on future outcomes *and* be income sensitive.

[50] Note that in this section we are implicitly presuming a model of human capital accumulation of the kind outlined by Case, Lubotsky and Paxson (2002). Whilst this is arguably the most plausible explanation for the empirically-demonstrated importance of transitory income, the mechanical nature of our model in section 4 means that we cannot prove this assertion conclusively.

8 Further Issues and Extensions

8.1 The Empirical Importance of Social Context: Developing vs. Developed Countries

The preceding sections provide support for the alternative model presented in section 4, and suggest a number of ways in which it could be tested using longer data sources in order to reach a more categorical conclusion. Although we hypothesise that the differences between IGEs calculated using this model and the standard model are likely to be significant in most cases, it is important to note that this will vary across societies. As is well known in the literature on intergenerational moblity, more developed societies in which there is substantial public provision of healthcare, education and other social services will be ones in which the importance of income of both a transitory and permanent nature is mitigated. Furthermore, such societies are likely to have better credit markets which are accessible to a greater proportion of the population, and be characterised by employment opportunities of a less volatile nature than in less developed societies. The result is that we would expect the bias arising from the standard model's exclusion of transitory income in the estimated equation for children's permanent income to be greater in less developed societies.

Indeed, although both Mayer (1998) and Shea (2000) argue that parental income is not an important causal factor for children's economic success in the United States, they acknowledge that:

1. This may be due in large part to the presence of "programs such as Food Stamps, housing subsidies, and Medicaid (which) have helped most American families meet their basic material needs" (Mayer, 1998: 148).

2. As a result the conclusions might be very different in less developed countries.

Shea therefore notes that Duflo's (2003) demonstration of the importance of pension income for children's outcomes in South Africa does not necessarily contradict his findings since, "the impact of parental resources on children may be higher in developing countries than in the contemporary US, where public investments in schooling and child health are relatively high" (Shea, 2000: 160). Or as Solon puts it: "the steady-state intergenerational earnings elasticity depends positively on the strength of the mechanical heritability of income-generating traits and the earnings return to human capital investment, and *it varies inversely with the progressivity of government investment in children's human capital (for example, through public provision of education or health care)*."(Solon, 2002: 65, my emphasis).

A recent piece of work (Karlan and Zinman, 2007), also reported in the Economist (2007), supports the notion that transitory income has a substantial impact in developing countries in the absence of credit. Karlan and Zinman find that marginal loan applicants whose applications were randomly approved were 19% less likely to be in poverty 6-12months later than those whose applications were rejected. This despite the fact that loans had to be repaid within 4months at an annual interest rate of 200%. If transitory income can impact poverty levels to this degree it is quite plausible that it thereby affects consumption of items that influence children's outcomes and future incomes.

We emphasise the broader point for two reasons. The vast majority of datasets available for making detailed analyses of intergenerational moblity of the kind we have been discussing are from highly developed countries. Whilst these present excellent opportunities for testing and confirming the theoretical assertions of this paper - as we do with the PSID in the previous section - it is important to emphasise that the social contexts these datasets represent are the ones in which the results from our alternative model are *least likely* to differ from those of the standard model. Nevertheless, it is important to emphasise - as we did in sections 2 and 4 - that the alternative model is *theoretically* superior to the simplistic permanent income model, requiring as it does fewer initial restrictions on the process of intergenerational tranfer. For that reason alone we argue that the former is simply the more correct way to conceptualise the IGE. The second reason is that, as data becomes available, researchers attempting to estimate the IGE in developing countries should be particularly cautious when drawing conclusions from estimates based on methods associated with the standard model. As demonstrated in section 5, these could be substantially biased in either direction.[51]

8.2 The Importance of the Age of Observation

One of the primary motivating factors for the proposal that transitory income matters was that it seems plausible to believe that parental income may be more or less important for children's outcomes depending on when it is earned (recall our two extreme examples in section 2). This was the reason that we first used β_q rather than simply β in (6) to characterise the importance of transitory factors for childhood income. In Section 5 two sets of simulations were constructed to incorporate a simplified version of this possibility, and in section 6 we attempted to explore the matter empirically. Our results, presented in Figure 2 and 3 - seem to provide strong support for this hypothesis. Other authors - some also using the PSID - have reached different conclusions however. In this subsection we examine the reasons for the difference.

Within the existing framework there appear to have been at least three attempts - by Mayer (1998), Case et al. (2002) and Hertz (2005) - in the literature to assess whether there is some difference in the importance of parental income at different ages of the child. Hertz (2005) estimates the IGE using three-year averages of family income taken at different ages of childhood (1-3years, 4-6years up to 16-18years), and further compares these over a period of 25years (1950-1975). Although there are clearly differences (sometimes substantial) between the estimates within a given year, no obvious pattern emerges. (Mayer, 1998: 72-75) also makes an attempt at assessing something akin to the effect of the age at which income is earned, by testing the null hypothesis that increasing parental income over childhood has a beneficial impact (controlling for the actual level of income over the period). She finds the gradient to be insignificant in her regressions and takes this as support for the assertion that 'parental income doesn't matter'. Finally, Case et al. (2002) attempt to assess whether it is permanent or current income that affects children's health status. They do this by comparing the coefficients on averages of log income from different stages of childhood.[52].

[51] South Africa, for instance, is just beginning its first National Income Dynamics Study through the South African Labour and Development Research Unit (SALDRU) at the University of Cape Town. Within a decade this will begin providing data with which we may make estimates of intergenerational mobility.

[52] In actual fact, it appears that the authors take logs of the averages rather than averages of the logs (Case et al., 2002: Table 5: 1321)

There are various problems with all these studies, stemming from the fact that none have an alternative model which actually incorporates the transitory income whose effect they wish to test. Mayer's approach is problematic in part because the structure she imposes on the varying salience of transitory income is so restrictive. The linear structure resembles the one constructed in our simulations in section 5 for illustrative purposes. However, as we point out in Section 6, this seems unlikely to represent the true pattern. Given our clearly non-linear graph in Section 6, it is therefore hardly surprising that she finds no significant result. In addition, it is not clear whether her estimation method of regressing children's outcomes on the gradient of parental income, whilst intuitively appealing, is econometrically valid.

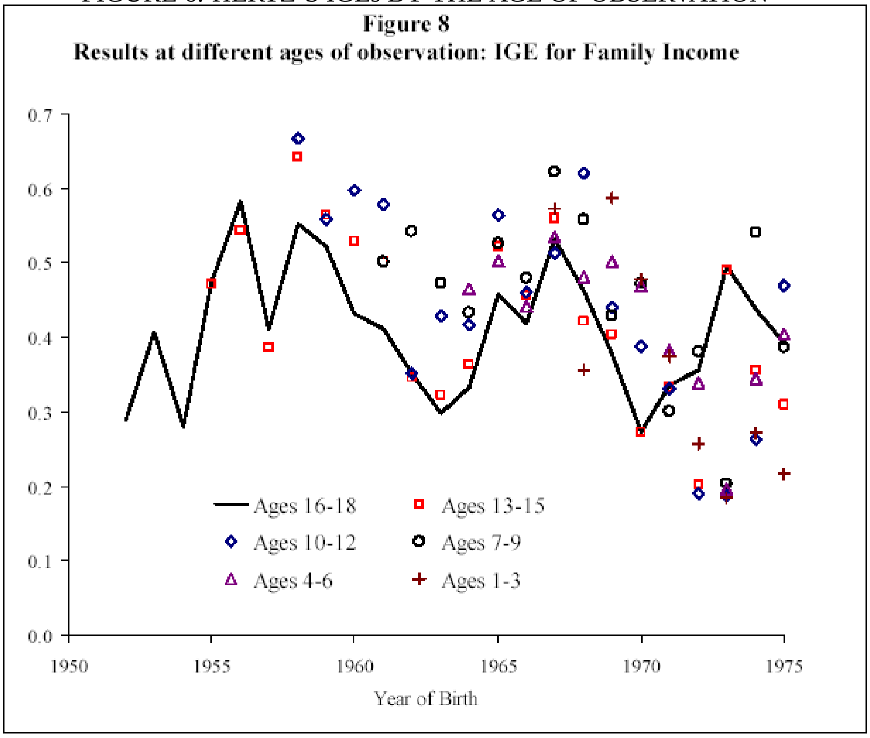
By contrast, the assessment by Case et al. (2002: 1321-1322) is much more nuanced in this respect. In ordered probits of health status they compare the coefficients on averages of household income from different periods of early childhood (0-3years, 4-8years and 9-12years) as well as that on an average of income from 6years before birth. Finding no statistically significant difference between these they then compare the coefficients on longer averages over: the child's lifetime (6.29years on average); their lifetime plus 6years before birth; their lifetime plus 9years before birth. In this case the coefficients on the second average are significantly higher than on the first, but do not increase significantly when three further years of pre-childhood income are included. The authors take this as an indication that, "our measure of permanent income becomes less noisy when we use these additional years of data" (Case et al., 2002: 1322). In fact, however, *none* of these results are incompatible with the possibility of transitory income being important for the outcomes under investigation.

To begin, note that the initial set of averages are taken over different lengths of time - from three to five years - which strictly speaking makes them incomparable given the effect this is likely to have on reducing the attenuation in the permanent component. Furthermore, the one average that is taken outside of childhood is still relatively close to that period. Equation (9) in section 4 demonstrates that, by virtue of the serial correlation in the transitory component, mere *proximity* to years of importance can falsely inflate the coefficients on averages over less important years. In addition our results in section 6 suggest that transitory income may be important up to three years before birth (though in that case the importance need not be a causal one). Furthermore, the results in the second set of estimations *concur* with the assertion in sections 4 and 6 (supported by the results in section 5) that the *direction* of averaging matters. If it were true that only permanent income mattered, then by Mazumder's analysis discussed previously there should be substantial benefits to increasing the average beyond a length of only twelve years.[53] Under his model this holds true for income from any period. By contrast, if transitory income matters as we suggest, then adding years of non-childhood income to an average which already contains a number of years from childhood may have little effect and may even *decrease* the coefficient.

The significance of transitory income is not Hertz's explicit interest (though he seems to believe that it does matter by indicating a preference for income earned whilst children were resident in the household). Nevertheless, one part of his paper - reproduced here as Figure 6 - is very similar to our analysis in section 6.

[53]This is actually the average length of the averages used by Case et al. (2002) because the children in the sample were of different ages, but the broader point remains valid.

FIGURE 6. HERTZ'S IGEs BY THE AGE OF OBSERVATION[54]



Hertz combines the calculation of the IGE using income from different stages of childhood with a comparison of IGEs across time. In terms of the former it is hard to see any consistent pattern *within* any given cohort - which contrasts with our results. (He makes no assessment of the importance of non-childhood income and is therefore unable to consider whether income within childhood is more important than that *outside* it).We suggest that there are two likely reasons for this:

- Dynamics and public policy: Due to changes in public policy and social structure one expects - as per the discussion in 7.1 - there to have been changes in the salience of income at different ages. As a result, the lack of a clear pattern between cohorts over a twenty-five year period is probably misleading.

- Sample size: Whilst restricting the analysis to individual cohorts is appealing as it deals with any concerns about variation - amongst these groups and over time - it is highly detrimental to the sample size in the regressions (which is precisely why we did *not* do this in section 6). Hertz notes that cohort sizes themselves range from 300 to less than 100 but, as we found, these are typically reduced substantially when the availability of income data is considered. Thus one suspects that the noise in his estimates is just that.

The main point regarding all these studies is that to assess whether transitory income matters it is preferable to compare coefficients on years well outside childhood (preferably by at least ten years) to those within it. This broader issue should not be confused

[54]We reproduce here the top half of Hertz's Figure 8. The second half simply adds a trendline based on the average of all his estimates; since we are not interested in issues of dynamics here, we exclude this part.

with assessing the *relative* importance of income at different *periods* in childhood, which is a different - more complicated - matter.

8.3 Decomposing the Intergenerational Correlation

As noted in the Introduction, a primary concern of the current literature is with decomposing the calculated intergenerational correlation in income; that is, determining the channels through which this correlation is being generated, and ascertaining their relative importance.[55] This can be done by estimating the relation between the variable representing the channel of interest (years of schooling for instance) and *children's* income in a multiple regression including variables representing all hypothesised channels, then multiplying this by the correlation between the channel and *parental* income.[56] By doing this we are breaking the IGC down into the 'direct' and 'indirect' effects of parental income.

Some channels which have received particular interest are those relating to the transfer of genetic ability (generally using twin and/or sibling studies), educational attainment and personality traits.[57] A consensus has yet to emerge however on even these three channels. For instance, Loehlin (2005) finds that personality traits explain little of the correlation, whereas Mayer (2005) in the same volume suggests they do explain a relatively large portion of it. In addition, some authors such as Grawe and Mulligan (2002) - following the models of family investment and intergenerational transfer of Becker and Tomes - emphasise the distinctive implications of economic models of transmission, whilst others (see Goldberger, 1989; Bowles and Gintis, 2002) take a more 'mechanical' approach.[58]

There is also a tendency to use analyses of these channels to infer the existence, or importance, of a causal relationship between parental income and children's outcomes (and hence income). One example of this sort is Solon's finding that the IGC only explains 0.16 of the 0.4 correlation between brothers' incomes and therefore that "of the 40% or so of permanent earnings inequality that arises from the family and community background factors shared by brothers, probably only a minority share is related to parental income" (Solon, 1999: 1784). This argument is based on Solon's decomposition of the sibling correlation in earnings (1999: 1777, eq.21). But the logic is potentiallly problematic because if the estimated IGC (0.4 in this example) is underestimated then it will be biased toward the conclusion that parental income per se is not particulary important.[59]

[55] Again it is important to emphasise that we are talking of the generation of the *correlation* rather than children's outcomes, since if anything the majority view appears to shy away from the notion of a causal relation.

[56] Where it is assumed that all variables are normalised. See Bowles and Gintis (2002) for a full discussion of the details.

[57] See Solon (1999) for a survey of studies on sibling correlations in income and their relation to the intergenerational correlation.

[58] "Unlike the models of parental and child behavior accounting for persistence pioneered by Becker and presented in this issue by Grawe and Mulligan, our approach is more diagnostic, not giving an adequate causal account of the transmission process, but indicating where to look to find the causes."(Bowles and Gintis, 2002: 9-10).

[59] In fact, based on Solon's decomposition, an IGC of 0.6 would explain $(0.6)^2 = 0.36$ of the 0.4 correlation between siblings leading to a very different conclusion.

Furthermore, in this instance there may be an additional problem: that calculated correlations in sibling income *do* pick-up the impact of transitory parental income to a greater degree than the IGC to which they are compared, thereby increasing the likelihood that we underestimate the salience of parental income as a causal factor. Until we have greater confidence in our estimates of the IGC (or - as we have suggested - the IGE) and, in particular, until we have accounted for the possible importance of transitory income it would be advisable to be somewhat cautious in the inferences drawn from such comparisons.

8.4 Implications for Public Policy

There are a number of reasons why we should be interested in intergenerational mobility from a policy perspective. Perhaps the primary one is that we may be interested in achieving, to the greatest degree possible, a society characterised by equal opportunity (with the caveat noted in the Introduction). To the extent that a high IGE indicates a failure in this regard it suggests the possible need for government intervention. The main point made in the literature on this subject is that one can only determine whether this is the case by decomposing the intergenerational relation into its constituent channels and making ethical determinations of their desirability. Swift (2005) puts forward one set of criteria in this regard which, though they may seem extreme to some, are appealing in as much as they take popular conceptions of what constitutes 'fair' transmission channels to their logical conclusions.

These arguments are based on the notion that intergenerational transmission occurs through permanent income. An additional appeal of considering transitory income is that *if* we can identify it to have a significant effect, the policy implications would appear to be more immediate. Essentially: if transitory income affects children's future economic status this demonstrates that income *per se* is affecting a child's opportunities, which is something that is generally incompatible with even relatively weak formulations of equality of opportunity. Thus the greater the importance of transitory income relative to permanent income, the stronger the case for government intervention. In addition, as we note in 7.3, the importance of transitory income implies a causal role for permanent income too - further strengthening the case for governnment income support of needy families.

The study by d'Addio (2007) clearly indicates an increased awareness amongst policy makers of the importance of intergenerational mobility, and by implication a greater role for mobility studies in influencing policy. Given that the explicit motivation for that study is derived from the statement by OECD Social Policy Ministers that: "the OECD should identify which interventions alleviate and will contribute to the eventual eradication of child poverty, break the cycle of intergenerational deprivation, and develop the capacity of children to make succesful transitions through the life course" (d'Addio, 2007: 10), it is quite clear that the role for mobility analyses from a policy perspective is going to be identifying the optimal areas for government intervention. The kind of problems identified in this paper that result from neglecting the importance of transitory income could well affect the validity of any such recommendations. But on a more positive note, as per the discussion in 7.2, they also present the possibility of identifying both the stages of childhood that are most important for children's eventual outcomes, as well as the relative extent of this importance.

One caution is that whilst transitory income may matter, this does not necessarily mean that government transfers will have the same effect. On the individual level the use of different 'mental accounts', and at the household level the complications of intrahousehold distribution of resources, can result in transfers being spent differently to income that is usually received.[60] In the South African context, for instance, Duflo (2003) and other authors such as Case and Deaton (1998) have found that government pension transfers are most beneficial for children's outcomes when they are received by *female* pensioners.[61] As always, the construction of policy upon analytical work must be done with due sensitivity to the assumptions involved in moving from the one sphere to the other.

9 Conclusion

In this paper we argue that the implicit structural model which underlies the literature on intergenerational correlations in income is incomplete, because it fails to allow for the influence of transitory parental income on children's permanent income. As a consequence, estimates of the IGC based on the associated simple regression model are likely to be flawed. In Section 4 we present an alternative model, and derive the probability limits of the estimators used to date in the context of these assumptions. Our simulation results in Section 5 confirm that inferences of the magnitude of the true IGE from empirical estimates derived using the standard methods in the literature, and using the logic of the crude permanent income model, are likely to be flawed. Section 6 and 7 use the implications of our alternative model to empirically ascertain the role of transitory income.

The primary objective of the paper is to provide a model which *allows* us to ask whether (transitory) income matters - as opposed to the rather unsatisfactory, *ad hoc* manner in which the question has been addressed in the literature to date. The *answer* to the question is, of course, to be found in empirical analysis. Evidence from PSID data which we present in Section 6 appears to support the notion that transitory income matters in the United States - to the extent that approximately 20% of the true IGE in the US may be due to transitory income. Furthermore, income - both transitory and permanent - may matter more at different stages of childhood; with our results suggesting that income in early childhood may be up to *five-times* as important as income in other periods.

Given that studies of intergenerational mobility appear likely to inform future government policies, the issue is not merely an academic one. The existing model is biased toward the conclusion that parental income is not a key determinant of children's future economic status, and whilst that conclusion cannot yet be categorically refuted without further evidence, the foregoing arguments and evidence suggest a much greater causal role for parental income than the recent literature implies. Considering the concomitant implications for redistributive social policy, we suggest that confirming this fact - and the broader validity of the complete model - should be a priority for future research.

[60] Useful references are Thaler (1990) - referred to previously - and Alderman, Chiappori, Haddad, Hoddinott and Kanbur (1995), respectively.

[61] Although Case and Deaton (1998) did find that aside from the impact of the gender of the household head, pension income was spent in much the same way as other income - allaying concerns about the effect of mental accounts in that context.

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Appendix

In this appendix we provide the derivations of the equations used in the text. We begin with the equations characterising the alternative model outlined in section 4, renumbered for this appendix.

$$
\begin{aligned}
y_{0is} &= y_{0i} + w_{0is} + v_{0is} && (12)\\
y_{1it} &= y_{1i} + w_{1it} + v_{1it} && (13)\\
w_{0is} &= \delta w_{0is-1} + \xi_{0is} && (14)\\
y_{1i} &= \rho y_{0i} + \sum_q \beta_q (z_{0iq}) + \varepsilon && (15)
\end{aligned}
$$

In these equations y_{0is} represents parental income in year s and y_{1it} represents the child's income in year t. Following Mazumder (2001) these are each expressed as functions of a permanent component (y_{0i} and y_{1i}), transitory component ($w_0 is$ and w_{1it}) and a white-noise component (v_{0is} and v_{1it}) respectively.

$$
\begin{aligned}
\text{Where} \quad & z_{0iq} = y_{0iq} - y_{0i} = (w_{0iq} + v_{0iq})\\
\text{and} \quad & q \in Q, s \in S, Q \subset S
\end{aligned}
$$

The second term in the fourth equation allows children's permanent income (y_{1i}) to be a function of *deviations* from parents' permanent income (z_{0iq}).

$$
\begin{aligned}
\text{Q is the set indexing childhood income, so that} \quad & \{q \in Q| \quad a < q < b, Q \subset \aleph\}\\
\text{K is the set indexing years used in the regression, so} \quad & \{k \in K| \quad r < q < m, Q \subset \aleph\}\\
\text{And the number of years used in the average are:} \quad & T = m - r
\end{aligned}
$$

Estimating the IGE using a Single-Year

If we estimate $y_{1it} = \beta^* y_{0ik} + \epsilon$ under the above assumptions, what is the probability limit of our estimated coefficient? We know that the regression coefficient β^* can be written as: $\hat{\beta}^* = \frac{\sum y_{1it} y_{0ik}}{\sum y_{0ik}^2}$

$$
\begin{aligned}
\text{so } plim\hat{\beta}^* &= \frac{plim(\sum y_{1it} y_{0ik})}{plim(\sum y_{0ik}^2)}\\
&= \frac{plim(1/n)(\sum y_{1it} y_{0ik})}{plim(1/n)(\sum y_{0ik}^2)}
\end{aligned}
$$

Because we are using only one year of parental income we know that:

$$
plim(1/n) \sum y_{0ik}^2 = \sigma_{y_0}^2 + \sigma_{w_0}^2 + \sigma_{v_0}^2
$$

so it remains to determine $plim(\sum y_{1it}y_{0ik})$:

Substituting equations (12) and (13) into equation (15) gives us:

$$\sum y_{1it}y_{0ik} = \sum(\rho y_{0i} + \sum_q \beta_q(w_{0iq} + v_{0iq}) + w_{1it} + v_{1it})(y_{0i} + w_{0ik} + v_{0ik})$$

Multiplying out this term, and eliminating combinations of independent variables we get:

$$\begin{aligned}
plim(1/n)(\sum y_{1it}y_{0ik}) &= plim(1/n)\left(\sum_i(\rho y_{0i}^2 + \sum \beta_q w_{0iq}w_{0ik} + \sum \beta_q v_{0iq}v_{0ik})\right)\\
&= \rho\sigma_{y_0}^2 + \beta_k\sigma_{v_0}^2 + plim(1/n)\sum_i\left(\sum \beta_q w_{0iq}w_{0ik}\right)\\
&= \rho\sigma_{y_0}^2 + \beta_k\sigma_{v_0}^2 + \sum \beta_q\left(plim(1/n)w_{0iq}w_{0ik}\right)
\end{aligned}$$

From equation (14) note that in general:

$$w_{0iq} = \delta^{|q-k|}w_{0ik} + \sum_{r=0}^{s-k-1}\delta^r\xi_{(s-r)}$$

so

$$\begin{aligned}
plim(1/n)\sum w_{0iq}w_{0ik} &= plim(1/n)\sum_i\{\delta^{|q-k|}w_{0ik}^2 + \sum_{r=0}^{s-k-1}\delta^r\xi_{(s-r)}w_{0ik}\}\\
&= \delta^{|q-k|}\sigma_{w_0}^2 + 0
\end{aligned}$$

therefore

$$\sum \beta_q(plim(1/n)\sum w_{0ik}w_{0iq}) = \sum \beta_q\delta^{|q-k|}\sigma_{w_0}^2$$

and

$$\begin{aligned}
plim(1/n)\sum y_{1it}y_{0ik} &= \rho\sigma_{y_0}^2 + \beta_k\sigma_{v_0}^2 + \sum \beta_q\delta^{|q-k|}\sigma_{w_0}^2, \text{ if } k \in Q\\
&= \rho\sigma_{y_0}^2 + \sum \beta_q\delta^{|q-k|}\sigma_{w_0}^2 \qquad , \text{ if k is not a year of childhood income}\\
&\approxeq \rho\sigma_{y_0}^2 \qquad , \text{ if } \delta \approxeq 0.5 \text{ and } min|q-k| \geqq 10
\end{aligned}$$

The first and second instances above are the basis for equations (7) and (8) in sections 4.2.1 and 4.2.2 respectively.

Estimating the IGE using a Multi-Year Average

If, instead of using one year of parental income we instead use an average of all parental income during childhood (where childhood is defined as being from year 'r' to year 'm') and estimate $y_{1it} = \beta^* \frac{\sum_q y_{0iq}}{m-r} + \epsilon$ the regression coefficient can be represented as:

$$\hat{\beta^*} = \frac{\sum_i y_{1it} \left(\frac{\sum_k y_{0ik}}{m-r} \right)}{\sum_i \left(\frac{\sum_k y_{0ik}}{m-r} \right)^2}$$

so $plim\hat{\beta^*} = plim(1/n) \sum_i \left(\frac{1}{m-r} \right) \left(y_{1it} \sum_k y_{0ik} \right) \div plim(1/n) \sum_i \left(\frac{1}{m-r} \right)^2 \left(\sum_k y_{0ik} \right)^2$

From reproducing Mazumder's result (Mazumder, 2001)[62] we know that:

$$plim(1/n) \sum_i \left(\frac{1}{T^2} \right) \left(\sum_k y_{0ik} \right)^2 = \sigma^2_{y_0} + (1/T)\sigma^2_{v_0} + (1/T)\sigma^2_{w_0} \left\{ 1 + 2\delta \left(\frac{T - \delta T - 1 + \delta^T}{T(1-\delta)^2} \right) \right\}$$

So we need to determine:

$$plim(1/n) \sum_i \left(\frac{1}{T} \right) \left(y_{1it} \sum_k y_{0ik} \right)$$

As in the previous section we substitute for y_{0ik} and y_{1it} using equations (12) and (13) respectively:

$$\begin{aligned}
\sum_i \left(y_{1it} \sum_k y_{0ik} \right) &= \sum (\rho y_{0i} + \sum_q \beta_q (w_{0iq} + v_{0iq}) + w_{1it} + v_{1it})(y_{0i} + w_{0ik} + v_{0ik}) \\
&= \sum_i \sum_k \Big(\sum \beta_q w_{0iq} y_{0i} + \sum \beta_q w_{0iq} w_{0ik} + \sum \beta_q w_{0iq} v_{0ik} + \rho y^2_{0i} + \\
&\quad \rho y_{0i} w_{0ik} + \rho y_{0i} v_{0ik} + w_{1it} y_{0i} + w_{1it} w_{0ik} + w_{1it} v_{0ik} + v_{1it} y_{0i} + \\
&\quad v_{1it} w_{0ik} + v_{1it} v_{0ik} + \sum \beta_q v_{0iq} y_{0i} + \sum \beta_q v_{0iq} w_{0ik} + \sum \beta_q v_{0iq} v_{0ik} \Big)
\end{aligned}$$

$$\begin{aligned}
\text{and } plim(1/n) \sum_i \left(y_{1it} \sum_k y_{0ik} \right) &= plim(1/n) \sum_i \sum_k \Big(\rho y^2_{0i} + \sum \beta_q w_{0iq} w_{0ik} + \sum \beta_q v_{0iq} v_{0ik} \Big) \\
&= T\rho\sigma^2_{y_0} + \sum_k \sum_q \beta q \; plim(1/n) \sum_i w_{0iq} w_{0ik} + \sum_k \beta_k \sigma^2_{v_0} \text{ , } \forall k \in Q
\end{aligned}$$

As noted in 1.1 above :

[62]The details of this calculation are available from the author.

$$plim(1/n)\sum_i w_{0ik}w_{0iq} = \delta^{|q-k|}\sigma^2_{w_0}$$

so

$$\begin{aligned}\sum_k\sum\beta_q plim(1/n)\sum_i w_{0ik}w_{0iq} &= \sigma^2_{w_0}\sum_k\sum\beta_q(\delta^{|q-k|}\\ &= \sigma^2_{w_0}\sum\beta_q\sum_k\delta^{|q-k|}\end{aligned}$$

and recall that $k \in K$ and $q \in Q$ (see the earlier definitions of these index sets).

We can then write this as:

$$\sigma^2_{w_0}\sum\beta_q\sum_k\delta^{|q-k|} = \sigma^2_{w_0}\sum\beta_q(\delta^{|q-r|}+\delta^{|q-r-1|}+\delta^{|q-r-2|}+\cdots+\delta^{|q-m+1|}+\delta^{|q-m|})$$

There are seven possible scenarios in terms of the overlap between the sets of childhood income (Q) and the years of income used in the regression (K). For the present purpose - and because in the text we are interested in the contrast with the case where one year of parental income is used - we shall focus on the scenario in which all years of childhood income are used in the regression so that $K = Q$. [63] In this instance the summed series in brackets above is increasing and then decreasing in $\delta^{|q-k|}$.

So we have:

$$\sum\beta_q(\delta^{|q-r|}+\delta^{|q-r-1|}+\delta^{|q-r-2|}+\cdots+\delta^0+\delta+\cdots+\delta^{|q-m+1|}+\delta^{|q-m|})$$

For a given year 'q' we split the sum in brackets into two finite geometric sequences as follows:

$$\left(\delta^{|q-r|}\frac{1-\left(\frac{1}{\delta}\right)^{|q-r|}}{1-\frac{1}{\delta}}\right)+\left(\delta^0\frac{1-\delta^{|q-m|+1}}{1-\delta}\right)$$

$$\begin{aligned}&= \delta^{|q-r|}\times\frac{\frac{\delta^{|q-r|}-1}{\delta^{|q-r|}}}{\frac{\delta-1}{\delta}}+\frac{1-\delta^{|q-m|+1}}{1-\delta}\\ &= \frac{\delta(\delta^{|q-r|}-1)}{\delta-1}+\frac{\delta^{|q-m|+1}-1}{\delta-1}\\ &= \frac{1+\delta-\delta^{|q-r|+1}-\delta^{|q-m|+1}}{1-\delta}\end{aligned}$$

[63] The reader is left to determine the remaining scenarios.

So we have :

$$\sigma_{w0}^2 \sum \beta_q \sum_k \delta^{|q-k|} = \sigma_{w_0}^2 \sum \beta_q \left(\frac{1+\delta-\delta^{|q-r|+1}-\delta^{|q-m|+1}}{1-\delta} \right)$$

Putting all this together :

$$plim(1/n) \sum_i \left(y_{1it} \sum_k y_{0ik} \right) = T\rho\sigma_{y_0}^2 + \sum \beta_k \sigma_{v_0}^2 + \sigma_{w_0}^2 \sum \beta_q \left(\frac{1+\delta-\delta^{|q-r|+1}-\delta^{|q-m|+1}}{1-\delta} \right)$$

And thus :

$$plim\hat{B}^* = \left(\frac{\rho\sigma_{Y_0}^2 + (1/T)\sum_q \beta_q \sigma_{V_0}^2 + (1/T)\sigma_{W_0}^2 \sum_q \beta_q \left(\frac{1+\delta-\delta^{|q-r|+1}-\delta^{|q-m|+1}}{1-\delta} \right)}{\sigma_{Y_0}^2 + (1/T)\sigma_{V_0}^2 + (1/T)\sigma_{W_0}^2 \left\{ 1+2\delta \left(\frac{T-\delta T-1+\delta^T}{T(1-\delta)^2} \right) \right\}} \right)$$

This is equation (9) in the text, where we substitute as follows :

$$\begin{aligned} \Lambda_q &= \left(\frac{1+\delta-\delta^{|q-r|+1}-\delta^{|q-m|+1}}{1-\delta} \right) \\ \alpha &= \left\{ 1+2\delta \left(\frac{T-\delta T-1+\delta^T}{T(1-\delta)^2} \right) \right\} \end{aligned}$$

TABLE 1. OUTPUTS OF IGE ESTIMATIONS ON SIMULATED DATA

PARAMETER VALUES	$\rho = 0.3$ $\beta_i = 0.2$	$\rho = 0.4$ $\beta_i = 0.1$	$\rho = 0.3$ $0 \leq \beta_i \leq 0.4,\ \beta_i \geq \beta_{i+1}$	$\rho = 0.3$ $0 \leq \beta_i \leq 0.4,\ \beta_i \leq \beta_{i+1}$
INDEPENDENT VARIABLE				
Single-year Outside Childhood	0.174	0.232	0.174	0.174
5year Average Outside Childhood	0.232	0.31	0.232	0.232
Single-year from Mid-Childhood	0.397	0.343	0.397	0.397
5year Average from Earliest Childhood	0.498	0.441	0.70	0.296
Full Childhood Average	0.617	0.536	0.614	0.609
Full Average Outside Childhood	0.289	0.377	0.28	0.297
Full Average over All Years	0.453	0.467	0.45	0.457
Lubotsky-Wittenberg Estimator	0.643	0.546	1.394	1.382
Childhood Years Separately with Full Non-childhood Average as a Control	2.489 0.452	1.315 0.459	2.489 0.452	2.489 0.452

Notes:
1. All simulations are for a sample of 10,000 parent-child pairs, with 10,000 repetitions.
2. The dependent variable in each case is one year of child's income.
3. In each case the true IGE ($\rho + \Sigma\beta_q$) is equal to 0.5.
4. No controls are used in the regressions. Life-cycle variation is not incorporated into the generation of the hypothetical income data.
5. The permanent, transitory and white-noise components of income are assumed to explain, respectively, 0.5, 0.3 and 0.2 of the overall variance in children's and parents' income (see the discussion in Section 5).