

# QUANTILE REGRESSION MEASURES OF MOBILITY

## IN THE US AND ABROAD

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### 1 Section I: Introduction

Studies of intergenerational mobility have devoted considerable effort to finding an unbiased estimate of the degree of income persistence. Solon (1992) and Zimmerman (1992) highlight the importance of transient income and measurement error; Jenkins (1987), Reville (1995), and Grawe (2001) point to the age-dependence of income persistence estimates; and countless studies using multiple data sets have attempted to find "the number" that best estimates financial mobility.

But a new line of literature seeks to move beyond the number in an effort to better understand the transmission of economic status. Even if estimates are biased, if the biases are similar across groups or across time, then the results may yet be useful. For instance, Couch and Lillard (2001) looks for cross-country (US and Germany) differences in regression non-linearities and Mayer (2001) examines trends in mobility estimates over time. This study joins this vein of the literature by providing several new pieces of information. First, the quantile regression slope is proposed as an alternative to the mean regression definition of mobility found in most of the literature. Second, I provide comparisons of mobility in the US and abroad with careful controls for sample selection rules. Finally, using two-sample instrumental variables techniques as well as a newly

developed quantile regression estimation procedure, I provide a first look at mobility in four developing nations: Ecuador, Nepal, Pakistan, and Peru.

The quantile regression

$$y_{sn} = \hat{\delta}_{1,n} + \hat{\delta}_{2,n}y_f \quad (1)$$

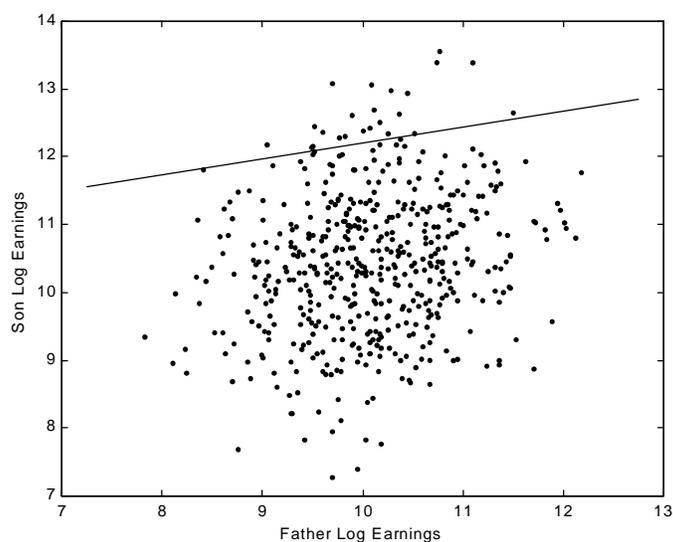
shows the estimated location of the  $n^{\text{th}}$  centile y-value conditional on the value of x. This is a direct analogy to mean regression. Koenker and Basset (1978) shows that regression (1) can be estimated by

$$\hat{\delta} = \min_{\delta} \frac{1}{K} \sum_{i=1}^K [n - .5 + .5\text{sgn}(y_i - x_i^0\delta)](y_i - x_i^0\delta) \quad (2)$$

where  $K$  is the sample size. (See Buchinsky (1998) for a summary of recent developments in quantile regression estimation.) In Figure 1, the 95<sup>th</sup> quantile regression line with slope 0.235 has been superimposed on the data from a simulated society. Consider two family types: a father in a family of type A earns the average (for fathers) income while a father in a family of type B earns twice as much. The 95<sup>th</sup> percentile son in a type-B family is estimated to earn 23.5% more than the 95<sup>th</sup> percentile son from a type-A family.

Quantile regression is not entirely new to studies of intergenerational mobility. Eide and Showalter (1999) estimates several quantile regressions using data from the Panel Study of Income Dynamics (PSID); Minicozzi (1997) presents median regression as corroborating evidence of mean regression. But without providing a clear motivation for an alternative approach, quantile regression is unlikely to attract many converts. I see at least three reasons for interest in quantile income regression.

First, researchers wondering *why* income is persistent may gain insight by considering regression rates at different points of the joint distribution. Is income persistence largely derived



### 1. Mobility as the potential earnings level.

from parents' abilities to pass on income advantages to their most able children? Or does income persistence reflect parental provision of a safety net for the least able children born into high-income families? Mean regression cannot distinguish these alternative depictions.

But the quantile regression is far more than simply a descriptive statistic. One reading of the welfare economics literature concerning equal opportunity suggests that the quantile regression best captures the notion of equal opportunity. Roemer (1998) presents a thesis defending a particular welfare function reflecting "social justice". In Roemer's view, proper social policy "levels the playing field" in the sense that individuals should not be held accountable for aspects of their circumstance which are beyond their control. Individuals who exert the same *degree* of effort (where the degree of effort is measured relative to others facing the same exogenous circumstances) should attain the same level of welfare.

According to Roemer, we should examine the group of individuals who reach the  $\pi^{th}$  centile conditional on their circumstance type. A just society will redistribute resources between

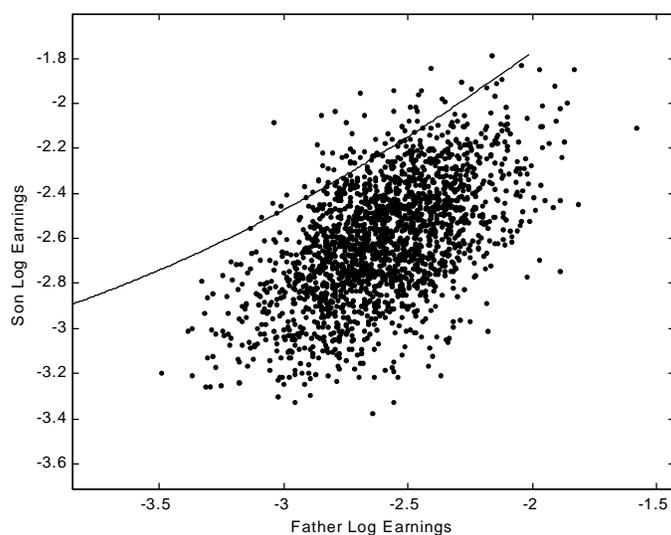
circumstance types so as to maximize the utility of the least fortunate member of this group. In the study of intergenerational income mobility, one natural definition of exogenous circumstance or type is parental income. Welfare is reduced when there is more inequality among incomes of the (conditionally)  $\pi^{th}$  centile sons. By this reasoning, the quantile regression is a natural measure of equality of opportunity.

Additional support for quantile regression as a measure of opportunity can be found in the literature on wage discrimination. The typical study of wage discrimination explores the regression

$$w_i = \alpha + \beta_1 HK_i + \beta_2 J_i + \beta_3 D_i + \varepsilon_i \quad (3)$$

where  $w_i$  is the wage (or log wage),  $HK_i$  is the human capital (education, experience, etc.),  $J_i$  measures characteristics of the individual's job (amenities or disamenities, occupation, industry, etc.), and  $D_i$  is a dummy variable representing race or sex. (See Blau and Kahn (1994), Ginther and Hayes (1999), or Hoffman (1976) for representative examples of such studies.) Wage differences *after controlling for human capital and job characteristic measures* are construed as evidence for discrimination. Implicit in the approach is the recognition that observed income combines both opportunity and preferences. If a woman's preferences lead her to choose to work part time, or to work in a low-wage occupation, or to acquire less education, this will be reflected in a lower income (and wage) even if opportunity is equal.

The result of this approach is to put a large emphasis on outliers in the data—women and minorities who make unusual choices. The question explored by regression (3) is: *Given* a woman has made the same choices as high-income males, does she too receive high compensation? The quantile regression similarly focuses on exceptional outcomes. While most sons will likely be



## 2. Identifying the credit constrained families.

very similar to their fathers in terms of choices and tastes, we might expect a few "deviants." Even if sons' average incomes are strongly correlated with fathers' incomes, the incomes of the highest paid sons from all family backgrounds should be fairly similar if an economy is open to upward mobility.

A final justification for my interest in quantile regression is its potential use in testing theory. Grawe (2001) shows that quantile regression can contribute to the literature testing for the presence and importance of intergenerational credit constraints which limit the ability to finance education. Figure 2 tells the story graphically. The data are simulated according to a variation of the simulations in Han and Mulligan (2001). The points represent the log earnings of fathers and sons that would result if credit markets were perfect. When markets fail, some sons are of such high ability relative to their fathers that their fathers cannot (or will not) self-finance the son's education. These fathers would like to give negative bequests, but are not permitted to do so. The line drawn in Figure 2 separates the families who are affected by the credit constraint from those

who are not. Any family located above the line could only reach this point if the father passes debt on to the son. A comparison of upper and lower quantile regressions distinguish potentially constrained from unconstrained families just as bequest information defines the two groups in Mulligan (1997, 1999). The conjecture in Becker and Tomes (1986) that constrained families experience slower income regression suggests that credit constraints will lead to slower income persistence in higher quantiles.

The remainder of the paper proceeds in three sections. In the next section I describe the data sets that are used and the methods of estimation that are employed. More detailed description of data selection criteria are available in an appendix upon request. The following section reports results including direct comparisons of the US to Canada, Germany, the UK, and Malaysia. Additional evidence is provided from Ecuador, Nepal, Pakistan, and Peru. Finally, I conclude with several alternative interpretations of the results.

## **2 Section II: Data and Methods**

Approximately half of the data in this study come from the standard data sets employed in intergenerational mobility studies. Table 1 lists the countries for which I have intergenerational panels and identifies the data set used. In this section I will briefly outline the considerations used to determine sample selection rules. A fuller description of the data sets and the exact sample selection criteria is included in an available appendix.

When conducting cross-country comparisons, care must obviously be taken to ensure that differences caused by sample differences are not mistakenly interpreted as real economic differences. Given the nature of intergenerational data sets, one important sample characteristic

Country	Data Set
United States	Panel Study of Income Dynamics (PSID)
United States	National Longitudinal Survey (NLS)
Canada	Intergenerational Income Data (IID)
Germany	German Socio-Economic Panel (GSOEP)
United Kingdom	National Child Development Survey (NCDS)
Malaysia	RAND Malaysian Family Life Survey (MFLS)

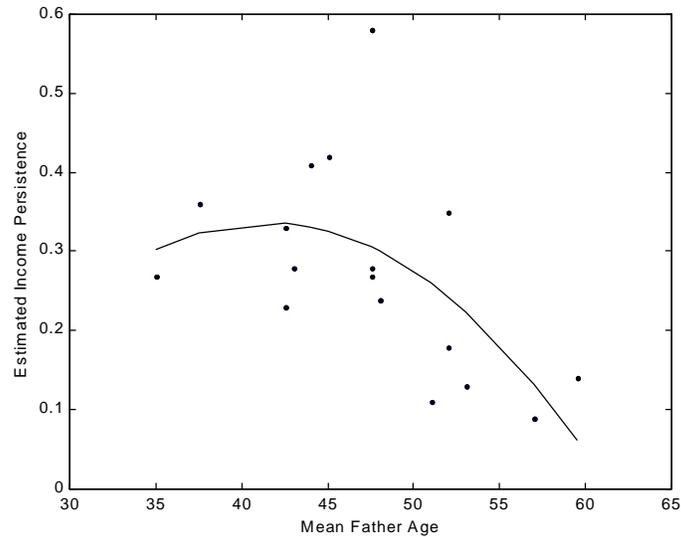
### 1. Intergenerational panel data sets.

is the age of the father and son at the point of measurement. Grawe (2001) points out that since income variance grows over the life cycle, estimates of income persistence based on data from mature fathers will naturally be lower than those based on young fathers. ( I will refer to this as a "life cycle bias".)

Figure 3, taken from Grawe (2001), summarizes the magnitude of the problem. The figure plots published estimates of income persistence against the average age of the fathers in each study. As predicted, since income variance is positively correlated with the fathers' age, income persistence estimates are negatively correlated. In fact, roughly one-third of the variation in income persistence estimates can be attributed to differences in fathers' age.

Figure 3 points out the dangers of meta-analysis in the comparison of income persistence across countries. If estimates are to be compared, the selection criteria must control for the age of the fathers in both data sets. Since the available data sets do not permit me to choose the same age in every country, I report only pair-wise comparisons between the US and other countries. In addition to controlling for the age of fathers, I control for the calendar year of observation in order to minimize the effects of business cycle fluctuations.

A second obvious concern is that of measurement error. Since data collection methods differ across data sets, it is likely that the magnitude of measurement error is also different. So proceeding with no correction and hoping that biases are similar across countries is insufficient.



### 3. Age-dependence of income persistence estimates.

Following Solon (1992) and Zimmerman (1992) I use a time average of fathers' and sons' incomes when possible. Unfortunately, only one year of income observation is available in the NCDS and MFLS data sets. In these cases I instrument father's income with father's education.

Corrections of mean regression for measurement error are well-studied. But the problem of measurement error in quantile regression is, to this point, unsolved (Buchinsky 1998). I propose a maximum likelihood-based correction of quantile regressions. (A detailed description of the method is available upon request.) The strategy I employ exploits the fact that quantile regression slopes differ from median regression slopes only insofar as the data are heteroskedastic. In a primary stage, I estimate the mean regression coefficient. Then in a second stage I estimate (using MLE) the variance about the regression line as a polynomial function of parent income

$$\sigma(y_f) = a + by_f + cy_f^2.$$

(If mean and quantile regression lines are linear, then  $c = 0$ .) Finally, the mean regression coefficient and the variance coefficients are combined to form estimates of the quantile regression

slopes. Standard errors are estimated with bootstrapping.

While quantile regression analysis of these data sets is an interesting contribution to existing work, perhaps even more interest lies in estimates of persistence from countries that have yet to be explored. Since the collection of intergenerational panels is costly and time consuming progress based on direct observation of father-son pairs is slow. However, Björklund and Jäntti (1997) suggests an interesting path around this problem: two-sample instrumental variables (TSIV) techniques. Intuitively, TSIV is identical to the more commonly used (single-sample) instrumental variables estimator. If you wish to know the effect on  $y$  of a marginal change in  $x$  you might look directly at the relationship between the two variables. Alternatively, you may find a third variable  $z$  which directly affects  $x$ , but does not directly affect  $y$ . Then a comparison of  $cov(y, z)$  and  $cov(x, z)$  yields the effect of  $x$  on  $y$ . In this description of instrumental variables estimation there is no mention of whether  $x$  and  $y$  are in the same data set. While we often do observe both  $x$  and  $y$  together,  $cov(y, z)$  and  $cov(x, z)$  can be estimated in two different samples so long as  $z$  is in both.<sup>1</sup>

Björklund and Jäntti apply TSIV to repeated cross-section data in Sweden and the US. Since repeated cross-sections are far cheaper than panels, this a significant step forward. But in many countries only a single cross-section exists. I propose to divide single cross-sections into two samples based on the age of the respondent in order to form two cross-sections—one with "fathers" and one with "sons". After briefly describing the data, I will discuss several of the limitations of this approach and what biases will likely result.

The World Bank has initiated Living Standards Measurement Surveys in many developing

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<sup>1</sup>Two-sample estimation is discussed in detail in Angrist and Krueger (1992) and Arellano and Meghir (1992).

nations. In four of the surveyed countries (Ecuador, Nepal, Pakistan, and Peru), the data includes a report of father's education—the variable necessary to create the TSIV estimates. The cross-sectional data sets were collected in 1994, 1995, 1991, and 1985 respectively. Two samples were created with data from each country. In Sample I, I record respondent's income, age, and the highest level of education attained by the respondent's father ( $ed_f$ ) for respondents between the ages of 24 and 40. These are the 'sons' in the study. Sample II includes observations of respondent's income, age, and own education for respondents between the ages of 45 and 60. This sample selection (on age) is chosen to represent the fathers of the Sample I respondents.

Consistent with all of the analysis in this paper, log incomes in both samples are initially regressed on the respondent's age and age-squared to adjust for the mean effects of age. All subsequent analysis is performed on the residuals of these first stage regressions. The first step of TSIV is to estimate the coefficients from a regression of log income on own education using the Sample II (father generation) observations. With these estimates in hand, estimates of father's log income can be constructed in Sample I ( $\hat{x}_I$ ) using  $ed_f$  as a predictor. In Sample II, the predicted father's income ( $\hat{x}_{II}$ ) is also estimated. If the Sample I respondent's log income is denoted  $y$ , then

$$\hat{\beta}_{TSIV} = \frac{Cov(y, \hat{x}_I)}{Var(\hat{x}_{II})}.$$

Before proceeding, several concerns about  $\hat{\beta}_{TSIV}$  should be identified. First, the life cycle bias identified above suggests that  $\hat{\beta}_{TSIV}$  (as estimated in these samples) will understate the true rate of income persistence since the fathers are of necessity observed late in their life cycles. As a result, estimates using the World Bank data should be considered lower-bound estimates of the desired parameter.

A second concern is that, over time, the covariance of education and father's earnings may

have changed due to technological change or other shifts in the economy. If the data had come from developed countries, countries in which the returns to skill have recently exploded, this problem would almost certainly be crippling. Its effect on the World Bank data is less clear since these economies do not share the industrialized world's modern technology. It should be noted, however, that to the extent that the returns to education increased among the fathers in the last twenty years, this would produce a downward bias in  $\hat{\beta}_{TSIV}$  since  $cov(x, z)$  would be larger than the true value. Once more this suggests that my estimates should be viewed as lower bounds.

Finally, the two-sample method requires a valid instrument. Grawe (2001) and Lillard and Kilburn (1995) show that education is probably not exogenous and so *IV* estimates include a positive bias. (Solon (1992) presents this hypothesis without testing it.) This offsets some of the bias introduced by the previous two concerns. In total, it is not clear whether the bias will be positive or negative. But the magnitudes of the life cycle and endogeneity biases found in Grawe (2001) are roughly equal in the PSID data. This suggests that the results of the two-sample approach may be fairly close to the true slopes.

With  $\hat{\beta}_{TSIV}$  in hand, it is possible to estimate the quantile regression slopes again using the method described above. However, the fact that no *OLS* estimate of  $\beta$  is available raises one additional complication; an estimate of (income) measurement error variance is no longer directly available since no signal-to-noise ratio can be estimated.<sup>2</sup> Since any consistent estimate of the variance of measurement error in the income variable can be used, a simple solution is available. I estimate the effect of log income on housing consumption using both *OLS* and *IV*. The signal-to-noise ratio is then estimated from this set of regressions. All standard errors are

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<sup>2</sup>The model is under-identified without this information.

estimated using bootstrapping.

### **3 Section III: Results**

Before examining mobility in the United States relative to other countries, I study mobility in the US alone. The previous section identified several important issues of sample selection. Before moving to international comparison, I want to establish a base line using the "best" sample selection rules possible without regard for data limitations created when trying to match sample characteristics across countries. The remainder of this section reports comparisons of mobility between the US and other countries with careful attention paid to holding sample selection criteria constant. Throughout, I will report linear regression results. Grawe (2001) explores non-linear regression.

#### **3.1 The United States**

The principle data sets used to estimate intergenerational income persistence in America have been the PSID and the Original Cohort NLS. The advantage of the PSID is its completeness. With observations in each year from 1968 to 1993, it is possible to compare the US to almost any other country, year-for-year, matching each selection rule. But the small sample size of the PSID suggests that including an analysis of the NLS data would be profitable.

Table 2 reports estimates of income persistence in the PSID and NLS. A linear log-log relationship is assumed. In the PSID, father's real income is observed from 1967 to 1971; inclusion in the data set requires positive income in at least three of these five years. Son's real income is observed from 1990 to 1993; positive income is required in three of the four years. In the NLS, father's real income is observed in 1965, 1966, 1968, and 1970 while son's are observed

	<b>NLS</b>	<b>PSID</b>
mean	0.154 (0.045)	0.473 (0.062)
q-10	0.275 (0.130)	0.355 (0.169)
q-25	0.248 (0.080)	0.494 (0.099)
median	0.261 (0.030)	0.535 (0.068)
q-75	0.157 (0.051)	0.457 (0.079)
q-90	0.005 (0.052)	0.396 (0.219)
rank correlation	0.335 (0.062)	0.423 (0.054)
sample size	233	354

## 2. Regression in the United States.

in 1978, 1980, and 1981. Once again, three years of positive income are required for inclusion in the sample.

The first notable result is that the NLS produces much lower estimates of income persistence than the PSID. This may be surprising to those familiar with the results of Solon and Zimmerman in the June 1992 volume of the *American Economic Review*. In both studies income persistence estimates hover near 0.45. The reason that my NLS estimates (and those of Altonji and Dunn (1991)) are roughly half those of Zimmerman is that I do not restrict the sample to fully employed fathers and sons. (Zimmerman's definition of fully employed is working 30 weeks per year, 30 hours per week.) I also restrict the age of sons to those under the age of 19 in 1966 in order to avoid oversampling sons who live with parents after high school. (See Solon (1992) for a discussion of the negative impacts of such oversampling.) Couch and Lillard (1998) shows that excluding unemployed periods and respondents raises persistence estimates. Zimmerman's restriction to the fully employed can be expected to increase estimates of income persistence because it eliminates low income parents and children.

Roughly half of the difference between PSID and NLS estimates can be explained by the life cycle bias. The average age of fathers in the PSID (50.2) and NLS (40.2) differ by a decade. (The difference in average son age is less than 7 years and is not likely to make a difference given the

stability of persistence estimates at this point in the life cycle.) The examination of the life cycle bias in Grawe (2001) indicates that income persistence would fall by as much as 0.1 if fathers in the PSID were measured 10 years later. Nevertheless, life cycle differences cannot explain the whole gap between the two samples; the PSID sample exhibits substantially higher income persistence than the NLS sample.

Looking at the quantile regressions, a clear pattern emerges in the NLS data. Income persistence is far weaker at the top of the joint distribution than at the bottom. This is similar to the results found in Eide and Showalter (1999). But my own analysis of the PSID does not find a clear pattern. In total, the results from the NLS and PSID are in conflict: Mobility is substantially faster in the NLS. Grawe (2001) shows that these differences are reduced when non-linearities are allowed; in both samples lower quantiles are roughly linear while the upper quantiles are convex.

### **3.2 Canada**

Canada shares much of the New World flavor of America while retaining more of the Old World approach to social policy. While labor union participation has fallen in the United States, it has remained high in Canada. Canada has provided unemployment benefits for longer durations. In addition, child benefits are distributed more widely and more generously than in the United States. (See Card and Freeman (1994) for a lengthy discussion of the social policy differences between the countries during the time that the children in this study were growing up.) These differences may result in different rates of mobility in Canada and the United States.

The data from Canada are taken from the Intergenerational Income Data (IID) organized by Statistics Canada from Canadian Tax files. A more detailed description of the organization of the data is found in Corak and Heisz (1999). I average the incomes of fathers and sons over five

	<b>IID</b>	<b>NLS</b>	<b>PSID</b>
mean	0.152 (0.004)	0.196 (0.046)	0.381 (0.090)
q-05	0.261 (0.017)	0.300 (0.310)	0.151 (0.539)
q-10	0.256 (0.011)	0.324 (0.149)	-0.021 (0.236)
q-25	0.211 (0.007)	0.258 (0.070)	0.219 (0.088)
median	0.157 (0.004)	0.264 (0.036)	0.409 (0.107)
q-75	0.110 (0.004)	0.152 (0.048)	0.502 (0.100)
q-90	0.086 (0.005)	0.031 (0.092)	0.363 (0.199)
q-95	0.075 (0.007)	0.045 (0.060)	0.560 (0.372)
rank correlation	0.205 (0.005)	0.329 (0.068)	0.334 (0.058)
sample size	47115	267	161

### 3. Regression in the US and Canada.

years. Fathers and sons are observed in 1978-1982 and 1994-1998 respectively. The log of mean income is regressed on the age and age-squared of both father and son to control for the change in mean income over the life cycle.

Table 3 presents regression coefficient estimates in the IID, NLS, and PSID data sets. While the NLS data were collected almost a decade prior to the IID and PSID data collection, the span between father and child observation is very similar across the three data sets. And the age of the sons at the point of observation is also very similar. Unless income regression has dramatically changed between the late 1960's and the late 1970's, the comparison to the NLS should be useful. (Mayer (2001) finds that income persistence has fallen during the time period covered by the PSID. So the NLS estimates might be upward biased relative to the IID and PSID.)

Looking at the results in the IID, income regression in Canada is very swift—faster even than the summary 0.2 figure cited in the early literature. Second, the rate of regression among exceptional sons is even faster. The share of parental advantage passed on to children at the 90<sup>th</sup> or 95<sup>th</sup> percentile is less than 10 percent. Like the NLS, the rate of regression is faster as you move from the bottom to the top of the joint distribution.

The comparison between the United States and Canada is fairly difficult to summarize since

the patterns in the PSID and the NLS are entirely at odds. The NLS results are nearly equal to those from the IID; the PSID results show the US to be far less mobile. Looking at the NLS data, the slowest mobility rates are found at the bottom of the joint distribution; the PSID data point to immobility in the highest quantiles. In short, nothing conclusive can be offered by way of a comparison of linear income regression in the United States and Canada.<sup>3</sup>

### 3.3 Germany

The traditional foil to the open society of North America is the statist system of Europe. Years of monarchy form the foundation of a society in which title and pedigree matter far beyond the impacts of natural ability. Rigid social classes restrict the educational choices of the younger generation, effectively passing status from one generation to the next. Or so the story goes.

The two European countries for which data is readily available are Germany and the United Kingdom. (More data has been collected in the Scandinavian countries. But access has not been forthcoming despite my efforts.) These two countries serve well as representatives of the stereotypical Europe. The United Kingdom, for instance, still holds firm to a symbolic monarchy. If the prejudice of class or status is maintained by any country, it might be suspected to be held by a country that still recognizes transfer of title through birth. And in Germany, a highly tracked educational system "locks" children into an educational path long before the end of high school. This may diminish the relationship between innate ability and educational attainment and increase the correlation in education across generations. In this section and the next I study mobility in the

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<sup>3</sup>There is some concern about the accuracy of the initial PSID releases. However, if I exclude the preliminary release data, the panel after 1978 is very short. I have repeated the analysis using only the 1993 incomes for sons and found results that were, by and large, very similar to those found using the 1994-1996 data as well. The only notable difference was that, like the IID and the NLS, the lower quantile regressions were steeper than the upper quantile regressions.

	<b>NLS</b>	<b>PSID</b>	<b>GSOEP</b>
mean	0.188 (0.048)	0.140 (0.080)	0.095 (0.101)
q-10	0.346 (0.170)	0.333 (0.402)	-0.280 (0.550)
q-25	0.198 (0.070)	0.238 (0.112)	-0.042 (0.086)
median	0.211 (0.033)	0.158 (0.064)	0.065 (0.079)
q-75	0.165 (0.043)	0.223 (0.121)	0.171 (0.094)
q-90	0.131 (0.060)	0.280 (0.167)	0.313 (0.159)
rank correlation	0.335 (0.062)	0.249 (0.070)	0.105 (0.084)
sample size	350	177	142

#### 4. Regression in the US and Germany.

German Socio-economic Panel (GSOEP) Cross-national Equivalent File and the National Child Development Survey (NCDS).

The GSOEP was intended to mimic the PSID. Begun in 1984, the study initially included a representative sample of the Federal Republic of Germany (West Germany). When Germany was united in 1990, East Germans were added to the sample. My analysis will only use households in the original sample.

In the GSOEP, I define father's income as the average (positive) income reported from 1984 to 1988; son's income is defined as the average (positive) income observed from 1995 to 1998. Unlike the other samples, I do not restrict the sons to those younger than age 18 in the year fathers and sons are matched. With the small sample sizes in the GSOEP, adding this restriction results in samples too small to be of practical use.

Table 4 reports the regression coefficient estimates in the three samples. The rate of mean regression is very swift in the GSOEP data. This might simply reflect the fact that it is a very short panel requiring the fathers to be relatively older (mean age 47.5). The sample size is also relatively small making precise estimation impossible. The estimates of income persistence at all quantiles cannot be differentiated statistically from zero. But the point estimates point toward less mobility among exceptional sons than among sons at or below the mean.

Consistent with the findings in Couch and Dunn (1997), the rate of mean regression appears similar in the GSOEP and the US data sets; the slight difference cannot be differentiated statistically. But this similarity masks differences in the regression pattern across quantiles. Again, the NLS shows a neat pattern with more mobility among exceptional sons—exactly the opposite pattern found in the GSOEP—while the PSID shows no difference among exceptional sons and slower mobility for sons below the mean. Unfortunately, the small sample sizes makes it difficult to distinguish between even large variation in point estimates.

### **3.4 The United Kingdom**

The National Child Development Survey (NCDS) (Crown Copyright) is a survey of all children born during the second week of March, 1958. The NCDS includes five waves of surveys following the birth of the child including one during the subject's adolescence and two during the subject's adulthood. Since the subjects were only 23 at the point of their first adult observation, I only use the second (1991) income observation in my analysis. (Respondents were 33 years old in 1991.) When the age-23 incomes are averaged with the age-33 incomes, estimates of income persistence are much lower; this is almost certainly due to the fact that income at age 23 is a particularly poor measure of status.

The data from the US are again taken from the NLS and PSID. The reader should note that it is not possible with the NLS (as it is with the PSID) to match year-for-year the observation years in the NCDS. I use 1965 (rather than 1969) to observe fathers and 1981 (rather than 1991) to observe sons. I have selected the samples such that the ages of fathers and sons are similar across samples.

In Table 5 the linear regression results are presented. As Dearden et al. (1997) found, income

	<b>NLS</b>	<b>PSID</b>	<b>NCDS</b>
q-10	0.587 (0.258)	0.937 (0.247)	0.344 (0.197)
q-25	0.428 (0.181)	0.753 (0.199)	0.455 (0.125)
median/mean	0.251 (0.104)	0.549 (0.165)	0.579 (0.069)
q-75	0.075 (0.076)	0.344 (0.165)	0.703 (0.099)
q-90	-0.084 (0.126)	0.160 (0.194)	0.814 (0.167)
sample size	350	267	1945

### 5. Corrected regression in the US and the UK.

persistence appears to be very high in the United Kingdom when *IV* estimates are used. In fact, the elasticity estimate is more than double the *OLS* estimate. In the United States, the *IV* results mirror those found previously in Altonji and Dunn (1991) and Solon (1992). If the NLS data are studied, the US appears to be far more open than the UK at the mean; little difference at the mean can be found if the PSID data are used.

Despite this conflict between the PSID and the NLS at the mean, there is no conflict in the upper and lower quantile regressions. In both the NLS and the PSID, the opportunities to rise to the top are far more equal across income groups than in the UK. In fact, in the UK there is more persistence at the top of the distribution than at the bottom. Depending on how you look at this, this is either very disturbing or very encouraging for advocates of equal opportunity in the United Kingdom. It means that while the outcomes of exceptionally able children are not very dependent (if at all) on parent income in the US, there is a very strong dependence in the UK. Taken at face value, this looks like an important limitation on low-income families trying to rise to the top. However, the faster rate of regression in low quantiles suggests two more agreeable pictures. Either there is a higher safety net for those with low income or children from high-income families who fail have similar outcomes as failing children of middle- and low-income families.

### 3.5 Malaysia

Intergenerational data sets are obviously rare. And only one exists in an Eastern country.

Collected by the RAND Corporation in 1976-77 and 1988-89, the Malaysian Family Life Survey (MFLS) includes earnings data for fathers and sons. Unfortunately, the sample of almost 1300 households only includes one contemporaneous measure of father's income. The survey does ask the fathers to fill out a work history including recollections of past earnings. But it is unlikely that these memories truly constitute independent income observations with independent measurement errors. And so I choose to use only the contemporaneous income measure. Lillard and Kilburn (1995) provides a more detailed account of the retrospective earnings history. As in the case of the United Kingdom, I will present the estimates using *IV* corrections for the mean regression; quantiles are corrected following the method described the previous section.

The NLS data are again unavailable in the particular years observed in the MFLS. I choose to observe fathers in 1965 and sons in 1978 to approximate the time gap between observations in the MFLS. Sample selection based on age mitigates life cycle bias concerns.

Table 6 records the results. Using the Malaysian sample selection rules, no income persistence is evident in the NLS. In the PSID, the results are comparable to those previously reported. The Malaysian *IV* estimates exhibit far more persistence than their uncorrected counterparts. But the standard errors are large. Like the countries already studied, mobility appears fastest at the top of the joint income distribution.

The mean/median slope coefficient of 0.537 is roughly twice that found in Lillard and Kilburn (1985). Their estimate is based on the retrospective earnings history collected by RAND and does not use the *IV* approach used here. The authors justify their choice not to employ *IV* methods

	NLS	PSID	MFLS
q-10	-0.055 (0.291)	0.439 (0.171)	0.791 (0.356)
q-25	-0.042 (0.216)	0.395 (0.133)	0.671 (0.282)
median/mean	-0.028 (0.141)	0.345 (0.098)	0.537 (0.215)
q-75	-0.013 (0.094)	0.296 (0.083)	0.404 (0.177)
q-90	0.000 (0.112)	0.212 (0.094)	0.283 (0.187)
sample size	326	198	153

#### 6. Corrected regression in the US and Malaysia.

by noting that father's education is likely an endogenous variable. But their method requires us to believe that the retrospective earnings can be averaged to form a less error-ridden measure of income than the available single-year measure. Since the retrospective data are likely to be influenced by current earnings, I am not convinced that this is true. If the estimates reported by Lillard and Kilburn are correct, then the signal-to-noise ratio in the single-year income measure must be 0.75. I am not sure that I believe that data collected in a country with a substantial agrarian component could be so pure. I certainly acknowledge that the *IV* estimates are likely to overstate income persistence. But I believe that the *IV* estimates are likely to be more or less equally biased in Malaysia and the US.

While the sample sizes in this exercise were small (and the standard errors large as a result), several items are worth noting. When instrumental variables is used to correct for measurement error, Malaysia appears to be less open than the US. In addition, regression is especially slow in the lower quantile regressions. This pattern of faster regression at the top of the joint distribution is qualitatively similar to the results reported in the US, Canada, Germany, and the UK.

### 3.6 First Evidence from Other Nations

The previous sections have studied five countries that share two important characteristics. First, they are all arguably industrialized nations. (Malaysia might be considered an exception to

this rule.) Many sociologists studying occupational mobility have argued that few and small differences exist among mobility rates of industrialized nations. (See Lipset and Bendix (1967) for the most famous presentation of this thesis.) So the fact that *income* mobility rates in these five countries are difficult to distinguish might seem like old news to students of *occupational* mobility.

The second characteristic common to all five of these countries is that students of intergenerational mobility have previously studied each one using the same data sets utilized in this work. I still believe the contributions of the previous sections are important: The present study represents one of only a few direct cross-country comparison using similar sample selection rules.<sup>4</sup> And the addition of quantile regression suggests that income persistence is faster at the top of the joint distribution than at the bottom. Yet this small sample of six data sets drawn from five countries is obviously limiting.

Of course, the lack of data is a limitation faced by all economists interested in intergenerational income mobility. Absent additional support by governments through the provision of tax return or social insurance data, the creation of new intergenerational data sets will take decades and more money than most statistical bureaus are willing to commit. It seems that either we must wait a very long time for the data to materialize or second-best alternatives must be sought out. This section exploits such a second-best approach using TSIV techniques to produce mobility estimates in Ecuador, Nepal, Pakistan, and Peru using World Bank Living Standards Measurement Study data.

The definition of earned income is less straightforward in underdeveloped nations. In

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<sup>4</sup>Couch and Dunn (1997) compares the US to Germany using an earlier wave of the GSOEP. Björklund and Jäntti (1997) compares Sweden to the US.

	<b>Ecuador</b>	<b>Nepal</b>	<b>Pakistan</b>	<b>Peru</b>
q-10	1.062 (0.270)	0.653 (0.527)	-0.204 (0.447)	0.923 (0.226)
q-25	1.096 (0.268)	0.498 (0.355)	0.005 (0.346)	0.802 (0.194)
mean/median	1.134 (0.294)	0.325 (0.197)	0.236 (0.301)	0.667 (0.172)
q-75	1.172 (0.316)	0.153 (0.197)	0.467 (0.355)	0.532 (0.172)
q-90	1.207 (0.355)	-0.003 (0.336)	0.675 (0.461)	0.411 (0.191)
sample size	1461 685	229 239	171 441	98 166

### 7. Regression in the World Bank data.

particular, many workers are self-employed farmers or small business owners. This raises the likelihood that labor and asset income will be mixed together. In the World Bank surveys of Ecuador and Peru, all respondents were asked to identify total wage income. But in Nepal and Pakistan, small business owners and farmers were not asked this question. Instead, profits from the family business or farm were calculated and added to earnings.

Table 7 reports estimated slopes for linear mean and quantile regressions in the four countries using these definitions of income. (The first sample size refers to the number of observations in the sons sample; the second refers to the number of observations in the fathers sample.) In Ecuador and Peru, substantially higher estimates of income persistence result. Recalling that the life cycle bias and any skill-biased technical change both suggest that these are lower-bound estimates, these results point to far less mobility in the non-industrialized countries.

In fact, it is impossible to reject the hypothesis that income does not regress to the mean in Ecuador. If it is true that incomes do not regress to the mean, then the variance of log incomes would grow across generations. An examination of the age-variance pattern does not contradict the hypothesis that income variance is at least equal among fathers and sons.<sup>5</sup> This finding

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<sup>5</sup>This may seem to be in conflict with rising income variance over the life cycle that would be required for a life cycle bias. This conflict is only illusory. It is perfectly plausible that the variance in fathers' incomes grew over their life cycle and yet that the variance of sons' incomes in youth exceeded the income variance of their fathers in maturity.

	<b>Nepal</b>	<b>Pakistan</b>
q-10	0.539 (0.320)	0.773 (0.244)
q-25	0.490 (0.252)	0.626 (0.172)
mean/median	0.436 (0.193)	0.463 (0.121)
q-75	0.381 (0.171)	0.300 (0.134)
q-90	0.333 (0.194)	0.153 (0.195)
sample size	116 104	98 199

#### 8. Regression in Nepal and Pakistan, wage income only.

supports the possibility that income regressed very slowly in Ecuador. The pattern of slower mobility at the mean is also generally found in the quantile regressions. In Nepal and Peru the upper quantiles again exhibit faster mobility. But in Ecuador and Pakistan, mobility is faster (by point estimates) at the bottom of the joint distribution.

One possible explanation for the higher estimates of income persistence in the non-industrialized nations is that the data is of lower quality. In particular, it is difficult to differentiate capital income from labor income in agricultural societies. If capital is passed from parent to child in such a way that a strong intergenerational correlation exists in asset income, then this could explain the result. This explanation does not fit the data, however. First, in Ecuador and Peru, the questionnaire specifically requests only labor income. Admittedly, respondents may not be able to easily distinguish labor and capital income in some cases. But in both Pakistan and Nepal, wage income and self-employment income are recorded separately. Table 8 shows that, in both cases, restricting the data set to only utilize wage income (excluding business and farm income) results in higher estimates of income persistence.

If the results of this section are taken at face value, a first step in understanding the results is to determine whether the large slope coefficients result from high values of  $cov(y_s, ed_f)$ , low values of  $cov(y_f, ed_f)$ , or both. Since the reports of father's education are only categorical in Ecuador,

	Ecuador	Nepal		Pakistan		Peru	NLS
		<i>a</i>	<i>b</i>	<i>a</i>	<i>b</i>		
yrs. education	0.123 (0.009)	0.130 (0.018)	0.146 (0.019)	0.129 (0.043)	0.125 (0.030)	0.153 (0.026)	0.089 (0.010)
correlation	0.432	0.417	0.597	0.261	0.456	0.413	0.516

#### 9. Relationship between log earnings and years of school in father generation.

Nepal, and Pakistan, it is difficult to compare the value of  $cov(y_s, ed_f)$  in, say, the NLS sample to the same value in the World Bank data sets. However, in Peru the years of father's education were recorded. The correlation between son's log income and father's education is 0.420 in the Peruvian sample compared to a correlation of only 0.063 in the NLS when the average of son's income is used or 0.146 when only the son's 1981 income is used. As a result, much of the higher slope estimates in Peru can be explained by the strong relationship between parent education and child earnings.

The second covariance— $cov(y_f, ed_f)$ —can be estimated with all of the World Bank data sets since the "fathers" are simply older survey respondents. Table 9 presents the results from regressing log income (adjusted for age and age-squared) on education in the father's generation in each of the four World Bank Data sets and in the NLS. The correlation coefficient is also included. The columns labeled *b* use only wage earnings. While the return to a year of education is higher in the World Bank data set than in the NLS, the correlations between education and log earnings do tend to be slightly lower. In total, it appears that the reason for the low rates of income mobility in Ecuador and Peru in particular are derived from a stronger relationship between the education of the parent and the earnings of the child.

## 4 Section IV: Conclusion

Consistent with the few existing cross-country comparisons, I do not find systematic

	US		Canada	Germany	UK	Malaysia	Ecuador	Nepal	Pakistan	Peru
	NLS	PSID								
mean	0.154 (0.045)	0.473 (0.062)	0.152 (0.004)	0.095 (0.101)	0.579 (0.069)	0.537 (0.215)	1.134 (0.294)	0.325 (0.301)	0.236 (0.301)	0.667 (0.172)
q-90	0.005 (0.052)	0.396 (0.219)	0.086 (0.005)	0.313 (0.159)	0.814 (0.167)	0.283 (0.187)	1.207 (0.355)	-0.003 (0.336)	0.675 (0.461)	0.411 (0.191)

### 10. Summary of results.

differences between the Old and New World in the rates of mean regression. Table 10 summarizes the results. To the extent that comparisons can be made, it looks like Canada may be more mobile than the UK and Germany. No difference is discernible between the US and Germany, but the US is more mobile than the UK if the NLS data is to be believed—no less mobile if the PSID data are used.

The introduction of quantile regression adds important insights to the discussion. Western nations consistently exhibit less income persistence in upper quantiles than in lower quantiles. (This is notably untrue in the United Kingdom.) The outcomes of exceptional children are less dependent on parent status than the outcomes of children at or below the mean (conditional on their family background). The relatively common patterns shared by Western countries is perhaps surprising given the large differences in the educational systems in the studied countries. Germany's early tracking and the *laissez faire* approach of the US look very dissimilar but do not result in identifiable differences. Either this calls into question the model presented in Becker and Tomes (1979, 1986) or it underscores the fact that human capital comes in many forms.

The sample selection rules prove to be critical determinants of income persistence underscoring the need carefully apply common criteria to each data set when making international comparison. But the cost of this care is seen in the sample sizes. In the end, sample sizes are so small and the standard errors are so large that it is impossible to reject multiple hypotheses. Absent efforts like

those in Canada which produced the IID, this problem cannot be directly solved.

Even if we did have such large data sets for something like 5 or 10 countries, we would still be very limited in our ability to understand why one country is mobile and another static. With many possible explanations, 5 or 10 data points is insufficient to draw any firm conclusions. In an effort to begin to address this problem, this paper provides the first estimates of income persistence for four developing nations. The results indicate that while industrialized countries might be fairly similar, income regression in developing countries may be much slower. In Ecuador it is not even clear that incomes are regressing to the mean. Perhaps the distinction between the developed and the underdeveloped world is by far more important. Since it is possible to pursue this question with cross-section data sets, this looks to be a promising area for future progress.

With this information in hand, it is possible to hypothesize concerning the cause of the higher rates of income persistence. Clearly it will be impossible to do much at this point to test these hypotheses since I only have data on a handful of countries. These ideas represent starting points for future research rather than conclusions.

First, it may be that credit constraints are prevalent in these economies. While industrialized nations have already invested heavily in education in an effort to mitigate these constraints, they may yet exist in countries with less educational support. Simulations in Han and Mulligan (2001) suggest that credit constraints will not explain differences of the magnitude exhibited in Table 10. As more data from under-developed economies become available, formal testing of this hypothesis will be possible.

A variation on the credit constraint hypothesis is that race or class discrimination is important. This may in part result from regional differences in the availability of education. For instance,

discrimination against indigenous peoples in Ecuador and Peru is said to be widespread. Unfortunately, the World Bank surveys did not record race variables. If in the future, World Bank surveys do include this information then income persistence could be estimated both within and across groups to test this hypothesis. Alternatively, information on distance from the nearest school could proxy for availability of education. As more data sets become available, this hypothesis could also be tested. Quantile regressions would be important to differentiate the group-discrimination hypothesis from the credit-constraint hypothesis; while group discrimination would reduce the incomes of all children born to the group, credit market imperfections only affect the most able children since low-ability children will not require much education.

Finally, it may be that industrialization itself is part of the answer. In agrarian economies, economic status depends in large part on physical characteristics. It may be that these characteristics can be developed passively with little work on the part of the individual. To the extent that characteristics are inheritable, economic status would then tend to be rather persistent. In an industrialized economy, however, intellectual capacity is more dominant. Since thinking skills are largely learned, the intergenerational correlation of market abilities may be lower.

Obviously it is impossible to make conclusive statements based on a cross-country analysis of nine countries. These results may be unique to this particular, small sample of economies. Future work expanding our existing knowledge to even more countries will be required to study this more. Even if panel studies are deemed too costly and governments refuse to make official data available, cross-section surveys like the World Bank Living Standards Measurement Studies are a possible source of additional estimates. I look forward to this data becoming available and updating this work in the future.

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