Education and Labor Market Risk: Understanding the Role of Data Cleaning*

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Abstract

This paper examines whether conclusions about the relationship between education and labor market risk depend on the use of commonly applied procedures to clean data of extreme values. The analysis uses fifteen years of data from the Panel Study of Income Dynamics to demonstrate that conclusions about the relationship between education and labor market risk are sensitive to how extreme values of labor income are treated. The untrimmed estimates imply that college graduates experience 75% less transitory labor market risk than high school dropouts. However, applying commonly used trimming procedures results in estimates of a one standard deviation transitory labor market shock for high school dropouts being reduced by between $2,700 and $4,500, or 14% and 24% of annual earnings. The results demonstrate that seemingly innocuous sample selection procedures can have substantive implications.

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1 Introduction

Human capital is one of the central assets most people hold. A vast body of economic research has investigated the question of how education affects the level of income (see Card (2001) for a review). Recently, however, economists have also studied the effect of education on the riskiness of labor income, as evidence that markets for sharing idiosyncratic labor income risk are incomplete has accumulated (i.e. Cochrane (1991)). When markets are incomplete the relevant rate of return to education is risk-adjusted, rather than based on expected income alone.

Crucial for the estimation of the risk-adjusted rate of return to education is the responsiveness of labor income risk to education. While much progress has been made, the findings remain mixed. Hubbard, Skinner and Zeldes (1994b), Carroll and Samwick (1997), and Guvenen (2009) all find little statistically significant relationship between education and labor income volatility. In contrast, Meghir and Pistaferri (2004) find a statistically significant relationship between education and labor income volatility.[1]

One important way the labor income volatility studies differ is in terms of the data cleaning procedures applied to minimize the impact of measurement error. As seemingly innocuous data cleaning procedures can have substantive effects (Bollinger and Chandra (2005)) and different studies of labor income volatility adopt different data cleaning procedures, this paper considers whether commonly applied data cleaning procedures can explain the mixed findings. Whether data cleaning procedures could account for the mixed findings is unclear. As the studies differ in many ways, from the exact methodology applied to the measure of income and time period studied, the effects of data cleaning procedures alone are difficult to separate. Furthermore, as Bollinger and Chandra (2005) show the effects of non-linear data cleaning procedures are difficult to characterize ana-

[1]In contrast, recent applications of mean-variance models of asset pricing to human capital indicate that education is more valuable than the expected rate of return alone would indicate. Palacios-Hureta (2003) shows that risk adjusted rates of return to education are greater than the effect of education on expected income alone. Hartog and Vijverberg (2007) show that broadly based school curriculums do indeed reduce risk. In addition, earnings losses from job displacement have been shown to decline with education (see Stevens (1997), Farber (1997), and Farber (2005)).

lytically, leaving any effects of data cleaning to be a largely empirical question.[2]

Data cleaning procedures are motivated by the desire to reduce problematic measurement error so that unbiased estimates are obtained. Commonly used sample trimming procedures generally trim observations which fall below a threshold level of labor income (or wages and hours) or have very large changes in labor income. Trimming the sample of extreme values is clearly appropriate if very large deviations from average labor income are primarily due to measurement error. For example, observations of annual earnings where hourly wages are far below the minimum wage are a likely candidate for poorly measured observations. However, in many cases researchers have little knowledge of which observations are well or poorly measured. Imposing sample trimming rules that do not trim only poorly measured observations from the sample may well lead to biased estimates.

Some evidence on measurement error suggests that sample trimming could generate bias in labor income volatility estimates. Two central studies of measurement error in longitudinal measures of labor income by Bound and Krueger (1991) and Bound, Brown, Duncan and Rodgers (1994) find that measurement error in labor income is mean-reverting. When measurement error is mean-reverting, trimming the sample of extreme values is likely to understate labor income volatility. Of course, as we lack detailed information on many other relevant aspects of measurement error, further investigation is warranted.

The central analysis in this paper examines whether, and how, the relationship between education and labor income volatility depends on the use of sample trimming procedures. The analysis uses 16 years of data from the Panel Study of Income Dynamics (PSID) and a simple variance decomposition procedure based on Carroll and Samwick (1997) to estimate labor income volatility. Three sets of sample trimming rules are examined. First examined are two sets of systematic trimming rules which simply trim the sample of extreme values based on different percentile of the labor income distribution (i.e. observations outside of the 99th and 1st percentiles are dropped from the sample). These two sets differ in terms

[2]Recent work by Jensen and Shore (2008) studies a related point. They find significant evidence that the increase in average volatility over recent decades that previous work has documented is driven largely by an increase in volatility for those with extremely high levels of volatility.

of how observations are selected for trimming. The first set trims annual observations if the trimming rule is violated, whereas the second set trims individuals who ever have annual earnings outside of the threshold from the sample. This analysis allows for a more general understanding of the effect of sample trimming rules before turning to the analysis of commonly applied sample trimming rules. The analysis of commonly applied trimming procedures compares conclusions about the education-labor income volatility relationship from the untrimmed estimates to those under the various sample trimming procedures. This analysis explicitly considers the effect of trimming the sample on estimates of labor income volatility.

The results reveal that sample trimming procedures significantly alter conclusions about the relationship between education and labor income risk. When the sample is not trimmed of extreme values transitory labor income volatility falls with education. Indeed, the untrimmed estimates imply that college graduates experience 75% less transitory labor income volatility than high school dropouts. The untrimmed estimates also reveal a relationship between education and permanent labor income risk.

The first comparison examines how the untrimmed results compare to those obtained under the two sets of systematic trimming rules. The results indicate that the application of many of the systematic trimming rules alters conclusions about the relationship between education and labor income volatility. For example, all of the estimates with trimming rules that drop individuals who ever have measured income outside of the trimming range show little relationship between education and labor income volatility. Similarly the estimates obtained under the more restrictive individual trimming rules show little relationship between education and labor income volatility.

The fact that alternative sample trimming procedures generate similar results is informative about whether outlier observations or outlier individuals are driving the results. As Jensen and Shore (2008) show in the context of rising income volatility, there is an important distinction between outlier individuals and outlier observations. In their framework, outlier individuals are those who experience far more than the typical level of volatility. In contrast, outlier observations are observations that are far from the level of labor income experienced by a typical individual. As different commonly used trimming rules trim different types of outliers, the results indicate that the distinction between outlier individuals and observations is not particularly important in this context.

The next comparison examines how the untrimmed results compare to those obtained under the trimming rules commonly applied in the literature. Again the results indicate that the application of the many commonly applied trimming rules substantially weakens the relationship between labor income volatility and education. In fact, only the application of Δ Log (Annual Earnings) trimming rule results in a relationship between labor income volatility and education. Interestingly, this is the procedure applied in the Meghir and Pistaferri (2004) study that finds a significant relationship between labor income volatility and education. Trimming the sample results in estimates of a one standard deviation transitory shock being understated by between \$2,700 and \$4,500 or 14% and 24% of annual earnings for high school dropouts. I show these findings are not sensitive to the definition of labor income and predictable labor income profile specification used. Thus, the results indicate that some of the mixed nature of the results in prior work can be explained by how different studies clean the data.

The most closely related literature to this paper examines the relationship between education and estimated labor income volatility as noted above. Other closely related work considers the role of measurement error in earnings dynamics.[3]

The paper proceeds as follows. The next section presents a discussion of measurement error in labor income and the effects of sample trimming. The third section presents the econometric approach. The data are discussed in the fourth section. Section five presents and discusses the results. The final section concludes.

[3]Pischke (1995) explicitly examines how incorporating measurement error affects estimates of labor income volatility. He uses data that have both a survey based measure and a non-survey based measure so that he can study the role of measurement error in estimates of labor income volatility. Kim and Solon (2005) find that the existence of mean-reverting measurement error implies that real wages may be more pro-cyclical than previous work indicated. Gottschalk and Huynh (2006) examine whether measurement error leads previous estimates of mobility and earnings inequality to be overstated. These studies do not examine the effect of sample trimming on estimates of labor market dynamics or the relationship between education and labor income volatility. Saks and Shore (2005) study the related question of the effect of labor income risk on career choice.

2 Measurement Error and Sample Trimming

This section first presents three commonly discussed measurement error processes. Second, evidence on the nature of measurement error in labor income are discussed. Third, the effects of sample trimming on labor income variance is discussed.

2.1 Labor Income Measurement Error Processes

White Noise. The first process considered is additive white noise. Additive white noise is often termed 'classical' measurement error, and serves as a useful starting point to understand the justification for sample trimming. Consider the case where the true value of labor income (y_i^*) and measurement error (η) generate the measured value of labor income (y_i). It is well known that additive white noise measurement error, with $y_i = y_i^* + \eta$ and $\mathrm{E}(\eta) = 0$, leads the measured variance to be greater than the variance in the actual level of labor income. Additive white noise measurement error however provides little justification for trimming the sample to reduce measurement error bias in estimates of labor income volatility. The reason is that while observations outside some acceptable region may be measured with error, observations within the acceptable region are also measured with error. In the case of white noise measurement error, sample trimming will reduce the measured volatility of labor income, but not necessarily reduce any bias arising from measurement error. Thus, the existence of white noise measurement error provides little justification for sample trimming.

Linear Measurement Error. A second process commonly considered is linear measurement error. In this case, $y_i = \delta + \gamma y_i^* + \eta$ where δ and γ are the parameters of the linear projection of y_i on y_i^*. In this case, it is possible that observations outside of some threshold are measured more or less poorly that those inside the threshold. Whether sample trimming can reduce bias labor income volatility estimates depends on the parameters δ and γ. Fortunately, we have some evidence on the nature of linear measurement error in survey reports of labor income. The best available evidence indicates that $\gamma < 1$ and $\delta > 0$ (see Bollinger (1998) and Bound and Krueger (1991)). This structure of measurement error is consistent with regression to the mean, as people report labor income values that they feel reflect an average value. Those below the mean are more likely to report higher values and those above the mean are likely to report lower values. In this case, reported labor income will display less variance than actual labor income suggesting that trimming the sample of extreme values will lead the researcher to underestimate labor income variance.

Contaminated Data. The last measurement error process discussed here is that of contaminated data. Simply put, the idea is that the distribution of measured labor income is a mixture of both the distribution of the true value of labor income and the distribution of measurement error. For each observation there is some probability we observe the true value of labor income and some probability that we observe only noise. In this case, if the contaminated (i.e. pure noise) observations can be identified bias in labor income volatility estimates can be reduced. Contaminated data seems to be the measurement error process many researchers have in mind. For example, if annual earnings observations implying a wage below the minimum wage are contaminated data, then eliminating them from the sample may well reduce the bias in labor income variance arising from measurement error. Thus, the existence of contaminated data does provide justification for sample trimming if the contaminated data can be identified by the researcher.

2.2 Evidence on Measurement Error in Labor Income

Much progress has been made in understanding the nature of measurement error in survey data.[4] Most relevant for the analysis here is the evidence on the extent of measurement error in labor income in Bound and Krueger (1991) and Bound, Brown, Duncan, and Rodgers (1994). In these papers the authors examine longitudinal earnings data where they have access to both survey reports and either employer or administrative reports of earnings. This allows for a unique opportunity to measure and quantify the nature of measurement error in survey based measures of labor income in a longitudinal setting. Bound and Krueger (1991) study a sample from the Current Population Survey (CPS) matched to social security earnings records in 1976 and 1977. They find that about 75%

[4]See Bound, Brown, and Mathiowetz (2001) for an excellent survey on the nature and consequences of measurement error in survey data.

of the variation in changes in measured earnings represent changes in actual earnings. Bound, Brown, Duncan, and Rodgers (1994) study a sample from the PSID which is matched to earnings records from one manufacturing plant in 1983 and followed up four years later. They find that about 78% of the variation in changes in earnings represent changes in actual earnings when outliers are included in the sample.

Recent work has also demonstrated that measurement error is often mean-reverting. Bound and Krueger (1991) and Bollinger (1998) find a point estimate for γ in the linear measurement model above of about 0.9. Indeed, mean-reverting measurement error appears to be a very general feature of survey data. Bollinger and Chandra (2005) state that "To our knowledge no study has ever found any variable with $\gamma > 1$ [measurement error positively correlated with the true value]" (Bollinger and Chandra (2005), pp. 241.)

Less is known about the relationship between education and measurement error in labor income, and the results are somewhat mixed. Bound and Krueger (1991) find little correlation between measurement error in labor income and education. Bound, Brown, Duncan, and Rodgers (1994) also find that measurement error is not significantly different across education groups. In recent work, Cristia and Schwabish (2007) find that the extent of measurement error in labor income is positively related to education using matched SIPP-SSA data. Thus, while it is possible that some differences in the variance of labor income across educational groups could reflect differences in measurement error the evidence does not unambiguously point in that direction.

2.3 Sample Trimming and Labor Income Variance Estimates

It is clear that trimming the sample of extreme values will reduce the variance of measured labor income, however the sign or magnitude of any *bias* in estimated labor income attributable to sample trimming is difficult to determine analytically without knowledge of the measurement error process (Bollinger and Chandra (2005)). Thus, obtaining the optimal trimming rule for data generated under commonly assumed measurement error processes is generally infeasible.

We can get a sense of the difficulty of obtaining a clear analytic expression for the general case by considering the more specific case where y_i is normally distributed. Consider a symmetric trimming rule where the researcher drops all values of y_i above a known constant, C, or below another known constant, c. While for most survey data C and c are unknown, a researcher may use a sensible trimming rule based on knowledge of the minimum wage for example. In the case where y_i is normally distributed the variance of y_i in the trimmed sample can be expressed as,

$$V(y|c \leq y_i < C) =$$

$$V(y)\left[1+\left[\frac{\frac{[c-E(y_i)]}{V(y_i)}\phi[\frac{c-E(y_i)}{V(y_i)}]-\frac{[C-E(y_i)]}{V(y_i)}\phi[\frac{C-E(y_i)}{V(y_i)}]}{\Phi[\frac{C-E(y_i)}{V(y_i)}]-\Phi[\frac{c-E(y_i)}{V(y_i)}]}\right]-\left[\frac{\phi[\frac{c-E(y_i)}{V(y_i)}]-\phi[\frac{C-E(y_i)}{V(y_i)}]}{\Phi[\frac{C-E(y_i)}{V(y_i)}]-\Phi[\frac{c-E(y_i)}{V(y_i)}]}\right]^2\right]$$

where Φ and ϕ are the cdf and pdf's of the distribution of y_i respectively (Bollinger and Chandra (2005)).

The expression for $V(y|c \leq y_i < C)$ demonstrates that any bias in the trimmed variance (i.e. $V(y|c \leq y_i < C) - V(y)$) depends on the first and second moments of the distribution in highly non-linear fashion.[5] As many researchers trim extreme observations to eliminate corrupted data, trimming is largely motivated by measurement error that is not drawn from a normal distribution. Thus, the special case of doubly truncated normal distribution is unlikely to be applicable. Because of the difficulties of specifying the optimal trimming rule under general assumptions, the approach taken in this paper is to instead examine whether commonly applied trimming rules affect labor income volatility estimates and play a role in explaining the mixed findings of the prior literature.

3 Econometric Approach

In this section the method used to estimate permanent and transitory labor income volatility is outlined. Because transitory shocks are generally easier for households to insure,

[5] It is important to note that this expression reflects a special case under distributional assumptions that may not be applicable to the data generating process to be studied. For example, income and any non-classical measurement error in labor income may not be normally distributed. I thank a referee for pointing out the importance of the assumption for the expression.

understanding which sources of labor income volatility are related to education has important implications for the welfare effects of market incompleteness. The decomposition of labor income variance into transitory and permanent components is based on the Carroll and Samwick (1997) procedure.[6] The essence of the variance decomposition approach is to first remove the predictable component of labor income growth. The resulting unpredictable component of earnings growth is then decomposed into permanent and transitory components.

The stochastic labor income process is described in the following way. The logarithm of permanent income for individual i at time t, y_{it}^{P}, is assumed to follow a random walk with drift so that,

$$y_{it}^{P} = g_{it} + y_{it-1}^{P} + \eta_{it}. \tag{1}$$

Predictable income growth is denoted by g_{it}, which is income growth related to the lifecycle and aggregate productivity growth, and η_{it} is the shock to permanent income in period t. The predictable income profile, g_{it}, is specified as including age, education (3 dummy variables), race (1 dummy variable), marital status (2 dummy variables), household size, 1 digit occupation dummies, 1 digit industry dummies, year dummies, and all variables interacted with age (except year dummies).[7] The log of current income y_{it}^{C} is given by the log of permanent income, and an additive transitory shock term ε_{it},

$$y_{it}^{C} = y_{it}^{P} + \varepsilon_{it}. \tag{2}$$

A crucial assumption here is that the errors ε_{it}, and η_{it} are uncorrelated with each other at all leads and lags. This assumption allows the variance of labor income to be decomposed into transitory and permanent components. To complete this decomposition I estimate and remove the predictable component of income growth g_{it} using a sample-wide OLS regression and rewrite (1) as,

$$y_{it}^{P} = y_{it-1}^{P} + \eta_{it}. \tag{3}$$

Define the d-year income difference as,

$$\begin{aligned} \Delta_{idt} &= y_{it+d}^{C} - y_{it}^{C} \\ &= y_{it+d}^{P} + \varepsilon_{it+d} - y_{it}^{P} - \varepsilon_{it}. \end{aligned} \tag{4}$$

[6] My exposition of this method draws heavily from Carroll and Samwick (1997).

[7] This is the specification used in Carroll and Samwick (1997) with race and education variables added.

The second equality comes from substituting (2) into the first equality. Substituting (3) into (4) recursively yields,

$$\Delta_{idt} = [\eta_{it+1} + \eta_{it+2} + \eta_{it+3} + ... + \eta_{it+d}] + \varepsilon_{it+d} - \varepsilon_{it}. \tag{5}$$

Taking the second moment of the right hand side of equation (5) I obtain

$$var(\Delta_{idt}) = d\sigma^2_{\eta i} + 2(\sigma^2_{\varepsilon i}), \tag{6}$$

Where $\sigma^2_{\eta i}$, and $\sigma^2_{\varepsilon i}$ are the variances of the permanent and transitory shocks to log income for individual i, respectively.

We can identify $\sigma^2_{\eta i}$ and $\sigma^2_{\varepsilon i}$ in equation (6) for any two values of d. The analysis again follows Carroll and Samwick (1997) in implementing the variance decomposition. The analysis proceeds in three steps. First, the predictable component of the labor income profile is removed from the differences in earnings. As transitory earnings shocks are allowed to follow an MA(2) process this means that the $d < 3$ differences are excluded from the decomposition.[8] Second, after taking out the predictable component of earnings growth, thirteen $var(\Delta_{dit})$'s are obtained for each i (with d taking on values 3 to 14 since there are fourteen potential differences). Lastly, to decompose the variance into transitory and permanent components we estimate an ordinary least squares model for each i where $var(\Delta_{dit})$ is regressed on d and a vector of thirteen 2's.[9] The coefficients on these regressors are the estimates of $\hat{\sigma^2_{\eta i}}$ and $\hat{\sigma^2_{\varepsilon i}}$ respectively. These are the permanent variance and the transitory variance components of the labor income process for each individual i.

The Carroll and Samwick (1997) approach implicitly assumes that deviations from the predictable permanent income profile reflect shocks to earnings and are not driven by individual labor market decisions. This assumption would be violated if individuals make labor market decisions based on expected shocks to earnings. For example, if an individual were to change occupations in response to a shock to labor income, the assumption would

[8] A number of other researchers (MaCurdy (1982), Abowd and Card (1989), and Meghir and Pistaferri (2004)) have also found transitory shocks to labor income to follow a second order moving average process.

[9] As the sample restriction that the panel is balanced is not imposed, some individuals have less than sixteen years of data. For these individuals, the available data is used to conduct the variance decomposition.

not hold. While it is important to recognize this concern in specifying the predictable component of earnings growth, a rich predictable income profile approach is used in the baseline analysis to be consistent with prior work. However, whether the results are robust to estimating the variances without a predictable income profile is examined in further analysis.[10]

Table 1 displays the different sample trimming rules to be used in the analysis. The first two sets of rules exclude extreme observations based on whether they are in the top or bottom tails of the earnings distribution. The first two rules differ in terms of whether they exclude only the annual observations outside of the threshold, or instead exclude individuals from the sample who ever have an earnings observation outside of the threshold. The general percentile rules examined are intended to provide context for the more specific trimming rules that have been used in the literature. They allow for a more complete understanding of whether conflicting results among commonly applied trimming procedures are due to systematic or idiosyncratic features of the alternative approaches.

In terms of the specific rules used in the literature, the wage and hour trimming procedure is applied in Guvenen (2007, 2009). The Δ log(annual earnings) trimming procedure is applied in Meghir and Pistaferri (2004), the annual earnings trimming rule one is applied in Carroll and Samwick (1997) and the annual earnings trimming rule two is applied in Hubbard, Skinner and Zeldes (1994a,b).

There are two characteristics of the commonly applied sample trimming procedures in Table 1 that are worth noting. First, the rules differ in terms of whether the data for an individual who ever satisfies a trimming rule is dropped or whether only those observations which trigger the trimming rule are dropped. In contrast to other sample trimming rules, the wage and hour trimming rule only trims out the outlying observations. Those individuals who experience an outlying observation remain in the sample in other years. Second, the trimming rules in Table 1 also differ in terms of whether they apply

[10] As pointed out by a referee one concern with a specification without a predictable income at all profile is that some variables included in the baseline specification are clearly not endogenous (i.e. age). To address this concern I have also estimated models in an (unreported) analysis where the forecastable component of labor income only contains a third order polynomial in age and a set of year dummies. The results of this analysis are very similar to those reported in the tables leaving conclusions about the role of sample trimming in determining the relationship between education and labor income risk unchanged.

symmetrically to high and low extreme values. The two annual earnings based trimming rules are asymmetric, dropping only observations with values below a threshold. The other two trimming rules however are more symmetric, dropping observations above a high threshold and below a low threshold.

4 Data

This study is conducted using 15 years (from 1979 to 1993) of data from the Panel Study of Income Dynamics (PSID).[11] The central estimates are based on examining the male household head's annual labor earnings alone, as defined by the PSID. Education is measured using categorical variables that divide individuals into one of four groups: high school dropout, high school graduate, some college attendance, and college graduate. The precise details of the key variable definitions are given in the Data Appendix. The summary statistics and observation counts by year are given in Table A1.

The sample exclusion rules imposed are the following.[12] Female household heads are dropped since gender differences in labor supply behavior are beyond the scope of this paper. Those who are not between 22 and 60 years of age in every year are dropped, so that the entire sample contains prime working age males for the length of the panel. I drop those who are not household heads as many of the variables of interest are only available for household heads. Those with missing values for education or race and those with less than nine years of non-zero labor income are dropped.[13] The final sample consists of 1,403 individuals.

It is important to note that the sample selection criteria do not include reported labor force status (i.e. employment), since being in a state of non-employment is likely to represent a significant source of labor income volatility, and may differ by education. The

[11] I use these years of data because of the major change in the data cleaning procedures that the PSID implemented in the 1994 survey year. Note that I index my sample years by the survey year, but by the year which the labor income report corresponds to, which is one year prior to the survey year.

[12] The exact number of observations lost due to each sample restriction is in Table A2.

[13] I restrict the sample to include only those with at least nine years of data following Carroll and Samwick (1997). I relax this assumption for a sensitivity analysis.

few observations with zero labor income for a full year are dropped from the analysis as I use the logarithm of income for my variance decomposition. Of course, as most labor force participation shocks (which do not result in the receipt of any transfer income) for prime working age males are shorter than a year, this restriction results in the loss of very few observations.

4.1 Sample Trimming and Educational Composition

I examine whether sample trimming procedures affect the educational composition of the sample is examined in Figures 1A, 1B, and 1C. Figure 1A shows that the less restrictive (i.e. one and two percentile) year trimming rules do not appear to affect the educational composition of the sample. In contrast, the more restrictive five percentile and ten percentile trimming rules do differentially trim by skill level. In Figure 1B we can see that the individual trimming rules have a similar effect on the sample regardless of how strict they are. Interestingly, this set of rules appears to differentially trim the less skilled from the sample in contrast to the set of rules applied in Figure 1B. Finally, Figure 1C presents the educational composition of the sample under the commonly applied trimming rules. The commonly applied trimming rules have quite similar effects on the sample composition. Figure 1C indicates that most trimming rules are more likely to drop the least skilled from the sample.

Taken together the results in Figures 1 suggest that sample trimming rules often differentially drop the least educated from the sample. Estimates of labor income volatility are next examined to see if sample trimming procedures have important effects on estimates of labor income volatility as the figures suggest.[14]

[14] One important question in understanding the potential effects of different trimming rules is whether they trim the same observations. A simple calculation suggests that commonly applied trimming rules do indeed trim different observations. Of the 895 individuals in the sample 300 are trimmed by at least one trimming rule in Table 4, but only 7 are trimmed by all trimming rules in Table 4.

5 Results

This section presents estimates of transitory and permanent labor income volatility for annual earnings using the decomposition methodology and sample trimming rules noted above. First, estimates using the percentile yearly observation based trimming rules are presented. Estimates using the individual based percentile trimming rules are then discussed. Lastly, estimates using the commonly applied sample trimming rules are discussed.

5.1 Percentile Year Trimming Procedure Estimates

In Table 2 estimates of transitory and permanent labor income volatility by education for alternative year trimming rule samples are presented. Each column of the table contains a set of labor income volatility estimates, with the top panel containing the transitory labor income volatility estimates and the bottom panel containing the permanent labor income volatility estimates. F test statistics for the hypothesis that labor income volatility differs across education groups are presented at the bottom of each panel. Each column presents results for a different sample with a different year based percentile trimming rule applied. These rules trim annual earnings observations that violate the trimming rule.

The results in Table 2 demonstrate that the relationship between education and labor income risk does depend on the stringency of the trimming rule. The first finding to note in Table 2 is that the untrimmed estimates in column one reveal a statistically significant relationship between education and transitory labor income risk. College graduates face 75% less transitory labor income volatility than high school dropouts. Moreover, the differences in the untrimmed transitory labor income volatility across educational groups are statistically significantly different at the 5 percent level. In the second and third columns we see that the results are very similar to those for the untrimmed estimates, so that less restrictive trimming rules have little effect on the relationship between education and transitory labor income volatility.

A different picture emerges in the fourth and fifth columns of Table 2 where more restrictive trimming rules are applied. The estimates in these columns indicate a much weaker relationship between transitory labor income volatility and education. In column five, the differences across education groups are no longer statistically significant.

The central differences between the transitory labor income volatility results in the fourth and fifth column of Table 2 and those in the first is that the magnitude of the estimates for the less skilled are much lower. For example, the estimated transitory labor income risk for high school drop-outs in the fifth column is only about 30 percent of that in the first column, while the college graduate transitory labor income volatility estimates are very similar across the columns. As the differences in transitory labor income risk across education categories seem to hinge on the magnitude of the estimate for the low skilled, the differing results are not likely due to a loss of precision from a smaller sample.

The second set of results to note in Table 2 are for permanent labor income risk in the second panel of the table. In a similar fashion to the transitory labor income volatility estimates, the relationship between education and labor income risk depends on how restrictive the trimming rule is. The untrimmed estimates in the first column show a statistically significant relationship between education and transitory labor income risk. A similar relationship is found with the less restrictive rule samples in columns two and three. Similar to the transitory labor income risk estimates, the relationship is far weaker with the more restrictive trimming rules applied in columns four and five.

5.2 Percentile Individual Trimming Procedure Estimates

In Table 3, I report estimates of transitory and permanent labor income volatility by education for alternative individual trimming rules. Each column presents results for a different sample with alternative individual based percentile trimming procedures applied. These rules trim all observations for an individual that ever has an annual earnings observation that violates the trimming rule.

Similar to Table 2, the results in Table 3 demonstrate that the relationship between education and labor income risk does depend on the application of a trimming procedure. The first column of Table 3 again presents the untrimmed estimates for reference.

Interestingly, the results in the top panel show that even with a less stringent trimming rule applied in the second and third columns, the relationship between education and transitory labor income risk is substantially weaker. The results in column 2 of Table 3 imply that college graduates face only 25% less transitory labor income volatility than high school dropouts. Moreover, the differences in the untrimmed transitory labor income volatility estimates across educational groups are not statistically significantly different at the 5 percent level. This pattern of different estimates for the untrimmed and trimmed samples remains in columns three to five. The differences between Table 2 and Table 3 suggest that the distinction between trimming individuals versus only the extreme annual observations has substantive implications for less restrictive trimming rules.

The second set of results to note in Table 3 are for permanent labor income risk in the second panel of the table. In a similar fashion to the transitory labor income volatility estimates, the relationship between education and labor income risk depends on the application of a trimming rule. The untrimmed estimates in the first column show a statistically significant relationship between education and transitory labor income risk. However, even when a less restrictive trimming rule is applied the relationship between permanent labor income risk and education is significantly weaker.

5.3 Common Trimming Procedure Estimates

In Table 4 I report estimates of transitory and permanent labor income volatility by education estimated applying commonly used trimming rules to obtain the sample. The goal of this table is to understand whether differences in how recent studies treat extreme values can account for differences in conclusions about the relationship between education and transitory labor income risk. Of the variance decomposition studies noted above only Meghir and Pistaferri (2004) find significant differences in transitory labor income risk by education. As Meghir and Pistaferri (2004) apply the Δ Log (Annual Earnings) trimming rule, the results in Table 4 allow us to see whether differences in how extreme values are treated can account for any of the mixed findings of the literature.

The results in the top panel of Table 4 reveal that imposing different trimming rules does indeed affect conclusions about the relationship between education and labor income risk. Particularly striking is that only the results using the Δ Log (Annual Earnings) trimming rule in column three demonstrate a statistically significant differences in labor income risk by education. As this is the rule utilized by Meghir and Pistaferri (2004), the results of the top panel of Table 4 indicate that differences in how extreme values are treated can explain some of the mixed findings in prior work. Interestingly, the estimates in column two do not simply replicate the magnitudes of the untrimmed estimates in column one. Instead, the estimates in column three are substantially smaller than those in the first column, but the education gradient is preserved. College graduates face about 75% less transitory labor income volatility than high school dropouts in both columns one and three. The estimates of transitory labor income risk in columns two, three and five show less relationship with education. Like the other trimming rules above, this lack of relationship appears to be due to the trimmed sample estimates of transitory labor income risk for the least skilled are particularly low.

Table 4 also presents results on how conclusions about the relationship between education and permanent labor income volatility depend on sample trimming. Interestingly, both the Wage and Hour trimming rule and the Δ Log (Annual Earnings) trimming rule lead to a statistically significant relationship between education and labor income risk. Again the estimates using these trimming rules lead to a relationship between permanent labor income risk and education that is similar to the untrimmed sample. The permanent labor income results using the two annual earning trimming rules in the last two columns reveal little significant relationship between education and permanent labor income risk. The result that permanent labor income volatility generally increases (though not monotonically) with education is also obtained by Meghir and Pistaferri (2004).

As only the Δ Log (Annual Earnings) trimming rule yields similar results to the untrimmed sample it is worth considering how this trimming rule differs from the others. As noted in Table 1, the Δ Log (Annual Earnings) trimming rule trims individuals from the sample who ever have observations outside of the threshold region. We can get a sense of whether this difference accounts for the differing results by comparing the results in column 3 of Table 4 to those from the systematic individual trimming rules in Table 3. The comparison reveals that the sample sizes and estimates obtained using the Δ Log (Annual Earnings) trimming rule do not precisely match any of the columns in Table 3. It seems that the Δ Log (Annual Earnings) results are not due to the trimming rule mimicking the sample selected by a systematic trimming rule yielding similar results to the untrimmed sample. Instead the comparison suggests that the similarity of the untrimmed and Δ Log (Annual Earnings) estimates are due to idiosyncratic features of the trimming procedure.

5.4 Trimming Effect Magnitude and Comparisons to Previous Estimates

Are the differing estimates in Table 4 economically significant? To examine this, I express the estimates in Table 4 in terms of the effect of shocks to the level of annual earnings in Table 5. The first column of Table 5 contains mean annual earnings (in 1993$) by education. These serve as a baseline level of annual earnings. The second through fifth columns contain the magnitude of a one standard deviation negative shock (based on the estimates in Table 4) to the level of annual earnings by education and trimming rule.

We can see in Table 5 that trimming the sample has an economically important effect on the size of a one standard deviation negative transitory shock. For high school dropouts sample trimming reduces the impact of the shock by between $2,700 and $4,500. This reduction in earnings volatility corresponds to between 14% and 24% of annual earnings. Thus, trimming has a substantively important effect on the magnitude of annual earnings volatility for the least skilled.

In Table A3 I display the labor income volatility estimates from previous studies. While the many differences between the present analysis and that in previous literature mean that the estimates are not strictly comparable, it is still worthwhile to consider their similarities. In general, my trimmed sample estimates are quite close to those of the authors who use the trimming rule in their estimates of idiosyncratic labor income volatility. Of course, as there are numerous differences in precise methodology and sample between the prior work and the analysis here, it is hardly surprising that my estimates are not identical.[15]

[15] The estimates for transitory labor income volatility in Meghir and Pistaferri (2004) are 0.055, 0.027, 0.005 for high school dropouts, high school graduates and college graduates respectively. My estimates are slightly higher, though quite close in magnitude. The transitory labor income volatility estimates

5.5 Sensitivity Analysis

This subsection considers how sensitive the conclusions on the importance of sample trimming are on a few dimensions. First, whether the results are sensitive to the precise measure of annual income used is examined. The results thus far examine only the labor income volatility measured by the household head's annual earnings. I now look at whether the results in Table 2 are robust to using other measures of labor income from the previous literature.[16] Both total household labor income and the head's annual earnings plus transfer income are considered in this section. These measures correspond to slightly different conceptions of what constitutes labor income uncertainty.

Household labor income volatility may well be lower than individual labor income volatility.[17] Within-household labor income insurance can be provided by labor supply responses of other household members to labor market shocks experienced by the household head. Household labor supply responses to shocks to the head's labor income would likely differ across educational groups. Those with very low levels of education are more likely to be credit constrained making the value of self-insurance larger. Thus, we would

in Guvenen (2009) (with the restricted income profile) are 0.052 and 0.047 for high school and college graduates respectively. These are both smaller than my comparable estimates in Table 4 column two. The estimates in Hubbard, Skinner and Zeldes (1994b) and Carroll and Samwick (1997) are based on household income (and also include transfer income). The point estimates in Hubbard, Skinner and Zeldes (1994b) of the variance of transitory labor income volatility are 0.040, 0.021 and 0.014 for high school dropouts, high school graduates and college graduates respectively. In Carroll and Samwick (1997) the point estimates for transitory labor income volatility are roughly 0.078, 0.043, 0.034, and 0.048 for high school dropouts. Because Carroll and Samwick (1997) disaggregate educational categories further that I do I have roughly averaged their estimates to aggregate education categories into comparable categories. My comparable estimates for transitory labor income volatility in Table 4 are all larger than those in Hubbard, Skinner and Zeldes (1994b) and those in Carroll and Samwick (1997), but quite close in magnitude. My estimates of permanent labor income volatility at lower levels of education are quite different than in either study, but at higher levels of education my estimates are very similar in magnitude. An important source of the differences is likely that Carroll and Samwick (1997) and Hubbard, Skinner and Zeldes (1994b) also include social insurance income which reduces transitory labor income volatility.

[16] I apply the trimming rules in Table 1 to the labor income definition that I consider. This results in the slightly different sample sizes across the estimates which define labor income differently.

[17] Recent work by Shore (2008) indicates that marriage undoes most if not all of the increased riskiness that comes to individuals in recessions

expect within-household labor supply responses to reduce the volatility of household labor income particularly for less educated individuals. In addition, both Hubbard, Skinner, and Zeldes (1994b) and Carroll and Samwick (1997) conduct their analysis with household labor income.

Also examined is whether including social insurance income affects the central findings in the previous tables.[18] To do so I return to examining only household head income so that the results are comparable to the baseline results in Table 2. The types of social insurance and transfer income I include are unemployment insurance, workers compensation payments, AFDC, food stamp and SSI income. Social insurance income would be expected to reduce labor income volatility particularly for the less educated as many social insurance programs are highly progressive. Dynarski and Gruber (1997) find that social insurance and other transfer income play a significantly more prominent role in insuring the head's labor income than with-in household labor supply responses.

Lastly, whether the results are sensitive to the use of a specific labor income profile or other sample restrictions is examined. Whether the results are sensitive to these specifcation and sample decisions is well worth examining for a number of reasons. The variance decomposition procedure I use implicitly assumes that there are forecastable and unforecastable innovations to labor income. The idea of the income profile is to effectively remove the predictable component of labor income shocks, say because of differences in the age profile by education, leaving only the unpredictable components behind. However, as individuals have more information than the econometrician it is possible that changes in the variables in the predictable labor income profile reflect individual choices in response to information about future unobserved labor income shocks. This would be the case if individuals switched industries in response to information about the possibility of future job loss. To address this important concern I also estimate the models above without a predictable labor income profile, so that endogenous changes in time varying

[18] Bound, Brown and Mathiowetz (2001) note that less is known about the measurement error process in transfer income than labor income. However, the fact that measurement error in social insurance income is not mean zero suggests that there is substantial measurement error in survey based measures of social insurance income. Thus, there is likely to be more measurement error in labor income plus transfers income than in labor income alone which suggests that estimates based on both sources of income to likely overstate the degree of uncertainty faced by individuals.

individual characteristics are not driving the results.

Also estimated are the models above without imposing the sample restriction that an individual reports labor income for at least nine years. Again, this assumption is imposed to be consistent with prior work, but is subject to debate and worthy of investigation. To better understand how this sample restriction affects the results models without restricting the sample to only individuals who have a given minimum number of observations are estimated.

The results of the sensitivity analysis are reported in Tables 6, 7, and 8. To conserve space I do not report each estimate of labor income volatility for each separate analysis. Instead, the tables report just the F-Statistics for the test of equality of the estimates across education groups. I again divide the tables into two panels, with the top panel for transitory labor income volatility and the bottom panel for permanent labor income volatility. The first row reports the relevant F-Statistics from the baseline results above for reference purposes. The second row of each panel reports the test results for labor income risk estimated using the identical specification to those above with household rather than individual earnings. Similarly, the third row of each panel reports the test results for labor income risk estimated using the identical specification to those above with individual labor and transfer income. The fourth row of each panel presents test results for estimate of labor income using models with no predictable labor income profile. Finally, the last row of each panel estimates identical models to those above, but without imposing the sample restriction of at least nine years of labor income reported.

Results examining the sensitivity of the above results for the annual trimming rules are presented in Table 6. In general, the results reported in Table 6 support conclusions that the relationship between education and labor income risk depend on whether and how the sample is trimmed. The one clear exception is that permanent labor income risk is not significantly related to education regardless of whether the sample is trimmed if the sample restriction of at least nine years of labor income reported is not imposed. The inclusion of transfer income does also serve to weaken the relationship between transitory earnings volatility and education.

Similarly, the results for the individual trimming rules estimates in Table 7 reveal similar patterns to those reported above. The results in Table 7 show that when the sample is untrimmed education does have a statistically significant relationship with labor income risk, with the possible exception of the transfer and labor income specification.

In Table 8 the results of the sensitivity analysis for the commonly applied trimming rules are presented. While the precise pattern of transitory risk results differ from those above, the application of Δ Log (Annual Earnings) trimming rule again leads to results most similar to untrimmed estimates. However, the results for permanent labor income risk vary significantly across the various sensitivity analyses. The results of this table lend further credence to the notion that differences in how the sample is trimmed can account for some of the mixed findings for the relationship between education and transitory labor income risk. However, they also demonstrate that choices about sample trimming are not likely to be central to explaining differences in the role of education in permanent labor income risk across studies.

5.6 Decomposition Weighted Estimates

In this last section, the effects of an alternative decomposition methodology are examined. As the Carroll and Samwick (1997) decomposition methodology treats all individuals the same, the efficiency of the decomposition approached may be enhanced by weighting the resulting decomposition estimates by their standard errors.[19] We might expect that individuals with less years of labor income available would have less precisely estimated transitory and permanent labor income volatility under Carroll and Samwick (1997) variance decomposition approach. In this case if we weight by the standard error of the estimated permanent and transitory labor income risk estimates, the efficiency of the variance decomposition approach in equation (6) may be enhanced.

The results of the decomposition weighted estimation procedure are presented in Tables 9, 10, and 11. The first finding to note from these tables is that the decomposition weighting does substantially improve the efficiency of the variance decomposition estimates. Interestingly, the results in Tables 9 and 10 demonstrate that the increase in

[19] I thank an anonymous referee for suggesting this alternative decomposition weighted approach.

precision due to the weighting procedure leads a significant education-earnings risk gradient regardless of the sample trimming rule imposed. These results suggest that concerns about the effects of simple systematic sample trimming rules can be effectively addressed through improving the efficiency of the variance decomposition methodology.

The results in Table 11 tell a somewhat different story. While the decomposition weighting methodology improves the precision of all of the samples, the education-transitory labor income risk gradient is only marginally significant for the two annual earnings trimming rule samples in columns four and five. This suggests that improving the efficiency of the decomposition methodology may alleviate concerns about the effects of sample trimming for rules which trim both upper and lower values. However, for sample trimming rules that are less closely related to the systematic symmetric trimming rules in Tables 9 and 10 the effects of sample trimming may not be accounted for in a straightforward manner.

6 Conclusion

Previous estimates of idiosyncratic labor income volatility have shown, at best, a mixed relationship with education. This paper shows that that many commonly used sample trimming procedures understate labor income volatility for the least educated. Furthermore, the results indicate that the mixed findings in prior studies may be accounted for by differences in how extreme values are treated. The only trimming procedure that results in an education - labor income risk relationship was used in the study that finds such a relationship.

The results show that when the sample is not trimmed of extreme values, transitory labor income volatility falls with education. The untrimmed estimates imply that college graduates experience 75% less transitory labor income volatility than high school dropouts. In contrast, when the sample is trimmed, no statistically significant relationship between education and transitory labor income volatility is found for most commonly applied trimming rules.

Commonly applied trimming procedures have a particularly large effect on the magnitude of the estimates of transitory labor income volatility for the less educated. For high school dropouts, trimming the sample reduces the impact of a one standard deviation transitory shock to annual earnings by between \$2,700 and \$4,500 or 14% and 24% of annual earnings. Some evidence that sample trimming weakens the relationship between education and permanent labor income volatility is also found, though the results are less robust.

Interestingly, the effects of sample trimming on the education - labor income risk relationship also depend on the method used to estimate labor income volatility. For symmetric sample trimming procedures the education - labor income risk relationship depends little on the trimming rule imposed when a decomposition weighted approach is used. However, under commonly applied asymmetric sample trimming procedures sample trimming affects conclusions about the education - labor income risk relationship even when a decomposition weighted approach is used.

The results of this paper suggest a few directions for further work. One direction would be to examine how data cleaning procedures affect estimates of labor income volatility in other contexts. In particular, the role of sample trimming procedures in estimates of the effect of business cycles (Storesletten, Telmer, and Yaron (2004)) on labor income volatility is worthy of further study. Another direction would be to improve our understanding of the nature of corrupted data with matched survey-administrative data so that sample trimming procedures could be appropriately redesigned. More broadly, the results of this paper indicate that seemingly innocuous sample selection decisions can have substantive implications.

7 Data Appendix

7.1 Variable Definitions

Head's Annual Labor Income: This measure includes the household head's wage or salary, bonuses, overtime, commissions, professional practice or trade income, and the labor part of farm, business, market gardening, and roomers and borders income. This is variable V17534 in 1989. Top-coded values are recoded as 1.5 times the top code income cutoff. It is deflated to 1993$ using the consumption GDP price deflator.

Head's Annual Transfer Income: This measure includes the head's unemployment compensation (variable V16456 in 1989), workers compensation (variable V16457 in 1989), AFDC (variable V16443 in 1989), food stamp (variable V16395 in 1989) and Supplemental Security Income (variable V16446 in 1989) income. It is deflated to 1993$ using the consumption GDP price deflator.

Household Annual Income: The values of this variable represent the sum of the Head's Annual Labor Income variable and the total wage and labor income of the wife (variable V16420 in 1989).

Head's Annual Hours Worked: The values of this measure represent the Head's total annual work hours (variable V16335 in 1989).

Head's Average Hourly Earnings: The values for this variable represent the Head's average hourly earnings in dollars and cents per hour. The formula used for this variable's generation is, Labor Income of Head (V17534 in 1989)/Hours of Work of Head (V16335 in 1989). This is variable V17536 in 1989. It is deflated to 1993$ using the consumption GDP price deflator.

Head's Job Displacement: This variable measures whether a head was employed in the previous year, is now unemployed, and involuntarily lost job due (1) to the company folding, (2) a strike or lockout, or (3) the head was laid off or fired. This is the measure utilized in Cochrane (1991). The measure is based on unemployed head's responses to a questions about what happened to their previous position (variable V16843 in 1989) and the Head's employment status in current and previous years (V16655 in 1989).

Head's Education Level: This variable measures the educational attainment of the head at the time that they first became a head (subsequent years of data carry this earlier response forward). This is variable v17545 in 1989. The educational categorical variables utilized in the analysis are the following: No High School = grades 0-5; grades 6-8; grades 9-11 High School Graduate = 12 grades (completed HS); 12 grades + academic training Some College = College, no degree College Graduate = College, Bachelor's degree; College, advanced or professional degree, some graduate work, close to receiving degree

Head's Age: This variable represents the actual age of the 1989 Head. It is variable V16631 in 1989.

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FIGURE 1A: Percent of Untrimmed Sample Size, By Education and Year PercentileTrim Sample

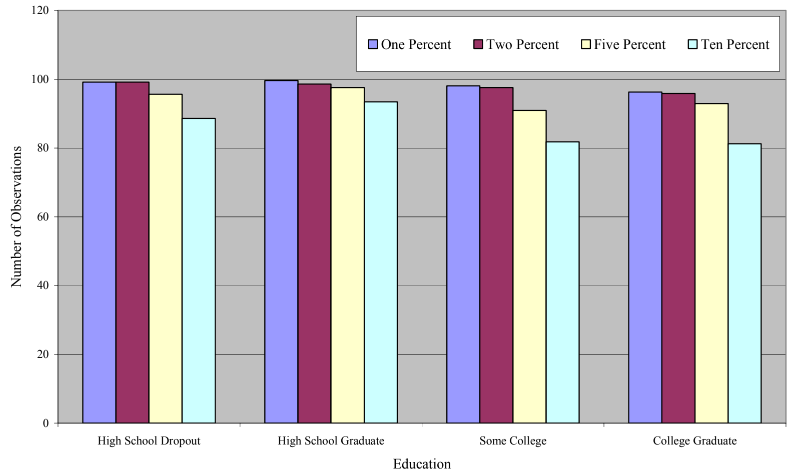


FIGURE 1B: Percent of Untrimmed Sample Size, By Education and Individual Percentile Trim Sample

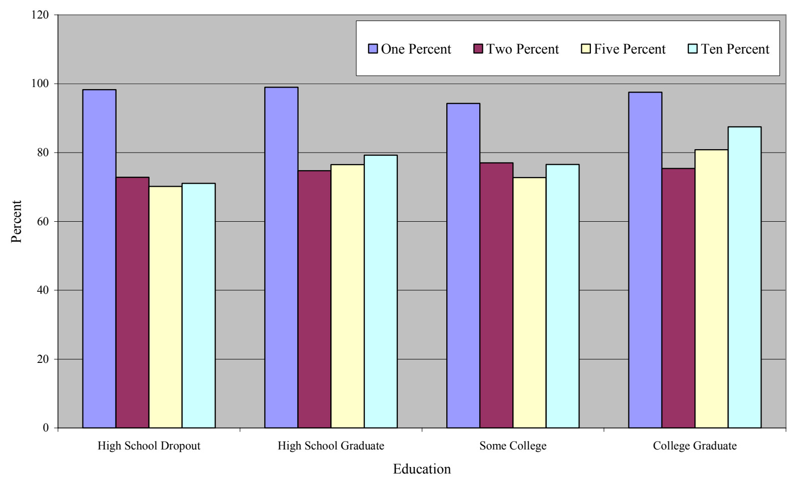
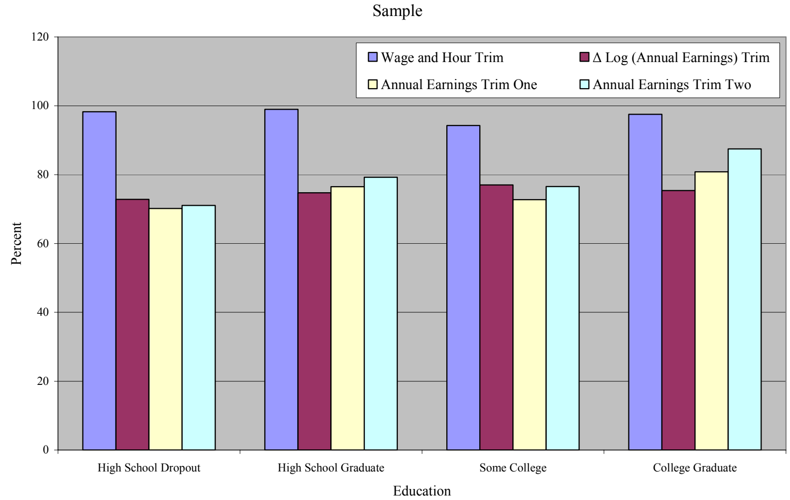


FIGURE 1C: Percent of Untrimmed Sample Size, By Education and Common Trimming Rule Sample



Notes: Source authors' calculations using PSID data. Each column is the number of individual observations in educational group.

TABLE 1: Trimming Rules

Name	Definition	Observations Trimmed
Year Percentile Trim	Exclude those observations with real annual earnings more than the top percentile or less than the bottom percentile listed.	Trim year observation only
Individual Percentile Trim	Exclude those observations with real annual earnings more than the top percentile or less than the bottom percentile listed.	Trim all observations for individual
Wage and Hours Trim	Exclude those observations with hourly labor earnings less than W_{min} and more than W_{max} , where W_{min} is \$2 and W_{max} is \$400 in 1993, and are adjusted for previous years using the personal consumption GDP deflator.	Trim year observation only
Δ Log (Annual Earnings) Trim	Exclude those observations with a change in log annual earnings greater than 5 or less than -1.	Trim all observations for individual
Annual Earnings Trim One	Exclude those observations with annual earnings in any year less than 20 percent of its average over the sample period.	Trim all observations for individual
Annual Earnings Trim Two	Exclude those observations with annual earnings are less than \$3000.	Trim all observations for individual

Notes: The Wage and Hours Trim rule is used in Guvenen (2005). The Δ Log (Annual Earnings) Trim rule used in Meghir and Pistaferri (2004), the Annual Earnings Trim One is used in Carroll and Samwick (1997) and the Annual Earnings Trim Two is used in Hubbard, Skinner and Zeldes (1994b).

TABLE 2: Transitory and Permanent Labor Market Risk, by Education and Year Percentile Trim Sample

Dependent Variable = Log (Annual Earnings)

	No Trimming	Top and Bottom 1%	Top and Bottom 2%	Top and Bottom 5%	Top and Bottom 10%
A. Transitory Labor Income Risk:					
High School Dropout	0.265	0.231	0.230	0.128	0.082
	(0.061)	(0.053)	(0.053)	(0.019)	(0.017)
	n=124	n=123	n=123	n=118	n=108
High School Graduate	0.207	0.200	0.201	0.089	0.062
	(0.033)	(0.033)	(0.041)	(0.012)	(0.010)
	n=311	n=310	n=307	n=305	n=283
Some College	0.050	0.048	0.043	0.080	0.061
	(0.042)	(0.042)	(0.041)	(0.014)	(0.009)
	n=216	n=212	n=211	n=194	n=172
College Graduate	0.064	0.087	0.058	0.060	0.048
	(0.024)	(0.027)	(0.026)	(0.014)	(0.010)
	n=244	n=234	n=229	n=215	n=179
F-Test: Education Differences	6.98	5.04	6.37	2.95	1.12
	[0.0001]	[0.0018]	[0.0003]	[0.0320]	[0.3409]
B. Permanent Labor Income Risk:					
High School Dropout	0.004	0.009	0.010	-0.001	-0.004
	(0.012)	(0.013)	(0.013)	(0.005)	(0.006)
	n=124	n=123	n=123	n=118	n=108
High School Graduate	0.010	0.012	0.010	0.014	0.004
	(0.006)	(0.007)	(0.007)	(0.004)	(0.004)
	n=311	n=310	n=307	n=305	n=283
Some College	0.083	0.081	0.083	0.019	0.011
	(0.021)	(0.023)	(0.022)	(0.005)	(0.003)
	n=216	n=212	n=211	n=194	n=172
College Graduate	0.041	0.024	0.036	0.019	0.013
	(0.009)	(0.011)	(0.011)	(0.005)	(0.004)
	n=244	n=234	n=229	n=215	n=179
F-Test: Education Differences	8.00	5.65	6.20	2.62	2.08
	[0.0000]	[0.0008]	[0.0004]	[0.0500]	[0.1012]

Notes: Source: Authors' calculations using the Panel Study of Income Dynamic data from 1979-1993. Entries in the table are the mean estimated labor market risk for the education * sample subgroup. Entries in parentheses are standard errors and entries beginning with n= are the number of individual observation in the education * sample subgroup. Entries in the 5th and 10th row are F-statistics for the hypothesis test that the differences in mean labor market risk between all educational subgroups within a sample are zero. The p-values for this hypothesis test as in square brackets.

TABLE 3: Transitory and Permanent Labor Market Risk, by Education and Individual Percentile Trim Sample

Dependent Variable = Log (Annual Earnings)

	No Trimming	Top and Bottom 1%	Top and Bottom 2%	Top and Bottom 5%	Top and Bottom 10%
	A. Transitory Labor Income Risk:				
High School Dropout	0.265	0.081	0.080	0.079	0.043
	(0.061)	(0.022)	(0.021)	(0.014)	(0.010)
	n=124	n=93	n=93	n=85	n=56
High School Graduate	0.207	0.136	0.136	0.070	0.040
	(0.033)	(0.021)	(0.021)	(0.011)	(0.008)
	n=311	n=263	n=261	n=215	n=157
Some College	0.050	0.082	0.084	0.059	0.051
	(0.042)	(0.031)	(0.032)	(0.013)	(0.010)
	n=216	n=172	n=165	n=129	n=79
College Graduate	0.064	0.061	0.048	0.033	0.024
	(0.024)	(0.023)	(0.023)	(0.013)	(0.007)
	n=244	n=202	n=165	n=115	n=61
F-Test: Education Differences	6.98	2.05	2.47	2.02	1.13
	[0.0001]	[0.1049]	[0.0607]	[0.1095]	[0.3361]
	B. Permanent Labor Income Risk:				
High School Dropout	0.004	0.012	0.012	0.003	0.000
	(0.012)	(0.007)	(0.007)	(0.003)	(0.003)
	n=124	n=93	n=93	n=85	n=56
High School Graduate	0.010	0.011	0.010	0.007	0.005
	(0.006)	(0.006)	(0.005)	(0.003)	(0.003)
	n=311	n=263	n=261	n=215	n=157
Some College	0.083	0.033	0.034	0.013	0.006
	(0.021)	(0.009)	(0.010)	(0.004)	(0.003)
	n=216	n=172	n=165	n=129	n=79
College Graduate	0.041	0.030	0.027	0.017	0.007
	(0.009)	(0.009)	(0.008)	(0.005)	(0.002)
	n=244	n=202	n=165	n=115	n=61
F-Test: Education Differences	8.00	2.21	2.54	2.29	0.55
	[0.0000]	[0.0854]	[0.0551]	[0.0779]	[0.6514]

Notes: Source: Authors' calculations using the Panel Study of Income Dynamic data from 1979-1993. Entries in the table are the mean estimated labor market risk for the education * sample subgroup. Entries in parentheses are standard errors and entries beginning with n= are the number of individual observation in the education * sample subgroup. Entries in the 5th and 10th row are F-statistics for the hypothesis test that the differences in mean labor market risk between all educational subgroups within a sample are zero. The p-values for this hypothesis test as in square brackets.

TABLE 4: Transitory and Permanent Labor Market Risk, by Education and Common Trim Rule Sample

Dependent Variable = Log (Annual Earnings)

	No Trimming	Wage and Hour Trim	Δ Log (Annual Earnings) Trim	Annual Earnings Trim One	Annual Earnings Trim Two
A. Transitory Labor Income Risk:					
High School	0.265	0.134	0.093	0.072	0.075
Dropout	(0.061)	(0.021)	(0.027)	(0.015)	(0.014)
	n=124	n=120	n=90	n=87	n=88
High School	0.207	0.110	0.056	0.074	0.077
Graduate	(0.033)	(0.015)	(0.016)	(0.012)	(0.012)
	n=311	n=308	n=229	n=238	n=247
Some	0.050	0.084	-0.007	0.061	0.082
College	(0.042)	(0.023)	(0.029)	(0.013)	(0.016)
	n=216	n=204	n=165	n=157	n=165
College	0.064	0.074	0.018	0.050	0.073
Graduate	(0.024)	(0.014)	(0.010)	(0.010)	(0.014)
	n=244	n=238	n=185	n=198	n=214
F-Test:					
Education	6.98	1.80	3.65	0.91	0.06
Differences	[0.0001]	[0.1460]	[0.0012]	[0.4340]	[0.9804]
B. Permanent Labor Income Risk:					
High School	0.004	-0.001	0.002	0.003	0.004
Dropout	(0.012)	(0.005)	(0.005)	(0.003)	(0.003)
	n=124	n=120	n=90	n=87	n=88
High School	0.010	0.011	0.011	0.007	0.012
Graduate	(0.006)	(0.005)	(0.003)	(0.003)	(0.004)
	n=311	n=308	n=229	n=238	n=247
Some	0.083	0.037	0.047	0.013	0.013
College	(0.021)	(0.016)	(0.013)	(0.004)	(0.004)
	n=216	n=204	n=165	n=157	n=165
College	0.041	0.022	0.020	0.015	0.017
Graduate	(0.009)	(0.005)	(0.004)	(0.003)	(0.004)
	n=244	n=238	n=185	n=198	n=269
F-Test:					
Education	8.00	2.64	6.60	2.11	1.24
Differences	[0.0000]	[0.0483]	[0.0002]	[0.0973]	[0.2937]

Notes: Source: Authors' calculations using the Panel Study of Income Dynamic data from 1979-1993. Entries in the table are the mean estimated labor market risk for the education * sample subgroup. Entries in parentheses are standard errors and entries beginning with n= are the number of individual observation in the education * sample subgroup. Entries in the 5^{th} and 10^{th} row are F-statistics for the hypothesis test that the differences in mean labor market risk between all educational subgroups within a sample are zero. The p-values for this hypothesis test as in square brackets.

TABLE 5: Implications of Transitory Labor Market Risk Estimates for Volatility in Levels of Labor Income, by Education and Common Trim Rule Sample

Predictable Income Profile Specification = Carroll and Samwick (1997)
Dependent Variable = Log (Annual Earnings)

		Impact of a One Standard Deviation Negative Transitory Shock on Mean Annual Earnings (1993$)				
	Mean Labor Income (1993$)	No Trimming	Wage and Hour Trim	Δ Log (Annual Earnings) Trim	Annual Earnings Trim One	Annual Earnings Trim Two
High School Dropout	18,398	9,471	6,734	5,611	4,937	5,038
High School Graduate	25,336	11,527	8,403	5,996	6,892	7,030
Some College	28,001	6,261	8,115	7,408	6,916	8,018
College Graduate	40,538	10,255	11,028	5,438	9,064	10,952
College Graduate – High School Dropout difference in one S.D. negative shock as a percentage of mean labor income	--	25%	9%	17%	4%	0%

Notes: Source: Authors' calculations using the transitory labor market risk estimates in Table 4.

TABLE 6: Test Statistics for Transitory and Permanent Labor Market Risk Education Differences, by Year Percentile Trim Sample: Sensitivity Analysis

Dependent Variable = Log (Annual Earnings) or as indicated

Sensitivity Analysis:	No Trimming	Top and Bottom 1%	Top and Bottom 2%	Top and Bottom 5%	Top and Bottom 10%
		A. Transitory Labor Income Risk: F-Test: Education Differences			
Baseline	6.98 [0.0001]	5.04 [0.0018]	6.37 [0.0003]	2.95 [0.0320]	1.12 [0.3409]
Household Earnings	5.41 [0.0011]	5.71 [0.0007]	4.45 [0.0041]	2.51 [0.0575]	1.01 [0.3892]
Earnings + Transfers	2.16 [0.0917]	1.03 [0.3806]	2.97 [0.0311]	1.79 [0.1474]	0.31 [0.8181]
No Income Profile	7.33 [0.0001]	5.47 [0.0010]	6.72 [0.0002]	3.18 [0.0234]	1.35 [0.2563]
Unrestricted Panel Sample	4.51 [0.0038]	1.48 [0.2196]	1.64 [0.1780]	0.87 [0.4542]	1.43 [0.2322]
		B. Permanent Labor Income Risk: F-Test: Education Differences			
Baseline	8.00 [0.0000]	5.65 [0.0008]	6.20 [0.0004]	2.62 [0.0500]	2.08 [0.1012]
Household Earnings	5.41 [0.0011]	5.35 [0.0012]	3.03 [0.0097]	3.09 [0.0265]	0.96 [0.4125]
Earnings + Transfers	3.61 [0.0129]	2.42 [0.0651]	2.10 [0.0992]	1.80 [0.1462]	0.88 [0.4521]
No Income Profile	7.50 [0.0001]	5.62 [0.0008]	6.26 [0.0003]	1.90 [0.1276]	3.02 [0.0290]
Unrestricted Panel Sample	1.12 [0.3408]	0.29 [0.8320]	0.24 [0.8718]	0.32 [0.8085]	0.75 [0.5251]

Notes: Source: Authors' calculations using the Panel Study of Income Dynamic data from 1979-1993. Entries in the table F-Statistics for the hypothesis test that estimated labor market risk differs by education group, with p-values in square brackets. See the text for precise details of each sensitivity analysis.

TABLE 7: Test Statistics for Transitory and Permanent Labor Market Risk Education Differences, by Individual Percentile Trim Sample: Sensitivity Analysis

Dependent Variable = Log (Annual Earnings) or as indicated

Sensitivity Analysis:	No Trimming	Top and Bottom 1%	Top and Bottom 2%	Top and Bottom 5%	Top and Bottom 10%
			A. Transitory Labor Income Risk: F-Test: Education Differences		
Baseline	6.98 [0.0001]	2.05 [0.1049]	2.47 [0.0607]	2.02 [0.1095]	1.13 [0.3361]
Household Earnings	5.41 [0.0011]	3.54 [0.0144]	3.69 [0.0118]	3.62 [0.0131]	0.056 [0.0422]
Earnings + Transfers	2.16 [0.0917]	0.41 [0.7434]	1.45 [0.2267]	2.82 [0.0385]	1.17 [0.3191]
No Income Profile	7.33 [0.0001]	2.26 [0.0798]	2.73 [0.0428]	3.11 [0.0262]	1.72 [0.1633]
Unrestricted Panel Sample	4.51 [0.0038]	2.04 [0.1052]	2.01 [0.1105]	2.13 [0.0953]	0.49 [0.6900]
			B. Permanent Labor Income Risk: F-Test: Education Differences		
Baseline	8.00 [0.0000]	2.21 [0.0854]	2.54 [0.0551]	2.29 [0.0779]	0.55 [0.6514]
Household Earnings	5.41 [0.0011]	2.82 [0.0382]	3.70 [0.0116]	3.01 [0.0299]	1.12 [0.3400]
Earnings + Transfers	3.61 [0.0129]	0.98 [0.4006]	1.76 [0.1543]	2.85 [0.0368]	0.50 [0.6810]
No Income Profile	7.50 [0.0001]	2.31 [0.0751]	2.65 [0.0476]	2.86 [0.0362]	1.05 [0.3707]
Unrestricted Panel Sample	1.12 [0.3408]	1.07 [0.3618]	0.92 [0.4310]	0.80 [0.4920]	0.14 [0.9334]

Notes: Source: Authors' calculations using the Panel Study of Income Dynamic data from 1979-1993. Entries in the table F-Statistics for the hypothesis test that estimated labor market risk differs by education group, with p-values in square brackets. See the text for precise details of each sensitivity analysis.

TABLE 8: Test Statistics for Transitory and Permanent Labor Market Risk Education Differences, by Common Trim Rule Sample: Sensitivity Analysis

Dependent Variable = Log (Annual Earnings) or as indicated

Sensitivity Analysis:	No Trimming	Wage and Hour Trim	Δ Log (Annual Earnings) Trim	Annual Earnings Trim One	Annual Earnings Trim Two
			A. Transitory Labor Income Risk: F-Test: Education Differences		
Baseline	6.98 [0.0001]	1.80 [0.1460]	3.65 [0.0012]	0.91 [0.4340]	0.06 [0.9804]
Household Earnings	5.41 [0.0011]	1.96 [0.1184]	2.01 [0.1112]	0.89 [0.4466]	1.76 [0.1541]
Earnings + Transfers	2.16 [0.0917]	0.97 [0.4079]	3.15 [0.0245]	0.87 [0.4537]	0.12 [0.9511]
No Income Profile	7.33 [0.0001]	1.87 [0.1337]	3.75 [0.0108]	1.64 [0.1782]	0.044 [0.7211]
Unrestricted Panel Sample	4.51 [0.0038]	1.63 [0.1819]	1.51 [0.2101]	1.86 [0.1348]	0.23 [0.8788]
			B. Permanent Labor Income Risk: F-Test: Education Differences		
Baseline	8.00 [0.0000]	2.64 [0.0483]	6.60 [0.0002]	2.11 [0.0973]	1.24 [0.2937]
Household Earnings	5.41 [0.0011]	2.38 [0.0684]	4.02 [0.0075]	2.37 [0.0694]	2.72 [0.0435]
Earnings + Transfers	3.61 [0.0129]	2.85 [0.0366]	4.75 [0.0028]	1.28 [0.2789]	1.27 [0.2823]
No Income Profile	7.50 [0.0001]	2.19 [0.0883]	5.63 [0.0008]	2.55 [0.0549]	1.40 [0.2425]
Unrestricted Panel Sample	1.12 [0.3408]	1.05 [0.3697]	0.24 [0.8713]	1.46 [0.2222]	1.21 [0.3039]

Notes: Source: Authors' calculations using the Panel Study of Income Dynamic data from 1979-1993. Entries in the table F-Statistics for the hypothesis test that estimated labor market risk differs by education group, with p-values in square brackets. See the text for precise details of each sensitivity analysis.

TABLE 9: Decomposition Weighted Transitory and Permanent Labor Market Risk, by Education and Year Percentile Trim Sample

Dependent Variable = Log (Annual Earnings)

	No Trimming	Top and Bottom 1%	Top and Bottom 2%	Top and Bottom 5%	Top and Bottom 10%
	A. Transitory Labor Income Risk:				
High School	1.116	0.841	0.837	0.284	0.276
Dropout	(0.139)	(0.121)	(0.121)	(0.029)	(0.037)
	n=124	n=123	n=123	n=117	n=106
High School	0.935	0.788	0.798	0.123	0.053
Graduate	(0.082)	(0.076)	(0.076)	(0.022)	(0.015)
	n=311	n=310	n=307	n=304	n=281
Some	-0.411	-0.423	-0.409	0.151	0.124
College	(0.110)	(0.117)	(0.116)	(0.023)	(0.013)
	n=216	n=211	n=211	n=194	n=167
College	-0.060	0.030	-0.166	0.092	0.097
Graduate	(0.057)	(0.054)	(0.076)	(0.026)	(0.016)
	n=244	n=232	n=228	n=210	n=174
F-Test:					
Education	60.88	45.86	45.85	7.46	21.06
Differences	[0.0000]	[0.0000]	[0.0000]	[0.0001]	[0.0000]
	B. Permanent Labor Income Risk:				
High School	0.015	0.087	0.088	-0.014	-0.071
Dropout	(0.029)	(0.031)	(0.032)	(0.001)	(0.016)
	n=124	n=123	n=123	n=117	n=106
High School	0.024	0.069	0.064	0.100	0.074
Graduate	(0.013)	(0.018)	(0.018)	(0.011)	(0.009)
	n=311	n=310	n=307	n=304	n=281
Some	0.558	0.618	0.605	0.062	0.055
College	(0.055)	(0.064)	(0.063)	(0.010)	(0.008)
	n=216	n=211	n=211	n=194	n=167
College	0.229	0.172	0.291	0.073	0.031
Graduate	(0.022)	(0.021)	(0.036)	(0.009)	(0.007)
	n=244	n=232	n=228	n=210	n=174
F-Test:					
Education	59.70	49.58	41.40	13.98	33.10
Differences	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]

Notes: Source: Authors' calculations using the Panel Study of Income Dynamic data from 1979-1993. Entries in the table are the decomposition weighted mean estimated labor market risk for the education * sample subgroup. Entries in parentheses are standard errors and entries beginning with n= are the number of individual observation in the education * sample subgroup. Entries in the 5th and 10th row are F-statistics

for the hypothesis test that the differences in mean labor market risk between all educational subgroups within a sample are zero. The p-values for this hypothesis test as in square brackets.
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TABLE 10: Decomposition Weighted Transitory and Permanent Labor Market Risk, by Education and Individual Percentile Trim Sample

Dependent Variable = Log (Annual Earnings)

	No Trimming	Top and Bottom 1%	Top and Bottom 2%	Top and Bottom 5%	Top and Bottom 10%
	A. Transitory Labor Income Risk:				
High School Dropout	1.116	0.039	0.041	0.159	0.091
	(0.139)	(0.049)	(0.048)	(0.019)	(0.017)
	n=124	n=93	n=93	n=85	n=56
High School Graduate	0.935	0.352	0.355	0.127	-0.001
	(0.082)	(0.044)	(0.044)	(0.021)	(0.018)
	n=311	n=263	n=261	n=215	n=157
Some College	-0.411	0.144	0.156	0.107	0.094
	(0.110)	(0.090)	(0.092)	(0.021)	(0.013)
	n=216	n=172	n=165	n=129	n=79
College Graduate	-0.060	-0.045	-0.098	-0.013	0.037
	(0.057)	(0.055)	(0.055)	(0.026)	(0.013)
	n=244	n=202	n=165	n=115	n=61
F-Test: Education Differences	60.88	8.18	9.08	8.60	7.23
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0001]
	B. Permanent Labor Income Risk:				
High School Dropout	0.015	0.084	0.081	0.004	-0.013
	(0.029)	(0.018)	(0.018)	(0.005)	(0.006)
	n=124	n=93	n=93	n=85	n=56
High School Graduate	0.024	0.091	0.090	0.062	0.095
	(0.013)	(0.013)	(0.014)	(0.009)	(0.013)
	n=311	n=263	n=261	n=215	n=157
Some College	0.558	0.200	0.197	0.030	0.012
	(0.055)	(0.025)	(0.025)	(0.005)	(0.004)
	n=216	n=172	n=165	n=129	n=79
College Graduate	0.229	0.212	0.170	0.079	0.022
	(0.022)	(0.022)	(0.019)	(0.010)	(0.004)
	n=244	n=202	n=165	n=115	n=61
F-Test: Education Differences	59.70	10.37	8.42	11.54	19.39
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]

Notes: Source: Authors' calculations using the Panel Study of Income Dynamic data from 1979-1993. Entries in the table are the decomposition weighted mean estimated labor market risk for the education * sample subgroup. Entries in parentheses are standard errors and entries beginning with n= are the number of individual observation in the education * sample subgroup. Entries in the 5th and 10th row are F-statistics for the hypothesis test that the differences in mean labor market risk between all educational subgroups within a sample are zero. The p-values for this hypothesis test as in square brackets.

TABLE 11: Decomposition Weighted Transitory and Permanent Labor Market Risk, by Education and Common Trim Rule Sample

Dependent Variable = Log (Annual Earnings)

	No Trimming	Wage and Hour Trim	Δ Log (Annual Earnings) Trim	Annual Earnings Trim One	Annual Earnings Trim Two
	A. Transitory Labor Income Risk:				
High School Dropout	1.116 (0.139) n=124	0.362 (0.036) n=119	0.396 (0.063) n=90	0.136 (0.022) n=87	0.151 (0.020) n=88
High School Graduate	0.935 (0.082) n=311	0.272 (0.032) n=308	0.413 (0.068) n=229	0.156 (0.021) n=238	0.132 (0.023) n=247
Some College	-0.411 (0.110) n=216	0.119 (0.036) n=203	-0.704 (0.084) n=165	0.121 (0.025) n=157	0.223 (0.033) n=165
College Graduate	-0.060 (0.057) n=244	0.079 (0.025) n=238	-0.021 (0.022) n=185	0.075 (0.018) n=198	0.133 (0.026) n=214
F-Test: Education Differences	60.88 [0.0000]	10.34 [0.0000]	67.54 [0.0000]	2.74 [0.0427]	2.52 [0.0568]
	B. Permanent Labor Income Risk:				
High School Dropout	0.015 (0.029) n=124	-0.019 (0.011) n=119	-0.024 (0.011) n=90	0.001 (0.006) n=87	0.007 (0.005) n=88
High School Graduate	0.024 (0.013) n=311	0.033 (0.014) n=308	0.039 (0.012) n=229	0.057 (0.008) n=238	0.072 (0.009) n=247
Some College	0.558 (0.055) n=216	0.099 (0.014) n=203	0.429 (0.036) n=165	0.027 (0.006) n=157	0.021 (0.007) n=165
College Graduate	0.229 (0.022) n=244	0.121 (0.012) n=238	0.078 (0.010) n=185	0.058 (0.007) n=198	0.061 (0.008) n=214
F-Test: Education Differences	59.70 [0.0000]	16.16 [0.0000]	80.71 [0.0000]	8.36 [0.0000]	11.81 [0.0000]

Notes: Source: Authors' calculations using the Panel Study of Income Dynamic data from 1979-1993. Entries in the table are the decomposition weighted mean estimated labor market risk for the education * sample subgroup. Entries in parentheses are standard errors and entries beginning with n= are the number of individual observation in the education * sample subgroup. Entries in the 5th and 10th row are F-statistics for the hypothesis test that the differences in mean labor market risk between all educational subgroups within a sample are zero. The p-values for this hypothesis test as in square brackets.

TABLE A1: Summary Statistics for the Analysis Sample

Year	Mean Age	Percent College Graduate	Percent Some College	Percent High School Graduate	Mean Real Annual Earnings	Number of Observations
1979	31.65	0.21	0.21	0.36	30,554	1403
1980	32.32	0.21	0.21	0.36	30,751	1462
1981	32.98	0.21	0.21	0.35	30,097	1510
1982	33.71	0.22	0.21	0.35	30,120	1556
1983	34.32	0.22	0.21	0.35	29,709	1612
1984	35.07	0.22	0.21	0.35	29,998	1656
1985	35.66	0.26	0.24	0.30	32,525	1702
1986	36.59	0.26	0.24	0.30	33,250	1701
1987	37.60	0.26	0.24	0.30	33,947	1697
1988	38.59	0.26	0.24	0.30	33,971	1671
1989	39.55	0.26	0.24	0.29	35,053	1641
1990	40.53	0.26	0.24	0.30	35,660	1624
1991	41.53	0.28	0.22	0.30	35,541	1585
1992	42.55	0.28	0.22	0.30	35,481	1570
1993	43.72	0.28	0.23	0.29	36,491	1508

Notes: Source Author's calculations using the Panel Study of Income Dynamics data from 1979-1993. All mean estimates are weighted by the individual probability of sampling weights. Real labor income is measured in $1992 using the GDP deflator

TABLE A2: Sample Selection

	Number of Observations
Baseline Sample	15,891
Exclude: Households with female household heads	11,352
Exclude: Households with head's age < 22 or age > 60	9,712
Exclude: Non-household heads	2,482
Exclude: Missing education or race	2,454
Exclude: Non-zero labor income for less than 6 years	1,862
Exclude: Immigrant or Latino recontact sample; main family nonresponse or mover-out nonresponse; individual was not a sample member; born or moved in after the interview (individual weight=0)	1,403
Final Sample	1,403

Notes: Source Author's calculations using the Panel Study of Income Dynamics data. The number of observations refers to the number of sample members in the 1979 cross-section.

TABLE A3: Previous Estimates of Transitory and Permanent Labor Market Risk, by Education and Authors

	Guvenen (2005)	Meghir and Pistaferri (2004)	Carroll and Samwick (1997)	Hubbard, Skinner and Zeldes (1994b)
	A. Transitory Labor Income Risk			
High School Dropout	--	0.055 (0.010)	0.078 (0.021)	0.040 (0.53)
High School Graduate			0.043 (0.013)	
Some College	0.052 (0.008)	0.027 (0.004)	0.034 (0.009)	0.021 (0.39)
College Graduate	0.047 (0.020)	0.005 (0.003)	0.044 (0.015)	0.014 (0.42)
	B. Permanent Labor Income Risk			
High School Dropout	--	0.033 (0.007)	0.020 (0.011)	0.033 (0.43)
High School Graduate			0.028 (0.007)	
Some College	0.011 (0.007)	0.028 (0.004)	0.024 (0.005)	0.025 (0.40)
College Graduate	0.0099 (0.013)	0.044 (0.007)	0.013 (0.008)	0.016 (0.040)
Most Comparable Table and Column	Table 2 Column 3	Table 2 Column 4	Table 5 Column 5	Table 5 Column 6

Notes: Sources: Guvenen (2005), Meghir and Pistaferri (2004), Carroll and Samwick (1997), and Hubbard, Skinner and Zeldes (1994b). Guvenen (2005), Meghir and Pistaferri (2004), and Hubbard, Skinner and Zeldes (1994b) do not present separate estimates for some college. The entries for Carroll and Samwick (1997) are aggregated to the same educational categories as I use in my analysis.