Multidimensional Skills and the Returns to Schooling: Evidence from an Interactive Fixed Effects Approach and a Linked Survey-Administrative Dataset*†

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Abstract

This paper presents new evidence on the returns to schooling based on an interactive fixed effects framework that allows for multiple unobserved skills with associated prices that are potentially time-varying. Skills and prices are both allowed to be correlated with schooling. The modeling approach can also accommodate individual-level heterogeneity in the returns to schooling. The framework thus constitutes a substantive generalization of most existing approaches that assume ability is unidimensional and/or returns are homogeneous. Our empirical analysis employs a unique panel dataset on earnings and education over the period 1978-2011 based on respondents from the Survey of Income and Program Participation (SIPP) linked with tax and benefit data from the Internal Revenue Service (IRS) and Social Security Administration (SSA). Our preferred specification yields a point estimate of the average marginal returns to schooling of about 2.7 percent relative to ordinary least squares and two stage least squares estimates which lie in the range 10.7-44.4 percent. A decomposition of the aggregate least squares bias shows that the omitted ability component is responsible for a larger fraction of the bias relative to the heterogeneity component. Finally, our heterogeneity analysis suggests larger returns for individuals born in more recent years, the presence of sheepskin effects, and considerable within-group heterogeneity.

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1 Introduction

The human capital hypothesis (Becker, 1962) states that in a competitive market, higher education leads to higher human capital and therefore higher wages. This hypothesis has led to decades of empirical discussion on the average marginal return to education based primarily on the Mincer regression (Mincer, 1974). The debate has centered around the omitted ability bias, with the assumption being that ordinary least squares (OLS) estimates of the growth rate of earnings with schooling are likely to be overstated due to the positive association between earnings and ability as well as ability and schooling (Griliches, 1977). In an attempt to correct for the potential upward bias, a large body of empirical work has emerged over the past four decades that adopted various econometric strategies to account for the endogeneity of schooling which could potentially deliver a reliable estimate of the returns to schooling. Such strategies include the use of instrumental variables (IV) estimates (e.g., Angrist and Krueger, 1991), utilizing within family variation in schooling (e.g., Ashenfelter and Krueger, 1994), and the use of observable proxies for ability (e.g., Heckman, Stixrud, and Urzua, 2006). However, each strategy suffers from its own set of issues and collectively they produce conflicting and sometimes surprising results (Card, 2001; Heckman, Lochner, and Todd, 2006; Caplan, 2018). These issues have led to a call for new approaches utilizing panel data econometrics and large administrative datasets (Heckman, Lochner, and Todd, 2006; Altonji, 2010).

This paper adopts an interactive fixed effects or common factor framework for estimating the returns to schooling that allows for multiple unobserved skills with associated prices that are potentially time-varying. The skills are represented by the factor loadings while their prices are represented by the common factors. Additive individual and/or time fixed effects are obtained as special cases of this framework. Skills and prices are both allowed to be correlated with schooling which addresses the endogeneity of the latter without resorting to external instruments or proxies for ability. The modeling approach can also accommodate individual-level heterogeneity in the returns to schooling. The framework thus constitutes a substantive generalization of most existing approaches that assume ability is unidimensional and/or returns are homogeneous. Moreover, it allows us to quantify two important sources of bias: one from ignoring the interactive fixed effects structure (the ability bias) and the other from ignoring potential parameter heterogeneity. Estimation is carried out using the methods developed by Bai (2009), Pesaran (2006), and Song (2013) that facilitate consistent estimation of the growth rate of earnings with schooling and enable statistical inference via asymptotically valid standard errors.

Using a common factor structure to model the earnings function is, however, not new to the literature. Hause (1980) employs an interactive effects framework (referring to it as "the fine structure of earnings") to decompose the covariance matrix of earnings time series into ability and on-the-job training components and evaluate the empirical significance of the latter. Heckman and Scheinkman (1987) employ a multifactor model for earnings in order to test the hypothesis of uniform pricing across sectors of the economy. More recently, Carneiro et al. (2003) use the common factor structure as a dimension reduction tool to model the dependence across unobservable ability components and estimate counterfactual distributions of outcomes while Heckman, Stixrud, and Urzua (2006) show that a low-dimensional vector of latent cognitive and non-cognitive skills modeled using a factor structure explains a variety of behavioral and labor market outcomes (see also Heckman et al., 2017). Westerlund and Petrova (2017) apply the interactive fixed effects framework to the returns to schooling and find smaller returns than OLS. However, their analysis was an empirical illustration of the performance of Pesaran's (2006) common correlated effects estimator under asymptotic collinearity, and leaves much room for work.[1] Our contribution differs from these studies in that we exploit the time series variation in schooling over the sample period as well as the high-dimensional nature of the panel dataset to simultaneously address the twin issues of heterogeneity in returns to schooling and the endogeneity of schooling thus enabling us to disentangle the biases associated with ignoring one or both of these features.

Our empirical analysis employs a unique panel dataset on earnings and education over the period 1978-2011 based on respondents from the Survey of Income and Program Participation (SIPP) linked with tax and benefit data from the Internal Revenue Service (IRS) and Social Security Administration (SSA). Combining nine SIPP survey panels and administrative earnings data provides a panel dataset that is of high quality, has a long time dimension, and includes a large number of individuals. Administrative data on earnings is advantageous relative to survey data due to rising measurement error and non-response in survey data (Abowd and Stinson, 2013; Meyer et al., 2015). This is particularly relevant for estimating the returns to schooling, given that the nature of earnings misreporting in survey data tends to vary with earnings and education levels (Pedace and Bates, 2000; Cristia and Schwabish, 2009; Chenevert et al., 2016). The availability of tax data from 1978-2011 and the linking of multiple SIPP panels to the tax data generates a dataset with a much larger time dimension and cross-section dimension than in the few existing panel studies on returns to schooling, which usually rely on the Panel Study of Income Dynamics (PSID) or the National Longitudinal Study of Youth (NLSY) (e.g., Angrist and Newey, 1991; Koop

[1]This includes the application of the framework to a larger dataset, use of additional estimators (Bai, 2009; Song, 2013), analysis of a variety of specifications to account for heterogeneity and experience, relation of the results to both the IV and the ability proxy literature, and accommodation of individual-level heterogeneity in the returns to schooling, all of which we address in this paper.

and Tobias, 2004; Ashworth et al., 2017; Westerlund and Petrova, 2017).[2]

Previewing our results, we first replicate the well established finding in the literature that the IV estimate of the growth rate of earnings due to schooling is larger than the corresponding OLS estimate, both using cross-section and panel data. The IV estimate is based on using either the quarter of birth or its interaction with the year of birth as instruments following Angrist and Krueger (1991). Next, our interactive fixed effects estimates are found to be considerably smaller than the OLS estimates, regardless of whether a pooled or heterogenous model is estimated. Our preferred specification based on a model with heterogeneous coefficients yields a point estimate of the average marginal returns to schooling of about 2.7 percent relative to ordinary least squares and two stage least squares estimates which lie in the range 10.7-44.4 percent. While both omitted ability and heterogeneity biases contribute to the overall OLS bias, a decomposition of the aggregate least squares bias shows that the omitted ability component is responsible for a larger fraction of the bias relative to the heterogeneity component. Lastly, we analyze both across-group and within-group heterogeneity in the returns to schooling. Although we find minimal evidence of heterogeneous returns across race, Hispanic status, or foreign born status, our results indicate that returns are larger for individuals born in more recent years. Our findings are also suggestive of “sheepskin effects” rather than diminishing marginal returns to years of schooling. Finally, we uncover considerable within-group heterogeneity in the returns to education within demographic groups and education levels.

The rest of the paper is organized as follows. Section 2 discusses issues related to the existing econometric strategies in the literature. Section 3 introduces the interactive effects framework including a brief description of the associated estimation methods. Section 4 details the administrative data used to conduct the empirical analysis. Section 5 presents the estimated specifications and results. Section 6 concludes.

2 Issues in the Existing Literature

In order to motivate the approach taken in this paper, it is useful to first highlight the issues associated with the different econometric strategies that have been employed in the literature to correct for the omitted ability bias inherent in OLS estimates of the returns to schooling. These issues have turned out to be of considerable importance from an empirical standpoint and have contributed to a general lack of consensus about the appropriate methodology to adopt when estimating the returns to schooling. We first discuss the two main approaches that are based

[2] Two other recent examples of panel analysis use administrative data from Norway and Sweden (Bhuller et al., 2017; Nybom, 2017).

on utilizing cross-sectional data: the instrumental variable (IV) approach and the ability proxy approach. This is followed by an assessment of existing panel data studies including a discussion of the relative advantages of our approach which should further help delineate our contribution to the literature.

The IV approach is based on exploiting natural variation in the data caused by exogenous influences on the schooling decision. For instance, the seminal study of Angrist and Krueger (1991) uses an individual's quarter of birth (interacted with year of birth or state of birth in some specifications) as an instrument for schooling based on the observation that compulsory schooling laws tend to lead individuals born earlier in the year to have less schooling relative to those born later in the year. Surprisingly, however, the IV estimates were found to be consistently larger than the OLS estimates thereby presenting an empirical puzzle regarding the interpretation of the IV estimates (See Card, 2001, Table II, for a summary of this literature). One potential explanation for the larger IV estimates is in terms of the Local Average Treatment Effect (LATE) on a selected sample (Imbens and Angrist, 1994) . That is, if the instrument has a larger impact on individuals with higher marginal returns to schooling, the IV procedure will tend to produce an overestimate of the average marginal returns to education. Heckman, Lochner, and Todd (2006) and Heckman, Urzua, and Vytlacil (2006), however, point out that the LATE interpretation of the IV estimate assumes away heterogeneity in the response of schooling choices to instruments. Card (2001) discusses other explanations for the puzzle including attenuation bias in the OLS estimates due to measurement error in schooling, short term credit constraints and specification search bias.[3,4] Carneiro and Heckman (2002) argue, using AFQT as a measure of ability, that the observed pattern of results can simply be a consequence of using poor or invalid instruments that are either only weakly correlated with schooling or correlated with ability. Heckman, Lochner, and Todd (2006) conclude in their survey of the literature that the IV approach is of limited use in uncovering a reliable estimate of the returns to schooling.

The ability proxy approach employs observable proxies for ability in order to mitigate the impact of the ability bias. Common proxies for cognitive ability include GPA, AFQT scores and other components in the ASVAB tests while those for non-cognitive ability include the Rotter Locus of Control Scale which measures the degree of control individuals feel they possess over their life and

[3]Card (2001) notes that measurement error in schooling cannot explain the observed difference in OLS and IV estimates while Carneiro and Heckman (2002) show that IV can exceed OLS even in the absence of credit constraints.

[4]Oreopoulos (2006) approximated the average treatment effect by looking at compulsory schooling policy change that affected a large group of people in U.K. and suggested that even when the sample is not subject to sample selection problems and credit constraints, the IV estimate is still larger than the one in OLS and therefore the answer to the puzzle remains far from satisfying.

the Rosenberg Self-Esteem Scale which measures perceptions of self-worth (Heckman, Stixrud, and Urzua, 2006). Heckman et al. (2017) provide a comparison of standard OLS estimates to estimates controlling for ability proxies using Bartlett cognitive and non-cognitive factors, and find that the latter are about 20-50 percent smaller, depending on the specification. Similar reductions are reported by Ashworth et al. (2017) in comparing the basic Mincer regressions to regressions that include ability proxies and actual experience using NLSY panel data.[5] A major challenge facing this literature is that the ability proxies, particularly those measuring non-cognitive ability or "soft skills" such as conscientiousness, conformity, self-esteem, etc., are far from perfect resulting in biased estimates of the schooling effect (see Heckman, Stixrud, and Urzua, 2006). Our paper contributes to the literature by providing a rigorous framework that allows the data to speak regarding the importance of multi-dimensional abilities without relying on imperfect proxies. Our preferred interactive fixed effects estimates suggest a reduction in the average marginal returns to schooling between 64-95 percent relative to OLS.

In contrast to the cross-section methods, the panel data approach identifies the effect of schooling based on time-series variation within individuals. Angrist and Newey (1991) and Koop and Tobias (2004) use panel data from the National Longitudinal Survey of Youth (NLSY) to estimate the returns to schooling (more precisely, the percentage growth rate of earnings due to schooling) although their modeling approaches are different. Both studies, however, assume that individual fixed effects can effectively capture the potential endogeneity of schooling. Angrist and Newey (1991) employ a standard panel data framework with homogeneous coefficients where unobserved heterogeneity is controlled for using individual and time fixed effects. They find that the fixed effects estimates are roughly twice as large as the OLS estimates which runs counterintuitive to the notion that ability bias tends to overstate the OLS returns and suggests that individual fixed effects are not sufficient to control for the potential upward bias. Koop and Tobias (2004) address the issue of cross-sectional heterogeneity in returns adopting a Bayesian framework to characterize the nature of such heterogeneity. Comparing results across a wide variety of specifications, they find strong evidence in favor of models that allow for heterogeneous slopes. Our modeling approach is considerably more general than those adopted in these studies in that we allow for multidimensional abilities with possibly time-varying prices as well as cross-sectional heterogeneity in the growth rate of earnings with schooling. As referenced in the introduction and discussed in detail in section 4, our empirical analysis uses a linked survey-administrative dataset

[5]Based on reviewing the earlier evidence, Caplan (2018, Chapter 3) suggests that cognitive ability bias is between 20-30 percent while non-cognitive ability bias is between 5-15 percent. He interprets the ability bias in the literature as a lower bound on the true bias due to the imperfect measure of abilities, especially the non-cognitive abilities.

which offers important advantages over survey-based data that have been employed in most of the existing panel data studies.

A potential drawback of the panel data approach is that it requires a sample of individuals with continuous earnings while increasing schooling; this may include, for example, traditional students who also work while obtaining a bachelors degree or individuals who return to school later in life, whether to finish an uncompleted degree or for additional degrees. This sample could be different from the traditional idea of a student who completes his/her schooling degrees consecutively and does not work while in school. Setting aside sample selection effects, there could also be issues comparing time-series earnings before, during, and after schooling, since earnings before or during schooling could be part-time or seasonal work and not truly reflect an individual's earnings ability (Lazear, 1977; Card, 1995). That said, we believe these concerns are mitigated somewhat by the fact that we do replicate well-established results from the cross-section literature with our sample; the fact that we find similar sample statistics and cross-section estimates if we instead use a sample that does not require continuous earnings while in school; and the fact that other research has shown that the student population who works during school is both large and growing (Hotz et al., 2002; Bacolod and Hotz, 2006; Bound et al., 2012; Scott-Clayton, 2012), and is thus an important population itself. Furthermore, unlike the cross-section approach, the use of panel data allows us to formally test for heterogeneity in the returns to schooling as well as explore its nature across and within subgroups.

3 Empirical Framework

This section presents the interactive fixed effects framework that forms the basis of our empirical analysis aimed at estimating the growth rate of earnings with years of schooling. Conditional on the common factor structure embedded in the framework that represents multiple skills with time varying prices, one can derive not only the aggregate OLS and IV biases but also provide a decomposition of the biases in terms of their omitted ability and heterogeneity components. Section 3.1 lays out the modeling framework including a description of the alternative estimation approaches. Section 3.2 outlines the derivations and details regarding the computation of the two sources of bias. A potential explanation for the pattern of results obtained from the empirics can be given based on these derivations.

3.1 The Interactive Fixed Effects Model

The general interactive fixed effects model with heterogeneous coefficients is specified as

$$y_{it} = c_i + x_{it}\beta_i + w_{it}'\gamma_i + v_{it} \tag{1}$$
$$v_{it} = \lambda_i' f_t + u_{it} \tag{2}$$

where y_{it} and x_{it} represent, respectively, the (log of) annual earnings and the years of schooling completed for person $i = 1,...,N$ at period $t = 1,...,T$, and w_{it} is a vector of observable characteristics that influence wages and are potentially correlated with education (e.g., experience). We include a set of person fixed effects c_i to control for time-invariant person characteristics such as gender and race. The parameter β_i measures the percentage change in annual earnings for person i due to an additional year of schooling. This parameter does not necessarily represent an internal rate of return to schooling unless the only costs of schooling are earnings foregone, and markets are perfect (Heckman, Lochner, and Todd, 2006). The error term v_{it} is composed of a common component $(\lambda_i' f_t)$ and an idiosyncratic component (u_{it}). Here λ_i represents a $(r \times 1)$ vector of unmeasured skills (factor loadings), such as innate abilities, while f_t is a $(r \times 1)$ vector of unobserved, possibly time-varying, prices (or common factors) of the unmeasured skills.[6] Both loadings and the factors are potentially correlated with the observables (x_{it}, w_{it}). The number of common components r is assumed unknown. The object of interest is the average marginal return $[E(\beta_i)]$ in the population. Note that while the returns to each of the skill components $(\lambda_i' f_t)$ are identified, the skills and their prices are not separately identified.[7] That is, the estimated factors and their loadings only estimate a rotation of the underlying true parameters and so cannot be given a direct economic interpretation. Unlike Heckman, Stixrud, and Urzua (2006), our paper does not attempt to distinguish between the role of cognitive and non-cognitive skills in explaining the behavior of earnings. Rather, we are interested in estimating the rate of growth of earnings with schooling employing the interactive fixed effects structure as a device to control for the different components of ability that may affect earnings and are potentially correlated with schooling.

Various panel data specifications used in the literature can be obtained as special cases of (1) and (2). The standard panel data model with person and time fixed effects considered by Angrist and Newey (1991) is obtained by setting $\beta_i = \beta$, $\gamma_i = \gamma$, $\lambda_i = \lambda$. Koop and Tobias (2004)

[6]While we refer to the factor loadings as unobserved skills/abilities throughout the paper, there are other time-invariant determinants with possibly time-varying prices, such as motivation and persistence, that can be captured by the factors loadings as well.

[7]For an arbitrary $(r \times r)$ invertible matrix A, we have $F\Lambda' = FAA^{-1}\Lambda' = F^*\Lambda^{*\prime}$, so that a model with common factors $F = (f_1,...,f_T)'$ and loadings $\Lambda = (\lambda_1,...,\lambda_N)'$ is observationally equivalent to a model with factors $F^* = (f_1^*,...,f_T^*)'$ and $\Lambda^* = (\lambda_1^*,...,\lambda_N^*)'$ where $F^* = FA$ and $\Lambda^* = \Lambda A^{-1\prime}$.

consider a restricted version of (1) and (2) that allows heterogeneity in returns to schooling but assumes that the endogeneity of schooling (i.e., the ability bias) is fully accounted for by the individual fixed effects c_i. Thus, their model does not allow for multiple skill components with time varying prices (Hause, 1980; Heckman and Scheinkman, 1987; Heckman, Lochner, and Todd, 2006). We consider estimating model (1) and (2) using two alternative econometric procedures: the principal components approach (Bai, 2009; Song, 2013) and the common correlated effects approach (Pesaran, 2006). We now briefly describe each of these methods.

3.1.1 The Principal Components Approach

Bai (2009) advocates an iterative principal components approach that treats the common factors and their loadings as parameters which are jointly estimated with the regression coefficients assuming cross-sectional homogeneity of the latter. Under both large N and large T, the estimator is shown to be $\sqrt{NT}$-consistent and asymptotically normal under mild conditions on the idiosyncratic components that allow for (weak) correlation and heteroskedasticity in both dimensions. To ensure that the asymptotic distribution is centered around zero, a bias corrected estimator is proposed. Our empirical analysis employs the bias corrected estimator which we refer to as the IFE (or interactive fixed effects) estimator.

Song (2013) develops a heterogeneous version of the IFE estimator that allows the regression coefficients to be unit-specific. The estimator is obtained by taking the cross-sectional average of the individual specific IFE estimates and is shown to be $\sqrt{N}$-consistent for the average return in the population. We refer to this estimator as the IFEMG (MG denoting mean group) estimator.

Both the IFE and IFEMG estimators require a choice on the number of common factors. Bai (2009) proposes estimating the number of factors employing the information criterion procedure of Bai and Ng (2002). Specifically, the number of factors is obtained by minimizing the criterion

$$IC(k) = \ln\left[(NT)^{-1}\sum_{i=1}^{N}\sum_{t=1}^{T}\hat{u}_{it}^{2}(k)\right] + k\left(\frac{N+T}{NT}\right)g(N,T)$$

over $k \in [0, k_{\max}]$, where $k_{\max}$ is a prespecified upper bound. The residuals $\{\hat{u}_{it}(k)\}$ are obtained from principal components estimation assuming k factors and $g(N,T)$ is a penalty function. When estimating a pooled model as in Bai (2009), the IFE estimate is used to construct the residual series while estimating a heterogeneous version as in Song (2013) entails the use of the individual level IFE estimate. We set $k_{\max} = 10$ and use $g(N,T) = \ln\left(\frac{NT}{N+T}\right)$ which corresponds to the "IC_{p1}" criterion in Bai and Ng (2002).

3.1.2 The Common Correlated Effects (CCE) Approach

Pesaran (2006) proposes to proxy for the unobserved common factors f_t using cross-sectional averages of the dependent and independent variables, i.e., unlike the principal components approach, the factors are treated as nuisance parameters rather than parameters of interest. Estimation is based on augmenting the regression (1) with the cross-sectional averages and does not require knowledge of the number of factors. Two estimators are suggested: (1) the common correlated effects mean group (CCEMG) estimator which allows for heterogeneous coefficients and is obtained by estimating person-specific time series regressions using ordinary least squares and taking the average of the person-specific estimates; (2) the common correlated effects pooled (CCEP) estimator which pools the observations over the cross-section units and achieves efficiency gains when the slope parameters are the same across units.

Based on a random coefficients formulation for the regression coefficients as well as the factor loadings, both estimators are shown to be $\sqrt{N}$-consistent and asymptotically normal as the cross-section dimension (N) and the time series dimension (T) jointly diverge to infinity. The finite sample performance of both estimators can be sensitive to a particular rank condition which requires that the number of factors does not exceed the total number of observed variables (see the Monte Carlo evidence in section 7 of Pesaran, 2006).

Pesaran (2006, p.1000) also suggests a two-step approach to estimation that involves combining the CCE and principal components approaches. For the model specified in (1) and (2), the first step entails obtaining the residuals

$$\hat{v_{it}} = y_{it} - \hat{c}_i - x_{it}\hat{\beta}_i - w_{it}'\hat{\gamma}_i$$

where $(\hat{c}_i, \hat{\beta}_i, \hat{\gamma}_i)'$ denote the individual level CCE estimates. The factors are then estimated by principal components treating the residuals as observed data where the number of factors is again selected based on the information criterion discussed in section 3.1.1. In the second step, the factor estimates (say $\{\hat{f}_t\}_{t=1}^{T}$) are then directly used as regressors in the regression equation

$$y_{it} = c_i + x_{it}\beta_i + w_{it}'\gamma_i + \lambda_i'\hat{f}_t + \xi_{it} \tag{3}$$

Given that the consistency of $\hat{\beta}_i$ hinges on the validity of the aforementioned rank condition, we replace $\hat{\beta}_i$ with the CCEMG estimate when computing the first step residuals. The estimate of β_i obtained from OLS estimation of (3) will be referred to as the "two-step CCE" estimate and the corresponding mean group version as CCEMG-2. For the pooled analog of (1), the first step residuals are obtained using the CCEP estimate and the resulting estimate is referred to as CCEP-2.

Our empirical analysis reports both the one and two-step CCE estimates. A potential advantage of the two-step approach is that the second-step estimate is based on factors estimated by principal components instead of observable proxies and is therefore possibly less sensitive to the fulfillment of the rank condition.[8]

3.2 Omitted Ability and Heterogeneity Biases

In the interactive effects environment, there are at least two potential sources of bias that can arise in panel Ordinary Least Squares (OLS)/Instrumental Variable (IV) estimation of the returns to schooling. The first is the omitted ability bias that emanates from ignoring the common factor structure (2). While OLS estimation treats the ability components as part of the error term leading to endogeneity of the schooling variable, the IV estimator can be subject to bias if the instruments are inappropriate in that they are unable to fully account for the OLS bias. The second source of bias arises from estimating a pooled specification when the true regression coefficients are heterogeneous. In practice, the two biases may reinforce or offset each other depending on their signs. In what follows, we consider each of these possible biases in turn and derive analytical expressions for them including conditions under which one would expect a given pattern in the relative magnitude of the regression parameter estimates. The interactive effects framework allows us to separately estimate the bias associated with each of the two sources.

3.2.1 Omitted Ability Bias

In this section, we employ the CCE and IFE estimators described in the preceding section to derive expressions for and estimates of the biases induced by the OLS and IV estimators assuming that the true model is given by (1) and (2). The interactive effects framework allows us to not only obtain estimates of the aggregate ability bias but also the bias attributable to each of the ability components. In particular, comparison of the component-specific OLS and IV biases allows us to isolate components that are responsible for exacerbating the IV bias relative to OLS from those where the instruments are effective at mitigating the bias. For instance, the instruments may reduce the bias associated with an ability component that is negatively correlated with schooling (e.g., high school skills) while worsening the bias associated with a component positively correlated with schooling (e.g., college skills).[9] To simplify the exposition, we consider a setup where ability

[8]The rank condition is potentially very relevant in this application, given that our empirical analysis based on panel data includes a small number of observed variables (2-4, depending on the specification).

[9]We borrowed the language of "high school" versus "college skills" from Heckman, Lochner, and Todd (2006) page 390. One can also think of it as "mechanical" versus "cognitive/non-cognitive skills" (See Prada and Urzua, 2017).

is two-dimensional ($r = 2$) and the regression coefficients are homogeneous.[10] The model is given by

$$y_{it} = x_{it}\beta + \lambda_{1i} f_{1t} + \lambda_{2i} f_{2t} + u_{it} \tag{4}$$

where y_{it} (x_{it}) is the residual obtained by regressing log wages (schooling) on the set of controls and a full set of time and person dummies. Note that given the set of dummies included, the means of y_{it} and x_{it} across i and t as well as their overall means (over i and t) are all zero. Let $c_{j,it} = \lambda_{ji} f_{jt}$ be the common component associated with factor j $(j = 1,2)$.

The probability limit of the OLS estimator can be expressed as

$$\begin{aligned} p\lim \hat{\beta}_{OLS} &= [Var(x_{it})]^{-1} Cov(x_{it}, y_{it}) \\ &= \beta + [Var(x_{it})]^{-1} Cov(x_{it}, c_{1,it}) + [Var(x_{it})]^{-1} Cov(x_{it}, c_{2,it}) \\ &= \beta + B1_{ols} + B2_{ols} \end{aligned} \tag{5}$$

where

$$Var(x_{it}) = plim_{N,T\to\infty} (NT)^{-1} \sum_t \sum_i x_{it}^2 \tag{6}$$

$$Cov(x_{it}, c_{j,it}) = plim_{N,T\to\infty} (NT)^{-1} \sum_t \sum_i x_{it} \lambda_{ji} f_{jt} \tag{7}$$

In (5), $B1_{ols}$ can be interpreted as the bias in the OLS estimator induced by f_1 and $B2_{ols}$ the bias induced by f_2. The aggregate OLS bias is given by

$$Bias(\hat{\beta}_{OLS}) = p\lim \hat{\beta}_{OLS} - \beta = B1_{ols} + B2_{ols} = B_{ols}$$

Now consider a two stage least squares (2SLS) estimator based on a set of K instruments z_{it} (as before, z_{it} is the residual from regressing the instruments on the set of controls and a full set of time and person dummies.) where $Cov(z_{it,k}, x_{it}) \neq 0$ where $k = 1,...,K$. Define the $(T \times 1)$ vector $Y_i = (Y_{i1},...,Y_{iT})'$, the $(T \times K)$ matrix $Z_i = (z_{i1},...,z_{iT})'$ and the $(NT \times K)$ matrix $Z = (Z_1',...,Z_N')'$. The first stage estimate is $\hat{\Pi} = \left(\sum_{i=1}^N Z_i'Z_i\right)^{-1} \sum_{i=1}^N Z_i'X_i$. The 2SLS estimate is

$$\hat{\beta}_{2SLS} = \left(\hat{\Pi}' \sum_{i=1}^N Z_i'Z_i\hat{\Pi}\right)^{-1} \left(\hat{\Pi}' \sum_{i=1}^N Z_i'Y_i\right)$$

Denote $\hat{X}_i = Z_i\hat{\Pi}$. Then we have

$$\begin{aligned} p\lim \hat{\beta}_{2SLS} &= \beta + [p\lim (NT)^{-1} \sum_{i=1}^N \hat{X}_i'\hat{X}_i]^{-1} \left\{ \begin{array}{c} p\lim (NT)^{-1} \left[\sum_i \hat{X}_i' F_1 \lambda_{1i}\right] \\ + p\lim (NT)^{-1} \left[\sum_i \hat{X}_i' F_2 \lambda_{2i}\right] \end{array} \right\} \\ &= \beta + B1_{iv} + B2_{iv} \end{aligned} \tag{8}$$

[10] The ability bias associated with the IFEMG estimator is derived in the Appendix B.

In (8), $B1_{iv}$ can be interpreted as the bias in the 2SLS estimator induced by f_1 and $B2_{iv}$ the bias induced by f_2. The aggregate 2SLS bias is given by

$$Bias(\hat{\beta}_{2SLS}) = p\lim \hat{\beta}_{2SLS} - \beta = B1_{iv} + B2_{iv} = B_{iv}$$

The 2SLS estimator has a larger aggregate bias than the OLS estimator if $B_{iv} > B_{ols}$ or

$$B2_{iv} - B2_{ols} > B1_{ols} - B1_{iv} \tag{9}$$

In accordance with our empirical results, we assume that $B1_{ols} + B2_{ols} = B_{ols} > 0$. We consider the following two cases depending on the magnitude and direction of the component-specific biases that turn out to be relevant in our context:

- **Case A:** $B1_{ols} > 0$, $B2_{ols} < 0$ such that $B1_{ols} > |B2_{ols}|$. Then $\hat{\beta}_{OLS}$ is upward biased with the positive bias induced by f_1 dominating the negative bias induced by f_2:

$$Bias(\hat{\beta}_{OLS}) = p\lim \hat{\beta}_{OLS} - \beta = B1_{ols} + B2_{ols} = B_{ols} > 0$$

The inequality (9) is consistent with any of the following four scenarios:

1. IV is effective in reducing the magnitude of the bias from *both* components: $|B2_{iv}| < |B2_{ols}|$, $|B1_{iv}| < B1_{ols}$.
2. IV is effective in reducing the magnitude of the bias from component 1 *only*: $|B2_{iv}| > |B2_{ols}|$, $|B1_{iv}| < B1_{ols}$.
3. IV is effective in reducing the magnitude of the bias from component 2 *only*: $|B2_{iv}| < |B2_{ols}|$, $B1_{iv} > B1_{ols}$.
4. IV is completely ineffective: $|B2_{iv}| > |B2_{ols}|$, $B1_{iv} > B1_{ols}$.

In general, if ability is multidimensional and one of its components is negatively correlated with schooling, it is possible for the aggregate 2SLS bias to exceed the aggregate OLS bias *regardless* of whether the instruments are fully, partially or not effective in reducing the magnitude of the bias in any or all of its components.

- **Case B:** $B1_{ols} > 0$, $B2_{ols} > 0$

The inequality (9) is consistent with any of the following three scenarios:

1. IV is effective in reducing the magnitude of the bias from component 1 *only*: $B2_{iv} > B2_{ols}$, $B1_{iv} < B1_{ols}$.

2. IV is effective in reducing the magnitude of the bias from component 2 *only*: $B2_{iv} < B2_{ols}$, $B1_{iv} > B1_{ols}$.

3. IV is completely ineffective: $B2_{iv} > B2_{ols}$, $B1_{iv} > B1_{ols}$.

In contrast to case A, if each of the ability components induce a positive bias in the OLS estimates, the instruments can be (at most) effective at reducing the bias associated with only a *subset* of the components at the expense of exacerbating the bias associated with the remaining components, for (9) to hold.

Under the factor model framework (4), each of the bias terms in (5) and (8) can be consistently estimated. This is because even though the factors and their loadings are not separately identified, their product, i.e., the common components $(c_{j,it})$ are. The estimated biases can be obtained as follows:

$$\begin{aligned}
\widehat{B1}_{ols} &= [SVar(x_{it})]^{-1} SCov(x_{it}, \hat{c}_{1,it}) \\
\widehat{B2}_{ols} &= [SVar(x_{it})]^{-1} SCov(x_{it}, \hat{c}_{2,it}) \\
\widehat{B1}_{iv} &= [SVar(\hat{X}_i)]^{-1} SCov(\hat{X}_i, \hat{F}_1 \hat{\lambda}_{1i}) \\
\widehat{B2}_{iv} &= [SVar(\hat{X}_i)]^{-1} SCov(\hat{X}_i, \hat{F}_2 \hat{\lambda}_{2i})
\end{aligned}$$

where, for $j = 1,2$, $\hat{c}_{j,it} = \hat{\lambda}_{ji} \hat{f}_{jt}$ are the Bai (2009) estimates of the common components and $SVar(x_{it})$, $SVar(\hat{X}_i)$, $SCov(\hat{X}_i, \hat{F}_j \hat{\lambda}_{ji})$, $SCov(x_{it}, \hat{c}_{j,it})$ denote the sample variance and sample covariances respectively, which are the sample analogs of the quantities defined in (6) and (7). Specifically, these quantities are computed as follows:

$$SVar(x_{it}) = (NT)^{-1} \sum_t \sum_i x_{it}^2 \tag{10}$$

$$SVar(\hat{X}_i) = (NT)^{-1} \sum_{i=1}^{N} \hat{X}_i' \hat{X}_i \tag{11}$$

$$SCov(\hat{X}_i, \hat{F}_j \hat{\lambda}_{ji}) = (NT)^{-1} \left[\sum_i \hat{X}_i' \hat{F}_j \hat{\lambda}_{ji} \right] \tag{12}$$

$$SCov(x_{it}, \hat{c}_{j,it}) = (NT)^{-1} \sum_t \sum_i x_{it} \hat{\lambda}_{ji} \hat{f}_{jt} = T^{-1} \sum_t \left\{ N^{-1} \sum_i x_{it} \hat{\lambda}_{ji} \hat{f}_{jt} \right\} \tag{13}$$

Note that in (10-13), we do not need to subtract the means since the variables already have mean zero. Note that $SCov(x_{it}, \hat{c}_{j,it})$ is the average (over time) of the cross-sectional correlation between

x_{it} and $\hat{c}_{j,it}$. Each of the terms in (10-13) can be computed based on our data and factor model estimates to examine the extent to which the component-specific biases offset or reinforce each other.

The CCE approach does not directly estimate the factors so we employ the following two-step procedure to estimate the bias components: (1) Obtain the residuals $\eta_{it} = y_{it} - x_{it}\hat{\beta}_{CCE}$, where $\hat{\beta}_{CCE}$ is either the CCEP or CCEMG estimator depending on whether one estimates a pooled or heterogeneous model; (2) Given a choice of the number of factors, estimate the common factor model $\eta_{it} = \lambda_i' f_t + u_{it}$ by principal components. Once the factor structure estimates are obtained, the biases attributable to each of the skill components can be estimated as discussed for the IFE estimator above. Note that since the CCE procedure proxies for the factors using cross-section averages of the variables, the aggregate bias estimated using the two-step procedure will not necessarily equal the difference between the OLS and the CCEP (or CCEMG). Our empirical results indicate that the difference is, however, minimal. For the CCEP-2 and CCEMG-2 estimates, the biases can be computed in the same way as for IFE and IFEMG, respectively.

3.2.2 Heterogeneity Bias

Heterogeneity bias arises when one estimates a pooled specification when the regression coefficients are in fact heterogeneous across the cross-section units. To analyze this source of bias, we consider the IFE estimator of Bai (2009). We can write (1) as

$$Y_i = X_i\beta_i + F\lambda_i + U_i \tag{14}$$

with Y_i, X_i, U_i being $(T\times 1)$ vectors defined as $Y_i = (y_{i1},...,y_{iT})'$, $X_i = (x_{i1},...,x_{iT})'$, $U_i = (u_{i1},...,u_{iT})'$ and $F = (f_1,...,f_T)'$ being the $(T\times r)$ matrix of common factors. Here we interpret y_{it} (x_{it}) as the part of log wages (schooling) unexplained by the controls w_{it} and person/time fixed effects.

The IFE estimator is given by

$$\hat{\beta}_{IFE} = \left(\sum_{i=1}^{N} X_i' M_{\hat{F}} X_i\right)^{-1} \left(\sum_{i=1}^{N} X_i' M_{\hat{F}} Y_i\right) \tag{15}$$

where $M_{\hat{F}} = I_T - \hat{F}\left(\hat{F}'\hat{F}\right)^{-1}\hat{F}'$, and $\hat{F}$ is the principal components (PC) estimate of F.

Under the heterogeneous model (14), we can write (15) as

$$\begin{aligned}
\hat{\beta}_{IFE} &= \left(\sum_i X_i' M_{\hat{F}} X_i\right)^{-1} \sum_i X_i' M_{\hat{F}} \left(X_i\beta_i + F\lambda_i + U_i\right) \\
&= \left(\sum_i X_i' M_{\hat{F}} X_i\right)^{-1} \sum_i X_i' M_{\hat{F}} \left(X_i\beta_i + (F-\hat{F})\lambda_i + \hat{F}\lambda_i + U_i\right) \\
&= \left(\sum_i X_i' M_{\hat{F}} X_i\right)^{-1} \sum_i X_i' M_{\hat{F}} X_i\beta_i + \left(\sum_i X_i' M_{\hat{F}} X_i\right)^{-1} \left(\sum_i X_i' M_{\hat{F}} (F-\hat{F})\lambda_i + \sum_i X_i' M_{\hat{F}} U_i\right) \\
&\underset{N,T\ large}{\simeq} \left(\sum_i X_i' M_{\hat{F}} X_i\right)^{-1} \sum_i X_i' M_{\hat{F}} X_i\beta_i
\end{aligned}$$

where the approximation in the last line holds since the other terms are negligible for large N,T [see Bai, 2009]. This gives

$$\hat{\beta}_{IFE} \underset{N,T\ large}{\simeq} \sum_i \omega_i\beta_i \tag{16}$$

where $\omega_i = \left(\sum_i X_i' M_{\hat{F}} X_i\right)^{-1} X_i' M_{\hat{F}} X_i$ is the weight on the individual i's return (note that $\sum_i \omega_i = 1$). This suggests that $\hat{\beta}_{IFE}$ is likely to exceed $\hat{\beta}_{IFEMG}$ (since $\hat{\beta}_{IFEMG}$ is an estimate of $N^{-1}\sum_i \beta_i$) if there exists positive correlation between β_i and ω_i, i.e., marginal returns are higher for those individuals who have higher time variation in the unexplained portion of schooling. This can be verified empirically by computing the cross-sectional correlation between $\hat{\beta}_i$ (the individual-specific IFE estimate) and ω_i.

4 Data

Our analysis uses large, high-quality panel data from the U.S. Census Bureau that includes self-reported educational history and administrative records of earnings over a large number of years. This section describes the details of the individual-level linked survey and administrative data, and the construction of the sample for analysis.

4.1 Linked Survey-Administrative Data

Linked survey and administrative data come from the U.S. Census Bureau Gold Standard File. The dataset is based on respondents from the Survey of Income and Program Participation (SIPP) linked with tax and benefit data from the Internal Revenue Service (IRS) and Social Security Administration (SSA). Nine SIPP panels are linked: 1984, 1990, 1991, 1992, 1993, 1996, 2001, 2004, and 2008. The linked dataset includes the respondents' SIPP survey information for the years during which they were in the survey and annual tax and benefit information that ranges from 1978 to 2011 for some variables and 1951 to 2011 for others.[11]

The SIPP is a household-level survey. A new set of households is sampled during each panel. These households are then surveyed longitudinally for 2-1/2 years to 4 years through several waves of interviews. Each panel consists of approximately 14,000 to 52,000 households. All household members age 15 or older are surveyed. The survey provides detailed social and economic information on the respondent during their SIPP panel years. Linking these individuals to tax and benefit data provides information, including earnings, over a much longer time frame.

Linking the SIPP with administrative data provides a unique panel dataset of education and earnings. The SIPP information includes the respondent's educational history, including not just the highest level of schooling completed, but also the year during which each level of school was completed. This within-person longitudinal schooling variable allows for within-person analysis of education and earnings. The data also provide a long history of detailed earnings data: when SIPP respondents are linked to IRS data, their annual detailed earnings can be observed for the entire time frame of 1978-2011.[12] The earnings data is based on W-2 records for employed workers and Schedule C records for self-employed workers, including deferred earnings.

Administrative data on earnings may be advantageous to survey data due to rising measurement error and non-response in survey data (Abowd and Stinson, 2013; Meyer et al., 2015). Previous work has shown that earnings data from surveys appears to be overstated at the bottom of the earnings distribution and understated at the top (Pedace and Bates, 2000; Cristia and Schwabish, 2009; Chenevert et al., 2016). Chenevert et al. (2016) also found that survey earnings data is overstated for lower education levels and understated for higher education levels. These findings have potential implications about the reliability of survey data for estimating the returns to schooling. Chenevert et al. (2016) estimated Mincer (1974) cross-section equations using OLS with different sets of earnings data from survey and administrative sources for individuals from the 2008 SIPP. They found that the source of earnings data has little affect on the estimated return to schooling, although it does affect estimates of the return to potential experience.

Combining survey data from the SIPP and administrative earnings data from the IRS provides

[11]The analysis is based on version 6.0 of the U.S. Census Bureau's SIPP Gold Standard File. Outside researchers can access a synthetic version of the Gold Standard File, known as SIPP Synthetic Beta. Researchers can then have their results validated on non-synthetic data. More information is available here: https://www.census.gov/programs-surveys/sipp/guidance/sipp-synthetic-beta-data-product.html.

[12]Summary earnings records are available back to 1951 from the SSA, but these summary earnings are capped at the taxable maximum.

a panel dataset that is of high quality, has a long time dimension, and includes a large number of individuals. This type of dataset is rare in the returns to schooling literature. Most studies have relied on cross-section analysis (e.g., Angrist and Krueger (1991); Card (1995); Staiger and Stock (1997); Card (1999)) or short panels (e.g., Carneiro and Heckman (2002); Carneiro et al. (2003); Cunha et al. (2005); Rubinstein and Weiss (2006); Carneiro et al. (2011); Park (2011)). Heckman, Lochner, and Todd (2006) conclude in their survey of the literature that the solution to improving the estimation of returns to schooling lies in rich panel data and new econometric approaches. The use of linked survey and administrative data addresses the former of those recommendations by providing a large, high-quality panel of earnings and education. It also addresses the latter recommendation; the use of rich panel data allows for an interactive fixed effects framework which cannot be applied to cross-section or short panel data. Altonji (2010) discusses these points and also the use of linked survey and administrative data specifically as avenues for future research.

Studies that use panel data typically use either the Panel Study of Income Dynamics (PSID) (e.g., Carneiro et al. (2003); Cunha et al. (2005); Rubinstein and Weiss (2006); Westerlund and Petrova (2017)) or the National Longitudinal Study of Youth (NLSY) (e.g., Angrist and Newey (1991); Carneiro et al. (2003); Koop and Tobias (2004); Cunha et al. (2005); Rubinstein and Weiss (2006); Ashworth et al. (2017)). These studies find similar results to cross-section studies, including that IV estimates are larger than OLS estimates (Angrist and Newey, 1991). The linked SIPP-administrative data has several advantages over the PSID and NLSY. These advantages include larger sample sizes, due to the combination of SIPP respondents across multiple SIPP panels;[13] more accurate earnings data, due to the removal of survey mis-reporting, non-response, and top-coding; less attrition, because SIPP respondents can be linked to administrative earnings records regardless of whether they answered survey questions about earnings; and a longer time dimension for earnings, due to administrative earnings data that covers many years. The drawbacks of the linked SIPP-administrative data relative to NLSY are the lack of parental information and the lack of an observed proxy for ability, such as AFQT scores.

4.2 Sample Selection and Summary Statistics

The sample of individuals in the analysis was selected from the linked SIPP-administrative dataset based on seven selection criteria: (1) males; (2) with variation in their years of schooling during 1978-2011; (3) with earnings observations in each year from 1978-2011; (4) without any missing data for the other variables included in the analysis; (5) between the ages of 16-65 during the

[13] Most panel studies in the literature analyze approximately 1,000-2,000 individuals, with the extremes being 888 in Westerlund and Petrova (2017) and 3,695 in Cunha et al. (2005).

entirety of 1978-2011; (6) at least 27 years of age at the time of their SIPP survey; and (7) not currently enrolled in school at the time of their SIPP survey.

The sample is restricted to males in order to analyze a population that historically is consistently and strongly attached to the labor market, and to be consistent with the majority of the literature so that results are comparable. Restricting the sample to those with variation in schooling allows for the estimation of person fixed effects models. Restrictions (3)-(4) are included to produce a balanced panel. Restriction (5) limits the analysis to individuals' prime working years. Restrictions (6) and (7) exclude individuals most likely to have incomplete educational histories in the survey data.

Table 1 Panel A shows summary statistics for the variables included in the analysis. The final sample includes 6,300 individuals.[14] Each column corresponds to a different set of the sample that is used in the analysis below. The full balanced panel sample is shown in column (3). This column shows the means and standard deviations for each variable over the full 6,300 individuals and 34 years of the panel. Columns (1) and (2) show means and standard deviations of the variables at two point-in-time cross-sections of the full balanced panel sample. Column (1) is based on the 6,300 individuals in the year 1990. Column (2) is based on the 6,300 individuals at age 40.

The variables included are annual earnings from W-2 or self-employment earnings records, including deferred earnings; years of school; age, measured in quarters;[15] and demographic controls for marital status, race ('White', 'Black', 'other race'), Hispanic origin, foreign born status, birth year, and state of residence at the time of the SIPP survey.

Years of school is a longitudinal variable based on survey responses indicating highest education level completed ('no high school degree', 'high school degree', 'some college', 'college degree', and 'graduate degree'), the year during which high school was completed, the year during which post-high school education began, the year during which post-high school education ended, and the year during which a bachelor's degree was earned. Collectively, these variables were used to build a longitudinal schooling variable.

First, individuals were assigned a highest-level-completed variable for each year. All individuals were assigned 'no high school degree' before the year during which they graduated high school and 'high school degree' beginning in their graduation year. Individuals whose highest

[14] All counts are rounded according to U.S. Census Bureau disclosure avoidance rules.

[15] We followed Angrist and Krueger (1991) and constructed age-in-quarters as the individual's age-in-quarters at the time of their SIPP survey. That is, the within-birthyear-birthquarter variation due to the differences in which quarter individuals were born and which quarter they were interviewed allows cross-section IV specifications that include birth year fixed effects, age controls, and the quarter-based IVs. When we moved to the panel setting, we calculated the age-in-quarters variable for their non-survey years by subtracting/adding four for each additional year away from the survey year for consistency and for the sake of estimating similar panel 2SLS specifications.

completed level was 'some college' and thus did not obtain a college degree were assigned 'some college' beginning in the year during which their post-high school education began. Individuals who obtained at least a college degree were assigned 'college degree' beginning in the year during which they obtained their bachelor's degree. Individuals who obtained a graduate degree were assigned 'graduate degree' beginning in the year during which their post-high school education ended.[16] Then, based on highest level completed at each year, individuals were assigned a years of school variable. Individuals with 'no high school degree' in a given year were assigned 10 years of school, individuals with 'high school degree' were assigned 12 years of school, individuals with 'some college' were assigned 14 years of school, individuals with 'college degree' were assigned 16 years of school, and individuals with 'graduate degree' were assigned 18 years of school.[17]

The sample is mostly made up of individuals who are of non-minority statuses: the sample is only 5.2 percent Black, 2.4 percent other race, 3.9 percent Hispanic, and 2.6 percent foreign born.[18] The small size of these groups is related to the sample restriction for earnings observations in each year from 1978-2011 and the sample restriction for no missing covariate information. Minority groups, including Black and Hispanic males, historically have higher unemployment rates and lower labor force participation rates than White males and are thus more likely to have gaps in their employment history (Altonji and Blank, 1999). There is also evidence that minority groups are more likely to have non-response items in survey data (Griffin, 2002; Chenevert et al., 2016). Finally, there is also evidence that individuals who are White are more likely to work while in

[16]Note that the variable for the year during which post-high school education ended could be before, the same as, or after the year during which a bachelor's degree was earned. If a person started college but did not obtain a bachelor's degree, then it indicates when the person dropped out. If a person obtained a bachelor's and then stopped, then it is the same as the bachelor's year variable. If the person obtained a graduate degree, then it indicates when they finished graduate school.

[17]Assigning years of school based on highest level completed in this way is common in the literature (e.g., Heckman, Lochner, and Todd, 2006; Henderson, Polacheck, and Wang, 2011). Another approach is to measure actual years spent in school, regardless of completed education levels. This is not feasible in the U.S. Census Bureau Gold Standard File as it is in some other datasets such as the NLSY, although it is not obvious that this approach would be preferable; variation in years of school that is independent of completed education levels (e.g., individuals who complete college in three versus five years) might introduce more measurement error into the variable, depending on beliefs about whether measured years of school should be tied to educational achievement. Another set of results not shown in this paper were based on a schooling variable that smoothed the discrete jumps in years of school described above by attempting to impute actual years spent in school. However, doing so was difficult based on available information in some instances, such as for individuals with long periods of time between the beginning and ending of post-high school education; while the educational history variables do report when post-high school education began and ended, it is not possible to know if or when individuals took breaks from school during college or between college and graduate school.

[18]Survey weights are not used in the sample statistics or regression analysis to re-weight to a nationally representative sample. SIPP survey weights would need to be adjusted not only for the linkage rate to administrative data and missing data, but also for the combination of many SIPP panels into one sample.

school than individuals who are Black or Hispanic (Hotz et al., 2002).

Panel B of Table 1 shows summary statistics for a comparison sample with the same individuals, except also including individuals who have missing earnings data while enrolled in school. As discussed earlier in the paper, using a panel approach forces the analysis to be based on individuals who have earnings data while increasing their education. This is a necessity of extending the analysis of returns to schooling to the panel setting, but it does introduce a sample of individuals who are potentially different with respect to observable or unobservable characteristics.

Table 1 shows that 62.38 percent of individuals in the comparison sample that relaxes the earnings-during-school restriction remain in the sample of analysis. This is generally consistent with evidence that as much as 92 percent of individuals gain at least some work experience during high school and 88 percent during in college (Hotz et al., 2002). Employment during college, in particular, has been on the rise and is one of the main variables related to increased time-to-graduation (Bound et al., 2012). The frequent occurrence of work while enrolled in school helps eliminate concerns that the sample of analysis is a small, non-representative group of individuals.

While the incidence of work during school appears to be frequent in the linked dataset and the literature, much of this work is likely part-time work. Thus, earnings may be artificially low during school, which could lead to larger estimates of the return to schooling from panel datasets. For this reason, the analysis below first estimates cross-section specifications in order to replicate the well-known pattern of OLS/2SLS results from the literature. This is done using two point-in-time cross-sections from the full panel sample: one based on values in the year 1990 and the other based on values at age 40. These cross-section samples are shown in the summary statistics table. Furthermore, the full panel sample will be used to generate OLS and 2SLS estimates, in addition to estimates from specifications based on an interactive fixed effects structure. Thus, to the extent that estimates of the return to schooling are larger or smaller based on panel data, all of the estimators will be affected by this, such that comparing estimates from OLS/2SLS with estimates from interactive fixed effects specifications still illustrates the effect of allowing for multiple unobserved skills whose prices can vary over time.

5 Empirical Results

The empirical results are organized into five subsections. Section 5.1 presents the set of specifications estimated that differ according to whether cross-section or panel data are employed, whether the effect of experience is accounted for, whether the regression parameters are allowed to be heterogeneous and whether interactive fixed effects are incorporated.[19] Section 5.2 reports the cross-section estimates which replicate the robust empirical finding in the literature that the instrumental variable estimate of the returns to schooling exceeds the OLS estimate. The former is based on using the quarter of birth or its interaction with year of birth as instruments (Angrist and Krueger, 1991). Section 5.3 presents the panel OLS, 2SLS, and interactive fixed effects estimates obtained by pooling the data across cross-section units assuming homogeneous parameters. Section 5.4 contains results for models that allow heterogeneity in the returns to schooling. Finally, Section 5.5 conducts a more in-depth analysis of the nature and degree of heterogeneity by examining the distribution of returns for various subgroups of the population.

5.1 Estimated Specifications

We estimate a total of fourteen specifications that are summarized in Table 2. We group the specifications as follows:

- **Group 1 [Specifications 1-2]:** Cross-section OLS and 2SLS regressions of log hourly earnings on schooling to verify the "IV > OLS" result commonly found in empirical studies (see Card, 2001 for a survey of these results). When age controls are included, we estimate the specification

$$y_i = c + x_i\beta + w_i'\gamma + a_i\rho_1 + a_i^2\rho_2 + u_i$$

  where w_i is a vector of demographic controls and a_i denotes the age of individual i. The age variables are included to account for the actual experience (we discuss this issue further below). Demographic controls include race, Hispanic status, foreign born status, marital status, state of residence during the SIPP survey and birth year. We also explore the sensitivity of the results to the omission of age controls by estimating the model

$$y_i = c + x_i\beta + w_i'\gamma + u_i$$

- **Group 2 [Specifications 3-7]:** Standard panel data specifications that include time and/or person fixed effects to control for unobserved heterogeneity. Here, we estimate five different specifications depending on the type of fixed effects included as well as whether age and demographic controls are included. The most general specification in this group takes the form

$$y_{it} = \delta_t + x_{it}\beta + w_{it}'\gamma + a_{it}\rho_1 + a_{it}^2\rho_2 + u_{it} \tag{17}$$

[19] Note that no tests to determine statistical significance have been performed except where indicated explicitly in the text or tables. Estimates of the return to schooling across the different sets of specifications listed here have not been tested to determine whether they are statistically different from one another.

where a_{it} denotes the age of individual i at period t. We consider the following variants of (17): (a) the age and demographic controls are excluded; (b) the age and demographic controls are replaced by a person fixed effect; (c) the demographic controls are excluded; (d) the age controls are excluded. Angrist and Newey (1991) consider a specification of the form

$$y_{it} = c_i + \delta_t + x_{it}\beta + w_{it}'\gamma + pe_{it}\rho_1 + pe_{it}^2\rho_2 + u_{it} \tag{18}$$

where pe_{it} denotes potential experience and is computed as $pe_{it} = a_{it} - x_{it} - 6$, where they define x_{it} as the highest grade completed. They estimate a reduced form schooling effect (expressed as a function of pe_{it} and a_{it}) based on the observation that the effect of schooling conditional on potential experience is not identified.[20] We present a derivation in the Appendix which shows that the effect of actual experience can be accounted for by including age and its square as controls as in (17).

- **Group 3 [Specifications 8-10]:** This group contains specifications that include interactive fixed effects while assuming that the regression coefficients are homogeneous. The nesting model takes the form

$$y_{it} = \delta_t + x_{it}\beta + a_{it}\rho_1 + a_{it}^2\rho_2 + \lambda_i' f_t + u_{it} \tag{19}$$

The following variants of (19) are considered: (a) the age controls are excluded; (b) the age controls are replaced by a person fixed effect.

- **Group 4 [Specifications 11-14]:** This group consists of specifications where the slope parameters are allowed to be individual specific. The general specification in this set is given by

$$y_{it} = x_{it}\beta_i + a_{it}\rho_{1i} + a_{it}^2\rho_{2i} + \lambda_i' f_t + u_{it} \tag{20}$$

We estimate three variants of (20): (a) the interactive fixed effects and age controls are replaced by a person fixed effect; (b) the person and interactive fixed effects are excluded; (c) the age controls are replaced by a person fixed effect.

[20] The returns to schooling literature often controls for potential experience, measured as (age−years of school−6), by assuming that individuals do not work while in school but do work during every other work-age year. This allows researchers to proxy for experience when no direct way to measure experience exists. We account for actual experience rather than potential experience because we can observe the accumulation of experience based on the presence of annual earnings and we specifically limit our sample to individuals who continue to work and earn income while increasing their schooling.

5.2 Cross-Section Estimates

Table 3 presents the cross-section OLS and 2SLS estimation results. Panel A reports the results for our main sample of analysis. Columns (1)-(6) report findings based on the cross-section at year 1990 while columns (7)-(9) report findings based on the cross-section at age 40. OLS results using the first cross-section indicate that the age controls have little impact on the estimated effect of schooling with a point estimate of about 9 percent in either case. The corresponding 2SLS point estimates are much larger when quarter of birth indicators are used as instruments. The estimated schooling effect depends crucially on the instruments used: when age controls are excluded, using the birth quarter as instruments results in a point estimate of about 28.6 percent while using the interactions of quarter of birth with year of birth as instruments yields an estimate of only 13.9 percent. The same pattern of results is observed for the cross-section at age 40 with the 2SLS point estimates exceeding the OLS estimate with the extent of the excess determined by the set of instruments employed.

Overall, these findings are in accordance with the literature summarized in Card (2001) which demonstrates the robustness of the "IV>OLS" result across different datasets as well as different instrument sets. For instance, the seminal study by Angrist and Krueger (1991) finds, based on the 1920-29 birth cohort using data on men from the 1970 Census, an OLS estimate of about 7 percent and a 2SLS estimate of about 10 percent when controlling for age and its square, race, marital status and urban residence.[21]

Panel B of Table 3 presents estimates based on the comparative sample from Panel B of Table 1. The OLS and 2SLS estimates are very similar to those in Panel A when individuals who have missing earnings data during school are included.[22] This helps alleviate concerns that our panel results are driven largely by sample selection effects.

[21] Our 2SLS estimates are slightly larger than those in the literature when only birth quarter is used as an instrument; OLS estimates generally range from 5 to 10 percent, while 2SLS estimates generally range from 10 to 16 percent (Card, 2001). The slightly larger 2SLS estimates could be due to the fact that we have a sample of continuous earners, which could make returns to schooling appear larger (Lazear, 1977; Card 1995); the fact that our sample is based on individuals born in more recent years than most of the literature and there is evidence that returns to schooling have been increasing over time (e.g., Card and Lemieux, 2001); the fact that our results are based on a different data source than most of the literature, and the only paper of which we are aware that has estimated returns to schooling based on SIPP and administrative data finds slightly larger OLS estimates than the literature (Chenevert et al., 2016); or an odd LATE interpretation for this sample.

[22] The number of observations in Panel B is slightly lower than in the summary statistics from Panel B of Table 1. This is because some individuals in the comparative sample have not yet completed their schooling and thus have missing earnings data at the time period of the cross-section analysis.

5.3 Pooled Estimates

The results from OLS and 2SLS estimation using panel data over 1978-2011 are presented in Table 4. Panel A results include demographic controls while Panel B results exclude these controls. Columns (1)-(3) report the OLS estimates while the 2SLS estimates are reported in columns (4)-(7). The following observations are readily apparent from these findings: (a) similar in spirit to the cross-section analysis, the OLS point estimates are smaller than the 2SLS point estimates across specifications; (b) the age controls only have a minor effect on the OLS estimate, irrespective of whether demographic controls are included while the 2SLS point estimates are noticeably smaller when age controls are included; (c) the demographic controls have little impact on the estimated schooling coefficient (OLS and 2SLS), once the age controls are included. Controlling for time and person fixed effects, Angrist and Newey (1991) obtain point estimates of the reduced form schooling effect in the range 3.4-9 percent across their estimation methods (OLS, 2SLS and its variants) while the range for our point estimates is 10.7-26.2 percent. Their results are, however, not directly comparable to ours, being based on a different dataset (NLSY). In addition to parameter estimates, Table 4 also reports the results of Pesaran's (2015) CD test for the presence of cross-section dependence for each estimated specification.[23] In all cases, the test provides evidence against no cross-section dependence (at the 1% level) which further motivates the use of the interactive fixed effects estimators.

Table 5 reports the results from estimating pooled specifications with interactive fixed effects. The estimators included are the IFE, CCEP and CCEP-2 estimators. Irrespective of whether one controls for interactive effects using principal components or cross-section averages of the observed variables, the point estimates are smaller in magnitude than the OLS and 2SLS estimates reported in Table 4. For instance, the IFE point estimate when age controls are included is about 4.2 percent while the corresponding OLS and 2SLS estimates are about 11.6 percent and 15.5 percent (or 21.5 percent if quarter of birth indicators are used as instruments), respectively. Under the assumption that the interactive effects specification represents the true model, the pattern of results suggests that the OLS and 2SLS estimates are both upward biased, with the magnitude of the 2SLS bias exceeding the OLS bias. This is consistent with the assumption that the IV approach suffers from poor instruments that are correlated with unobserved abilities or skills, which the interactive fixed effects specifications can account for. The CCEP point estimates are larger than the IFE estimates reflecting the difference in how the unobserved common factors are accounted for in the

[23] The test is based on estimated pairwise correlation coefficients between the pooled OLS/2SLS residuals for each pair of cross-section units. The test has a standard normal asymptotic distribution under the null hypothesis of no cross-section dependence.

two approaches. However, the CCEP-2 estimates that employ the estimated factors are much closer to the IFE estimates, especially so when age controls are included. Finally, an interesting pattern emerges across the three specifications (columns 1-3) when using the IFE estimate - the estimated number of common factors corresponding to the most general specification (column 3) is one less than that when a person and time fixed effect are included and two less than that when only a time fixed effect is included. This is precisely the pattern that one would expect a priori if the factors are able to pick up the components that are not controlled for in a particular specification. For example, the difference between the specifications in columns 2 and 3 amounts to the presence of a person-specific trend in the latter which is accounted for by the additional factor estimated for the specification with only time and person fixed effects.

Our interpretation of the interactive fixed effects structure as capturing unobserved skills or abilities hinges on the assumption that there are no suitable proxies to fully account for their effects. Alternatively, such a structure could be potentially capturing time-varying returns to time invariant individual-specific characteristics such as demographics or these characteristics could serve as useful proxies for individual skills or abilities. To investigate this possibility, we estimated the following specification with demographic-by-year fixed effects, denoted $d_i'\theta_t$, by OLS:

$$y_{it} = \delta_t + x_{it}\beta + w_{it}'\gamma + d_i'\theta_t + v_{it}$$

The estimates, reported in columns (1)-(3) of Appendix C, are only marginally smaller than those reported in columns (1)-(3) of Panel B in Table 4, with a reduction of about 10 percent for our preferred specification with age controls. These findings lend support to our interpretation of the factor loadings as skills and that interactive fixed effects are needed to fully model these skills.

As discussed in Section 3.2.1, the interactive fixed effects estimates can be used to obtain estimates of the OLS and 2SLS biases associated with each of the skill components. Table 6 shows the biases corresponding to the first four common factors for each of the IFE, CCEP and CCEP-2 estimates. The contribution of additional factors to the total bias (reported in column (5)) is marginal in all cases. For all three estimation approaches, the aggregate 2SLS bias exceeds the OLS bias across specifications with the magnitude of the excess being relatively greater for the IFE and two-step CCE approaches, consistent with the findings reported in Tables 4 and 5. While including age controls mitigate the biases to some extent, the magnitudes remain considerable even in this case. For instance, the aggregate OLS bias with age controls using the IFE approach accounts for about 64 percent of the estimated OLS effect of schooling (Table 4, column 3). The corresponding aggregate 2SLS bias accounts for about 73 percent of the estimated 2SLS effect of schooling (Table 4, column 7). Similar magnitudes are obtained when the biases are based on the two-step CCE approach.

The disaggregate bias estimates reveal some interesting patterns. First, the leading common component is the major contributor to the aggregate OLS bias, accounting for at least 50 percent of the bias across specifications/estimators and nearly all of the bias when only year fixed effects are used to control for unobserved heterogeneity. In contrast, the first two common components are important contributors to the 2SLS bias, with the first component being relatively more important when age controls are included and vice-versa. Notably, a sizeable negative bias component (corresponding to the second common factor) emerges in both OLS and 2SLS cases when age controls are included (the exception being the case where the 2SLS bias is computed using the IFE estimates). In the 2SLS case, this component makes a substantial contribution in the (one-step) CCE approach serving to reduce the resulting aggregate estimated bias sufficiently to a value smaller than the estimated bias using the IFE approach, even though the positive bias emanating from the first common component is larger using the former approach. The negative bias component can be interpreted as the presence of mechanical skills that are negatively correlated with schooling but make a positive contribution to earnings (Heckman, Lochner, and Todd, 2006; Prada and Urzua, 2017). Finally, it is useful to note that the 2SLS estimator is only successful at ameliorating the bias associated with the common components which only make a negligible contribution to the total bias (i.e., components other than the first two), at the expense of aggravating the bias in the two leading components. These results show that, assuming an underlying interactive factor structure, the consistent "IV $>$ OLS" finding in the literature could be due to the use of instruments that actually worsen the ability bias.

5.4 Mean Group Estimates

Table 7 presents results from estimating the specifications 11-14 in Table 2 (i.e., those corresponding to Group 4) that allow the slope parameters to be individual-specific. In addition to the CCEMG, CCEMG-2 and IFEMG estimators, we also include the OLSMG estimator that entails taking the average of the individual specific time series OLS regressions of log earnings on a constant and schooling. Note that a mean group 2SLS estimate cannot be computed since the instruments are time-invariant. To confirm the presence of heterogeneity, Table 7 also reports the results from conducting two slope homogeneity tests recently proposed by Ando and Bai (2015) and Su and Chen (2013).[24] Both tests provide evidence against the null of slope homogeneity at

[24]The Ando and Bai (2015) test is based on the (scaled) difference between the individual level estimates and the IFEMG estimate while the Su and Chen (2013) test is based on the Lagrange Multiplier (LM) principle that utilizes IFE residuals computed under the null of slope homogeneity. Both tests have a standard normal asymptotic null distribution. We refer the reader to the original articles for details.

the 1% significance level.

When the interactive effects are ignored, point estimates of the return to schooling are considerably larger - when age controls are included, the OLSMG point estimate is about 35 percent, which is more than ten times as large as the IFEMG (2.7 percent) and CCEMG-2 (2.9 percent) estimates and larger than three times the CCEMG estimate (9.7 percent). Consistent with the foregoing pooled results, the one-step CCE approach yields a larger point estimate of the average marginal returns to schooling relative to the IFE and two-step approaches which yield very similar estimates. Given that the pooled estimates exceed the corresponding mean group estimates for both the IFE and two-step CCE approaches, we should expect a positive correlation between the individual level estimate $\hat{\beta}_i$ and the weight on individual $i's$ return ω_i according to the heterogeneity bias analysis in Section 3.2.2. Indeed, the IFE-based correlations were estimated to be .009 and .005 with and without age controls, respectively, while the corresponding two-step CCE correlations were estimated as .019 and .022, respectively. The one-step CCE results were also in agreement with the predicted signs, except when age controls are included, although in this case the difference between the pooled and mean group estimate was rather small (.3%) [indeed smaller (in absolute value) than the difference between any other pair of estimates in Tables 5 and 7].[25] The pattern of findings for the estimated schooling effect obtained from the IFE and two-step CCE approaches therefore suggest that ignoring potential heterogeneity is likely to induce an upward bias in the parameter estimates. We also computed the CD test for cross-section dependence based on the OLSMG estimate and found evidence against no cross-section dependence for both specifications at the 1% level.[26]

As in the pooled case, we compute the biases associated with the OLSMG estimate using the CCEMG, CCEMG-2 and IFEMG estimates of the common structure. The findings are reported in Table 8. When age controls are excluded, the first common component is responsible for at least 80 percent of the aggregate bias across three estimation approaches. Consistent with the pooled results, the inclusion of age controls only alleviate the aggregate bias to a limited extent: the bias reduction using both the IFE and two-step approaches is about 22 percent while that based on the one-step CCE procedure is about 23 percent. In either case, the aggregate bias is very large: the aggregate OLSMG bias accounts for up to 95 percent of the estimated OLSMG effect of schooling in Table 7, depending on the specification. An important difference with the pooled bias results in Table 6 is that the biases associated with each of the skill components are now positive, regardless

[25]When age controls are excluded, $Corr(\hat{\beta}_i, \hat{\omega}_i) \approx -.054$ based on the one-step CCE approach, in accordance with $\hat{\beta}_{CCEP} < \hat{\beta}_{CCEMG}$. With age controls, $Corr(\hat{\beta}_i, \hat{\omega}_i) \approx -.008$ but $\hat{\beta}_{CCEP} > \hat{\beta}_{CCEMG}$.

[26]The results are available upon request.

of whether one controls for experience. This finding suggests that the emergence of a negative bias component in the pooled case might be a consequence of the failure to incorporate cross-sectional heterogeneity in the returns to schooling.

Finally, since the interactive effects framework allows for both individual slope heterogeneity and cross-sectional dependence modeled through a common factor structure, it is possible to obtain estimates of the biases emanating from each of the two sources. We can use the decomposition $\hat{\beta}_{POLS} - \hat{\beta}_{IFEMG} = \left(\hat{\beta}_{POLS} - \hat{\beta}_{IFE}\right) + \left(\hat{\beta}_{IFE} - \hat{\beta}_{IFEMG}\right)$, where $\hat{\beta}_{POLS}$ denotes the OLS estimate assuming a homogeneous slope parameter. The first term in the decomposition may be interpreted as the bias arising from ignoring the common factor structure while the second term denotes the bias from ignoring potential parameter heterogeneity. The results are shown in Figure 1. Based on the results for our preferred specification that includes age controls, we find $\hat{\beta}_{POLS} - \hat{\beta}_{IFEMG} \simeq 8.9$ percentage points, $\hat{\beta}_{POLS} - \hat{\beta}_{IFE} \simeq 7.4$ percentage points, $\hat{\beta}_{IFE} - \hat{\beta}_{IFEMG} \simeq 1.5$ percentage points. A similar calculation using the two-step CCE estimate yields $\hat{\beta}_{POLS} - \hat{\beta}_{CCEMG-2} \simeq 8.7$ percentage points, $\hat{\beta}_{POLS} - \hat{\beta}_{CCEP-2} \simeq 7.4$ percentage points, $\hat{\beta}_{CCEP-2} - \hat{\beta}_{CCEMG-2} \simeq 1.3$ percentage points. For the one-step CCE method, we obtain $\hat{\beta}_{POLS} - \hat{\beta}_{CCEMG} \simeq 1.9$ percentage points, $\hat{\beta}_{POLS} - \hat{\beta}_{CCEP} \simeq 1.6$ percentage points, $\hat{\beta}_{CCEP} - \hat{\beta}_{CCEMG} \simeq 0.3$ percentage points. For all three estimation approaches, the omitted ability bias captured using the interactive fixed effects structure appears to be the more important contributor to the total bias of the least squares estimator that does not incorporate the common factor structure or slope parameter heterogeneity.

5.5 Heterogeneity Analysis

Estimates of the return to schooling in Table 7 show the average of the individual returns across all individuals. This section discusses heterogeneity of the individual-level returns. We focus on the distributional characteristics of the individual returns, differences in mean returns across and within subgroups and characteristics associated with extreme returns. Most papers in the literature assume that the return to schooling is the same for all individuals, but there are exceptions (Harmon et al., 2003; Koop and Tobias, 2004; Henderson et al., 2011; Li and Tobias, 2011; Zhu, 2011). The results for heterogeneity across and within subgroups discussed below are most comparable to the results from Henderson et al. (2011). They use cross-section nonparametric kernel regression methods to study heterogeneity in returns and summarize the heterogeneity across and within subgroups, but their method does not address omitted ability bias.

5.5.1 Distribution of Individual Returns

Figure 2 shows the distribution of individual returns for each estimator based on kernel density plots.[27] The figure illustrates that there are large differences in returns across individuals. Most of the density associated with the heterogeneous OLS model falls between approximately a negative 50 percent return and a positive 100 percent return. The common factor models clearly shift the distribution to the left, which is consistent with evidence that the common factor models are removing positive bias in the OLS estimates due to unobserved multidimensional skills.

The common factor models place greater density immediately around the modal return, which is illustrated by the height of the density plots compared to OLS. The OLS returns are somewhat left-skewed compared to the common factor model returns. Finally, despite the relatively large difference between the CCEMG estimate and the IFEMG and CCEMG-2 estimates reported in Table 7, the three common factor model distributions look very similar; the difference in the CCEMG return appears to be due to relatively small differences along the left tail and right side of the distribution.

The most striking result from the figure is that each of the estimators shows a considerable fraction of individuals with negative returns to schooling. Overall, 13.0 percent of individuals have negative returns in the heterogeneous OLS model, 45.2 percent in the heterogeneous IFE model, 38.4 percent in the heterogeneous CCE model, and 45.9 percent in the heterogeneous CCE-2 model. The OLS fraction is similar to Henderson et al. (2011), who find that 15.2 percent of individuals who are White have negative returns to schooling. Heckman et al. (2017) and Prada and Urzua (2017), who use ability proxies from the NLSY to address ability bias, appear to find fractions of negative returns between the OLS and common factor models: Prada and Urzua (2017) report that 19 percent of individuals in their sample would have had higher annual earnings if they had decided not to attend a 4-year college. Heckman et al. (2017) do not report an exact fraction, but the log wage panel in their Figure 7 appears to indicate a fraction less than zero of approximately 20-40 percent.

5.5.2 Across-Group Heterogeneity

Table 9 reports the mean and variance of the individual returns separately by several subgroups: race (White, Black, other race), Hispanic status, foreign born status, birth cohort (born before 1950,

[27] The kernel density plots are based on a standard normal (Gaussian) kernel with a bandwidth of 0.1.

born 1950-1954, born 1955-1959, born after 1959), and highest education level completed.[28,29] Mean returns for individuals who are non-White are statistically tested against the mean for individuals who are White. For the other subgroups, the mean for each group is statistically tested against the mean for the group listed directly above it within each panel of the table.

Based on the OLSMG model, the mean return to schooling is statistically larger for individuals who are Black compared to White and for individuals born in later birth cohorts. According to highest completed education level, the mean of the individual returns is statistically larger for individuals with a high school degree compared to some college, statistically larger for individuals with some college compared to a bachelor's degree, and statistically larger for individuals with a graduate degree compared to a bachelor's degree. These results are similar to those from Henderson et al. (2011), who find that returns to schooling are larger for individuals who are Black, natives, individuals born in more recent years, and individuals with lower levels of educational attainment.

Mean individual returns from the heterogeneous common factor models are smaller than those from the OLSMG model for every subgroup, which is consistent with the main results discussed in the previous sections. In addition to smaller mean returns, the common factor models show three differences in the relative size of returns across subgroups compared to the OLSMG model: (1) There are no statistically significant differences by race; (2) CCEMG shows statistically smaller returns for Hispanic and foreign-born individuals; (3) There is a different pattern across highest level of education completed. Both OLSMG and the common factor models show the largest mean return to schooling for individuals who ultimately stop at high school, but they show different relative returns for other education levels: common factor models show the next largest mean returns for individuals whose highest achievement is a bachelor's degree or a graduate degree, whereas OLSMG shows the second largest returns for individuals who begin college but do not finish. The common factor model returns are statistically smaller for some college compared to high school (IFEMG and CCEMG), statistically larger for a bachelor's compared to some college (CCEMG), and statistically smaller for a graduate degree compared to a bachelor's (CCEMG).

The statistically larger returns for more recent birth cohorts, found across all four heterogeneous models, is consistent with evidence that returns to schooling have risen over time (Card and

[28]The CCE model from Pesaran (2006) makes a random coefficients assumption on the individual-level returns. This assumption only affects the CCE standard errors and therefore analysis of the mean and variance of individual-level returns by particular characteristics is feasible without violating assumptions of the model.

[29]Due to the limited sample size as explained in section 4.2, the results of non-white, Hispanic, and foreign born individuals should be interpreted with caution. It is possible that these groups in our sample have unique attributes and are not representative of the rest of the population or that we lack the statistical power to detect significant differences.

Lemieux, 2001).[30] With regard to the results across completed education levels, the OLSMG results suggest diminishing marginal returns to years of schooling, at least until graduate school. The common factor model results are more suggestive of "sheepskin effects" (Layard and Psacharopoulos, 1974; Hungerford and Solon, 1987; Jaeger and Page, 1996); if the value of additional years of school is partly related to the value of degree attainment rather than knowledge obtained in each year, then returns may be larger for individuals who complete bachelor's and graduate degrees than for individuals who drop out of college. Henderson et al. (2011) find evidence of diminishing marginal returns that are similar to those from the OLSMG model: larger returns to schooling for individuals with some college than individuals with a bachelor's or more.

5.5.3 Within-Group Heterogeneity

The heterogeneous models also allow for the analysis of heterogeneity within subgroups. Table 9 shows the variance of the individual returns within each subgroup. The common factor models show larger variance than OLSMG for every subgroup except for individuals born before 1950 (CCEMG). Larger variance suggests that, in addition to removing positive bias in the mean return as discussed above, accounting for multidimensional skills with time-varying prices that are potentially correlated with schooling also increases the overall dispersion of returns around the mean.

The common factor models change the relative variance across subgroups compared to OLSMG in two cases: (1) The heterogeneous common factor models generally show larger variance for more recent birth cohorts, whereas OLSMG shows smaller variance;[31] (2) The common factor models show much larger variance for individuals who only obtain a high school degree than any other education level, whereas OLSMG shows similar variance between these individuals and those with higher levels of education.

Table 10 reports the 25th, 50th, and 75th percentiles of the distribution of individual returns by subgroup. The common factor model estimators show smaller returns at each percentile, which is once again consistent with previous results. The difference between the 25th and 75th percentiles is often larger for OLSMG than the heterogeneous common factor models. This is inconsistent with the larger variance associated with the common factor models in Table 9 . However, this can be reconciled by analyzing the distributional plot in Figure 2. The common factor estimators place

[30] The differences across birth cohorts for CCEMG-2 do not show statistically significant differences when a given birth cohort is only tested against the cohort directly above it in the table, but statistically larger returns for more recent birth cohorts do exist when the 1955-1959 or after-1959 cohorts are tested against the before-1950 cohort.

[31] The one notable exception is that IFEMG shows much larger variance for individuals born before 1950 than any other cohort. But IFEMG still shows increasing variance over time for the subsequent cohorts.

relatively more density immediately around the mode than OLS, which produces a smaller range between the 25th and 75th percentiles than OLS. But the factor model estimators also have longer tails than OLS, which increases the overall variance.

5.5.4 Extreme Returns

Given that the results discussed above show significant heterogeneity in returns both across and within subgroups, Table 11 shows characteristics that are associated with being in the top 5 percent and bottom 5 percent of returns for each estimator. In addition to the characteristics discussed above, these tables also include whether the individual finished high school late (at age 20 or later), began college late (three or more years after finishing high school), and finished college late (did not obtain their bachelor's until age 26 or later).

Based on OLSMG, individuals in the top 5 percent of returns are statistically more likely to have begun college late, have finished college late, be Black, have been born after 1959, and have obtained only some college. They are also statistically less likely to be married at age 40, have been born in 1950-1954, and hold a bachelor's or graduate degree. Individuals in the bottom 5 percent of returns are statistically more likely to have finished high school late, have finished college late, have been born in 1950-1954, and hold a bachelor's or graduate degree. They are also statistically less likely to have begun college late, have been born in 1955-1959, have been born after 1959, or have obtained only some college. These results are fairly consistent with those from Henderson et al. (2011), who find that individuals born in more recent years and individuals who are Black are more likely to have returns in the top 5 percent, while individuals born in earlier years and individuals who are married are less likely to have returns in the top 5 percent.

Some aspects of the OLSMG results are intuitive. For example, individuals born in more recent birth cohorts being statistically more likely to have a top 5 percent return and statistically less likely to have a bottom 5 percent return is consistent with the literature that returns to schooling have increased over time. Other results are less intuitive, such as individuals who hold a bachelor's or graduate degree being statistically less likely to have a top 5 percent return and statistically more likely to have a bottom 5 percent return, compared to the opposite pattern for individuals with only some college.

The heterogeneous common factor models show that individuals in the top 5 percent of returns are statistically more likely to have begun college late, have finished college late (IFEMG and CCEMG-2), have finished high school late (CCEMG-2), and have been born after 1959. They are also statistically less likely to have been born in 1950-1954 (CCEMG-2), have been born in 1955-1959 (IFEMG and CCEMG), have obtained only some college (CCEMG only), have a bachelor's degree, and have a graduate degree (IFEMG and CCEMG-2). The CCEMG model also shows that individuals in the top 5 percent are statistically less likely to be a race other than White or Black, be Hispanic, or be foreign-born. Individuals in the bottom 5 percent are statistically more likely to have finished high school late (CCEMG), have begun college late (CCEMG and CCEMG-2), have finished college late, be Hispanic (IFEMG), have been born after 1959 (IFEMG and CCEMG-2), and have obtained only some college. They are also statistically less likely to have been married at age 40, have been born in 1955-1959 (CCEMG and CCEMG-2), have a bachelor's degree, and have a graduate degree (IFEMG and CCEMG-2).

The common factor model results seem to correct some of the less intuitive results from OLSMG. The common factor models show that individuals with a bachelor's or graduate degree are statistically less likely to end up in the bottom 5 percent of returns while those with only some college are statistically more likely to end up in the bottom 5 percent. This is the opposite of the OLSMG result and is consistent with the potential "sheepskin effects" discussed previously. The result that individuals born after 1959 are statistically more likely to end up in both the top and bottom 5 percent according to the common factor models, despite having larger mean returns in Table 9 , is consistent with the larger variance in returns for more recent birth cohorts.

Finally, all three common factor estimators find evidence that individuals who begin college late or finish college late are more likely to be in both the top and bottom 5 percent of returns. In addition to suggesting large within-group heterogeneity for these individuals, this result is also consistent with multiple potential selection biases that have different predictions for the return to schooling: (1) Some individuals who begin and finish college late may do so because of poor grades or lack of motivation, which could also be related to lower earnings after college; (2) Other individuals who begin and finish college late may do so because their realized outcomes in the labor market without a college degree suggested that they had the most to gain from continuing their education; (3) Still more individuals who begin and finish college late may have delayed because of large financial or psychic costs associated with attending college, in which case those who eventually went to and completed college must have had large potential returns in order to take on the large costs (Becker, 1964; Heckman, Lochner, and Todd, 2006).

6 Conclusion

This study explores the viability of an interactive fixed effects approach to estimating the returns to schooling employing a large panel dataset that links survey data with tax and benefit information obtained from administrative records. This research is possible due to the linking of SIPP survey data with administrative records from the Social Security Administration (SSA) and Internal Revenue Service (IRS). SIPP provides longitudinal education information, while administrative records provide a long history of high-quality earnings data. The generality of the interactive fixed effects approach over most existing approaches is apparent in at least three dimensions: (1) Unobserved ability is allowed to be multidimensional where each component is characterized by its own contribution to earnings with skill prices that can vary over time; (2) The endogeneity of schooling is accounted for through estimation of or proxying for the skill prices that is made possible by the high-dimensional nature of the the panel without the need to resort to external instruments or proxies for ability; (3) Individual-level heterogeneity in the returns to schooling can be accommodated that allows us to simultaneously address the twin sources of bias that can arise due to unmeasured skills (the omitted variable bias) and assuming that the marginal returns to schooling are homogeneous across individuals.

The estimates from our preferred specification indicate considerably lower average marginal returns to schooling compared to traditional methods such as ordinary least squares or two stage least squares. While both aforementioned sources of bias contribute to the aggregate least squares bias, our estimates point to a relatively more important role for the bias induced by omission of time-varying returns to skills. The two biases operate in the same direction serving to explain the gap in the heterogeneous interactive fixed effects estimates and the homogeneous panel OLS estimates. Our subgroup heterogeneity analysis suggests interesting differences among methods both within and across subgroups. For example, OLS or standard nonparametric regressions suggest the presence of diminishing marginal returns to schooling, at least until graduate school. In contrast, our preferred estimates are suggestive of "sheepskin effects" so that degree attainment can have an important impact in determining the value of additional years of schooling.

Several extensions of our analysis are in order. First, it would be interesting to investigate the extent of heterogeneity in returns at different quantiles of the earnings distribution using the quantile interactive effects approach recently developed by Harding and Lamarche (2014). Second, while our results indicate important differences both across and within subgroups, our sample only includes men. Analysis of heterogeneity from a gender standpoint is a promising avenue for future research. Third, our paper only considers cross-sectional heterogeneity but as the nonparametric analysis of Henderson et al. (2011) documents, returns vary not only across individuals but also across time. A limitation of our analysis in this context is that splitting the sample by time periods would leave us with relatively few observations in each subsample (splitting by, say, half would imply a time series dimension of seventeen for each subsample) to estimate the individual specific parameters. Fourth, our analysis assumes that the skill prices are homogeneous across individuals although they are allowed to vary over time. Heckman and Scheinkman (1987) find evidence in favor of a model where skill prices are sector-specific which suggests the presence of a grouped factor structure for earnings which allows heterogeneity in skill prices across sectors of the economy but possibly homogeneous for individuals within a particular sector. We leave analyses of these and related issues as possible directions for further research.

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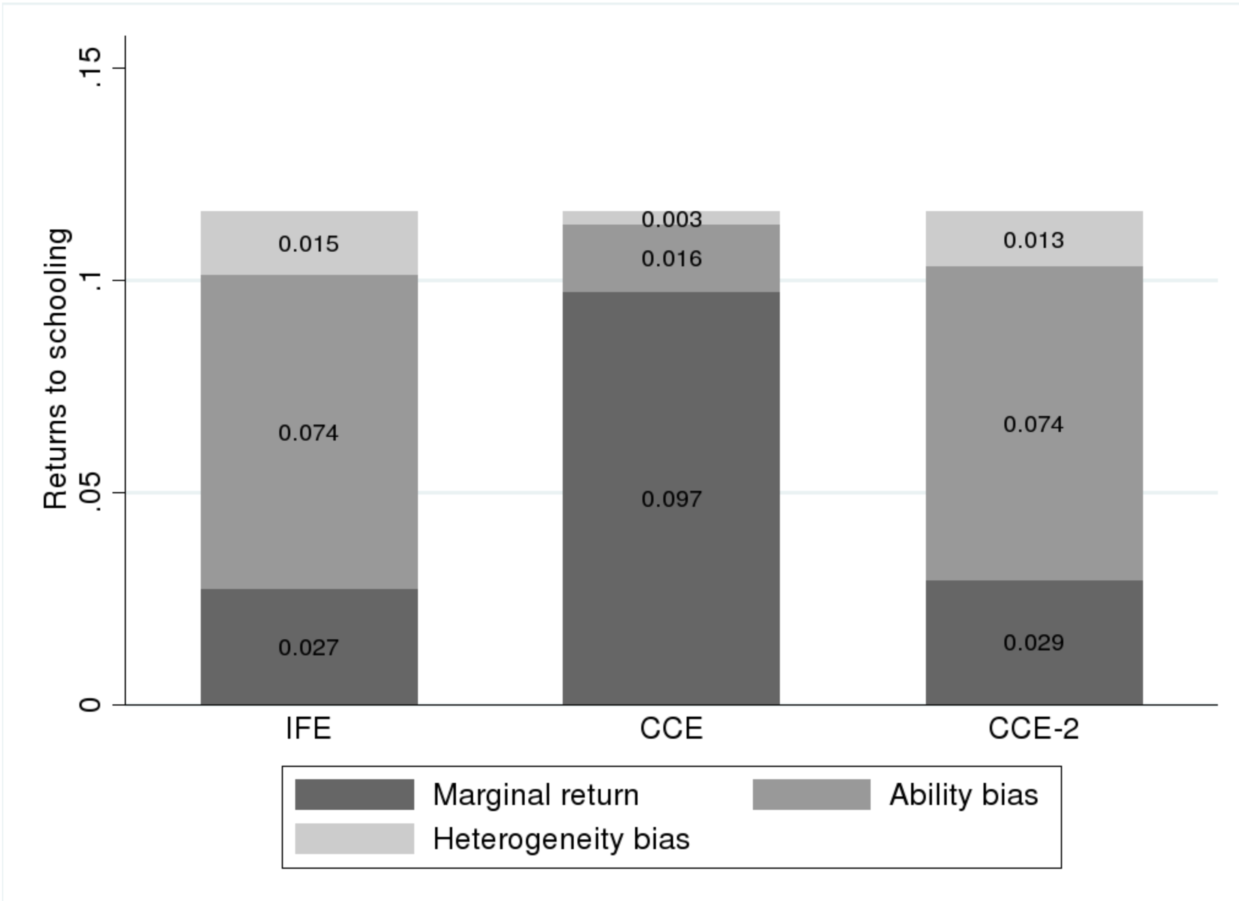
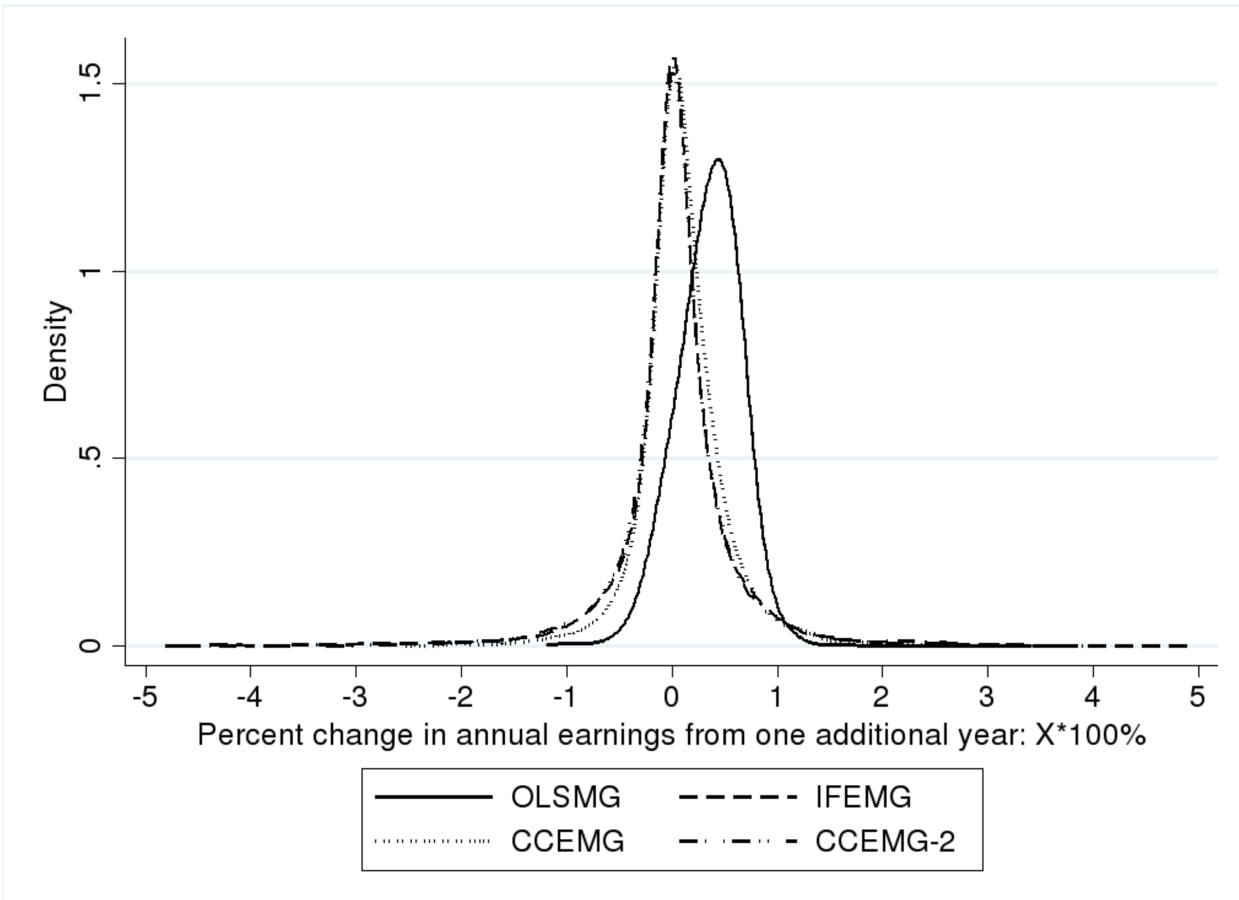
Figure 1: Bias Decomposition of Pooled OLS Estimate



Source: SIPP respondents linked to IRS and SSA data in the U.S. Census Bureau Gold Standard File.
Note: The total stacked bar for each estimator represents the pooled OLS estimate of the returns to schooling with year fixed effects and age controls, corresponding to column (3) in Panel B of Table 4. We use the decomposition $\hat{\beta}_{POLS} - \hat{\beta}_{IFEMG} = \left(\hat{\beta}_{POLS} - \hat{\beta}_{IFE}\right) + \left(\hat{\beta}_{IFE} - \hat{\beta}_{IFEMG}\right)$, where $\hat{\beta}_{POLS}$ denotes the OLS estimate assuming a homogeneous slope parameter. The first term in the decomposition may be interpreted as the bias arising from ignoring the common factor structure while the second term denotes the bias from ignoring potential parameter heterogeneity. The same calculations are applied using CCE and CCE-2 estimates.

Figure 2: Distribution of Marginal Returns to Schooling



Source: SIPP respondents linked to IRS and SSA data in the U.S. Census Bureau Gold Standard File.

Note: Each line is a kernel density plot of individual returns based on the heterogeneous model for the given estimator. Results are based on the specification with age controls, which corresponds to columns (2), (4), (6), and (8) in Table 7.

Table 1: Summary Statistics

	(1) Year 1990 sample	(2) Age 40 sample	(3) Panel sample
A. Sample of Analysis - with earnings-in-school restriction			
Annual earnings	43,920	59,270	50,660
	(35,540)	(61,390)	(76,890)
Years of school	14.60	14.83	14.46
	(2.076)	(2.004)	(2.138)
Age (in quarters)	133.2	160.3	151.2
	(16.65)	(1.715)	(42.63)
Married	0.750	0.805	0.698
	(0.433)	(0.396)	(0.459)
Black	0.052	0.052	0.052
	(0.222)	(0.222)	(0.222)
Other race	0.024	0.024	0.024
	(0.152)	(0.152)	(0.152)
Hispanic	0.039	0.039	0.039
	(0.193)	(0.193)	(0.193)
Foreign born	0.026	0.026	0.026
	(0.159)	(0.159)	(0.159)
Birth year	1957	1957	1957
	(4.160)	(4.160)	(4.160)
Observations	6,300	6,300	213,000
B. Comparative Sample - without earnings-in-school restriction			
Annual earnings	42,570	62,300	50,410
	(35,730)	(71,820)	(81,400)
Years of school	14.75	15.01	14.56
	(2.164)	(2.093)	(2.249)
Age (in quarters)	130.8	160.3	148.8
	(16.51)	(1.716)	(42.58)
Married	0.720	0.792	0.669
	(0.449)	(0.406)	(0.471)
Black	0.057	0.057	0.057
	(0.232)	(0.232)	(0.232)
Other race	0.039	0.039	0.039
	(0.194)	(0.194)	(0.194)
Hispanic	0.041	0.041	0.041
	(0.198)	(0.198)	(0.198)
Foreign born	0.052	0.052	0.052
	(0.222)	(0.222)	(0.222)
Birth year	1957	1957	1957
	(4.117)	(4.117)	(4.117)
Observations	10,100	10,100	342,000

Source: SIPP respondents linked to IRS and SSA data in the U.S. Census Bureau Gold Standard File.
Note: Each column reports averages and the standard deviations in parentheses for the given sample. Columns (1) and (2) report averages at given points in time from the panel sample. The panel sample used for analysis in Panel A includes males with earnings observations in each year from 1978-2011, variation in level of education during 1978-2011, between the ages of 16-65 during the entirety of 1978-2011, age 27 or older at the time of the SIPP survey, not currently enrolled in school at the time of the SIPP survey, and without any missing data. The sample includes a balanced panel of N=6,300 individuals over T=34 years. Annual earnings are adjusted for inflation to 1999 dollars. The comparative samples shown in Panel B are the same as those from above, except they also include individuals without earnings data while enrolled in school.

Table 2: Summary of Estimated Specifications

	Specification	Controls	Estimator
1.	$y_i = c + x_i\beta + w_i'\gamma + u_i$	Demographic controls	CSOLS, CS2SLS
2.	$y_i = c + x_i\beta + w_i'\gamma + a_i\rho_1 + a_i^2\rho_2 + u_i$	Demographic controls	CSOLS, CS2SLS
3.	$y_{it} = \delta_t + x_{it}\beta + u_{it}$	time fixed effects	POLS, P2SLS
4.	$y_{it} = c_i + \delta_t + x_{it}\beta + u_{it}$	time and person fixed effects	POLS
5.	$y_{it} = \delta_t + x_{it}\beta + a_{it}\rho_1 + a_{it}^2\rho_2 + u_{it}$	time fixed effects, age controls	POLS, P2SLS
6.	$y_{it} = \delta_t + x_{it}\beta + w_{it}'\gamma + u_{it}$	time fixed effects, Demographic controls	POLS, P2SLS
7.	$y_{it} = \delta_t + x_{it}\beta + w_{it}'\gamma + a_{it}\rho_1 + a_{it}^2\rho_2 + u_{it}$	time fixed effects, Demographic and age controls	POLS, P2SLS
8.	$y_{it} = \delta_t + x_{it}\beta + \lambda_i' f_t + u_{it}$	time and interactive fixed effects	IFE, CCEP, CCEP-2
9.	$y_{it} = c_i + \delta_t + x_{it}\beta + \lambda_i' f_t + u_{it}$	time, person and interactive fixed effects	IFE, CCEP, CCEP-2
10.	$y_{it} = \delta_t + x_{it}\beta + a_{it}\rho_1 + a_{it}^2\rho_2 + \lambda_i' f_t + u_{it}$	time and interactive fixed effects, age controls	IFE, CCEP, CCEP-2
11.	$y_{it} = c_i + x_{it}\beta_i + u_{it}$	person fixed effects	OLSMG
12.	$y_{it} = x_{it}\beta_i + a_{it}\rho_{1i} + a_{it}^2\rho_{2i} + u_{it}$	age controls	OLSMG
13.	$y_{it} = c_i + x_{it}\beta_i + \lambda_i' f_t + u_{it}$	person and interactive fixed effects	IFEMG, CCEMG, CCEMG-2
14.	$y_{it} = x_{it}\beta_i + a_{it}\rho_{1i} + a_{it}^2\rho_{2i} + \lambda_i' f_t + u_{it}$	interactive fixed effects and age controls	IFEMG, CCEMG, CCEMG-2

Note: The estimators are abbreviated as follows: (1) CSOLS: Cross-section ordinary least squares; (2) CS2SLS: Cross-section two stage least squares; (3) POLS: Panel ordinary least squares; (4) P2SLS: Panel two stage least squares; (5) IFE: pooled interactive fixed effects estimator [Bai, 2009]; (6) IFEMG: mean group interactive fixed effects estimator [Song, 2013]; (7) CCEP: common correlated effects pooled estimator [Pesaran, 2006]; (8) CCEMG: common correlated effects mean group estimator [Pesaran, 2006]; (9) OLSMG: mean group ordinary least squares estimator; (10) CCEP-2: two-step CCEP estimator [Pesaran, 2006]; (11) CCEMG-2: two-step CCEMG estimator [Pesaran, 2006].

Table 3: Cross-Section OLS and 2SLS Estimates of the Return to Schooling for Males

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	At year 1990						At age 40		
	OLS	OLS	2SLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS
A. Sample of Analysis - with earnings-in-school restriction									
Years of school	0.090***	0.090***	0.286***	0.231*	0.139***	0.137**	0.116***	0.312***	0.194***
	(0.005)	(0.005)	(0.102)	(0.126)	(0.043)	(0.045)	(0.006)	(0.088)	(0.040)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age & age-squared	No	Yes	No	Yes	No	Yes	No	No	No
Instrument			Quarter	Quarter	Quarter x year	Quarter x year		Quarter	Quarter x year
First stage F-stat			12.4	12.3	8.2	8.1		11.6	7.6
Observations	6,300	6,300	6,300	6,300	6,300	6,300	6,300	6,300	6,300
B. Comparative Sample - without earnings-in-school restriction									
Years of school	0.097***	0.097***	0.297***	0.234	0.169***	0.157**	0.136***	0.277***	0.175***
	(0.005)	(0.005)	(0.110)	(0.155)	(0.043)	(0.037)	(0.004)	(0.082)	(0.040)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age & age-squared	No	Yes	No	Yes	No	Yes	No	No	No
Instrument			Quarter	Quarter	Quarter x year	Quarter x year		Quarter	Quarter x year
First stage F-stat			15.9	15.6	10.09	10.0		15.5	9.8
Observations	9,900	9,900	9,900	9,900	9,900	9,900	10,000	10,000	10,000

Source: SIPP respondents linked to IRS and SSA data in the U.S. Census Bureau Gold Standard File.

Note: The dependent variable is the log of annual W-2 earnings or self-employment earnings. Columns (1)-(6) are based on a cross-section in 1990. Columns (7)-(9) are based on a cross-section at age 40. Demographic controls include race, Hispanic status, foreign born status, marital status, state of residence during the SIPP survey, and birth year. Years of school is instrumented for with quarter of birth indicator variables in columns (3), (4), and (8) and with quarter of birth indicator variables interacted with year of birth indicator variables in columns (5), (6), and (9). Panel A shows the results from the sample of analysis where the earnings-in-school restriction is imposed. The analysis in Panel B is the same as Panel A, except based on the comparative sample shown in Panel B of Table 1. The number of observations is slightly less than the number reported in Panel B of Table 1 because some individuals in the comparative sample had not yet finished schooling and had missing earnings data at the point of the cross-section time period. Standard errors are clustered at the state level and shown in parentheses. Significance is as follows: one-percent=***, five-percent=**, and ten-percent=*.

Table 4: Panel OLS and 2SLS Estimates of the Return to Schooling for Males

	(1) OLS	(2) OLS	(3) OLS	(4) 2SLS	(5) 2SLS	(6) 2SLS	(7) 2SLS
A. with demographic controls							
Years of school	0.108***		0.107***	0.262***	0.213**	0.185***	0.167***
	(0.004)		(0.004)	(0.082)	(0.096)	(0.030)	(0.033)
Age & age-squared	No		Yes	No	Yes	No	Yes
Year FE	Yes		Yes	Yes	Yes	Yes	Yes
Instrument				Quarter	Quarter	Quarter x year	Quarter x year
First stage F-stat				353.9	371.5	246.6	259.8
CD test stat	139.3		136.9	85.3	97.5	109.7	115.0
Observations	213,000		213,000	213,000	213,000	213,000	213,000
B. without demographic controls							
Years of school	0.130***	0.142***	0.116***	0.235**	0.215**	0.237***	0.155***
	(0.004)	(0.005)	(0.004)	(0.105)	(0.102)	(0.008)	(0.016)
Age & age-squared	No	No	Yes	No	Yes	No	Yes
Person FE	No	Yes	No	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Instrument				Quarter	Quarter	Quarter x year	Quarter x year
First stage F-stat				854.9	1,009	376.7	410.7
CD test stat	129.2	124.0	134.1	89.4	96.9	88.9	120.0
Observations	213,000	213,000	213,000	213,000	213,000	213,000	213,000

Source: SIPP respondents linked to IRS and SSA data in the U.S. Census Bureau Gold Standard File.
Note: Panel A replicates the results from Table 3, except with panel data on earnings and education from 1978-2011 rather than point-in-time cross-section data. Panel B is the same as panel A, but without demographic controls. A specification with person fixed effects is also added in column (2).

Table 5: Common Factor Model Estimates of the Return to Schooling for Males - Pooled Model

	(1) IFE	(2) IFE	(3) IFE	(4) CCEP	(5) CCEP	(6) CCEP	(7) CCEP-2	(8) CCEP-2	(9) CCEP-2
Years of school	0.033***	0.033***	0.042***	0.087***	0.076***	0.100***	0.041***	0.041***	0.042***
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.006)	(0.005)	(0.005)
Age & age-squared	No	No	Yes	No	No	Yes	No	No	Yes
Person FE	No	Yes	No	No	Yes	No	No	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	213,000	213,000	213,000	213,000	213,000	213,000	213,000	213,000	213,000

Source: SIPP respondents linked to IRS and SSA data in the U.S. Census Bureau Gold Standard File.
Note: Columns (1)-(3) are based on Interactive Fixed Effects (IFE) estimates (Bai, 2009) with 9, 8 and 7 factors, respectively, selected by the IC_{p1} procedure in Bai and Ng (2002). Columns (4)-(6) are based on Common Correlated Effects pooled (CCEP) estimates (Pesaran, 2006). Columns (7)-(9) are based on the two-step CCE procedure with 8, 7, and 8 factors, respectively, selected by the IC_{p1} procedure in Bai and Ng (2002) applied to residuals based on the CCEP estiamtes. IFE and CCE standard errors are calculated following Bai (2009) and Pesaran (2006), respectively. Significance is as follows: one-percent=***, five-percent=**, and ten-percent=*.

Table 6: Bias Associated with OLS and 2SLS Estimates, Due to Common Factor Structure

	(1) Factor skill 1	(2) Factor skill 2	(3) Factor skill 3	(4) Factor skill 4	(5) All others	(6) Total
IFE						
A. Covariates: Year fixed effects						
OLS	0.081	0.008	0.004	0.002	0.002	0.097
2SLS	0.100	0.107	-0.002	-0.0002	-0.001	0.204
B. Covariates: Person and year fixed effects [1]						
OLS	0.062	0.020	0.014	0.006	0.008	0.110
C. Covariates: Year fixed effects and age controls						
OLS	0.073	-0.008	0.005	0.002	0.002	0.074
2SLS	0.083	0.025	0.003	0.00003	0.001	0.113
CCEP						
A. Covariates: Year fixed effects						
OLS	0.040	0.001	0.002	0.001	0.0001	0.044
2SLS	0.050	0.102	-0.003	0.0001	0.0001	0.150
B. Covariates: Person and year fixed effects						
OLS	0.045	0.013	0.008	0.003	0.002	0.071
C. Covariates: Year fixed effects and age controls						
OLS	0.035	-0.021	0.002	0.001	0.0004	0.017
2SLS	0.216	-0.162	0.0003	0.00003	-0.0004	0.054
CCEP-2						
A. Covariates: Year fixed effects						
OLS	0.074	0.007	0.004	0.002	0.001	0.089
2SLS	0.088	0.110	-0.003	0.0001	0.0001	0.196
B. Covariates: Person and year fixed effects						
OLS	0.058	0.020	0.013	0.005	0.006	0102
C. Covariates: Year fixed effects and age controls						
OLS	0.081	-0.015	0.005	0.002	0.002	0.075
2SLS	0.267	-0.152	0.0001	-0.00005	-0.0003	0.113

Source: SIPP respondents linked to IRS and SSA data in the U.S. Census Bureau Gold Standard File.
Note: Bias estimates for OLS are based on the part of years of school that is unexplained by the covariates listed in the panel title. Similarly, bias estimates for 2SLS are based on the part of quarter of birth indicators interacted with year of birth indicators that is unexplained by the other covariates listed in the panel title. The OLS and 2SLS estimates correspond to the specifications in Table 4 Panel B that include the covariates listed in the panel title. The common factors in the IFE panels are based on the IFE results, and in the CCE panels are based on the principal components procedure applied to residuals based on the CCEP estiamtes in Table 5 that correspond to the specifications in the panel titles. Column (5) includes common factors up to 9 in the IFE panel (8 in the CCE panels) for the specifications with only year effects, 8 in the IFE panel (7 in the CCE panels) for the specifications with person and year effects and 7 in the IFE panel (8 in the CCE panels) for the specifications with age controls.
[1] The discrepancy of the total bias (0.110 versus 0.109 which is the difference between OLS estimate in Table 4 Panel B column (2) and IFE estimate in Table 5 column (2)) is due to rounding.

Table 7: Common Factor Model Estimates of the Return to Schooling for Males - Heterogeneous Model

	(1) OLSMG	(2) OLSMG	(3) IFEMG	(4) IFEMG	(5) CCEMG	(6) CCEMG	(7) CCEMG-2	(8) CCEMG-2
Years of school	0.444***	0.353***	0.025**	0.027***	0.105***	0.097***	0.028***	0.029***
	(0.005)	(0.004)	(0.011)	(0.008)	(0.005)	(0.005)	(0.007)	(0.007)
Age & age-squared	No	Yes	No	Yes	No	Yes	No	Yes
Su-Chen slope test			21.9	36.9	24.6	20.7	24.3	19.6
Ando-Bai slope test			741.0	64,760.0	334.9	778.8	631.5	782.0
Observations	213,000	213,000	213,000	213,000	213,000	213,000	213,000	213,000

Source: SIPP respondents linked to IRS and SSA data in the U.S. Census Bureau Gold Standard File.
Note: Each estimator is replaced with a version that allows regression coefficients to vary across individuals (Pesaran and Smith, 1996; Pesaran, 2006; Song, 2013). The individual-level regression coefficients are then averaged across all individuals to produce a "mean group" (MG) estimate. IFEMG estimates are based on 9 and 7 factors for columns (3) and (4), respectively, selected by the IC_{p1} procedure in Bai and Ng (2002). Two-step CCE estiamtes are based on 7 and 8 factors for columns (7) and (8), respectively, selected by the IC_{p1} procedure in Bai and Ng (2002) applied to residuals based on the CCEMG estimates. The Su-Chen and Ando-Bai slope homogeneity tests are based on Su and Chen (2013) and Ando and Bai (2015).

Table 8: Bias Associated with Heterogeneous Model OLS Estimates, Due to Common Factor Structure

	(1) Factor skill 1	(2) Factor skill 2	(3) Factor skill 3	(4) Factor skill 4	(5) All others	(6) Total
IFEMG						
A.Covariates: Person fixed effect						
OLSMG	0.389	0.019	0.003	0.003	0.005	0.419
B. Covariates: Age controls						
OLSMG	0.283	0.019	0.018	0.004	0.003	0.326
CCEMG						
A. Covariates: Person fixed effect						
OLSMG	0.304	0.019	0.017	0.005	0.001	0.345
B. Covariates: Age controls						
OLSMG	0.188	0.043	0.027	0.007	0.001	0.265
CCEMG-2						
A. Covariates: Person fixed effect						
OLSMG	0.343	0.026	0.030	0.008	0.009	0.416
B. Covariates: Age controls						
OLSMG	0.201	0.064	0.025	0.015	0.018	0.324

Source: SIPP respondents linked to IRS and SSA data in the U.S. Census Bureau Gold Standard File.

Note: Bias estimates are based on the common factor estimates from the heterogeneous factor model results in Table 7. The CCE common component estimates are based on applying principal components to $\eta_{it} = y_{it} - x_{it}\hat{\beta}_{CCE}$. Column (5) includes common factors up to 9 in the IFE panel (7 in the CCE panels) for the specifications with person fixed effects and 7 in the IFE panel (8 in the CCE panels) for the specifications with age controls.

Table 9: Mean and Variance of Heterogeneous Model Estimates by Characteristic Group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLSMG		IFEMG		CCEMG		CCEMG-2		Group
	Mean	Variance	Mean	Variance	Mean	Variance	Mean	Variance	Size
A. Race									
White	0.350	0.095	0.028	0.386	0.098	0.170	0.030	0.313	5,800
Black	**0.395**	0.100	0.024	0.397	0.089	0.149	0.035	0.320	350
Other race	0.364	0.092	-0.008	0.276	0.066	0.115	0.010	0.256	150
B. Hispanic status									
Non-Hispanic	0.353	0.095	0.028	0.387	0.099	0.167	0.031	0.309	6,000
Hispanic	0.345	0.098	0.002	0.312	**0.051**	0.178	-0.015	0.389	250
C. Foreign born status									
Native	0.354	0.095	0.026	0.385	0.098	0.168	0.030	0.314	6,100
Foreign Born	0.327	0.092	0.054	0.353	**0.041**	0.159	0.021	0.228	150
D. Birth cohort									
Born before 1950	0.160	0.136	-0.033	0.640	0.005	0.131	-0.010	0.207	500
Born 1950-1954	**0.299**	0.102	0.003	0.313	**0.044**	0.158	0.020	0.263	1,200
Born 1955-1959	**0.374**	0.084	0.013	0.341	**0.089**	0.143	0.026	0.284	2,600
Born after 1959	**0.405**	0.080	**0.073**	0.413	**0.160**	0.206	0.049	0.401	2,000
E. Highest education level completed									
High school	0.538	0.068	0.120	0.762	0.202	0.357	0.049	0.678	850
Some college	**0.458**	0.093	**0.001**	0.418	**0.042**	0.164	0.013	0.351	2,500
Bachelor's	**0.191**	0.057	0.023	0.216	**0.129**	0.076	0.034	0.139	1,700
Graduate	**0.240**	0.077	0.022	0.285	**0.091**	0.160	0.042	0.225	1,200

Source: SIPP respondents linked to IRS and SSA data in the U.S. Census Bureau Gold Standard File.

Note: Means and variances reported in the table are characteristic-specific summary statistics of the individual-level coefficients from the heterogeneous model results in Table 7. Estimates are based on the specifications from Table 7 that include age and age-squared. Bold numbers indicate means that are statistically different than within-panel counterparts at at least the ten-percent level. For Panel A, mean returns for individuals who are Black and other races are both tested against the mean for individuals who are White. For the remaining panels, each subgroup mean is tested against the subgroup mean directly above it, beginning with the second subgroup listed. For example, in Panel D, the mean return for individuals born in 1950-1954 is tested against the mean for individuals born before 1950, the mean for individuals born in 1955-1959 is tested against the mean for individuals born in 1950-1954, and the mean for individuals born after 1959 is tested against the mean for individuals born in 1955-1959.

Table 10: Quantiles of Heterogeneous Model Estimates by Characteristic Group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	OLSMG			IFEMG			CCEMG			CCEMG-2		
	p25	p50	p75	p25	p50	p75	p25	p50	p75	p25	p50	p75
All individuals	0.160	0.378	0.563	-0.126	0.021	0.208	-0.079	0.056	0.263	-0.138	0.020	0.202
A. Race												
White	0.158	0.375	0.561	-0.127	0.022	0.210	-0.079	0.056	0.260	-0.139	0.020	0.203
Black	0.213	0.426	0.616	-0.113	0.011	0.196	-0.082	0.061	0.272	-0.140	0.015	0.199
Other race	0.180	0.394	0.549	-0.097	0.016	0.152	-0.071	0.052	0.248	-0.112	0.014	0.167
B. Hispanic status												
Non-Hispanic	0.161	0.378	0.563	-0.126	0.022	0.208	-0.078	0.057	0.264	-0.136	0.020	0.204
Hispanic	0.122	0.371	0.575	-0.143	0.018	0.191	-0.105	0.046	0.218	-0.226	-0.002	0.173
C. Foreign born status												
Native	0.161	0.378	0.564	-0.126	0.022	0.208	-0.079	0.057	0.265	-0.139	0.020	0.202
Foreign Born	0.104	0.363	0.545	-0.120	0.008	0.145	-0.083	0.020	0.189	-0.114	0.011	0.181
D. Birth cohort												
Born before 1950	-0.032	0.158	0.371	-0.084	0.007	0.112	-0.098	0.008	0.126	-0.121	0.001	0.126
Born 1950-1954	0.099	0.337	0.515	-0.118	0.004	0.134	-0.098	0.022	0.148	-0.112	0.009	0.145
Born 1955-1959	0.195	0.391	0.568	-0.131	0.018	0.187	-0.079	0.052	0.245	-0.143	0.018	0.189
Born after 1959	0.215	0.435	0.607	-0.140	0.052	0.303	-0.058	0.130	0.361	-0.160	0.040	0.275
E. Highest education level completed												
High school	0.424	0.578	0.694	-0.198	0.082	0.481	-0.105	0.157	0.489	-0.282	0.032	0.397
Some college	0.311	0.489	0.636	-0.142	0.014	0.186	-0.125	0.032	0.210	-0.159	0.011	0.194
Bachelor's	0.025	0.209	0.359	-0.107	0.019	0.185	-0.032	0.089	0.276	-0.105	0.018	0.185
Graduate	0.058	0.260	0.435	-0.096	0.019	0.157	-0.062	0.048	0.209	-0.091	0.029	0.175

Source: SIPP respondents linked to IRS and SSA data in the U.S. Census Bureau Gold Standard File.
Note: Quantiles reported in the table are characteristic-specific summary statistics of the individual-level coefficients from the heterogeneous model results in Table 7. Estimates are based on the specifications from Table 7 that include age and age-squared. p25 = 25th percentile, p50 = 50th percentile, p75 = 75th percentile. Sample sizes for each group are shown in Table 9.

Table 11: Percentage Difference in Characteristics Associated with Extreme Returns

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Top 5 percent of returns				Bottom 5 percent of returns			
	OLSMG	IFEMG	CCEMG	CCEMG-2	OLSMG	IFEMG	CCEMG	CCEMG-2
Married at 40	**-0.077**	-0.033	-0.023	-0.002	-0.006	**-0.043**	**-0.049**	**-0.060**
Finished HS late	0.026	-0.008	-0.018	**0.032**	**0.093**	-0.008	**0.042**	-0.018
Began college late	**0.306**	**0.132**	**0.127**	**0.096**	**-0.061**	0.035	**0.085**	**0.094**
Finished college late	**0.283**	**0.069**	0.041	**0.066**	**0.081**	**0.049**	**0.126**	**0.105**
Black	**0.030**	-0.001	-0.021	-0.011	0.003	0.006	0.012	-0.001
Other race	-0.001	-0.008	**-0.015**	-0.008	-0.005	0.005	-0.005	0.005
Hispanic	0.003	-0.004	**-0.020**	-0.004	-0.001	**0.026**	0.016	0.017
Foreign born	-0.014	-0.012	**-0.017**	-0.004	-0.001	-0.011	-0.004	-0.004
Born 1950-1954	**-0.067**	-0.034	-0.014	**-0.038**	**0.151**	-0.028	0.033	-0.027
Born 1955-1959	0.025	**-0.054**	**-0.073**	-0.023	**-0.130**	-0.023	**-0.067**	**-0.056**
Born after 1959	**0.093**	**0.129**	**0.124**	**0.099**	**-0.149**	**0.062**	0.008	**0.090**
Some college	**0.256**	-0.034	**-0.070**	0.010	**-0.144**	**0.033**	**0.147**	**0.055**
Bachelor's	**-0.271**	**-0.123**	**-0.160**	**-0.143**	**0.143**	**-0.096**	**-0.217**	**-0.156**
Grad degree	**-0.151**	**-0.060**	0.001	**-0.030**	**0.095**	**-0.054**	-0.020	**-0.063**

Source: SIPP respondents linked to IRS and SSA data in the U.S. Census Bureau Gold Standard File.

Notes: Each entry shows the difference in the mean value for the row characteristic between individuals in the top (bottom) 5 percent and the bottom (top) 95 percent of individual-level returns for the estimator listed in the column heading. Specifically, the average of each characteristic for individuals in the bottom (top) 95 percent is subtracted from the average of each characteristic for individuals in the top (bottom) 5 percent and a t-test for statistical significance is performed. Bold numbers indicate statistical significance at at least the ten-percent level. The results for each estimator correspond to the specifications from Table 7 with age controls included.

Appendices

A Accounting for Experience

Consider the pooled specification

$$y_{it} = \delta_t + x_{it}\beta + e_{it}\rho_1 + e_{it}^2\rho_2 + \lambda_i{}' f_t + u_{it} \tag{A.1}$$

where e_{it} denotes actual experience and x_{it} denotes schooling. Let $e_{it} = e_{i0} + t$, where e_{i0} is initial experience and t is the time trend. Therefore,

$$y_{it} = \delta_t + x_{it}\beta + (e_{i0} + t)\rho_1 + (e_{i0} + t)^2\rho_2 + \lambda_i{}' f_t + u_{it}$$

or,

$$y_{it} = (e_{i0}\rho_1 + e_{i0}^2\rho_2) + (2e_{i0}\rho_2)t + (\rho_1 t + \rho_2 t^2 + \delta_t) + x_{it}\beta + \lambda_i{}' f_t + u_{it}$$

or,

$$y_{it} = \widetilde{\rho}_{1i} + \widetilde{\rho}_{2i}t + \widetilde{\delta}_t + x_{it}\beta + \lambda_i{}' f_t + u_{it} \tag{A.2}$$

where

$$\begin{aligned} \widetilde{\rho}_{1i} &= e_{i0}\rho_1 + e_{i0}^2\rho_2,\ \widetilde{\rho}_{2i} = 2e_{i0}\rho_2 \\ \widetilde{\delta}_t &= \rho_1 t + \rho_2 t^2 + \delta_t \end{aligned}$$

Thus, from $(A.2)$ in the pooled model, besides time fixed effect, we should include a person fixed effect and person-specific linear trend, which is equivalent to a pooled model that includes age and age-squared terms instead of the person fixed effect and person-specific linear trend.

In the heterogeneous model,

$$y_{it} = x_{it}\beta_i + e_{it}\rho_{1i} + e_{it}^2\rho_{2i} + \lambda_i{}' f_t + u_{it}$$

or

$$y_{it} = \breve{\rho}_{1i} + \breve{\rho}_{2i}t + \rho_{2i}t^2 + x_{it}\beta_i + \lambda_i{}' f_t + u_{it} \tag{A.3}$$

where

$$\breve{\rho}_{1i} = e_{i0}\rho_{1i} + e_{i0}^2\rho_{2i},\ \breve{\rho}_{2i} = \rho_{1i} + 2e_{i0}\rho_{2i}$$

From $(A.3)$, we should include a person fixed effect, person-specific quadratic trend, which is equivalent to a heterogeneous specification that includes age and age-squared terms instead of the person fixed effect and person-specific quadratic trend.

B Bias in the OLS Mean Group [OLSMG] Estimator

The aggregate bias in the OLSMG estimator (based on the IFE approach) can be expressed as

$$\begin{aligned}\hat{\beta}_{OLSMG}-\hat{\beta}_{IFEMG} &= N^{-1}\sum_i\left\{\left(\sum_t X_{it}^2\right)^{-1}\sum_t X_{it}\hat{\lambda}_i'\hat{f}_t\right\} \\ &= \sum_{j=1}^{r}\left[N^{-1}\sum_i\left\{\left(\sum_t X_{it}^2\sum_t X_{it}\hat{\lambda}_{ji}\hat{f}_{jt}\right)^{-1}\right\}\right]\end{aligned} \tag{A.4}$$

assuming r common factors. In $(A.4)$, $\hat{\lambda}_i=(\hat{\lambda}_{1i},\hat{\lambda}_{2i},...,\hat{\lambda}_{ri})'$ so that $\hat{\lambda}_{ji}$ represents the j-th factor loading for individual i. The contribution of the j-th factor to the aggregate bias is therefore

$$N^{-1}\sum_i\left\{\left(\sum_t X_{it}^2\sum_t X_{it}\hat{\lambda}_{ji}\hat{f}_{jt}\right)^{-1}\right\}$$

For the CCE approach, since the factors are not directly estimated, we follow a two-step procedure to estimate the component-specific biases as described in Section 3. The only difference is that the residuals in the first step are now computed using $\hat{\beta}_{CCEMG}$.

C Time-Varying Returns to Demographics as Proxies for Interactive Fixed Effects

	(1) OLS	(2) OLS	(3) OLS
Years of School	0.105***	0.116***	0.104***
	(0.004)	(0.004)	(0.004)
Age & age-squared	No	No	Yes
Person FE	No	Yes	No
Year FE	Yes	Yes	Yes
Demographics-by-Year FE	Yes	Yes	Yes
CD test stat	112.1	110.8	112.2
Observations	213,000	213,000	213,000

Source: SIPP respondents linked to IRS and SSA data in the U.S. Census Bureau Gold Standard File.
Note: Columns (1)-(3) are identical to columns (1)-(3) in Panel B of Table 4, except with demographic-by-year fixed effects included. These additional fixed effects are intended to proxy for the interactive fixed effects structure. That is, whereas a general version of the pooled interactive fixed effects approach estimates $y_{it}=\delta_t+x_{it}\beta+w_{it}'\gamma+\lambda_i'f_t+u_{it}$, here we estimate $y_{it}=\delta_t+x_{it}\beta+w_{it}'\gamma+d_i'\theta_t+v_{it}$. The demographic variables included in d_i are race, Hispanic status, foreign born status, marital status, birth year, and state of residence in the SIPP survey.