Three Essays on
Career and Education Choices

by

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A dissertation submitted in partial fulfillment
of the requirement for the degree of
Doctor of Philosophy
(Economics)
in The University of Michigan
2012

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To my parents, for love, inspiration, and every kind of support.

Acknowledgements

I thank my committee members, Matthew Shapiro, Jeff Smith, Charlie Brown, and Tyler Shumway for extensive discussion, advice, and support. I am particularly grateful to my co-chairs, Matthew Shapiro and Jeff Smith, for years of collaboration and friendship. Chapter 4 of this dissertation is co-authored with Jeff Smith. For helpful comments, I thank other members of the University of Michigan faculty, including Rudiger Bachmann, John Bound, Sue Dynarski, Miles Kimball, Dmitriy Stolyrov, my classmates including Matthew Backus, Cynthia Doniger, Brad Hershbein, David Ratner, Ryan Nunn, and Gabe Ehrlich, and also Roc Armenter, Dan Black, William Johnson, Sarah Turner, and seminar participants at the University of Michigan's macro and labor seminars, the Federal Reserve Board of Governors, and the Federal Reserve Bank of Philadelphia. The Institute for Social Research and Rackham Graduate School at the University of Michigan and the Federal Reserve Board of Governors provided valuable financial support during the development of this dissertation.

Table of Contents

List of Tables

List of Figures

Abstract

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Early in life, people make education and career decisions that affect their income and wellbeing for the rest of their lives. Understanding how individuals make these human capital investments helps economists evaluate the efficiency and equity of individual sorting into schools and occupations and predict the pace of labor market adjustment following changes in labor demand. The second chapter of this dissertation estimates the relationship between earnings uncertainty and expected earnings across occupations. Rational, risk-averse workers require higher average compensation to enter occupations where they face greater uncertainty about lifetime earnings. Compensation for earnings risk explains 17% of the differences in average earnings across occupations, but only a small share of total earnings inequality. Lifetime earnings risk, which is largely uninsurable, creates inefficiencies in the labor market: products become more expensive to cover this compensation, but workers are no happier than they would be with lower, safer earnings. Moreover, workers sort into occupations partially based on their

preferences for risk, rather than their relative skills. The third chapter estimates the responsiveness of college enrollment decisions to changes in the relative average earnings of workers with and without a college degree. Growth in the college earnings premium can explain more than half of the 10 percentage point rise from 1980 to 2002 in four-year college enrollment for men. As the relative supply of workers with a college degree rises, some of the recent rise in their relative earnings should be reversed. The fourth chapter studies the causes of mismatch between student ability and college quality, measuring college quality with peer student ability and resources per student. Additional wealth and information about college lower the probability that a student will attend a college of low quality relative to their ability and raise the probability that she will attend a relatively high quality college. Programs that provide information about college to less informed students may increase the equity of student sorting into colleges. However, if all well-informed students seek to attend the highest quality colleges, only increasing the overall quality of the college stock can improve welfare.

Chapter 1

Introduction

Early in life, people make decisions that affect their income and wellbeing for the rest of their lives: whether to attend college, if so what college to attend, and what occupation to enter afterwards. This dissertation examines the information people use when making these decisions and how information and budget constraints affect their choices. Individuals collect and act on a broad and nuanced set of information when making these lifetime decisions, however not everyone has access to the same set of information. The factors people consider when making these education and career decisions have implications for the pace of adjustment to shocks in the labor force and for the efficiency and equity of individual sorting into schools and occupations.

The next chapter measures sources of uncertainty about lifetime earnings and considers the relationship between the level of this uncertainty and the expectation of lifetime earnings across occupations. If workers are risk-averse and understand the different degrees of earnings uncertainty across occupations, then they will require additional compensation to enter careers that start in the riskier occupations. I measure several sources of uncertainty, including earnings risk and employment risk, and measure riskiness of starting occupation in a lifecycle context, incorporating the possibility that workers will change occupations over the course of their career. I find a positive relationship across occupations between my measure of lifetime earnings uncertainty and average lifetime earnings, indicating that workers do recognize different degrees of riskiness across occupations and demand compensation for this risk. Moreover, workers sort into occupations based partially on risk preference; less risk-averse workers are more likely to enter the riskiest occupations.

Compensation for earnings risk is an important source of differences in average earnings in an occupation, explaining 17% of the differences in expected lifetime earnings for workers starting in different occupations. However, these differences in average earnings across occupations are not an important source of earnings inequality. A far larger source of total earnings inequality is the differences in earnings within occupations due to different resolutions of earnings uncertainty.

Public programs that seek to condense the distribution of earnings, such as progressive income taxes, unemployment insurance, and food stamps, will decrease earnings inequality directly and improve welfare by reducing the earnings uncertainty faced by workers. These programs may also increase the efficiency of the labor market. Workers will require less compensation for earnings risk, lowering the cost of the goods and services they produce, and can pay more attention to their special skills when choosing an occupation, rather than their risk preferences. However, the classical principle-agent model theorizes that managers may need to tie workers' earnings to the variable productivity of the firm to insure high effort. The potential efficiency gains from reducing earnings uncertainty must be weighed against the potential losses from lowering the incentives for high worker effort.

The third chapter measures the responsiveness of college enrollment to changes in the relative earnings of workers with and without a college degree. During the 1970s, earnings for college-educated workers fell relative to earnings for high school graduates. However, since 1980 the gap in earnings between college- and high school-educated workers rose substantially, nearly doubling between 1980 and 2002. College enrollment rates followed a similar pattern over the same period. The expected lifetime earnings gap between workers with and without a college degree at the time a student graduates high school is an important predictor of whether he enrolls in college, even controlling for other factors such as parents' income and education and local tuition rates. On average, a 10% increase in the lifetime earnings gap between workers with and without a college degree will increase the probability that a high school graduate enrolls in college by 1%. The rise in this earnings gap between 1980 and 2002 can explain the majority of the 10 percentage point rise in the four-year college enrollment rate for men over that period.

This relationship between the return to a college education and college enrollment is an important channel for labor market adjustment. The relative earnings of more and less educated workers depend partially on the relative supplies of each type of worker. Regardless of the causes of the recent rise in the relative earnings of college-educated workers, the increasing supply of college-educated workers should eventually push their relative earnings back down.

Chapters 2 and 3 consider the sources of information the average person uses when making education and occupation choices with lifetime implications. The fourth chapter of this dissertation considers differences across individuals in the type of information available and how free these individuals are to act on that information.

Future earnings depend not only on whether an individual goes to college, but on the quality of the college they attend. Traditional earnings models predict that higher-ability students will reap greater rewards from higher quality colleges. While students at high quality colleges have higher ability on average, many individual students appear to be mismatched with their college: high ability students at relatively low-quality colleges or lower ability students at relatively high-quality colleges. Chapter 4 examines the sources of this apparent mismatch between student ability and college quality.

We find that both types of mismatch are primarily the result of choices made by the student and their families, not by college admissions offices. The vast majority of students who end up mismatched with their college either did not apply to any schools with which they would be well-matched or were accepted to at least one well-matched school and chose to attend a mismatched school instead. One plausible explanation for over-qualification, when strong students attend relatively low-quality colleges, is that students are financially constrained and cannot afford to attend the higher-quality colleges that would be a better match. We find that students from the wealthiest families are less likely to be over-qualified. However, many factors that we predicted would reduce both types of mismatch instead lower the probability of over-qualification but raise the probability of under-qualification. One exception is the public university system; students are less likely to end up mismatched in either direction if they have a school with which they are well-matched within their home state university system. In addition to affecting the students' private outcomes, the match between student and college characteristics also affects how efficiently the substantial investments made by federal and state governments work to grow the supply of workers with college degrees.

Chapter 2

Risk and Return Tradeoffs in Lifetime Earnings

I. Introduction

Workers in occupations with greater uncertainty about total lifetime earnings receive higher total earnings on average. This compensation for uncertainty about lifetime earnings is justified when risk-averse workers invest their time in learning occupation-specific skills, making it costly to change their career later in life. When they are uncertain about their lifetime earnings, workers can either build up precautionary savings, which may keep consumption low early in life, or they can leave themselves vulnerable to swings in consumption. Either way, this uncertainty lowers the expected utility of risky earnings streams for risk-averse workers relative to a certain stream with the same expected value. I study compensation for lifetime earnings risk by estimating a structural model of job and consumption choices under multiple sources of earnings uncertainty. I find that compensation for greater earnings uncertainty is an important explanation for differences in expected lifetime earnings across careers.

Labor income risk is a largely uninsurable and un-diversifiable risk faced by virtually all households. Most households receive income from at most two careers and there are few private mechanisms to insure labor income. Understanding the magnitude of uninsurable earnings risk and its effect on workers' utility from lifetime earnings highlights the benefit of public programs, such as unemployment insurance and food stamps, which smooth earnings risks that are not insured by the private market. Shocks to earnings over a worker's lifetime also represent an important source of earnings inequality. The realizations of these shocks generate far more earnings inequality than the differences in average earnings across occupations. Policymakers seeking to reduce income inequality must recognize that providing young people with equal starting opportunities, while important, misses an important source of inequality.

I study earnings risk in a lifecycle framework where workers face uncertainty about how much they will earn when they work, how much time they will spend out of work, and whether they will change occupations over the course of their careers. Movements between occupations can represent an added source of risk, if workers change occupations unwillingly after losing their job, but they can also mitigate risk if workers choose to change occupations to escape low earnings in their old occupation. To accurately capture the relationship between risk and occupation mobility I estimate a simple labor search model with frictions that includes both exogenous separations into non-employment, which may result in an occupation change, and endogenous decisions to search for work in new occupations. Workers in different occupations face different variances of shocks to earnings, different probabilities that their job will be destroyed, and different arrival rates of offers for new jobs if they move into non-employment. Earnings rise with tenure in an occupation, so established workers experience a fall in earnings if they change occupations.

I model workers' optimal employment and consumption choices in the face of these multiple sources of lifetime earnings risk. I use data from the Current Population

Survey and the Panel Study of Income Dynamics to estimate the occupation-specific parameters describing these risks. I estimate the occupation-specific determinants of earnings and variances of earnings shocks from moments of observed earnings data. I then use these earnings parameters and the solution to the worker's optimization problem to estimate the labor market parameters using indirect inference. I define all individuals who begin their working lives in the same broad occupation category as following the same career, even if some of them later transition to other occupations. I use my estimated parameters and the model solution to simulate a series of lifetime earnings streams for workers starting in each occupation and compare the mean and variance of discounted lifetime earnings for workers starting in the same occupation.

The only source of variation in these simulated earnings streams between workers starting in the same occupation is different realizations of risk. The determinants of earnings and earnings shocks are estimated allowing for individual fixed effects, but I then omit these effects from the simulations. Some of my modeled changes in earnings are due to workers' decisions about whether to quit work or accept new job offers. I include these endogenous moves as part of my measure of risk because any move away from continuing to work in one's current occupation only becomes optimal ex post as a best response to receiving certain shocks. The variance in the discounted value of these simulated earnings streams therefore represents a total measure of riskiness that includes both employment uncertainty and earnings uncertainty and captures how they interact under optimizing worker behavior. My framework allows me to discuss the magnitude as well as the sign of the relationship between expected earnings and riskiness and to separate and quantify the sources of risk.

I find a clear positive relationship between the riskiness of lifetime earnings, measured as the ratio of the variance of these simulated lifetime earnings streams for workers in each career divided by the mean in that career, and expected earnings in that career. Moving from the 25th to the 75th percentile of lifetime earnings risk increases expected earnings by an average of $4,000 per year, or 6% of the mean annual earnings of $63,000. Compensation for earnings uncertainty can explain 17% of the variation in expected lifetime earnings across careers. Permanent shocks to individual earnings, which have a fairly large standard deviation of about 0.08 on average and compound over working lives, are by far the largest source of lifetime earnings risk, dwarfing persistent but mean-reverting occupation-wide shocks. Employment risk, particularly the possibility of changing occupations, is also an important determinant of lifetime risk.

The idea that earnings in an occupation should reflect compensation for characteristics of that occupation was first articulated by Smith (1776) and formalized by Rosen (1986). The earliest reference to compensation for earnings risk that I have found is Friedman and Kuznet's 1954 study of professional incomes. Since then, several papers have found a positive relationship between cross-sectional or single period variance of earnings and the mean of earnings across occupations, including King (1974), Hartog and Vivjerberg (2007), and McGoldrick and Robst (1996). However, these papers study only a cross-sectional or single-period measure of earnings variance, which misses the correlation in income shocks over time, and ignore the additional earnings risk from non-employment and occupation transitions.[1] As I will discuss in this chapter, considering a

[1] McGoldrick and Robst include the predicted probability of changing jobs in their regressions along with the variance of earnings in each occupation. They find positive effects of both earnings risk and mobility risk on expected earnings, but a negative effect for the interaction term, illustrating that the ability of workers to change jobs can help insulate them against earnings shocks.

single period model of occupation choice also provides a misleading interpretation of the slope of the risk-return tradeoff.

In this chapter, I incorporate a more detailed approach to isolating and estimating earnings uncertainty, similar to those developed in Moffitt and Gottschalk (2002), Meghir and Pistaferri (2004), Low, Meghir, and Pistaferri (2010), and Guvenen and Smith (2010). While these recent papers are able to more precisely estimate earnings risk, they do not look at differences in risk across occupations or the way workers choose the riskiness of their earnings stream by sorting into occupations.

The next section presents a model of workers' optimal career and consumption choices in the face of uncertain earnings. Section III describes solving for the policy rule that optimizes this model. Section IV discusses the data and methods for estimating the parameters of the earnings and mobility process and presents the estimates. Section V analyzes the relationship between expected lifetime earnings and earnings riskiness and Section VI discusses interpretations of the slope of this risk-return tradeoff. Section VII concludes.

II. A Model of Career and Lifetime Earnings

My model depicts a labor market where jobs are differentiated by occupation and workers face multiple sources of lifetime earnings risk from shocks to earnings and the possibility of job destruction. Non-employed workers receive job offers from all occupations and may choose to accept an offer that involves a change in occupation from their previous work. I do not include an out-of-the-labor-force state in which people neither work nor search for work.[2] To capture the effect of workers' risk aversion on career choice I give workers decreasing marginal utility of consumption and model their choices to borrow and save to smooth over earnings fluctuations. My aim is to model working life in the simplest possible terms that still capture the major sources of uncertainty in lifetime earnings and allow workers to mitigate negative earnings shocks through occupational mobility.

All variations in earnings and earnings uncertainty in this model come at the occupation level. I make no distinction between different employers within an occupation or between different industry categories, except insofar as industry definitions and occupation definitions overlap. Shaw (1984) and Kambourov and Manovskii (2009) find that while firm, industry, and occupation tenure all affect earnings, occupation tenure is the most important single determinant of earnings. In my estimation I use 19 occupation categories listed in Table A2.1.

In this model workers are assigned a starting occupation, although they may later choose to transition to a new occupation. In reality, workers choose their starting occupations; these choices, by risk-averse workers, drive the relationship between riskiness and expected earnings. However, the aim of this working model is not to recreate this initial choice, but rather to capture average earnings and earnings risk conditional on first occupation. Without incorporating differences across occupation in the cost of initial training, the arduousness of the work, and other factors, workers in my model would all flock to the highest-paying professions like law and health. While in my simulations, as in life, workers from lower-paying occupations like community service

[2] In my estimation I focus on men between the ages of 25 and 65, for whom this omission is relatively benign.

are more likely to eventually change occupations, I prevent wholesale herding into a few occupations by matching the distribution of new offers to workers in each occupation to observed transition rates between occupations.

A. Employment

Individuals live for L periods and work for the first T of them. Each period is a quarter and I set L=200, T=160. Each working period $t \leq T$ individual i receives stochastic earnings Y_{ikt}, which depend on employment status, $N_{it} \in \{0,1\}$, occupation, $k_{it} \in [1, K]$, and other determinants of potential earnings which I summarize as Ω_{it}. Prior to the first working period, t=0, individuals receive a starting occupation. In the first working period, t=1, all individuals are employed in that occupation and learn and receive their starting earnings. This framework for the start of working life resembles a world where individuals sort into careers while still in school and have a position lined up by the time they are ready to begin work.

In all subsequent working periods employed workers face an occupation-specific probability $0 \leq \delta_k \leq 1$ of losing their job and entering non-employment. To greatly ease the computational burden I do not allow workers to receive outside job offers while working, but workers may quit if they wish to search for work in other occupations. Non-employed workers who were most recently employed in occupation k receive a job offer from their current occupation with per-period probability $0 \leq \lambda_{ck} \leq 1$ and from a new occupation with probability $0 \leq \lambda_{nk} \leq 1 - \lambda_{ck}$. The per-period probability that a worker most recently employed in occupation k receives an offer from a new occupation k' is defined as $\lambda_{kk'}$, where $\sum_{k' \neq k} \lambda_{kk'} = \lambda_{nk}$. Non-employed workers may choose to accept an offer if they receive one or remain non-employed for another period. Each working period starts with shocks to potential earnings, job destruction shocks for some employed workers, and new offers for some non-employed workers. Workers then choose to quit, to accept a job offer if they have one, and decide how much to consume.[3]

B. Consumption

I assume individuals have standard time-separable constant relative risk aversion utility over consumption with coefficient of relative risk aversion γ and discount rate β. For simplicity, I further assume individuals get no utility from leisure.[4] Individuals can save and borrow over their lives at a constant risk-free interest rate *r*, but they cannot buy state-dependent assets to insure against idiosyncratic earnings risk. The worker's problem is therefore to choose each period his consumption, C_{it}, employment, and occupation to maximize

$$\max_{C_{it}, N_{it}, k_{it}} E_t \left[\sum_{s=t}^{L} \beta^{s-t} \frac{C_{is}^{1-\gamma}}{1-\gamma} \right] \tag{2.1}$$

subject to a terminal asset condition $A_{iL} \geq 0$ and the dynamic budget constraint

$$A_{it+1} = (1+r)(A_{it} + Y_{ikt} - C_{it}). \tag{2.2}$$

I assume that everyone begins life with no assets, $A_{i1} = 0$.

The consumption and employment decisions can be viewed sequentially: workers first identify their best consumption choice under each possible employment situation this period and then choose among employment situations. The value of an employment

[3] The timing of employment choices relative to the revelation of earnings shocks is important. If workers observe their earnings shocks, but have the opportunity to avoid receiving the shock by quitting into non-employment then they will cherry pick only positive shocks, which can distort simulated earnings.

[4] The amount of hours worked and flexibility of hours represents another important dimension of differences across occupations that may affect how workers sort into them. Including disutility from work and variation in hours worked across occupations is a non-trivial but interesting extension to this model.

situation is a function of assets and the determinants of potential earnings and can be expressed as a Bellman equation,

$$V_t^{N_{it},k_{it}}\left(A_{it},\Omega_{it}\right)=\max_{C_{it}}\frac{C_{it}^{1-\gamma}}{1-\gamma}+\beta E_t\left[V_{t+1}\left(A_{it+1},\Omega_{it+1}\right)\middle|A_{it},\Omega_{it},N_{it},k_{it}\right]. \tag{2.3}$$

The value of a period depends on the choice of employment situation,

$$V_t\left(A_{it},\Omega_{it}\right)=\max_{N_{it},k_{it}}\left\{V_t^{N_{it},k_{it}}\left(A_{it},\Omega_{it}\right)\right\}. \tag{2.4}$$

C. Earnings

A worker's earnings include a deterministic component based on his total labor market experience, ex_{it}, his tenure in his current occupation, ten_{ikt}, and fixed effects for himself, μ_i, and his current occupation, μ_k. The inclusion of occupation tenure in the earnings function captures the cost of changing occupations part way through life. The occupation fixed effect and the different effects of occupation tenure generate differences in expected earnings across occupations. Because the individual fixed earnings effect multiplies earnings in any occupation, additive in the log, it does not affect the relative earnings across occupations or occupation choice. For identification, I assume that the intercept for earnings is captured in the occupation effect and $E\left[\mu_i\right]=0$. Including this individual effect helps differentiate between cross-worker earnings variation from known differences between workers and from realizations of earnings shocks.

Earnings risk is captured by three stochastic components. First, the log earnings potential in an occupation has an AR(1) component, ε_{kt}, with occupation-specific persistence ρ_k and innovation $e_{kt}\sim N\left(0,\sigma_{ek}^2\right)$,

$$\varepsilon_{kt}=\rho_k\varepsilon_{kt-1}+e_{kt}. \tag{2.5}$$

I estimate that all occupation productivities are mean reverting, $\rho_k < 1$, and that shocks have an average half-life of about 2 quarters.[5] Shocks to occupation productivity affect the earnings of all workers in the same occupation each period, but workers can escape low occupation productivity by searching for work in other occupations.

Workers also experience idiosyncratic and fully permanent shocks to their log productivity,

$$\zeta_{it} = \zeta_{it-1} + u_{it} . \tag{2.6}$$

While the variance of idiosyncratic productivity shocks is also occupation specific, $u_{it} \sim N\left(0, \sigma_{ku}^2\right)$, workers carry their current level of individual productivity between occupations, so they cannot escape negative shocks through occupation changes.[6] Carrying individual productivity across occupations makes sense if it consists mainly of general skills and physical capacity or if a worker's most recent wage affects his bargaining power at his next job.

Finally, a worker starting in an occupation draws a match quality, $\alpha_{ik} \sim N\left(0, \sigma_{\alpha}^2\right)$, that remains fixed during his time in that occupation. The distribution of match is the same across occupations and individuals. This worker-occupation match captures an additional level of uncertainty about untried occupations and will generate some churning in the early periods of working life as workers who are poorly matched with their starting

[5] While these productivity fluctuations could co-vary with each other or with an aggregate shock I have left them independent in this paper. An aggregate productivity shock affects all occupations, and is therefore less relevant for distinguishing differences in riskiness across occupations. Occupation productivity could also follow a time trend, but there is little evidence that it does, at least in the broad occupation categories I use, in my 1988-2007 data sample.

[6] The random walk assumption is necessary for identification. With fully permanent earnings shocks the variance of changes in earnings for workers in the same occupation depends only on the variance of idiosyncratic shocks in that occupation. With a general AR process, the change in earnings could depend on the variance of all past shocks, and therefore the complete occupation history of each worker, which I do not observe in the data.

occupations quit and search elsewhere. Idiosyncratic productivity shocks, occupation productivity shocks, and match quality are all independent of one another.

Combining these elements, a worker's log potential earnings are determined by

$$\log\left(P_{ikt}\right) = \mu_k + \mu_i + \phi\left(ex_{it}\right) + \psi_k\left(ten_{ikt}\right) + \alpha_{ik} + \varepsilon_{kt} + \zeta_{it}. \tag{2.7}$$

In practice, $\phi\left(ex_{it}\right) = \phi ex_{it}$ and $\psi_k\left(ten_{ikt}\right) = \psi_{1k} ten_{ikt} + \psi_{2k} ten_{ikt}^2$.[7] While working, workers also experience an i.i.d. transitory earnings disturbance, $\xi_{it} \sim N\left(0, \sigma_\xi^2\right)$. When not employed workers receive a fraction, b, of their potential earnings. Earnings are therefore

$$Y_{ikt} = \begin{cases} P_{ikt} \exp\left(\xi_{it}\right) & N_{it} = 1 \\ bP_{ikt} & N_{it} = 0. \end{cases} \tag{2.8}$$

This estimated fraction of earnings captures both monetary unemployment benefits and the monetary equivalent of other benefits of not working.

Non-employed workers continue to be affected by productivity shocks in their most recent occupation, but they do not experience further idiosyncratic productivity shocks, reflecting the idea that many of these individual shocks come from new skills learned or capacities lost while working. Workers accumulate labor market experience whenever they are employed and this experience does not depreciate during non-employment. Workers accumulate occupation-specific tenure while working in that occupation. Tenure does not depreciate during non-employment, but it is lost when a worker changes occupations. For example, a worker who spends five years in manufacturing then loses his job will start with five years of tenure if he takes a new job

[7] A piecewise linear function of tenure generates similar results. The effects of higher moments of experience and tenure are imprecisely estimated in my data, which is problematic when the point estimates are used in the simulations.

in manufacturing, but no tenure if he takes a new job in sales. If he later returns to manufacturing from sales he will re-start with no tenure. This assumption is necessary for the estimation since I do not observe the full occupation histories of most workers in my data.

Finally, during retirement individuals receive a fraction, pen, of their earnings in their last period of work as a pension. The worker has no uncertainty about this pension once his earnings in his last period of working life are revealed. If the worker in employed in period T this pension is $pen \bullet P_{ikT}$. If he is not employed in period T, his pension is $pen \bullet b \bullet P_{ikT}$.

III. Optimal Choices under Earnings Uncertainty

The model described in the last section illustrates two causes for moves into non-employment and out of non-employment into new occupations. In some cases, workers are forced into non-employment when their job is destroyed. These workers may accept a job in a new occupation rather than spending more periods with low non-employment earnings if offers from their current occupation are rare relative to offers from new occupations. In other cases, workers in an occupation with low current productivity or with which they are poorly matched choose to enter non-employment with the goal of finding work in a new occupation. These two sources of occupation mobility have very different implications for the riskiness of the starting occupation. In the first case, frequent occupation changes imply that the starting occupation is quite risky because losing one's job is likely to also lead to a costly occupation change. In the second case, frequent occupation changes imply lower riskiness of the starting occupation because workers can easily escape low earnings.

The key difference between these two types of transitions is that the second type will be correlated with earnings: workers are more likely to willingly leave when their current earnings are low. A simpler model of lifetime earnings that included only exogenous transition probabilities between employment states would miss this negative correlation between earnings and mobility and overstate the overall riskiness of occupations. Instead, I solve for a policy rule that determines when workers will choose to move into non-employment and include both types of transitions.

The solution to the multi-period model consists of workers' optimal choices of consumption, employment, and occupation each period. Workers must find the level of consumption that maximizes the Bellman equation (2.3) in order to assess the value of each employment possibility and choose between them. The choices available to the worker will depend on his employment status and occupation after jobs have been destroyed and new offers made at the start of each period. If a worker is still employed, his employment decision is whether or not to quit into non-employment. If a worker is not employed and receives a job offer, his employment decision is whether to accept, which may involve changing occupations. Workers who have just had their job destroyed or who are start non-employed and receive no offers have no choice but non-employment.

To determine optimal consumption individuals must build expectations of their value of entering next period with different levels of assets, corresponding to different consumption choices today. In all but the last working period, the expected value of entering next period with a certain level of assets is a probability-weighted average of the employment situation-specific values tomorrow. If a worker is employed this period, he will be employed or not employed in the same occupation next period and

$$\begin{aligned} E_t\left[V_{t+1}\left(A_{t+1},\Omega_{t+1}\right)\middle|A_t,\Omega_t,N_t=1,k_t\right] &= \delta_{k_t}E_t\left[V_{t+1}^{0,k_t}\left(A_{t+1},\Omega_{t+1}\right)\right]+ \\ &\left(1-\delta_{k_t}\right)E_t\left[\max_{N_{t+1}}\left\{V_{t+1}^{0,k_t}\left(A_{t+1},\Omega_{t+1}\right),V_{t+1}^{1,k_t}\left(A_{t+1},\Omega_{t+1}\right)\right\}\right], \end{aligned} \tag{2.9}$$

where Ω_t denotes the determinants of potential earnings and the individual i subscripts have been omitted for brevity. If a worker is not employed in period t, then in period $t+1$ he may receive a job offer from his old occupation, k_t, a job offer from a new occupation k', or no job offers,

$$\begin{aligned} E_t\left[V_{t+1}\left(A_{t+1},\Omega_{t+1}\right)\middle|A_t,\Omega_t,N_t=0,k_t\right] &= \left(1-\lambda_{ck_t}-\lambda_{nk_t}\right)E_t\left[V_{t+1}^{0,k_t}\left(A_{t+1},\Omega_{t+1}\right)\right]+ \\ &\lambda_{ck_t}E_t\left[\max_{N_{t+1}}\left\{V_{t+1}^{0,k_t}\left(A_{t+1},\Omega_{t+1}\right),V_{t+1}^{1,k_t}\left(A_{t+1},\Omega_{t+1}\right)\right\}\right]+ \\ &\lambda_{nk_t}\sum_{k'}\lambda_{k_tk'}E_t\left[\max\left\{V_{t+1}^{0,k_t}\left(A_{t+1},\Omega_{t+1}\right),V_{t+1}^{1,k'}\left(A_{t+1},\Omega_{t+1}\right)\right\}\right]. \end{aligned} \tag{2.10}$$

The value of each employment state can be re-written factoring out potential earnings,[8] which highlights the role of expectations of earnings growth under each employment possibility,

$$v_t^{1,k_t}\left(a_t,\Omega_t\right)=\max_{c_t}\left\{\begin{aligned}&\frac{c_t^{1-\gamma}}{1-\gamma}+\beta\delta_{k_t}E_t\left[\left(bg_{t+1}\right)^{1-\gamma}v_{t+1}^{0,k_t}\left(a_{t+1},\Omega_{t+1}\right)\right]+\\&\beta(1-\delta_{k_t})E_t\left[\max_{N_{t+1}}\left\{\left(bg_{t+1}\right)^{1-\gamma}v_{t+1}^{0,k_t}\left(a_{t+1},\Omega_{t+1}\right),g_{t+1}^{1-\gamma}v_{t+1}^{1,k_t}\left(a_{t+1},\Omega_{t+1}\right)\right\}\right]\end{aligned}\right\} \tag{2.11}$$

$$v_t^{0,k_t}\left(a_t,\Omega_t\right)=\max_{c_t}\left\{\begin{aligned}&\frac{c_t^{1-\gamma}}{1-\gamma}+\beta\left(1-\lambda_{ck_t}-\lambda_{nk_t}\right)E_t\left[\left(bg_{t+1}\right)^{1-\gamma}v_{t+1}^{0,k_t}\left(a_{t+1},\Omega_{t+1}\right)\right]+\\&\beta\lambda_{ck_t}E_t\left[\max_{N_{t+1}}\left\{\left(bg_{t+1}\right)^{1-\gamma}v_{t+1}^{0,k_t}\left(a_{t+1},\Omega_{t+1}\right),g_{t+1}^{1-\gamma}v_{t+1}^{1,k_t}\left(a_{t+1},\Omega_{t+1}\right)\right\}\right]+\\&\beta\lambda_{nk_t}\sum_{k'}\lambda_{k_tk'}E_t\left[\max\left\{\left(bg_{t+1}\right)^{1-\gamma}v_{t+1}^{0,k_t}\left(a_{t+1},\Omega_{t+1}\right),g_{t+1}^{1-\gamma}v_{t+1}^{1,k'}\left(a_{t+1},\Omega_{t+1}\right)\right\}\right]\end{aligned}\right\},$$

[8] The derivation of this reformulation, which follows Carroll (2004), is described in Appendix 2.

where $V_t\left(A_t,\Omega_t\right)=P_t^{1-\gamma}v_t\left(a_t,\Omega_t\right)$, lowercase letters denote the ratio with potential earnings, $a_t=\frac{A_t}{P_t}$, and $g_{t+1}=\frac{P_{t+1}}{P_t}$ is growth in potential earnings.

Growth in potential earnings depends on employment situation this period and last:

$$g_t=\begin{cases}\exp\left(\phi+\psi_{1k}+\psi_{2k}\left(2ten_{ikt}+1\right)-\left(1-\rho_k\right)\varepsilon_{kt-1}+e_t+u_t\right) & N_{t-1}=1\\ \exp\left(-\left(1-\rho_k\right)\varepsilon_{t-1}+e_t\right) & N_{t-1}=N_t=0\\ \exp\left(-\left(1-\rho_k\right)\varepsilon_{t-1}+e_t+u_t\right) & N_{t-1}=0,N_t=1,k_{t-1}=k_t\\ \exp\left(-\psi_k\left(ten_{k,t-1}\right)+\mu_{k'}-\mu_k+\alpha_{k'}-\alpha_k+\varepsilon_{k't}-\varepsilon_{kt-1}+u_t\right) & N_{t-1}=0,N_t=1,k_{t-1}\neq k_t.\end{cases}\tag{2.12}$$

Potential earnings growth after a period of employment includes predictable growth in experience and tenure, predictable decay of occupation productivity, and new shocks to occupation and individual productivity. Workers who are continuing in non-employment are affected by only the change in occupation productivity. Individuals moving from non-employment to employment in their current occupation are affected by changes in occupation productivity and individual productivity, but gain no experience or tenure. Finally, workers moving from non-employment to employment in a new occupation lose the effects of their accumulated tenure in their old occupation, switch to a new occupation match quality, fixed effect, and variable productivity, and experience a shock to their individual productivity.

Equations (2.11) and (2.12) make clear that while fixed individual earnings power, μ_i, total work experience, ex_{it}, and individual productivity, ζ_{it}, affect the level of potential earnings, they never affect expected earnings growth and are therefore not relevant for the policy rule. The set of earnings determinants included in the value function is therefore occupation tenure, occupation productivity, and match quality:

$\Omega_{it} = \{ten_{ikt}, \varepsilon_{kt}, \alpha_{ik}\}$. In all, the model solution depends on six state variables: a_{it}, k_{it}, N_{it}, $ten_{ikt}, \varepsilon_{kt}, \alpha_{ik}$. The first three--assets, current occupation, and employment status--evolve endogenously based on individual decisions, as well as stochastic separation shocks and job offer arrivals. Conditional on employment decisions today, occupation tenure evolves deterministically and occupation productivity evolves stochastically. Occupation match quality never changes between periods: individuals always begin a period with the same match they had last period, although they may decide to accept an offer with a new match over the course of the period.

The optimal behavior of individuals in the full multi-period model cannot be solved for analytically and must be found computationally using backwards induction from the retirement period. I describe this solution method in Appendix 1.

Because earnings are expected to grow over the lifetime and workers are impatient, individuals will prefer to consume more than their earnings early in life. Working against that inclination, uncertainty about future earnings will cause people to build up precautionary savings to guard against negative earnings shocks, lowering their lifetime utility relative to the case of risk-free earnings. The size of the precautionary savings motive will depend on how freely people are able to borrow against future earnings during low-earnings spells. I assume that individuals face a natural borrowing constraint as in Aiyagari (1994) equal to the discounted value of a "worst case scenario" per-period earnings for all remaining working periods.[9] Workers cannot borrow against their pensions, $a_{T+1} \geq 0$. This loose constraint emphasizes the welfare cost of uncertainty

[9] In this model, the worst case scenario is being non-employed for all remaining periods with constant very negative occupation productivity shocks.

rather than the welfare cost of borrowing constraints. If borrowing is more restricted, reducing workers' ability to smooth, they will require higher risk compensation.

IV. Data and Parameter Estimation

A key difficulty in estimating this model is that some model parameters do not correspond exactly with observable statistics. For example, we observe transitions from non-employment to employment, which occur only when an offer is accepted, but not offer arrivals. To estimate these parameters I use a two-stage approach. I first estimate the parameters describing the determinants of earnings using method of moments and observed earnings data. I then estimate the remaining parameters by indirect inference. In this second stage, I simulate employment histories and earnings paths for workers starting in each occupation, using the policy rule described in the previous section and the parameters estimated in the first stage, and search for values of the remaining parameters that best align characteristics of the simulated and observed data. Gourieroux, Monfort, and Renault (1993) prove that this approach can consistently estimate structural parameters even if they cannot by analytically mapped to the observed data moments.

A. The Data

I use two data sources for this estimation: the Current Population Survey (CPS), which surveys a large sample of workers each month but keeps respondents in the sample for only two years, and the Panel Study of Income Dynamics (PSID), which follows a smaller sample of workers over many years. Table 2.1 lists the parameters I estimate and the method and data source I use for each. The PSID is my primary dataset. The long panel and detailed questions allow me to measure total work experience, occupation changes, and occupation tenure and to separate individual fixed effects, persistent shocks to individual productivity, and transitory shocks. I take advantage of the larger sample of workers in each occupation each month in the CPS to measure the fixed and time-varying occupation-specific contributions to earnings. In both datasets my sample covers 20 years from 1988-2007 and includes men between the ages of 25 and 65 who are not currently in the armed forces or enrolled in school. I further restrict the sample to workers with at least some college, on the theory that the menu of possible occupations is likely to differ for workers with and without post-secondary education and that the model of investing in career-specific skills is particularly relevant for this more educated group. More details on my use of both data sets can be found in Appendix 3.

B. Occupation-Level Determinants of Earnings

To identify occupation fixed effects and productivity I use reports of usual weekly earnings in the CPS to estimate a log-earnings regression, including a full set of occupation-quarter fixed effects. The CPS interviews a household for four consecutive months, then again in the same four calendar months a year later. Every month respondents are asked about their employment status and current or more recent occupation. In their 4^{th} and 8^{th} interviews, employed respondents are asked an earnings supplement that includes a question about their usual weekly earnings in their current job. I include dummies for race/ethnicity, region of the United States, living in a rural area, and having less than a bachelor's degree to control for time-invariant differences between workers, and a quadratic of potential experience, age minus years of school minus 6, as a rough control for differences in total work experience and occupation tenure. Log weekly earnings, net of these observed worker characteristics, are described by

$$y_{ikt}^{CPS} = \hat{\mu}_k + \hat{\alpha}_{ik} + \hat{\varepsilon}_{kt} + \hat{\zeta}_{it} + \hat{\xi}_{it}. \tag{2.13}$$

Measurement error for the effects of experience and tenure and any elements of the individual fixed effect not captured by the set of control variables will be absorbed into my estimate of the transitory shock, ξ_{it}.

By construction, the average match quality across workers within an occupation, α_{ik}, is equal to zero. The average values of individual productivity, ζ_{it} and the transitory shock are also equal to zero across all workers. The average earnings residuals in each occupation-quarter cell therefore isolates the occupation fixed effect and time-varying productivity

$$\overline{y}_{kt}^{CPS} = \hat{\mu}_k + \hat{\varepsilon}_{kt}. \tag{2.14}$$

I estimate the occupation effect and the variance and persistence of occupation productivity with the consistent AR(1) moments[10]

$$\begin{aligned} E\left[\overline{y}_{kt}^{CPS} \middle| k\right] &= \hat{\mu}_k \\ \operatorname{cov}\left(\overline{y}_{kt}^{CPS}, \overline{y}_{kt-1}^{CPS}\right) &= \hat{\rho}_k \\ \operatorname{var}\left(\overline{y}_{kt}^{CPS} - \hat{\mu}_k - \hat{\rho}_k \overline{y}_{kt-1}^{CPS}\right) &= \sigma_{ke}^2. \end{aligned} \tag{2.15}$$

These parameter estimates are presented in Table 2.2. The estimated average occupation-specific intercept for quarterly earnings is \$10,188 in 2000 dollars. If individual earnings power, μ_i is correlated with initial occupation choice, then my estimates of the occupation effect would include the non-zero expected value of μ_i conditional on occupation choice.

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[10] In practice, I first de-mean the residuals for my estimate of $\hat{\mu}_k$. I then seasonally adjust the de-meaned residuals by regressing them on a set of quarterly dummies because these seasonal movements are predictable and do not represent risk. Finally, I regresses the de-meaned and adjusted residuals on their lagged values to estimate $\hat{\rho}_k$.

C. Total Work Experience and Occupation Tenure

Because the PSID interviews the same respondents year after year I am able to build a detailed work history and measure actual total labor market experience and occupational tenure for each respondent in each year. Measurement error in occupation codes can bias down estimates of occupation tenure. From year to year, the respondent may use slightly different words to describe the same job, or occupation coders may assign different codes to the same description, resulting in more changes in occupation codes than there are actual job changes. I use the method developed by Kambourov and Manovskii (2009) to reduce measurement error in occupation changes by comparing changes in occupation codes with reported employer and position changes. This approach is described in Appendix 4.

I estimate a log weekly earnings regression using PSID respondents' reports of their usual weekly earnings in their current main job. Along with total experience and a quadratic of occupation tenure, I include the same set of worker demographic variables as in the CPS regression to partially control for individual time-invariant earnings power. Rather than estimate noisy occupation-year fixed effects using the relatively small PSID sample, I subtract the estimated effects for the corresponding years from the CPS before running the regression.

In Table 2.3, occupation tenure has a larger effect on earnings than total experience. I estimate that an additional year of any work experience raises earnings by 0.9%. The first year of occupation-specific tenure raises earnings by almost 3% on average across occupations. The average worker's earnings will rise by 19.5% over the first 5 years in an occupation. My estimates are similar to other papers that estimate the effects of experience and occupation separately, including Shaw (1984) and Kambourov and Manovskii (2009).

D. Idiosyncratic Earnings Shocks and Match Quality

I use the residual from this PSID earnings regression to identify the variance of individual productivity shocks, individual-occupation match quality, and the transitory earnings shock. From equations (2.5) and (2.6), the residual from this PSID log earnings regression comprises

$$y_{ikt} = \hat{\alpha}_{ik} + \hat{\zeta}_{it} + \hat{\xi}_{it}. \tag{2.16}$$

However, the residual may also include elements of the individual effect that were not captured by the set of dummy variables. To avoid errantly identifying fixed individual variation as unexpected shocks I identify the variance of these parameters off the annual growth in this residual within workers, $\Delta y_{ikt} = y_{ikt} - y_{ikt-1}$, which eliminates any remaining individual fixed effects in the residual.[11] I identify the variance of these remaining earnings shocks with the over-identified set of moments

$$\begin{aligned} E\left[\Delta y_{ikt}^2 \middle| k_{it} = k_{it-1}\right] &= 4\hat{\sigma}_{uk}^2 + 2\hat{\sigma}_{\xi}^2 \\ E\left[\Delta y_{ikt}^2 \middle| k_{it} \neq k_{it-1}\right] &= 4\hat{\sigma}_{uk}^2 + 2\hat{\sigma}_{\xi}^2 + 2\hat{\sigma}_{\alpha}^2 \\ E\left[\Delta y_{ikt}\Delta y_{ikt-1} \middle| k_{it} = k_{it-1} = k_{it-2}\right] &= -\hat{\sigma}_{\xi}^2. \end{aligned} \tag{2.17}$$

The variance of the change in residual log earnings over a year for workers who remained employed in the same occupation includes the cumulative variance of four quarterly,

[11] Measurement error will inflate the variance of residual earnings growth. While measurement error should mainly load onto the estimated variance of transitory shocks, Whalley (2011) points out that data trimming, a usual approach to reducing measurement error, has a substantial effect on the estimates of both persistent and transitory variance. I exclude earnings observations that are more than 4 times or less than $1/4$ of each respondent's average real earnings, following Carroll and Samwick (1997). This exclusion rule cuts 3.8% of the sample and leads to an average standard deviation of the permanent shock of 0.083. Tightening the cutoffs to 3 times or $1/3$ reduces the average standard deviation to 0.065 while loosening to 5 or $1/5$ raises the average estimate to 0.106.

permanent shocks to individual earnings ability and the variance of the transitory shocks to the starting and ending earnings. For a worker who changes occupations over the year, the variance also includes the effects of losing his old occupation match and drawing a new one. Finally the expected covariance of two consecutive changes in residual earnings for workers who remain employed in the same occupation for three years contains only the variance of the transitory earnings shock in the middle period.[12]

The parameters estimated from the PSID earnings residuals are presented in Table 2.4. Idiosyncratic shocks are more than twice the size, on average, of occupation-wide shocks, with average standard deviations of 0.083 and 0.032 respectively. The relative importance of idiosyncratic shocks will be even larger for lifetime earnings, since they are permanent and compound over a lifetime while the AR(1) occupation-wide shocks are mean-reverting. While some occupations are risky in multiple dimensions, others have highly variable occupation-wide productivity but relatively little idiosyncratic variation. Agricultural workers have the highest occupation-wide earnings risk while computer scientists have the lowest. Agricultural workers also have the highest idiosyncratic productivity shocks while engineers have the lowest.

E Job Destruction and Offer Arrival Rates

I estimate the exogenous job destruction rate, the current-occupation and new-occupation offer arrival rates, and the share of potential earnings received during non-employment using indirect inference. For this approach, I simulate 40 years of earnings

[12] In these moments, I assume that workers who change occupations over the year do so at the beginning of the year, so that all four of the quarterly individual shocks are drawn from the distribution of the new occupation.

and employment moves for 300 workers starting in each occupation.[13] I generate a lifetime of shocks for each worker, then simulate working lives using the policy rule described in section III to guide workers through the shocks they encounter, the earnings parameters estimated as described above, and guesses of the remaining parameters. I then compare characteristics of these simulated data to characteristics of observed data and update my guess of the labor market parameters until the characteristics of the simulated and observed data align. The set of shocks is held constant across simulations.

The data characteristics I target are the average duration of completed non-employment spells by last occupation, the average occupation tenure of employed workers by occupation and age bracket, and the annual probability of changing occupations by starting occupation and age bracket, all measured from the PSID.[14] I do not observe enough non-employment spells to match non-employment duration separately by occupation and age bracket. In theory, average non-employment duration could increase with age as workers with more tenure in their current occupation wait longer for an offer in that same occupation, but this effect does not show up strongly in the PSID data. Many workers remain in the same occupation for all the years I observe them in the PSID, so looking at the length of completed spells rather than average tenure would both reduce my observations and understate the persistence of workers in occupations.

The parameters estimated with this method are presented in Table 2.5. Table 2.6 assesses how well these parameter estimates fit the simulated data to the observed data. The average duration of non-employment is slightly higher in the simulated data than in

[13] The number of simulated workers is chosen to roughly match the total number of individuals observed in the PSID sample.
[14] I assume that workers enter the simulations at the age of 25 to match the data.

the real data. The gap is partially due to the discrete time structure of the simulations. Many non-employment spells in the PSID last only one or two months, but spells in the simulations must last at least one quarter. If I count the PSID spells of less than a quarter as lasting one quarter the average duration of non-employment rises to about two quarters.

The simulations do a good job of matching the average occupation tenure by age, but I produce too few occupation changes, particularly for young workers. Some of these early occupation changes may reflect a search for a good match in other dimensions of an occupation that I do not model. Young workers may also take short term jobs while they prepare for a planned career in a different occupation, but this type of anticipated occupation change is also outside my model. The occupations with the worst fit on the probability of changing occupation for young workers are office support (36% probability in the data against 6% in the simulations) and construction (27% and 8%), which supports the theory that these starter jobs are driving some of the gaps. Finally, while I have tried to reduce the number of misidentified occupation changes, my observed changes may still be too high because of measurement error. As shown in Table 2.7, I do a better job of matching the probability of changing occupations at least once over longer time horizons, which would be the case if my observed changes are biased up by individuals moving back and forth the between two related occupation codes while continuing to do the same work. Table 2.7 also shows that my simulations match the accumulation of total experience over the lifecycle, although experience is systematically lower in the simulations because the PSID respondents generally have some work experience when they enter the sample at age 25 while the simulated workers all start with none.

F. Calibrated Parameters

In the indirect inference estimation and in the simulations below, I set the quarterly discount rate, β, to 0.987, equivalent to a 0.95 annual rate, and the quarterly risk-free interest rate, r, to 0.5%, a 2% annual rate. I assume that workers receive $pen = 0.75$ share of their period T earnings during retirement. I set the coefficient of relative risk aversion, γ, to 1.5, taken from Attanasio and Weber (1995). Finally, I set the transition matrix for workers who receive a job offer from a new occupation, $\lambda_{kk'}$ to the observed distribution of quarterly occupation to occupation moves in the CPS. For a non-employed occupation *k* worker, the probability that he receives an offer from a new occupation is estimated, but the share of new offers from each occupation, conditional on receiving an outside offer, is imposed.

V. Uncertainty and the Value of a Career

To approximate the expected value of lifetime earnings in a given career and the variance around that expectation I simulate possible earnings streams for 500 workers starting in each occupation. These simulations use the same policy rule to determine labor choices as the indirect inference simulations. However, in the estimation simulations all workers in the same occupation each period have the same occupation productivity contribution to their earnings to match observed data while in this exercise each worker faces a different sequence of occupation productivity shocks to capture all sources of earnings uncertainty. I use the final set of parameter estimates from the last section to generate the earnings and employment shocks. I calculate the discounted stream of realized earnings for each simulated worker using the quarterly real interest rate r=0.5% and take the mean and variance of these discounted lifetime earnings for workers starting in each occupation as an approximation of the expectation and variance of lifetime earnings in that career.

The variance of lifetime earnings among workers starting in the same occupation is far larger than the variance in average lifetime earnings across occupations. Sales workers experience about the median level of lifetime earnings uncertainty. In my simulations, among workers who start in sales, a worker in the 75th percentile of lifetime earnings earns about 1.4 times more over his life than a worker in the 25th percentile of lifetime earnings. The inter-quartile range of average earnings across occupations is much smaller. Finance workers, who represent the 75th percentile of average earnings by occupation, can expect to earn only 20% more over their lives than mechanics, who represent the 25th percentile.

A. Estimated Expected Lifetime Earnings and Earnings Risk

The first column of Table 2.8 presents an OLS regression of the mean of these simulated lifetime earnings streams in each occupation on a constant and the ratio of the variance of lifetime earnings and the mean. This relationship between the mean and variance of earnings across occupations is also plotted in Figure 2.1.[15] The estimated slope of the risk-expected return frontier is 0.22. Compensation for risk can explain 17% of the variation in expected lifetime earnings across starting occupations. To the extent that lifetime earnings risk correlates with other sources of compensating differences,

[15] Lifetime earnings are bounded below by zero and have a long right tail. In distributions with this shape the mean and variance are mechanically related because higher variance generates more extreme observations on the right than the left. Plotting the ratio of the variance to the mean mitigates this effect.

probability of physical risk is a particularly likely example, these plots will overstate the effect of earnings risk on expected lifetime earnings.

This positive relationship between earnings uncertainty and average earnings reinforces the findings of Campbell (1996) and Jagannathan and Wang (1996) that labor income risk is difficult to insure against. If risk-averse workers could pay to insure a steady earnings stream, then they would require only the price of this insurance as additional compensation to make them indifferent between more and less risky earnings streams. My results imply that managers, for example, would give up almost a quarter of their expected lifetime earnings in exchange for eliminating earnings risk, suggesting that earnings insurance is either woefully expensive or unavailable at any price.

B. The Role of Employment Risk and Occupation Transitions

The second column of Table 2.8 estimates the mean-variance relationship using the mean and variance of lifetime earnings simulated assuming workers remain employed in their starting occupation in all periods and face uncertainty only from earnings shocks. This exercise is closer to earlier papers estimating the relationship between the mean and variance of earnings, although my results still differ from these earlier papers by estimating lifetime risk. This relationship is also plotted in Figure 2.2.

The effect of removing endogenous employment transitions is ambiguous in theory. The variance of earnings could be higher without endogenous labor choices because workers cannot escape earnings shocks by changing occupations. The variance could also be lower because workers cannot choose to search out occupations with which they are well matched, so there is less churning and therefore less variance. Without employment risk and transitions, the variance of earnings falls for some occupations, most notably manufacturing, but rises for others, most notably artists and entertainers. The estimated mean of lifetime earnings is also somewhat smaller because workers cannot search for work in a new occupation even if they draw a very low match with their starting occupation. The net effect of these changes and increased measurement error from excluding some sources of risk is to lower the estimated risk-return tradeoff from 0.22 to 0.09 and reduces the R-squared of the regression from 0.17 to 0.09.

Another possibility is to include employment and occupation mobility in the simulations via a transition matrix. For this exercise, I define 38 states, employed or not employed in each of the 19 occupations, and estimate a quarterly transition matrix between each state using observed transitions in the CPS. This approach captures some of the risk of non-employment and occupation changes without making any assumptions about workers' utility or solving for a policy rule. However, it destroys the relationship between earnings shocks and mobility because workers are not endogenously choosing their transitions. This method effectively raises the job destruction rate since all observed transitions into non-employment are now treated as exogenous. The relationship between the mean and variance of simulated earnings with this exogenous mobility framework is presented in the third column of Table 2.8 and in Figure 2.3.

Figure 2.3 shows that the estimated mean and variance of lifetime earnings both fall when earnings are simulated with this exogenous transition matrix instead of the endogenous search model. In addition to eliminating the option for workers with low earnings to look for work in a new occupation, high earners face a larger probability that their job will be destroyed. The R-squared remains low in this specifications and the estimated risk-return tradeoff falls even farther to 0.07.

C. My Results in Context

I have not found other papers that calculate the total variance of lifetime earnings as I do, but the risk measures I use to construct these lifetime variances are in line with those estimated elsewhere. I estimate an average standard deviation of idiosyncratic earnings shocks of 0.083, which is the range of random walk earnings shocks estimated by Meghir and Pistaferri (2004), 0.105, Carroll and Samwick (1997), 0.074, and Low, Meghir, and Pistaferri (2010), 0.052.[16] Meghir and Pistaferri also include an MA(1) earnings shock, similar to the AR(1) occupation-wide productivity shocks I estimate, and also find that this medium-term shock has a standard deviation a little less than half the size of the permanent shock. Low, Meghir, and Pistaferri (2010) include a job search process similar to mine with variance of match quality, although they define match at the level of job rather than occupation. Their variance of match quality, 0.228, is slightly lower than my estimate of 0.251, which makes sense since they are comparing jobs within the same occupation as well as across occupations. Their quarterly job destruction and offer arrival rates are quite similar to my estimates if I sum the same-occupation and new-occupation arrival rates to compare to their single arrival rate.

Guvenen and Smith (2010) find that controlling for individual differences in the slope of earnings growth reduces their estimated variance of shocks to earnings. If workers know in advance how their lifetime earnings profile will differ from the average then these differences should not count as risk and failing to account for them will overstate workers' uncertainty about lifetime earnings. I partially control for differences in earnings growth by allowing accumulated tenure to affect earnings differently across

[16] Author's calculations of quarterly standard deviations of shocks based on the estimates reported in each paper.

occupations. Differences in earnings growth are also partially captured in my model by the different probabilities of moving in and out of work and between occupations. Increased mobility will lower earnings growth through spells of non-employment and through tenure loss. While the structure of shocks in Guvenen and Smith's paper makes it difficult to compare estimates directly, my collection of earnings shocks generate about the same annual standard deviation in earnings changes for workers who stay in the same occupation as their shocks.

Like earlier papers exploring risk-return trade-offs across occupations, I am assuming that the riskiness of occupations remains constant over time. My formulation captures fixed differences in the riskiness of earnings across careers, for example earnings fluctuate with the business cycle more in some occupations than others or the difference between individual success and failure is greater in some occupations than others. It does not capture the risk of a permanent change in occupation productivity due to technological change. Plots of the occupation effects over time do not show much evidence of time trends over my 20 year sample.[17] If this sort of technological shock is unforeseeable, then it represents a kind of unknown risk for which workers will not demand compensation. However, if riskiness evolves gradually over time, as in Meghir and Pistaferri (2004), then younger workers entering an occupation will have different expectations of risk, and require different compensation, from older workers who entered under different circumstances. My estimates would then give an average of both the riskiness and the expected earnings over the sample period.

[17] The breadth of my occupation categories works against these trends. While some sectors of manufacturing certainly declined from 1988 to 2007 the production sector as a whole does not exhibit a downward trend.

VI. Interpreting the Slope of the Risk-Return Frontier

For a relationship between risk and expected earnings to hold, workers must be somewhat substitutable across occupations; there must be a threat that risk-averse workers will flock toward less risky occupations unless occupations with higher risk become more attractive to workers through higher expected earnings. If workers were identical and occupations differed only by riskiness and expected return, than in equilibrium all populated occupations would line up along a frontier of expected lifetime earnings and earnings risk. Any occupation with higher expected earnings relative to its risk would attract all workers in the market. Any occupation below it would attract none.

In the simplest case of a single period of risky earnings, the slope of this frontier can be derived analytically as a function of the workers' coefficient of relative risk aversion. In equilibrium, workers must expect the same utility from entering any risky career, *k*, as from receiving riskless earnings Y_0,

$$E\left[\frac{Y_k^{1-\gamma}}{1-\gamma}\right]=\frac{Y_0^{1-\gamma}}{1-\gamma}. \tag{2.18}$$

After taking Taylor expansions of both sides around $E[Y_k]$ and rearranging, we see that in this case the expected earnings from risky career *k* is positively related to the ratio of the variance of earnings in career *k* to the expectation of earnings in that career,

$$E[Y_k]\approx Y_0+\frac{\gamma}{2}\frac{Var[Y_k]}{E[Y_k]}. \tag{2.19}$$

Under the intuition of equation (2.19), my estimated risk-return trade-off implies a coefficient of relative risk aversion of 0.43, much lower than the 1.5 value I use in my simulations and on the low end of the range of values estimated elsewhere.[18] In reality, however, the relationship between expected lifetime earnings and earnings uncertainty will differ from equation (2.19), perhaps substantially. Firstly, occupations and workers differ from one another along many dimensions beyond the scope of this chapter.[19] Compensating earnings differences for these other occupation traits, or rents for rare innate skills required for some occupations, will push the expected earnings from occupations away from the risk-return frontier. While I remove some worker heterogeneity from my simulations, my occupation-specific earnings intercepts are estimated from observed earnings and will therefore also reflect these additional sources of earnings differences across occupations. My estimated risk-return tradeoff is also the final product of several stages of estimation and is attenuated by measurement error.

Additionally, in a multi-period model where earnings shocks are revealed gradually, consumption is generally not equal to earnings. Workers will save and borrow to smooth consumption over the lifecycle. Once consumption differs from income, the indifference curve over the mean and variance of earnings can no longer be derived analytically. Instead, I computationally solve a model with two periods of risky earnings, where workers can choose to save or borrow in the first period, and compute the risk premia that make workers indifferent between a risk free stream of earnings and a set of risky options. In this two-period model the computed indifference curves become convex. For low levels of the ratio of the variance of total income to the mean the slope of the indifference curve is slightly lower in the two period case than in the single period

[18] Chetty (2006) surveys measures of the coefficient of relative risk aversion from a variety of observed labor decisions and finds estimates ranging from 0.44 to 1.78. Kimball, Sahm, and Shapiro (2008) estimate an average coefficient of relative risk aversion of 8.2 based on survey responses.

[19] Rosen (1986) surveys the large literature on compensating earnings differences for work characteristics ranging from the risk of physical harm to the public-mindedness of the work.

case above, but the required risk compensation rises at an increasing rate as the ratio of variance to the mean increases.

Finally, workers have different aversions to risk. If less risk-averse workers tend to enter occupations with higher earnings risk, than the slope of the observed risk-return frontier will reflect the risk aversion of the marginal worker at each level of riskiness rather than the average risk aversion across workers. Figure 2.4 plots possible convex indifference curves for two workers with different coefficients of relative risk aversion. The example demonstrates that if workers sort into occupations by risk preference, than the observed relationship between the earnings riskiness of occupations and their expected lifetime earnings could be much flatter than the slope of a single indifference curve using an average degree of risk aversion for all workers.

To measure the degree of occupational sorting by risk preference I use a survey-based measure of risk aversion. In 1996, the PSID asked heads of households in their sample a series of questions designed to elicit their willingness to accept uncertain income streams. The PSID question is based on the HRS risk tolerance question developed by Barsky, Juster, Kimball, and Shapiro (1997). Respondents were asked:

> Suppose you had a job that guaranteed you income for life equal to your current, total income. And that job was (your/your family's) only source of income. Then you are given the opportunity to take a new, and equally good, job with a 50-50 chance that it will double your income and spending power. But there is a 50-50 chance that it will cut your income and spending power by a third. Would you take the new job?

Respondents who said they would accept the risky job were then asked up to two additional versions of this question with larger downside risks. Respondents who rejected the initial risky option were asked up to two additional versions of the question with smaller downside risks. Individuals can therefore be grouped into six categories of risk aversion based on which, if any, of the risky job options they were willing to accept. Table 2.9 tabulates this categorization for the male heads of household in 1996 between the ages of 25 and 65 and with at least some college education.

Kimball, Sahm, and Shapiro (2008) describe a method for imputing cardinal measures of risk tolerance and risk aversion from these gamble responses, taking into account some measurement error in the questions as well as true variation in risk preferences across individuals. The fifth and sixth columns of Table 2.9 show the mean of these imputed preference parameters for individuals in each response category. I discuss this imputation method in more detail in Appendix 5. The PSID respondents demonstrate a wide range of risk preferences. Individuals who rejected all the risky jobs, the most common response, have an average imputed risk aversion of almost 6, while those who accepted all the risky jobs have an average imputed risk aversion of 1.2.

To assess the degree of sorting into occupations by risk preference, I calculate the mean imputed risk aversion for individuals in the 1996 risky job question sample by the occupation of their first observed job. Figure 2.5 plots this mean risk aversion for workers starting in each occupation against my occupation-specific measure of lifetime earnings risk. The plot shows a loose, negative relationship between the riskiness of an occupation and the average risk aversion, which we expect to see if workers are aware of the relative earnings riskiness of occupations, but weigh their aversion to risk against other factors, such as tastes and skills for particular types of work, when choosing a starting occupation. The correlation between average imputed risk aversion and lifetime earnings risk is -0.26, with an R-squared of 0.07.

Figure 2.5 indicates that workers do sort into occupations partially by risk preference, so we should expect the risk-return tradeoff in the market to be somewhat flatter than the indifference curve of an average worker. The extent of this flattening will depend not only on the degree of sorting but on the relative shape of the distributions of risk preferences and occupation riskiness. The risk premium will be higher if relatively risky occupations are plentiful relative to relatively less risk-averse workers, since this means that high risk occupations will have to attract some more risk-averse workers. As shown in Table 2.9, the modal risk preference category in the PSID is the most risk-averse one and the lowest risk aversion categories are the least populated. Figure 2.1 shows that the riskiest occupations, such as law and agriculture, tend to be smaller, which may reflect an equilibrium where few employers are able to pay wages high enough to induce the more risk averse workers to enter these careers.

The slope of the estimated risk-return tradeoff is positive, but lower than the prediction of a simple single-period model where the slope of the tradeoff is equal to half the coefficient of relative risk aversion. However, this lower slope is consistent with a multi-period model where workers can save to smooth consumption and where workers have heterogeneous risk preferences that influence their occupation choice. This slope can be interpreted as placing a lower bound of 0.43 on the average level of risk aversion among workers. As shown in Table 2.9, the actual average risk aversion is much higher.

VII. Conclusions

The expected discounted value of lifetime earnings in a career is positively related to the variance of lifetime earnings around that expectation. I describe and solve a model of optimal worker decisions over employment and occupation and simulate earnings streams that combine shocks to earnings while working, shocks to employment, and workers' optimal employment decisions in the face of these shocks. I then compare the mean and variance of the discounted value of simulated earnings streams for workers starting in different occupations. Increasing the ratio of the variance of lifetime earnings in an occupation to the mean lifetime earnings by $100,000 increases predicted discounted expected lifetime earnings by $22,000. The slope of this tradeoff is consistent with a model where workers can save and borrow to self-insure over earnings fluctuations and where workers have different preferences for risk and partially sort into occupations based on these preferences.

Compensation for lifetime earnings risk is quantitatively large. By my estimates, workers who start their career in financial occupations, who are in about the 75th percentile of occupation riskiness, would give up 20% of their expected lifetime earnings in exchange for completely eliminating earnings uncertainty. They would give up 8% of their expected lifetime earnings to reduce their uncertainty to the level of mechanics and electricians, the least risky occupation in my sample. Compensation for lifetime earnings uncertainty can explain 17% of the variation in average earnings across occupations. This explanatory power is substantially larger than compensating differences estimated for other job characteristics, even risk of injury and death (Deleire and Levy 2004).

However, compensation for earnings risk explains a far smaller share of overall income inequality. Far more of the variation in earnings comes from the realization of earnings shocks, the riskiness for which workers demand compensation, or differences in individual worker characteristics than from the differences in expected earnings across occupations. Subtracting my predicted compensation for earnings risk for each occupation reduces the total cross-sectional variance of real weekly earnings in the PSID by only 1%. In contrast, netting out the predicted differences in earnings by total work experience and occupation tenure reduces the cross-sectional variance by 45%.

The strong relationship between lifetime earnings riskiness and expected earnings suggests that this earnings uncertainty is not insurable. If it were, workers would require only the price of this insurance to enter a riskier occupation. This uninsurable risk generates inefficiencies in the labor market. Workers with different preferences for risk sort occupations partially based on the riskiness of earnings in that occupation, rather than matching only on their relative skills in different occupations and enjoyment of the work. The extra compensation workers demand for enduring earnings uncertainty raises the price of the goods and services they provide without increasing the workers' utility. Public programs like unemployment insurance, food stamps, and progressive income taxes compress earnings dispersion and can reduce these inefficiencies while increasing workers' welfare. However, the classic principal-agent model suggests that employers must share the risk of uncertain outcomes with their employees to induce high work effort. Some earnings uncertainty is inevitable and probably desirable, but the optimal level of earnings uncertainty, and the accompanying earnings inequality, is unclear. An interesting avenue for future work would be comparing the potential relative costs of reducing worker's incentives and reducing their disutility from uncertainty.

A1: Solving for the Policy Rule by Backward Induction

During retirement individuals face no uncertainty and no labor choices, so optimal consumption follows the standard Euler equation

$$C_{it+1} = \left(\beta R\right)^{1/\gamma} C_{it}, \tag{A2.1}$$

where $R = 1 + r$, and consumption becomes a linear function of assets at the start of retirement and pension income,[20]

$$C_{it} = \ell_0^t A_{iT+1} + \ell_0^t \ell_1 Y_{iL}. \tag{A2.2}$$

The value of entering the first period of retirement, T+1, with a ratio of assets to potential income A_{T+1} is therefore

$$V_{T+1}\left(A_{iT+1}, \Omega_{iT+1}\right) = \sum_{s=T+1}^{L} \beta^{s-T+1} \frac{\left(\ell_0^s A_{iT+1} + \ell_0^s \ell_1 Y_{iL}\right)^{1-\gamma}}{1-\gamma}. \tag{A2.3}$$

The value of arriving in the last period of work with each possible set of state variables, including occupation and employment status, and the optimal consumption under these states can be determined by plugging equation (A2.3) into equation (2.3) in the text. Optimal employment decisions are determined by comparing the value in each period of each employment option under optimal consumption choices. The value and optimal consumption choice in each earlier working period can then be determined the same way using backwards induction and the expectation of next period's employment state laid out in equations (2.9) and (2.10) in the text.

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[20] The parameters of this linear function are derived from plugging the Euler equation into the lifetime budget constraint as of the start of retirement. $\ell_1 = \frac{1-\left(\frac{1}{R}\right)^L}{1-\left(\frac{1}{R}\right)}$. In the first period of retirement,

$\ell_0^{T+1} = \frac{1-\beta^{1/\gamma} R^{1/\gamma - 1}}{1-\left(\beta^{1/\gamma} R^{1/\gamma - 1}\right)^L}$ and for the remaining periods, $\ell_0^{t+1} = \left(\beta R\right)^{1/\gamma} \ell_0^t$.

A2: Factoring Potential Earnings Out of the Value Functions

This derivation follows Carroll (2004). By plugging equation (A2.3) into equation (2.3) in the text, the value of being employed in the last period of working life, T, is

$$V_T^{1,k_t}\left(A_T,\Omega_T\right)=\max_{C_T}\left\{\frac{C_T^{1-\gamma}}{1-\gamma}+\beta\sum_{s=T+1}^{L}\beta^{s-T+1}\frac{\left(\ell_0^s A_{T+1}+\ell_0^s\ell_1 Y_L\right)^{1-\gamma}}{1-\gamma}\right\}. \qquad \text{(A2.4)}$$

Adding in the relationship between pension income and potential earnings in the last period of work, equation (A2.4) can be re-written as

$$P_T^{1-\gamma}v_T^{1,k_t}\left(a_T,\Omega_T\right)=P_T^{1-\gamma}\max_{c_T}\left\{\frac{c_T^{1-\gamma}}{1-\gamma}+\beta\sum_{s=T+1}^{L}\beta^{s-T+1}\frac{\left(\ell_0^s a_{T+1}+\ell_0^s\ell_1 pen\right)^{1-\gamma}}{1-\gamma}\right\}, \qquad \text{(A2.5)}$$

where lowercase letters denote the ratio with potential earnings, $a_t=\frac{A_t}{P_t}$.

The dynamic budget constraint becomes

$$a_{t+1}=\frac{R}{g_{t+1}}\left(a_t+1-c_t\right), \qquad \text{(A2.6)}$$

where $g_{t+1}=\frac{P_{t+1}}{P_t}$ is growth in potential income, which could be less than 1.

The equivalent expression for workers not employed in the last period is

$$\left(bP_T\right)^{1-\gamma}v_T^{0,k_t}\left(a_T,\Omega_T\right)=\left(bP_T\right)^{1-\gamma}\max_{c_T}\left\{\frac{c_T^{1-\gamma}}{1-\gamma}+\beta\sum_{s=T+1}^{L}\beta^{s-T+1}\frac{\left(\ell_0^s a_{T+1}+\ell_0^s\ell_1 pen\right)^{1-\gamma}}{1-\gamma}\right\}. \qquad \text{(A2.7)}$$

Now consider the value of being employed in occupation k in period T-1 from equations (2.3) and (2.9) in the text,

$$V_{T-1}^{1,k_{T-1}}\left(A_{T-1},\Omega_{T-1}\right)=\max_{C_{T-1}}\left\{\begin{array}{l}\dfrac{C_{T-1}^{1-\gamma}}{1-\gamma}+\beta\delta_{k_t}E_{T-1}\left[V_T^{0,k_t}\left(A_T,\Omega_T\right)\right]+\\ \beta\left(1-\delta_{k_t}\right)E_{T-1}\left[\max_{N_T}\left\{V_T^{0,k_t}\left(A_T,\Omega_T\right),V_T^{1,k_t}\left(A_T,\Omega_T\right)\right\}\right]\end{array}\right\}. \quad (A2.8)$$

Dividing through by $P_{T-1}^{1-\gamma}$ and incorporating (A.5) and (A.7) yields

$$v_{T-1}^{1,k_{T-1}}\left(a_{T-1},\Omega_{T-1}\right)=\max_{c_{T-1}}\left\{\begin{array}{l}\dfrac{c_{T-1}^{1-\gamma}}{1-\gamma}+\beta\delta_{k_t}E\left[\left(bg_T\right)^{1-\gamma}v_T^{0,k_{T-1}}\left(a_T,\Omega_T\right)\right]+\\ \beta\left(1-\delta_{k_t}\right)E\left[\max_{N_T}\left\{\left(bg_T\right)^{1-\gamma}v_T^{0,k_{T-1}}\left(a_T,\Omega_T\right),g_T^{1-\gamma}v_T^{1,k_{T-1}}\left(a_T,\Omega_T\right)\right\}\right]\end{array}\right\}. \quad (A2.9)$$

The same process can be worked backwards for the value of non-employment in T-1 and for all earlier periods.

A3. Data Notes

Current Population Survey

In my CPS sample I am restricted to considering only men who have worked sometime in the past five years because people who have not been employed in more than five years are not asked about their current or most recent occupation. I must also exclude the self-employed because they are not asked the ORG earnings supplement. I also exclude respondents with imputed occupations and those with imputed earnings. Within this sample, I count people classified as both "unemployed" and "not in labor force" as not employed.

To calculate quarterly transition rates between occupations I match the 1st and 4th interviews and the 5th and 8th interviews with each household. CPS interviews are conducted by address, so if a family moves between surveys they drop out of the sample and the new family that moves into that address is surveyed in their place. Some families also drop out of the survey by choice, or are misidentified so that they cannot be linked with their previous interviews. I use a method based on the one used by Madrian and Lefgren (2000) to check the matches by household identifier by comparing the age and race of household members. I am able to match 88% of 4th and 8th interview respondents to their 1st or 5th interview three months earlier.

Panel Study of Income Dynamics

The PSID interviewed a set of U.S. households in 1968 and has continued to re-interview those households and their descendants annually until 1997 and bi-annually since then. The PSID sample began with both a cross-section of U.S. households and an oversample of low-income households and has undergone several additions and subtractions over the survey period. I use inverse probability weights constructed by the PSID to adjust for the oversamples and differential non-response rates over time. Only heads of households and their wives (the PSID convention is to identify the male member of heterosexual couples as the head of household) are asked the detailed work and earnings questions I need for my analysis. Since I look only at men my sample consists of male heads of household. This PSID sample is the same as the CPS sample with the noted exception that I can and do include self-employed workers in the PSID sample, since self-employment captures an important element of risk.

The PSID only calculates weights for original sample family members or blood descendants of these family members. When the daughter of a sample family marries a man, the man becomes the head of household, and therefore a member of my sample, but his PSID individual weight is equal to zero unless he is also a descendent of a sample family. Excluding men who marry into the PSID sample would reduce my sample by nearly half. Instead, I assume that the husband's probability of being in the sample is approximately equal to his wife's and use her weight for the husband. The PSID makes the same assumption when calculating family-level weights. Using this approach I am able to construct sampling weights for 95% of the respondents for whom I have earnings data.

A4: Measuring Occupation Changes and Occupation Tenure in the PSID

I use surveys from 1981-2007 to measure experience and tenure for the 1988-2007 estimation sample. Since 1981, the PSID has asked about the starting dates and/or the length in months of heads' time with their current employer and time in their current job position. Kambourov and Manovskii (2010) develop a method of using these questions in the PSID to reduce the number of errantly identified occupation changes. Following their approach, I consider a respondent to have changed occupations only if their occupation code differs from the one assigned in the previous year and they also report having changed employers or positions since the last interview. I consider a worker to have changed employer or position if their reported start date (or start date implied by their interview date and reported tenure) falls after the date of their previous interview. All respondents who are not employed at the time of the interview are counted as remaining in their previous occupation.

I set occupation tenure to current position tenure (or employer tenure if position tenure is missing) in the first year a respondent appears in the sample. For each following year, occupation tenure increases by the time since the last interview if the respondent worked at least 20 weeks in the previous year and did not change occupations.[21] Occupation tenure stays the same if the respondent did not change occupations but worked less than 20 weeks in the previous year. If the respondent changed occupations since the last interview their new occupation tenure is set to half the time since the last interview. Total labor market experience is set to potential experience (age minus schooling minus 6) in the first year of the survey. In subsequent years, total labor market experience increases by the time since the last interview if the respondent worked at least 20 weeks in the previous year and stays the same if the respondent worked less than 20 weeks. Increases in tenure and experience do not depend on current labor market status, only on work during the previous year.

A5. Imputing Risk Aversion from PSID Survey Responses

I follow the method described in Kimball, Sahm, and Shapiro (KSS 2009) for imputing a cardinal measure of risk aversion from the responses to a series of risky job choice questions asked in the 1996 PSID. More documentation on this imputation method is available at http://www-personal.umich.edu/~shapiro/data/risk_preference/. Kimball, Sahm, and Shapiro (2008) use multiple waves of these gamble responses in the Health and Retirement Survey (HRS) to separately identify the true variance of risk preferences and the variance of transitory response error. In that paper they also estimate the status-quo bias induced by asking respondents to choose between their current job and a new risky job, the framework of the question in the PSID, rather than presenting a choice between two new jobs, as is done in the more recent waves of the HRS. I impose estimates of the true variance of risk preferences and the status quo bias estimated from

[21] Setting the bar for accumulating experience and tenure at 10 or 30 weeks worked does not appreciably change the results.

the HRS and reported in KSS and estimate the mean of log risk tolerance and the variance of transitory response error from the single wave of PSID responses using maximum likelihood on the probability of a respondent ending up in each response category. These estimates are reported in the first column of Table A2.2.

I use all 3,682 male heads of household between the ages of 25 and 65 to estimate these parameters of the distribution of risk preferences. However, since I estimate the riskiness of occupations only for workers with at least some college education, I allow these workers to have a separate mean log risk tolerance and error variance. These questions were asked only in the 1996 survey to heads of household of all ages. KSS find that risk aversion increases predictably by age. Since I want to compare risk preference to the choice of first occupation, I allow the mean of log risk tolerance to depend on a quadratic of age and then net out these age effects when calculating imputed risk preferences. The second column of Table A2.2 shows the estimated effects of age and education on the mean and error variance of log risk tolerance.

I use the formulas in KSS to impute risk aversion, γ, and risk tolerance, $1/\gamma$, for each respondent using the parameters reported in Table A2.2. Table A2.3 reports the mean of these risk preferences by age using the estimates with calibrated response error, the estimates with calibrated response error allowing the mean and error variance to depend on covariates, and with calibrated response error and covariates after netting out the expected changes in risk preferences with age. This last specification is the one I use in Table 2.9 and Figure 2.5.

Table 2.1: Estimated Parameters

Symbol	Description	Count	Estimation method	Dataset
ϕ	Effect of total labor experience on earnings (linear)	1	Method of moments	PSID
ψ_k	Effect of occupation tenure on earnings (quadratic)	2*K	Method of moments	PSID
μ_k	Occupation fixed effect on earnings	K	Method of moments	CPS
ρ_k	Persistence of occupation-wide variable earnings effect	K	Method of moments	CPS
σ_{ke}^2	Variance of shock to occupation-wide variable earnings effect	K	Method of moments	CPS
σ_{α}^2	Variance of worker-occupation match	1	Method of moments	PSID
σ_{ku}^2	Variance of shock to individual earnings ability	K	Method of moments	PSID
σ_{ξ}^2	Variance of transitory earnings shock	1	Method of moments	PSID
δ_k	Exogenous separation rate	K	Indirect Inference	PSID
λ_{ck}	Current occupation job offer arrival rate	K	Indirect Inference	PSID
λ_{nk}	New occupation job offer arrival rate	K	Indirect Inference	PSID
b	Share of potential earnings received during non-employment	1	Indirect Inference	PSID

K=19 is the number of occupation categories. Total number of parameters to estimate is 9*K+4=175.

Table 2.2: Occupation Fixed Effect and Stochastic Productivity

	μ_k : occupation fixed effect	ρ_k : persistence of occupation productivity shock	σ_{ke} : std. dev of occupation productivity shock
Cross-occupation	*6.664*	*0.656*	*0.032*
	(0.005)	*(0.063)*	*(0.003)*
Management	6.897	0.839	0.025
	(0.003)	(0.034)	(0.002)
Financial	6.716	0.696	0.031
	(0.005)	(0.053)	(0.002)
Computers	6.837	0.709	0.022
	(0.004)	(0.074)	(0.002)
Engineering	6.876	0.800	0.031
	(0.005)	(0.046)	(0.003)
Sciences	6.695	0.699	0.05
	(0.008)	(0.058)	(0.004)
Community	6.293	0.379	0.046
	(0.009)	(0.094)	(0.003)
Legal	7.050	0.569	0.065
	(0.012)	(0.071)	(0.006)
Education	6.455	0.379	0.030
	(0.006)	(0.091)	(0.003)
Entertainment	6.537	0.376	0.049
	(0.012)	(0.089)	(0.005)
Health	6.737	0.716	0.044
	(0.007)	(0.074)	(0.004)
Protection	6.604	0.406	0.038
	(0.007)	(0.074)	(0.003)
Maintenance	6.077	0.261	0.048
	(0.011)	(0.093)	(0.004)
Sales	6.604	0.779	0.026
	(0.004)	(0.053)	(0.002)
Office support	6.397	0.568	0.031
	(0.005)	0.074)	(0.003)
Agriculture	6.245	0.420	0.076
	(0.019)	(0.072)	(0.005)
Construction	6.647	0.421	0.032
	(0.006)	(0.089)	(0.003)
Mechanics	6.63	0.559	0.024
	(0.005)	(0.089)	(0.002)
Manufacturing	6.566	0.691	0.025
	(0.004)	(0.061)	(0.002)
Transportation	6.387	0.617	0.034
	(0.006)	(0.071)	(0.003)

Source: CPS, 1988-2007. Block-bootstrapped standard errors from 100 replications in parentheses. The first row is observation-weighted averages across occupations. Standard deviation and persistence are for shocks at quarterly frequency. The occupation fixed effects are reported in log form for weekly earnings. The estimated average occupation effect in real quarterly earnings is 13*exp(6.6)≈ $10,188 in 2000 dollars.

Table 2.3: Effects of Occupation Tenure on Log Weekly Earnings

	Years / 10	(Years/10)^2	Effect of 5 years
Total Experience	0.091		0.046
	(0.009)		
Occupation Tenure			
Cross-occupation	*0.373*	*-0.087*	*0.137*
	(0.065)	*(0.026)*	
Management	0.248	-0.039	0.115
	(0.048)	(0.018)	
Financial	0.376	-0.084	0.167
	(0.068)	(0.025)	
Computers	0.321	-0.084	0.140
	(0.035)	(0.012)	
Engineering	0.387	-0.115	0.165
	(0.056)	(0.023)	
Sciences	0.549	-0.168	0.232
	(0.071)	(0.028)	
Community	0.274	-0.086	0.115
	(0.111)	(0.059)	
Legal	0.629	-0.169	0.272
	(0.102)	(0.033)	
Education	0.268	-0.049	0.122
	(0.050)	(0.017)	
Entertainment	0.497	-0.128	0.217
	(0.075)	(0.035)	
Health	0.599	-0.164	0.259
	(0.103)	(0.044)	
Protection	0.357	-0.068	0.162
	(0.066)	(0.026)	
Maintenance	0.300	-0.013	0.147
	(0.152)	(0.067)	
Sales	0.526	-0.129	0.231
	(0.063)	(0.024)	
Office support	0.358	-0.067	0.162
	(0.053)	(0.022)	
Agriculture	0.759	-0.186	0.333
	(0.104)	(0.037)	
Construction	0.256	-0.066	0.111
	(0.068)	(0.029)	
Mechanics	0.329	-0.081	0.144
	(0.053)	(0.019)	
Manufacturing	0.349	-0.087	0.153
	(0.050)	(0.019)	
Transportation	0.561	-0.150	0.243
	(0.130)	(0.050)	

Source: PSID, 1988-2007. Block bootstrapped standard errors from 100 replications in parentheses. Estimated by OLS from a log weekly earning regression (N=25,835). First row reports observation-weighted averages for occupation tenure parameters. Coefficients reported are the effect of quarters of tenure or experience /10 on log weekly earnings. Effects of 5 years of tenure are calculated from columns 1 and 2.

Table 2.4: Estimates from Individual Earnings Residuals

	σ_{uk} : std. dev. of individual productivity shock	σ_{α} : std. dev of match quality	σ_{ξ} : std. dev. of transitory earnings shock
Cross-occupation	*0.083*	*0.251*	*0.181*
	(0.011)	*(0.013)*	*(0.005)*
Management	0.077		
	(0.007)		
Financial	0.087		
	(0.011)		
Computers	0.065		
	(0.008)		
Engineering	0.065		
	(0.021)		
Sciences	0.085		
	(0.012)		
Community	0.129		
	(0.015)		
Legal	0.090		
	(0.018)		
Education	0.077		
	(0.011)		
Entertainment	0.105		
	(0.012)		
Health	0.099		
	(0.013)		
Protection	0.065		
	(0.010)		
Maintenance	0.116		
	(0.018)		
Sales	0.105		
	(0.011)		
Office support	0.090		
	(0.010)		
Agriculture	0.152		
	(0.023)		
Construction	0.096		
	(0.022)		
Mechanics	0.070		
	(0.018)		
Manufacturing	0.053		
	(0.009)		
Transportation	0.090		
	(0.010)		

Source: PSID, 1988-2007, for male workers not currently in school or in the armed forces with at least some college education. Estimated using GMM using the within-person change in residuals from the wage regression presented in Table 2.3. Block-bootstrapped standard errors from 100 replications in parentheses. Standard deviation of productivity shock is for shocks at quarterly frequency.

Table 2.5: Labor Market Parameters

	δ_k : exogenous job destruction rate	λ_{ck} : offer arrival rate for current occupation	λ_{nk} : offer arrival rate for new occupation	b : share of earnings received in non-employment
Cross-occupation	*0.041*	*0.293*	*0.415*	*0.029*
	(0.004)	*(0.026)*	*(0.037)*	*(0.003)*
Management	0.037	0.263	0.437	
	(0.004)	(0.015)	(0.044)	
Financial	0.046	0.382	0.456	
	(0.004)	(0.032)	(0.028)	
Computers	0.037	0.346	0.267	
	(0.004)	(0.028)	(0.026)	
Engineering	0.048	0.254	0.459	
	(0.004)	(0.016)	(0.032)	
Sciences	0.052	0.195	0.218	
	(0.006)	(0.021)	(0.035)	
Community	0.067	0.425	0.472	
	(0.006)	(0.042)	(0.050)	
Legal	0.053	0.328	0.455	
	(0.005)	(0.023)	(0.032)	
Education	0.020	0.373	0.495	
	(0.002)	(0.045)	(0.037)	
Entertainment	0.046	0.393	0.312	
	(0.005)	(0.028)	(0.032)	
Health	0.049	0.321	0.547	
	(0.005)	(0.028)	(0.042)	
Protection	0.048	0.241	0.371	
	(0.004)	(0.024)	(0.040)	
Maintenance	0.091	0.177	0.239	
	(0.006)	(0.026)	(0.022)	
Sales	0.038	0.264	0.437	
	(0.004)	(0.030)	(0.056)	
Office support	0.022	0.158	0.506	
	(0.002)	(0.028)	(0.036)	
Agriculture	0.052	0.412	0.288	
	(0.004)	(0.032)	(0.031)	
Construction	0.044	0.211	0.269	
	(0.004)	(0.025)	(0.026)	
Mechanics	0.033	0.432	0.431	
	(0.003)	(0.037)	(0.039)	
Manufacturing	0.038	0.341	0.473	
	(0.003)	(0.024)	(0.028)	
Transportation	0.054	0.273	0.371	
	(0.004)	(0.033)	(0.040)	

Source: Estimated by indirect inference from PSID 1988-2007. Separation and offer arrival rates are at quarterly frequency. Block bootstrapped standard errors from 100 replications in parentheses.

Table 2.6: Fit for Indirect Inference Matched Moments

Measure	Average moment from data	Average moment from simulations	Average gap	Max (abs(gap))
Duration of non-employment				
All ages	1.4	2.4	-1.0	2.0
Average Tenure				
Age 25-32	15.3	12.2	3.2	5.8
Age 33-48	35.5	36.5	-1.0	14.1
Age 49-65	59.0	58.8	0.2	3.4
Pr(change occupation)				
Age 25-32	0.158	0.064	0.094	0.306
Age 33-40	0.096	0.062	0.034	0.180
Age 41-65	0.070	0.053	0.017	0.159

The moments are calculated separately by starting occupation. Moments from data are calculated from the PSID. Duration of non-employment and average tenure are measured in quarters. Probability of occupation change is measured over a year. Data and simulated moments and average gap are inverse variance-weighted averages across occupations. Gap is data moment minus simulated moment. The last column is the maximum absolute different between the data and simulated moments across occupations, that is, the worst fit.

Table 2.7: Experience and Occupation Mobility in Observed and Simulated Data

	Total Experience		Pr(change occupation at least once)		
	Mean	Std. dev.	4 years	10 years	20 years
Observed					
Age 25 to 32	7.8	3.3	0.39	0.56	0.71
Age 33 to 40	15.8	3.5	0.26	0.45	0.62
Age 41 to 49	23.5	3.2	0.20	0.37	0.43
Age 49 to 56	31.2	3.1	0.18	0.33	--
Age 57 to 65	39	3.1	0.14	--	--
Simulated					
Age 25 to 32	3.6	2.2	0.23	0.49	0.72
Age 33 to 40	10.9	2.3	0.21	0.43	--
Age 41 to 49	18.2	2.4	0.19	0.38	0.57
Age 49 to 56	25.5	2.5	0.16	0.33	--
Age 57 to 65	32.8	2.7	0.15	--	--

Observed moments from PSID, 1988-2007. Experience is measured in years. Share of respondents who change occupations at least once over 4-, 10-, or 20-year spans. Spans are non-overlapping within individual, but the same respondent may contribute more than one 4-, 10-, or 20-year span. I observe at most one 20-year span in the 26 years of unbalanced panel PSID data. I observe exactly two non-overlapping spans in the 40 years of simulated data.

Table 2.8: Expected Earnings and Earnings Uncertainty

	Full risk specification	Earnings risk only	Exogenous employment transitions
Constant	1,602,279	1,574,188	1,351,433
	(116,993)	(117,147)	(46,655)
$Var[Y_k]/E[Y_k]$	0.215	0.092	0.068
	(0.073)	(0.105)	(0.049)
N	19	19	19
R-squared	0.165	0.025	0.034

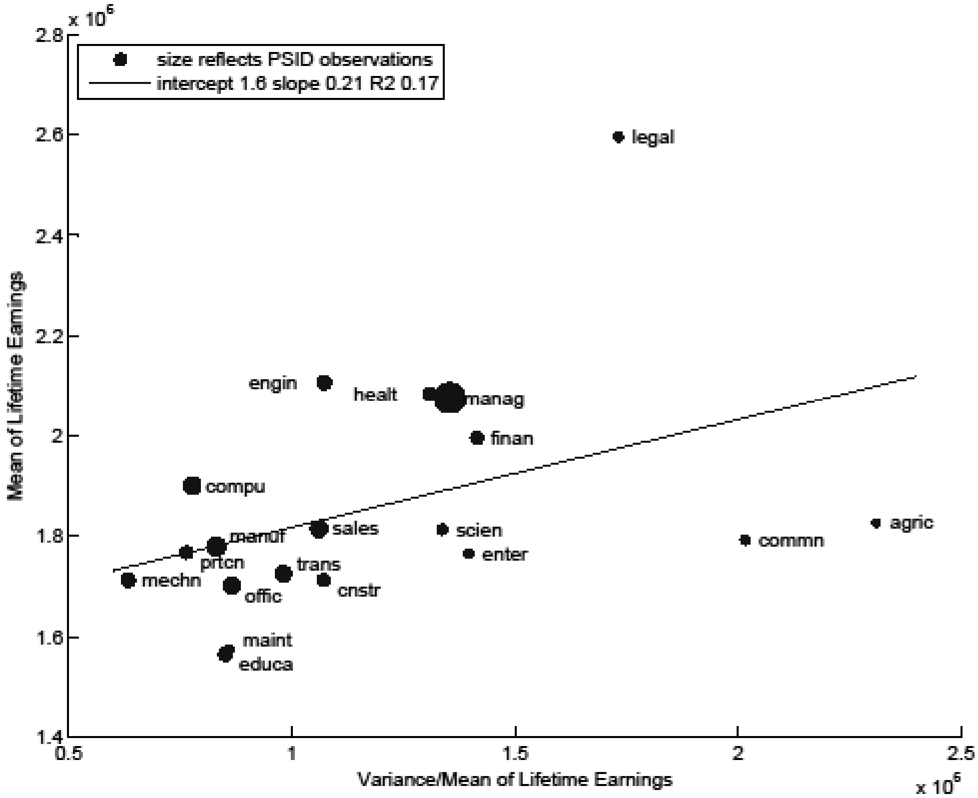
This table presents the results of an OLS regression of the mean of simulated total discounted lifetime earnings on a constant and the ratio of the variance of total discounted lifetime earnings to the mean. Block bootstrapped standard errors from 100 replications in parentheses.

Table 2.9: Responses to Risky Job Questions in the PSID

Response Category	Downside Risk of Risky Job		Count	Average Imputed Risk Aversion	Average Imputed Risk Tolerance
	Accepted	Rejected			
1	None	1/10	486	5.82	0.30
2	1/10	1/5	376	3.70	0.45
3	1/5	1/3	330	2.99	0.56
4	1/3	1/2	303	2.42	0.69
5	1/2	3/4	298	1.79	0.94
6	3/4	None	149	1.15	1.51

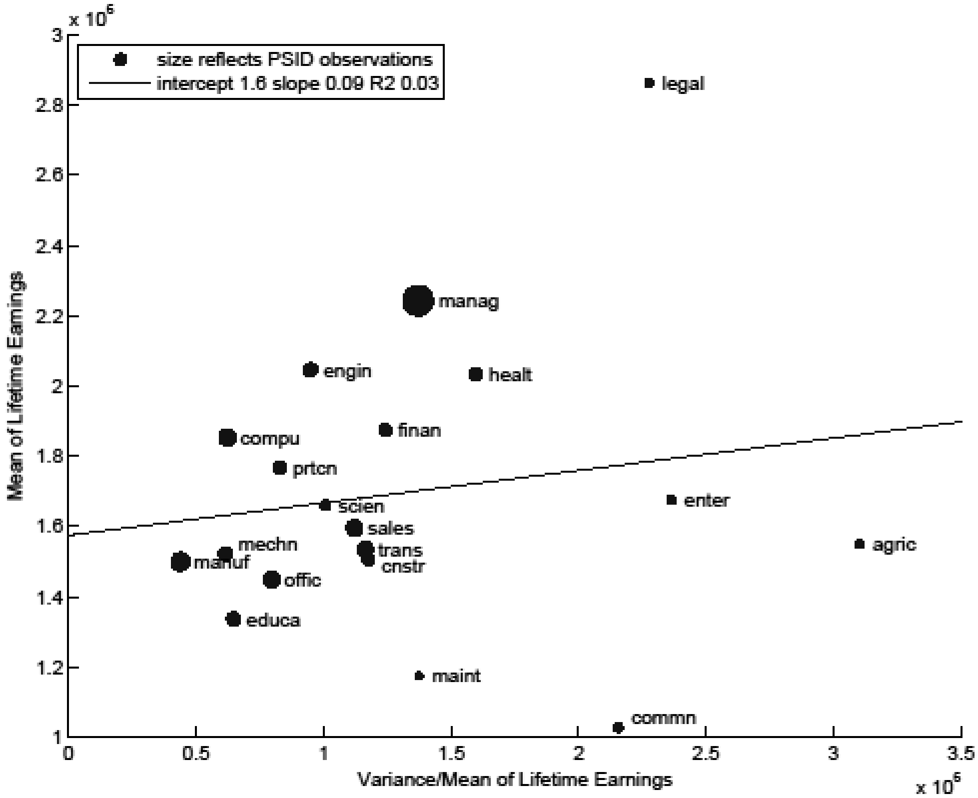
Responses from male heads of household between the ages of 25 and 65 with at least some college education in the 1996 PSID.

Figure 2.1: Expected Value and Variance of Lifetime Earnings



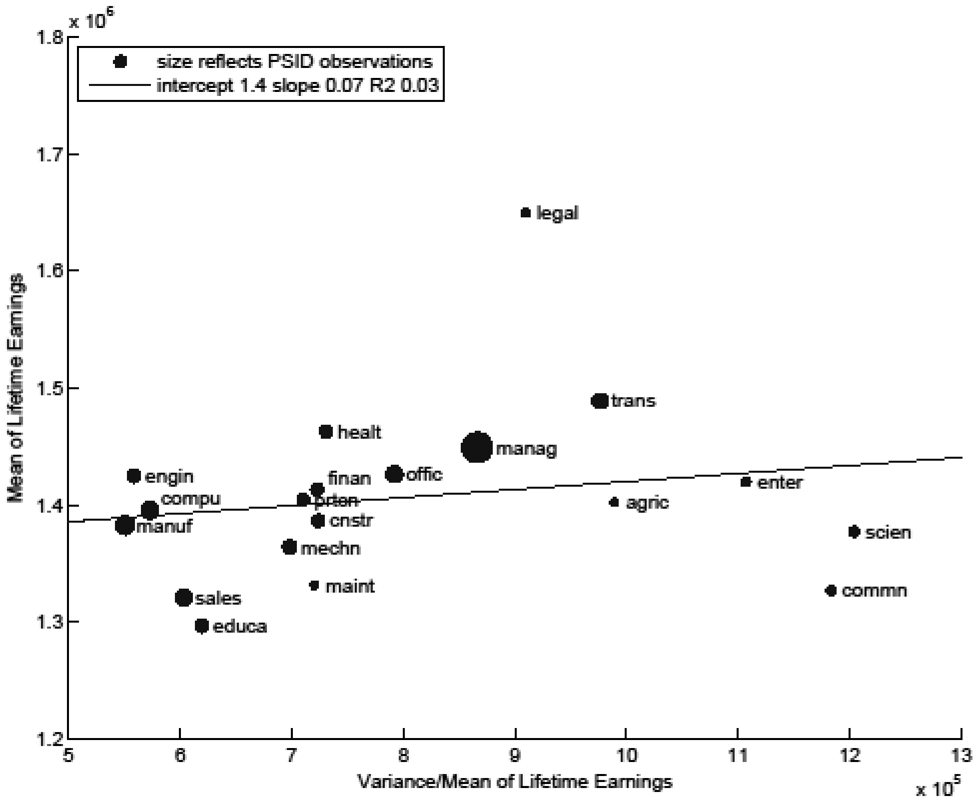
Source: Simulated lifetime earnings streams for workers starting in each occupation, as described in the text. Standard errors for the best fit line reported in Table 2.8.

Figure 2.2: Expected Value and Variance of Lifetime Earnings, Earnings Risk Only



Source: Simulated lifetime earnings streams for workers starting in each occupation, as described in the text. Standard errors for the best fit line reported in Table 2.8.

Figure 2.3: Expected Value and Variance of Lifetime Earnings, Exogenous Mobility



Source: Simulated lifetime earnings streams for workers starting in each occupation, as described in the text. Standard errors for the best fit line reported in Table 2.8.

Figure 2.4: Risk-Return Tradeoffs with Heterogeneous Risk Preferences

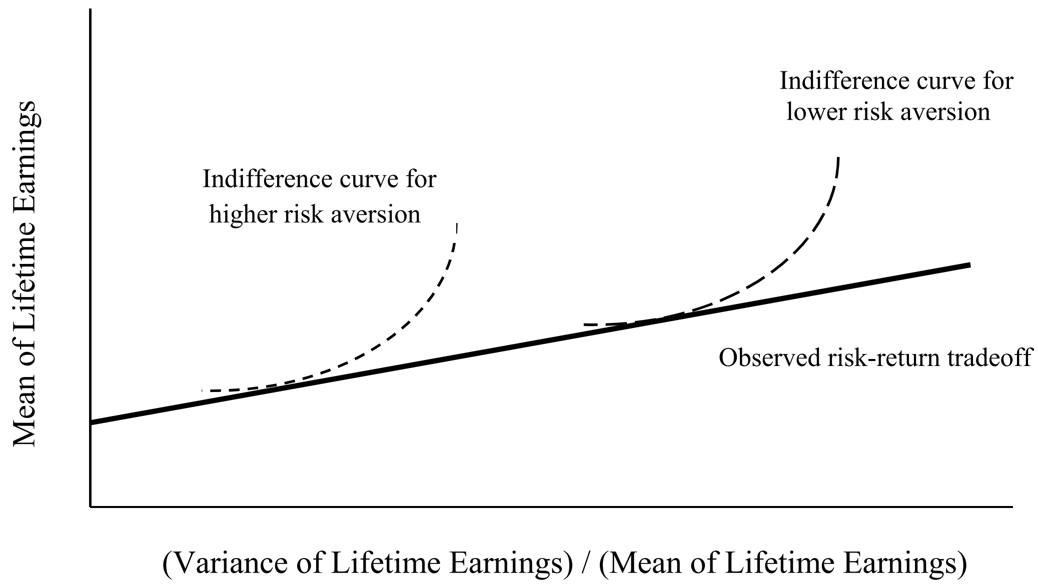


Figure 2.5: Risk Aversion and Riskiness of First Occupation

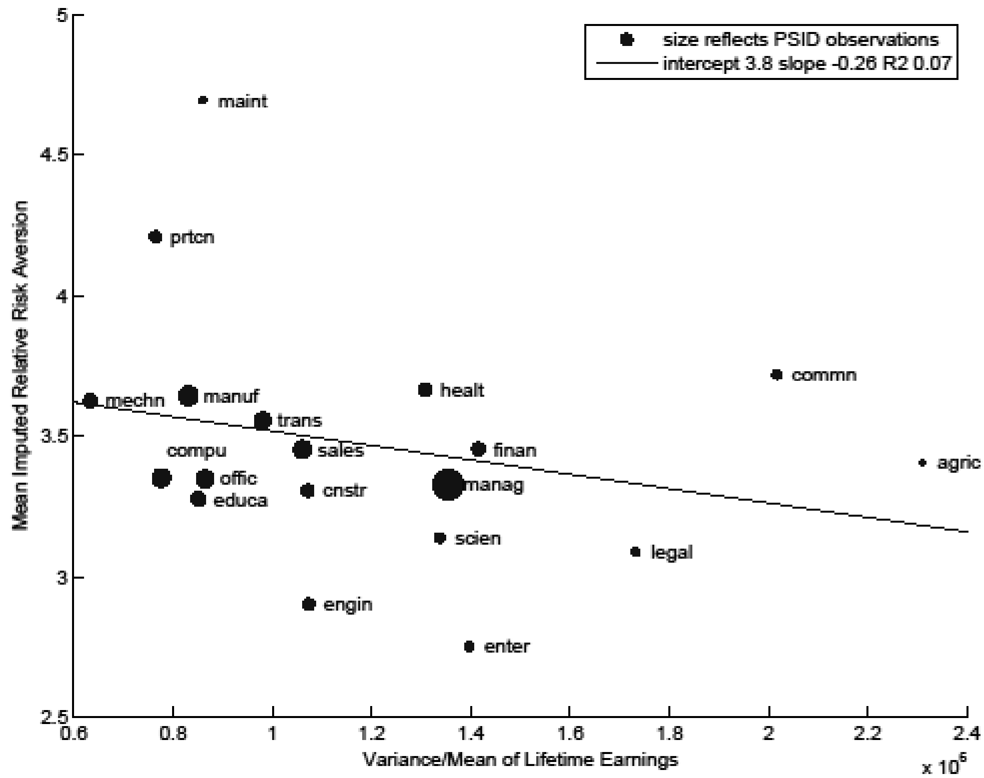


Table A2.1: Description of Occupation Categories

Number	Name	Description
1	Management	Management occupations
2	Financial	Business, insurance, and financial operations occupations
3	Computers	Computer and mathematical occupations
4	Engineering	Architecture and engineering occupations
5	Sciences	Life, physical, and social science occupations
6	Community	Social service and religious occupations
7	Legal	Lawyers, judges, and legal support occupations
8	Education	Education, training, and library occupations
9	Entertainment	Arts, design, entertainment, sports, and media occupations
10	Health	Healthcare practitioners, technicians, and support occupations
11	Protection	Law enforcement, firefighters, investigators, guards, and other protection
12	Maintenance	Building and grounds keeping, household workers, and maintenance
13	Sales	Sales and advertising occupations
14	Office support	Office and administrative support occupations
15	Agriculture	Farming, fishing, and forestry occupations
16	Construction	Construction, including skilled craftsmen, and extraction occupations
17	Mechanics	Mechanical and electrical installation and repair occupations
18	Manufacturing	Manufacturing and production occupations
19	Transportation	Transportation and material moving occupations

This grouping is based on the 2000 Census 22 Major Occupation Groups, excluding armed services, defined by Bureau of Labor Statistics. I combine health professionals and technicians with health support occupations and exclude food and personal service workers because there are too few observations of workers in these occupations with at least some college education. Occupations are coded at the 1980, 1990, and 2000 3-digit Census code level in the CPS and the 1970 and 2000 3-digit Census code level in the PSID. I use the mapping developed by CPS IPUMS to map different years of Census codes into 2000 Census 3-digit codes, and the CPS Utilities mapping from those into the broad occupation groups.

Table A2.2: Determinants of Log Risk Tolerance

	Calibrating Response Error	Conditioning on Age and Education
Log of risk tolerance		
Mean	-1.01	-1.01
	(0.03)	(0.03)
At least some college		0.07
		(0.22)
Age		-0.04
		(0.03)
Age-squared		0.34
		(0.06)
Variance	0.76	0.76
Status-quo bias	-0.21	-0.21
Transitory response error		
Variance	1.62	1.56
	(0.08)	(0.07)
At least some college		-0.16
		(0.15)

Note: Asymptotic standard errors are in parentheses. Estimates use job gamble responses from 3,682 male heads of household in the 1996 PSID between the ages of 25 and 65.

Table A2.3: Responses to Risky Job Questions by Age

		Average Imputed Risk Aversion			Average Imputed Risk Tolerance
Age Category	Count	Calibrating Response Error	Conditioning on Age and Education	Adjusting for Age Effects	Adjusting for Age Effects
Age 25 to 32	374	3.53	2.63	3.42	0.61
Age 33 to 40	585	3.77	3.09	3.50	0.61
Age 41 to 49	605	3.90	3.66	3.42	0.62
Age 49 to 56	267	3.95	4.32	3.28	0.68
Age 57 to 65	111	4.39	6.11	3.37	0.68

Responses from male heads of household between the ages of 25 and 65 with at least some college education in the 1996 PSID.

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Chapter 3

The College Earnings Premium
and Changes in College Enrollment

I. Introduction

From 1980 to 2010, annual earnings for workers with at least a college degree increased over 30% relative to earnings of workers with only a high school diploma. Over the same period, the share of high school graduates starting at four-year colleges rose from 34% to 42%. I explore how high school graduates respond to the rising returns to college when deciding whether to enroll. Students considering starting college must weigh the short-term costs of attendance, including both direct tuition and effort costs and the opportunity cost of staying in school instead of entering the labor force immediately, against the delayed payoff of higher lifetime earnings if they have a college degree. Students cannot know their own earnings in advance, but they can build expectations of their earnings at different education levels based on earnings in the current labor market. If students are in fact looking at the current labor market as a guide to their own potential earnings, then the recent rise in the college earnings premium has made college a much more attractive option for new high school graduates.

The responsiveness of college enrollment to changes in the earnings benefits of college is an important mechanism for labor market adjustment. Increases in the demand for more educated workers, driven perhaps by the greater use of new technologies, will push up the relative earnings of college-educated workers. Over time, these higher relative earnings should raise the supply of college-educated workers, which in turn should push the relative earnings of these workers back down. Goldin and Katz (2008) document a century of changes in the share of workers with high school and college education and their relative earnings that is suggestive of a series of shocks to the demand for increasingly educated workers followed by a gradual rise in the supply of educated workers. However, this adjustment will only take place if high school graduates, or at least their parents, recognize the increasing earnings benefits of a college education and respond to it by entering college at greater rates.

I develop a model in which high school graduates make the choice of whether to enroll in college based on the direct and indirect costs of college, their expectation of their future earnings with and without a college degree, and their own abilities, which also affect their potential earnings. The higher earnings associated with a college degree are not the only benefit of attending college and may not be the primary motivation for all college-goers. Social and economic backgrounds and pressures play an important, and perhaps a dominant, role for many students. The education and job opportunities that college provides are desirable because they are interesting and often more stable as well as because they pay well. However, the monetary value of a college degree can be important for students who are on the margin of deciding to start college; they are the relevant students when thinking about changes in college enrollment over time.

I use 41 years of data from the 1970 to 2010 Current Population Surveys to estimate the relationship between the college premium and college enrollment, controlling for changing college costs and for students' backgrounds. I find clear evidence that students do respond to changes in the relative earnings of college-educated workers when deciding whether to begin college themselves. On average, a 10% increase in my measure of the relative lifetime earnings for college-educated workers raises the probability that a high school graduate will attend college by 1%. Increases in these relative lifetime earnings for college graduates can explain more than half of the rise in college enrollment between 1980 and 2000. My model also explains the flat enrollment rate since 2000.

The feedback from the market returns to education to schooling choices has been studied from several angles. Richard Freeman (1975, 1976) estimates a strong reduced form relationship between the market college earnings premium and the contemporaneous share of young white men enrolling in college from the 1950s to the mid-1970s. Willis and Rosen (1979) used each cohort's actual ex-post earnings as a proxy for their expected earnings at different levels and found that earnings expectations played an important role in determining college enrollment choice. More recently, Heckman, Lochner, and Taber (1998) and Laitner (2000) developed models of the relationship between returns to education and school choice and demonstrated how an increase in the returns to schooling, through a less progressive income tax in Heckman, Lochner, and Taber and through skill-biased technological change in Laitner, would increase educational attainment.

I expand on the existing literature by combining an explicit model of college choice with estimation over a long period of data, including both the decline in the relative earnings of college-educated workers in the early 1970s and the dramatic increase since then. Like Freeman, I look specifically at how potential college students react to what they can see in the labor market at the time they are making their enrollment choice, as opposed to their actual ex post earnings. Cunha and Heckman (2007) compare college choice to ex post earnings and conclude that students foresaw some, but not all, of the recent rise in the college earnings premium. In addition to the empirical evidence in Freeman (1976), survey evidence suggests that students are also using current earnings to forecast their own future earnings.[22] Unlike Freeman, I control for changes in college tuition and individual student characteristics when measuring the effect of expected earnings on college attendance. I also construct a measure of projected lifetime earnings using current market earnings that controls for variations in the age and other demographics of the labor force.

The next section presents a simple model of earnings and college choice. Section III summarizes my data and the trends in relative earnings for college educated workers and college enrollment. In Section IV I present my construction of expected future lifetime earnings at each level of education using current earnings and discuss some of the complications and limitations of my approach. Section V estimates the relationship between expected relative lifetime earnings and college attendance and Section VI concludes.

II. The Labor Market and College Choice

High school students choosing whether to enroll in college must weigh the costs of attendance against their expected higher earnings with a college education. The expected monetary return to a college education will vary over time and across

[22] See Freeman (1976), Manski and Wise (1983), and Dominitz and Manski (1996).

individuals, incorporating changes in the market return to college and differences in individual abilities that complement education. I assume that students know their own ability by the end of high school,[23] and that they forecast the market return to college over their working lives based on the current labor market.

I consider an economy with only two types of workers: those with a high school education and those with a college education. I posit a standard Mincer (1974)-style model of earnings determination and allow the parameters of the earnings function to vary by education. Log annual earnings for workers with a high school education are determined by a polynomial in experience at year s, ex_{is}, a single-dimensional measure of individual ability, θ_i, and other characteristics, x_{is},

$$\log\left(y_{is}^{0}\right) = x_{is}\alpha + \gamma^{0}\left(ex_{is}\right) + \theta_i + u_{is}. \tag{3.1}$$

Log annual earnings for workers with a college education are

$$\log\left(y_{is}^{1}\right) = x_{is}\alpha + b + \gamma^{1}\left(ex_{is}\right) + \theta_i + u_{is}. \tag{3.2}$$

The earnings of the two groups differ in two ways: college-educated workers have a baseline earnings boost of b and also collect different returns to experience. $u_{is} \sim N\left(0,\sigma^2\right)$ is a transitory earnings shock. In practice, the earnings of college educated workers will also depend on additional characteristics of their education, for example major choices and college quality.[24] I do not observe students' expectations about these dimensions of their future education, but if they enter linearly into the log earnings equation, as is standard in the literature, then they will serve the same role as

[23] The framework and conclusions are the same if people continue to learn about their ability during college and their working lives and forecast their expected earnings with their best guess of their ability at the time they graduate high school.
[24] See, for example, Arcidiacono (2004) on major and Dale and Krueger (2002) or Black and Smith (2006) on college quality.

unobserved ability: an individual multiplier on average earnings for college educated workers known by the individual but not the econometrician.

A student graduating high school in year t can anticipate discounted lifetime earnings of

$$\sum_{s=0}^{T} \delta^s \exp\left(\theta_i\right) \exp\left(\tfrac{\hat{\sigma}^2}{2}\right) \exp\left(x_{is}\widehat{\alpha_t} + \widehat{\gamma_t^0}\left(s\right)\right) \tag{3.3}$$

if he goes directly into the workforce and

$$\sum_{s=4}^{T} \delta^s \exp\left(\theta_i\right) \exp\left(\tfrac{\hat{\sigma}^2}{2}\right) \exp\left(x_{is}\widehat{\alpha_t} + \hat{b} + \widehat{\gamma_t^1}\left(s-4\right)\right) \tag{3.4}$$

if he goes on to complete college. δ is an annual discount rate. All workers retire T years after they graduate high school. $\hat{\alpha}$, $\hat{b}$, and $\hat{\gamma}$ indicate the graduate's best guess at year t of the wage parameters during his working life. This chapter has two time concepts: a lifetime of earnings and a point in time when a cohort graduates high school and considers the current labor market conditions. Throughout, s denotes a year of working life and t denotes a cohort of high school graduates. In this simplified economy individuals either work or attend school and everyone completes their degree in four years, so the lifetime earnings for college graduates include four years of zero earnings while they finish their education. These years out of the workforce are also reflected in his accumulated experience: s years after he graduates high school a worker who went directly to work would have s years of experience, while one who went on to college would have *s-4*.

If all new high school graduates use the same information when developing their expectations of future earnings then the difference in lifetime earnings with and without a college degree can be decomposed into privately observed ability and a publicly observed multiplier:

$$\exp(\theta_i)\exp\left(\tfrac{\hat{\sigma}^2}{2}\right)\hat{\beta}_t = \exp(\theta_i)\exp\left(\tfrac{\hat{\sigma}^2}{2}\right)\begin{bmatrix} \sum_{s=4}^{T}\delta^s \exp\left(x_{is}\widehat{\alpha_t} + \hat{b} + \widehat{\gamma_t^1}(s-4)\right) - \\ \sum_{s=0}^{T}\delta^s \exp\left(x_{is}\widehat{\alpha_t} + \widehat{\gamma_t^0}(s)\right) \end{bmatrix} \tag{3.5}$$

Labor market information about the current value of a college education available to students graduating high school in year t is summarized by $\hat{\beta}_t$.

While higher ability individuals will have higher earnings at either education level they are more likely to elect to go to college because their abilities multiply the returns to college. Going to college involves costs beyond the opportunity cost of delaying the start of work. Let C_{it} summarize individual costs of college, which might depend on tuition at nearby colleges and the student's parents' ability to support him while he is studying instead of working. An income-maximizing student will attend college if the expected returns to college, $exp(\theta_i)\widehat{\beta_t}$, exceed these costs. In log form, he will go on to college if

$$\theta_i \geq \log(C_{it}) - \log\left(\widehat{\beta_t}\right) - \tfrac{1}{2}\hat{\sigma}^2, \tag{3.6}$$

that is, if his ability exceeds a threshold that depends on his individual costs of college and the returns to college he observes when he graduates high school. Higher-ability students may also have lower individual costs of college through lower effort costs of learning. My framework encompasses this possibility if the log costs of college are a linear function of ability.

III. Trends in College Enrollment and the College Earnings Premium

In this chapter I use 41 years of U.S. data from the 1970-2010 March Annual Demographic Supplements and the October Schooling Supplements of the Current Population Survey (CPS). The March CPS supplement includes information on total earnings in the previous calendar year, so the March 1970 supplement surveys earnings over 1969. While the college earnings premium has risen for women, their labor force participation and college enrollment choices changed in important ways over this period for reasons beyond the scope of this chapter, so I restrict my analysis to men. My earnings sample includes approximately 1.1 million civilian men ages 18 to 55 who worked at least 14 weeks in the previous year and who did not spend any time out of the labor force in the previous year. I use CPS-provided weights designed to make the earnings supplement sample representative of the U.S. working population. My earnings measure is total income from all jobs in the previous year including income from farms and other businesses.[25]

The earnings premium for workers with a college education rose 36% between 1970 and 2010, with most of the increase between 1980 and 2000. Figure 3.1 plots the ratio of annual earnings for workers with exactly 4 years of college relative to workers with exactly a high school diploma.[26] In 1980, college-educated workers made about 1.5 times as much as their high school-educated coworkers. By 2000, college-educated workers were earning almost twice as much as workers with only a high school diploma.

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[25] The CPS topcodes labor income, which could bias my estimates of the college wage premium because the topcode will disproportionately affect more educated workers. Through 2002 I use cell means for income above the topcode calculated by Larrimore et al (2008) from internal March CPS data. Since 2003 the Bureau of Labor Statistics has calculated these cell means themselves and filled them in for top-coded observations in the CPS.

[26] The high school graduate sample does not include people who received a GED.

Relative earnings for college-educated workers have remained fairly stable since 2000, with a slight dip and recovery around 2004, perhaps related to the slump in the technology sector.

This rise in relative earnings combines several changes in the labor market. College educated workers have higher starting wages than high school educated workers. Katz and Murphy (1992) and Elsby and Shapiro (2012) find that returns to experience were higher for high school graduates than college graduates in the 1980s. Elsby and Shapiro add that returns to experience have fallen for high school graduates and risen or remained steady for college graduates, so that by 2000 returns to experience were higher for college graduates. Finally, college-educated workers have a higher employment rate and this pattern has also become more pronounced over time (Juhn, Murphy, and Topel, 2002). The first line of Figure 3.1 Panel A plots the ratio of total reported annual earnings. The sample includes only men who spent the full year in the labor force and worked at least 14 weeks, but within that sample the total earnings measure incorporates lower earnings for workers who spent part of the year non- or under-employed. The second line in this panel plots the ratio of earnings adjusted to full-time full-year equivalent earnings using reported weeks worked and usual weekly hours. The ratio of these full-time equivalent earnings has risen more slowly than realized earnings, increasing 26% from 1970 to 2010 while realized earnings rose 36%. This adjusted measure is more comparable to other papers that have tracked the difference in weekly or hourly earnings, but it misses an important dimension of the differences in annual earnings across education groups.

The bottom two panels of Figure 3.1 plot the college earnings premium separately by race and by age. The college earnings premium has moved fairly consistently across race and ethnicity, but it has not evolved evenly across age groups. The college earnings premium began rising among 22-31 year old workers in the mid-1970s while remaining flat for older workers. The premia for older workers began to rise in later decades as the cohort of workers who entered the labor force in the 1970s aged. Meanwhile, the premium continued to increase for new workers entering the labor market through the 1990s. Card and Lemieux (2001) postulated that the isolated rise in the college earnings premium among young workers through the 1980s could be explained by a vintage capital model where successive generations of college graduates are imperfect substitutes for one another.

College enrollment roughly mirrors the movements in the college earnings premium over the past 40 years. Figure 3.2 shows enrollment falling slightly with the college premium in the 1970s and rising with the premium through the 1980s. I measure college enrollment using the annual October schooling supplement to the CPS. My sample includes 17 to 19 year old men who report in October that they graduated high school in the current calendar year (mostly in the spring). Students are considered to have started college if they report being currently enrolled in a 4-year college. This definition somewhat understates the true share of the population that eventually attends college since it does not include students who take time off between high school and college. Cameron and Heckman (2001) track college enrollment among all 21 to 24 year old high school graduates, which better captures students who take time off between high school and college, and find a very similar pattern of college enrollment over the same time period.

Higher expected earnings are not the only consideration when deciding whether or not to enroll in college. Figure 3.3 plots the average in-state tuition at public universities (the relevant choice for the majority of high school graduates), which more than doubled from $2,017 in 1970 to $5,552 in 2010, in 2000 dollars.[27] However, throughout the period this direct cost of college has been dwarfed by the opportunity cost of college, represented in Figure 3.3 by the average annual earnings of 19 to 21 year olds with exactly a high school diploma from the March CPS sample. Willis and Rosen (1979) estimate that students who go on to college would have earned less as high school graduates than workers who stopped at high school, so these earnings may somewhat overstate the opportunity cost of college for college attendees. Nevertheless, the magnitude difference between college tuition and potential earnings suggests that, unless students are credit constrained, the direct costs of college should play a relatively minor role in determining changes in college enrollment.

IV. Measuring the Expected Relative Earnings of College-Educated Workers

I use data from the March CPS to estimate the determinants of earnings for workers with and without a college degree over time. I then use these year-specific parameter estimates to construct a measure of the lifetime earnings gap between the two

[27] Tuition data are from the National Association of State Universities and Land Grant Colleges for 1970-71 and from the Washington Higher Education Coordinating Board from 1972-2008. For 2009 and 2010 I collected the in-state tuition data for the individual schools used in the Washington HECB report and constructed my own state averages.

education groups as described in equation (3.5). For each year of the sample, I estimate the parameters of the earnings equation

$$\log\left(y_{it}\right) = x_{it}\widehat{\alpha_t} + \widehat{b_t}col_i + \widehat{\gamma_t^0}\left(ex_{it}\right) + col_i \bullet \widehat{\gamma_t^1}\left(ex_{it}\right) + e_{it}, \tag{3.7}$$

which combines equations (3.1) and (3.2).[28] x_i includes a constant and dummies for being black, Hispanic, or other non-white. $col_i = 1$ if the worker is in the college group. $\widehat{\gamma_t^0}(ex_{it})$ is a quadratic function of potential experience, defined as age-years of school-6, the parameters of which can vary across education groups. Table 3.1 presents the parameters estimated from the log earnings equation for select years. The separate intercepts by race and ethnicity are economically and statistically significant. As has been shown in numerous studies, surveyed in Altonji and Blank (1999), white workers earn more than other groups and the gaps have remained fairly constant over time. As expected, college-educated workers earn substantially more than high school educated workers and the gap has grown over time.

I use these estimated parameters from all years to construct a projected gap in lifetime earnings as defined in equation (3.5):

$$\widehat{\beta_t} = \sum_{s=4}^{T}\delta^s exp\left(x_{is}\widehat{\alpha_t} + \hat{b} + \widehat{\gamma_t^1}\left(s-4\right)\right) - \sum_{s=0}^{T}\delta^s exp\left(x_{is}\widehat{\alpha_t} + \widehat{\gamma_t^0}\left(s\right)\right). \tag{3.8}$$

I assume that everyone discounts future earnings at $\delta = \frac{1}{1.02}$ and that T=35, so that everyone retires at the age of 53 and high school graduates work for more years than college graduates. This lifetime earnings difference includes the opportunity cost of college because the earnings for college workers do not begin until year 4. The discount

[28] Each year I observe earnings for about 16,000 total workers. To reduce spurious changes in predicted earnings from measurement error I include two years of lagged data. So, the parameters used to construct expected earnings for 1980 are estimated using 1978-1980 earnings data. I use data from the 1968 and 1969 March supplements to help construct my expected earnings estimates for 1970 and 1971.

rate is based on a two percent real interest rate, which is the average annual real interest rate over the sample period as measured by the difference between the market yield on 1-year Treasury Securities and PCEPI inflation. The retirement age is set to match the oldest workers in the estimation sample, but the details of retirement have little effect on the estimated lifetime earnings gap because the end of the earnings stream is heavily discounted, $\left(\frac{1}{1.02}\right)^{35} = 0.5$.

Figure 3.4 presents the full time series of these estimated gaps in lifetime earnings. I use actual annual earnings in my baseline estimates to capture differences across education groups in both wages and weeks worked. For comparison, I also plot projected lifetime earnings using full-time equivalent annual earnings. As in Figure 3.1, the gap in lifetime earnings using full-time equivalent earnings grew more slowly than my baseline specification. My baseline estimates define the college sample as those with exactly 16 years of education, which assumes that everyone forecasting their potential earnings plans to complete a bachelor degree and go no further. I also include a specification where the college sample includes everyone who started college, that is anyone with more than 12 years of education. This specification is consistent with high school graduates probability-weighting the possible results of starting college if they base their probabilities on the population distribution of outcomes. The gap between high school earnings and the earnings of all college starters is smaller than the gap with college graduates, but the changes over time are quite similar.

This constructed lifetime earnings gap has evolved somewhat differently than the average college earnings premium plotted in Figure 3.1. The difference is that my projected lifetime earnings gap controls for the changing composition of the labor force.

The bottom panel of Figure 3.1 shows that relative earnings for college workers rose more for younger workers from the mid-1970s to 1990 and more rapidly for older workers since then. Figure 3.5 shows the changing age composition of the labor force, mostly reflecting the movement of the baby boomers through each age range. The rise in the average college premium in the 1970s was driven by the experience of the young workers who made up the largest share of workers in that period. My measure, which estimates the difference in starting earnings and the effects of experience and then simulates lifetime earnings, is not affected by these changes in the age distribution of workers.

However, the bottom panel of Figure 3.1 also makes clear that the synthetic cohort assumption I use to project lifetime earnings has not been a good forecast of workers' actual experiences over my sample period. By estimating the effects of experience on a cross-section of workers, I assume that workers of various ages in the current labor market reflect what a young worker will earn as he ages in the future. In this framework, a student graduating high school in 1970 expects that when he graduates college and begins working in 1974 he will earn about 1.2 times as much as a 22 year old with a high school diploma. This forecast is fairly close to what new college graduates actually earned in 1974. However, the relative earnings of older workers in 1970 would lead him to expect about 1.6 times the earnings of high school graduates his age by the time he turned 50. In fact, when that worker turned 50 in 2002 the college premium for older workers was almost 2. This same point was made using Census data by Heckman, Lochner, and Todd (2007).

While these ex ante forecasts of lifetime earnings are poor predictors of ex post experiences during this period, they may still give an accurate portrait of students' expectations at the time they graduated high school if students are not able to predict future developments in the college earnings premium. Cuhna and Heckman (2007) estimate that young people forecast some but not all of the recent rise in relative earnings when making their college decisions. Betts (1996) and Dominitz and Manski (1996), among others, fielded surveys to ask high school and college students about their beliefs about current earnings of workers with different levels of education and their expectations about their own future earnings. While students varied widely in their perceptions of the current and future labor markets, their answers to these two questions were quite correlated. These results suggest that students seem to think that current earnings are a good predictor of their own future earnings, although they may have highly error-ridden perceptions of current earnings. Exploring different frameworks for constructing expectations of future earnings would be a fruitful avenue for future research.

As is clear from equations (3.1) and (3.2), earnings should also depend on individual ability. The CPS includes no suitable measure of individual ability and it is therefore not included in the estimation equation.[29] If, as my model supposes, unobserved ability is positively correlated with the decision to attend college than this omission will bias up my estimate of $\widehat{b_t}$, the coefficient on college. Put another way, I estimate the average earnings effect of a college education. This average effect confounds the actual return to college and the effect of being the type of high ability

[29] Proxies for ability based on instrumenting for college attendance by cohort with average SAT scores or historical college tuition were too closely correlated with potential experience (which in a single year is simply a measure of cohort) to provide a meaningful control for ability.

person who is likely to go to college. Someone who is just on the margin of attending college probably has lower ability than the average college student (or else unusually high costs of attendance) and should therefore expect to earn less than the mean college graduate.

Several previous studies have attempted to measure what share of the return to college is actually a return to unobserved ability. Murnane, Willet, and Levy (1995) and Taber (2001) use panel datasets to measure the college premium controlling for high school test scores. They find that the estimated college earnings premium falls between 25% and 50% when they control for test scores, although Heckman and Vytlacil (2001) argue that this type of exercise may not reliably separate returns to schooling and ability because the two are so closely correlated. Chay and Lee (2000) find that returns to ability explain at most 40% of the rise in the college premium in the United States between 1979 and 1991. Carneiro and Lee (2011) observe that increased college enrollment rates have lowered the average ability of college graduates and that this compositional change has biased down the estimated increase in the college premium. The combined conclusions of these studies is that my biased estimates of the returns to a college education are too high, but that they move in the right direction over time and may have less variation over time than the actual returns. Overstating the magnitude of the college premium should bias my estimated effect of the premium on college enrollment toward zero. Understating the inter-temporal variation in the premium should also bias the effect of the premium toward zero. Therefore, my estimates should be viewed as lower bounds on the true effect of the college premium on enrollment decisions.

V. Estimating the Role of Earnings Expectations in College Choice

As shown in equation (3.6), students should condition their choice to go to college on their expectation at the time they are ready to start college of the additional discounted lifetime earnings they will receive with this additional education. To test this relationship I match the earnings gaps estimated in the previous section to the college choices of new high school graduates in the October CPS Schooling supplement. I assume that unobserved ability has a normal distribution and estimate a probit model of going to college for individual i in cohort t

$$Pr\left(col_{it}\right) = \Phi\left(\varphi_0 + \varphi_\beta log\left(\widehat{\beta_{it}}\right) - C_{it}\varphi_C\right). \tag{3.9}$$

The constant term, φ_0, will include an estimate of the variance term $-\frac{1}{2}\hat{\sigma}^2$. I pair each new high school graduate with the estimated lifetime earnings gap calculated for his race and ethnicity using labor market data from the previous year, so the college choice of someone graduating high school in spring 1970 depends on the 1969 labor market. C_{it} is a broad set of covariates to control for the costs of college faced by each individual and may also capture heterogeneous preferences for college. The set of covariates includes log in-state tuition at public universities in the state where the student's parents live, parent's education and log income, and race and ethnicity. To control for other changes in college enrollment trends over the period I also include a quadratic time trend. All monetary variables, including the lifetime earnings benefit of college, are CPI-deflated to 2000 dollars.

Table 3.2 presents the results of a probit estimation of equation (3.9). The first column presents the baseline specification using the log difference in lifetime earnings between workers with exactly 4 years of college and those with exactly 12 years of school. Because the enrollment regression includes non-linear transformations of pre-estimated parameters the standard errors are difficult to calculate. Instead I report bootstrapped standard errors based on repeated draws from both the March and October data samples.

In my baseline specification the estimated mean marginal effect of the log college earnings gap is 0.107. If the log college wage gap rises by 1, college enrollment should rise by 10.7%. In a probit model, the values of all covariates influence the effect of each variable on enrollment. The gap in lifetime earnings approximately doubled between 1980 and 2002, generating an increase in the log gap of about 0.8. If I hold college tuition and the characteristics of high school graduates and their families constant at 1980 levels, but increase the projected lifetime earnings gap to its 2002 level, my estimates predict that college enrollment rates should increase 8.7 percentage points. Increases in the expected lifetime earnings benefits of college can explain the majority of the rise in the college enrollment rate from 35% in 1980 to 45% in 2002. Within this period, enrollment rose 7% between 1980 and 1990 and only 3% from 1990 to 2002, while the change in the earnings gap predicts a steadier rise over the two decades.

Figure 3.6 plots my predicted college enrollment, incorporating observed changes in all the independent variables, against observed enrollment rates over the sample period. My predicted enrollment rates track actual enrollment through the fall in the 1970s, the rise in the 1980s and 1990s, and the flattening out since 2002.

Both the education and the real income of the parents of high school graduates are important determinants of college enrollment. After the change in relative earnings, increases in the education and income of parents explain the second largest share of the rise in college enrollment. Controlling for other factors, non-white students are more likely to attend college than their white counterparts, although this effect is not statistically significant for Hispanic students. Students from the Western United States are substantially less likely to attend college than students from the Midwest, the omitted category, and students from the Northeast are somewhat more likely to attend college. Over my sample period, the share of high school graduates that are Hispanic, other non-white, or from the western United States has risen. However, the demographic shifts explain very little of the rise in college enrollment, while the regional shifts imply a slight fall in enrollment.

The remaining columns of Table 3.2 present the results from the alternative specifications of the log difference in lifetime earnings. Column (2) shows that the difference in lifetime full-time equivalent earnings has a smaller effect on college enrollment than my baseline measure. Unlike the baseline measure, full-time equivalent earnings ignores the difference in employment rates between education groups. The smaller estimated effect may indicate that students respond to both relative wages and relative employment rates in the baseline model. The full-time adjustment introduces additional measurement error by using the reported hours and weeks worked, which may also produce attenuation bias. The last two columns of Table 3.2 use the lifetime earnings difference calculated using a broader definition of the college group that includes all workers with at least some college, including those who went on to graduate degrees, rather than only those with exactly a 4-year college degree. Neither the magnitude nor the significance of the estimated parameters is strongly affected by the college definition.

College enrollment, the relative earnings of college graduates, and many of the other covariates in Table 3.2 all have a positive time trend over all or most of my sample period. The negative or flat growth in both college enrollment and relative earnings in the 1970s and 2000s is important for separating the effect of the earnings gap from these other factors. In my baseline specification I include a quadratic time trend. A Durbin-Wu-Hausman specification test rejects the exclusion of the linear time trend and is indeterminate on the inclusion of the quadratic term (excluding the quadratic term is rejected with 30% confidence). While these time trends are only marginally significant in the baseline specification, my estimates are sensitive to their inclusion.

As shown in the second column of Table 3.3, the effect of the log college wage gap is indistinguishable from zero when I exclude the linear time trend. With no other covariates, the coefficient on the college earnings gap falls but remains positive when the time trend is excluded (columns 3 and 4 of Table 3.3). The complete sensitivity to the inclusion of a time trend stems from including both the college wage premium and other covariates, in particular parents' education. When I exclude the dummy variables for parents' education, the coefficient on the college earnings gap is robust to the exclusion of the time trend (columns 5 and 6 of Table 3.3).

Parents became steadily more educated of my sample period. The share of high school dropouts fell from 33% in 1970 to 6% in 2010 while the share of college graduates rose from 6% to 27%. The fall in the R-squared statistic from 0.098 in the first column of Table 3.2 to 0.057 in the fifth column of Table 3.3 highlights the importance of parents' education as a determinant of college enrollment. However, these parents are experiencing the changes in relative earnings over the sample period that are captured in the log college earnings gap, so it is not surprising that the estimated effect of this gap is particularly sensitive to the inclusion of parents' education and income.

A weakness of using the October CPS to look at college enrollment is that respondents are surveyed by household, so family information is only available for youths who are still living at home or are living in group quarters such as a college dorm (in which case they are still considered part of their parents' household). In consequence, my regression sample does not include young people who moved out to live on their own immediately after graduating high school.[30] Cameron and Heckman (2001) find that youths who remain dependents of their parents tend to have higher family incomes. This bias will cause me to underestimate the effect of family income on college choice.

This selection bias may also affect my estimate of the importance of market-based expectations on college choice, but the direction of the bias is unclear. Betts (1996) found that students from families with lower incomes tended to systematically underestimate the returns to college, in which case I will overestimate the effect of expected earnings on college enrollment. However, if students from richer families are less motivated by the monetary returns to college—Brand and Xie (2010) find that high-income students experience the lowest earnings benefits of college but are the most likely to attend—then I may understate the true effect. The final column of Table 3.3 uses all 17 to 19 year old new high school graduates in the October CPS and includes a dummy for respondents missing family background data. Including these additional graduates causes my estimate of the role of predicted lifetime earnings to fall. The large positive

[30] 91% of college attendees in the full sample group are still dependents of their parents in the fall after high school graduation, as are 79% of students who graduated high school in the past year but did not go on to college.

coefficient on the indicator for not having family data is a puzzle, since college attendees are more likely to be matched with their parents.

VI. Conclusions

I find that the current earnings returns to a college degree at the time a student graduates from high school exert a modest but important influence on that student's decision to enroll in college. On average, a 10% increase in the lifetime earnings gap between workers with and without a college degree will increase the probability that a high school graduate enrolls in college by 1%. The rise in this earnings gap between 1980 and 2002 raised predicted college enrollment rates 8.7 percentage points. Expectations about the average market return to a college education are only one influence on a student's college decision, along with expected individual returns based on one's abilities, personal preferences, and financial constraints. Nevertheless, this estimated effect of the expected relative earnings of college-educated workers can explain the majority of the 10 percentage point rise in the four-year college enrollment rate for men between 1980 and 2002.

This relationship between the return to a college education and college enrollment rates is an important channel for labor market adjustment. The relative earnings of more and less educated workers depend partially on the relative supplies of each type of worker. Regardless of the causes of the recent rise in the relative earnings of college-educated workers, the increasing supply of college-educated workers should eventually push their relative earnings back down. This adjustment will unfold slowly. The share of college educated workers changes mainly through the decisions of young people; older workers are justifiably reluctant to leave established jobs and invest the necessary time and money in schooling when they have fewer working years remaining in which to reap the benefits of a college degree. Moreover, an increasing share of students who start college never complete their degree (Bound, Lovenheim, and Turner 2007). A 4% increase in the share of young people starting college produces a smaller increase in the share of young people graduating college and a far smaller increase in the fraction of workers with a college degree.

The relative earnings of college graduates have stopped increasing over the past decade. This stabilization may be due entirely to the recent economic downturn, but it may also signal the beginnings of this rebalancing of relative earnings. As the high college premium raises enrollment rates for successive cohorts of high school graduates and these cohorts make their way into the labor market the earnings gap between more and less educated workers should gradually narrow, unless the forces increasing the gap outpace the change in relative supplies.

This gradual readjustment of the relative earnings of more and less educated workers also has implications for the future of income inequality in the United States. As has been documented by numerous studies,[31] the rise in the college earnings premium has accompanied and contributed to a dramatic increase in income inequality. Inequality has been a recent focus of policy makers and news agencies and a source of widespread public protests. If higher levels of college enrollment do begin to gradually push the earning of more and less educated workers closer together then income inequality may decrease somewhat in the future.

[31] See, for example, Card and DiNardo (2002), Autor, Katz, and Kearney (2006), and Goldin and Katz (2008).

Table 3.1: First Stage Estimates of Determinants of Earnings

	1970	1980	1990	2000	2010
Black	-0.335	-0.280	-0.316	-0.264	-0.262
	(0.021)	(0.019)	(0.017)	(0.015)	(0.013)
Hispanic		-0.116	-0.211	-0.215	-0.227
		(0.014)	(0.018)	(0.013)	(0.013)
Other non-white	-0.201	-0.209	-0.238	-0.195	-0.139
	(0.039)	(0.026)	(0.023)	(0.023)	(0.017)
College educated	0.393	0.373	0.691	0.644	0.659
	(0.120)	(0.059)	(0.078)	(0.058)	(0.064)
Experience	0.086	0.082	0.085	0.071	0.073
	(0.011)	(0.005)	(0.006)	(0.005)	(0.006)
(Experience^2)/100	-0.180	-0.161	-0.158	-0.126	-0.133
	(0.026)	(0.013)	(0.015)	(0.012)	(0.013)
College*experience	0.008	0.001	-0.017	0.004	0.011
	(0.014)	(0.007)	(0.009)	(0.007)	(0.007)
College* (experience ^2)/100	-0.039	-0.002	0.028	-0.033	-0.047
	(0.034)	(0.019)	(0.023)	(0.020)	(0.018)
Intercept	9.635	9.571	9.388	9.446	9.364
	(0.102)	(0.048)	(0.060)	(0.043)	(0.054)
Observations	37,613	50,912	53,460	43,711	64,909
R-squared	0.262	0.277	0.263	0.258	0.273

Source: March CPS. Notes: Bootstrapped standard errors in parentheses. Results from OLS regressions of real annual earnings from select years. Regressions for each year include observations from the current and previous two years. Hispanics were not distinguished from “other non-white” before 1972, so that variable is omitted from the 1970 regression. All analysis uses CPS-generated inverse probability weights.

Table 3.2: Probit Estimation of the Choice to Enroll in College

Dependent Variable:	1 4 yr college	2 4 yr college	3 4 yr college	4 Any college
Log(lifetime earnings gap)	0.107			
	(0.033)			
Log(lifetime earnings gap),		0.059		
FT equivalent		(0.021)		
Log(lifetime earnings gap),			0.131	0.100
all college starters			(0.032)	(0.036)
In-state tuition	0.011	0.012	0.019	-0.017
	(0.012)	(0.012)	(0.012)	(0.012)
Family Income	0.096	0.096	0.096	0.102
	(0.006)	(0.006)	(0.006)	(0.005)
Parent HS dropout	-0.113	-0.113	-0.113	-0.134
	(0.010)	(0.010)	(0.010)	(0.010)
Parent some college	0.100	0.099	0.100	0.108
	(0.009)	(0.009)	(0.009)	(0.008)
Parent college graduate	0.270	0.270	0.270	0.228
	(0.009)	(0.009)	(0.009)	(0.007)
Time	-0.002	0.017	-0.001	-0.003
	(0.001)	(0.015)	(0.002)	(0.002)
Time^2/1000	-0.034	-0.002	-0.01	0.106
	(0.034)	(0.018)	(0.049)	(0.05)
Black	0.035	0.126	0.006	-0.003
	(0.017)	(0.020)	(0.016)	(0.015)
Hispanic	0.009	0.000	-0.009	0.040
	(0.018)	(0.001)	(0.018)	(0.018)
Other non-white	0.139	-0.049	0.119	0.124
	(0.022)	(0.037)	(0.020)	(0.019)
Northeast	0.033	0.033	0.032	0.034
	(0.009)	(0.009)	(0.009)	(0.009)
South	-0.015	-0.014	-0.012	-0.011
	(0.010)	(0.010)	(0.010)	(0.010)
West	-0.133	-0.133	-0.129	-0.010
	(0.011)	(0.011)	(0.011)	(0.012)
N	22,999	22,999	22,999	23,343
Pseudo R-squared	0.098	0.098	0.098	0.101

Source: October CPS with difference in lifetime earnings calculated from March CPS. Notes: Coefficients reported are the average marginal effects. Bootstrapped standard errors in parentheses.

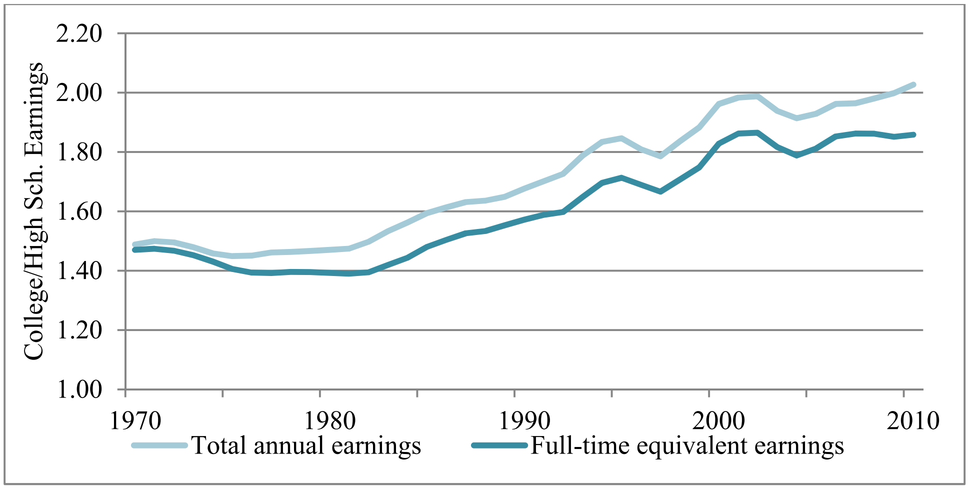
Table 3.3: The Choice to Enroll in College, Alternate Specifications

	1	2	3	4	5	6	7
Dependent Variable:	4 yr college	4 yr college	4 yr college	4 yr college	4 yr college	4 yr college	4 yr college
Log(lifetime earnings gap)	0.094	-0.004	0.261	0.158	0.125	0.113	0.091
	(0.031)	(0.021)	(0.029)	(0.014)	(0.035)	(0.021)	(0.032)
In-state tuition	0.007	-0.005			0.006	-0.001	0.018
	(0.011)	(0.012)			(0.012)	(0.012)	(0.011)
Family Income	0.096	0.097			0.158	0.158	0.097
	(0.006)	(0.006)			(0.005)	(0.005)	(0.006)
Parent HS dropout	-0.113	-0.109					-0.112
	(0.010)	(0.010)					(0.010)
Parent some college	0.100	0.096					0.093
	(0.009)	(0.009)					(0.009)
Parent college graduate	0.270	0.266					0.278
	(0.009)	(0.009)					(0.010)
Time	-0.003		0.001		0.002		0.000
	(0.001)		(0.001)		(0.001)		(0.001)
Time^2/1000			-0.090		-0.067		-0.054
			(0.034)		(0.035)		(0.031)
Black	0.032	0.002			0.041	0.038	0.032
	(0.017)	(0.015)			(0.017)	(0.016)	(0.016)
Hispanic	0.006	-0.017			-0.051	-0.055	0.007
	(0.018)	(0.017)			(0.017)	(0.017)	(0.016)
Other non-white	0.136	0.112			0.161	0.159	0.148
	(0.021)	(0.020)			(0.021)	(0.020)	(0.020)
Northeast	0.033	0.035			0.039	0.039	0.036
	(0.009)	(0.009)			(0.010)	(0.010)	(0.009)
South	-0.016	-0.021			-0.008	-0.010	-0.007
	(0.010)	(0.010)			(0.010)	(0.010)	(0.009)
West	-0.135	-0.140			-0.122	-0.125	-0.112
	(0.011)	(0.011)			(0.011)	(0.011)	(0.01)
No parent information							0.553
							(0.005)
N	22,999	22,999	22,999	22,999	22,999	22,999	26,400
Pseudo R-squared	0.098	0.097	0.007	0.006	0.057	0.057	0.085

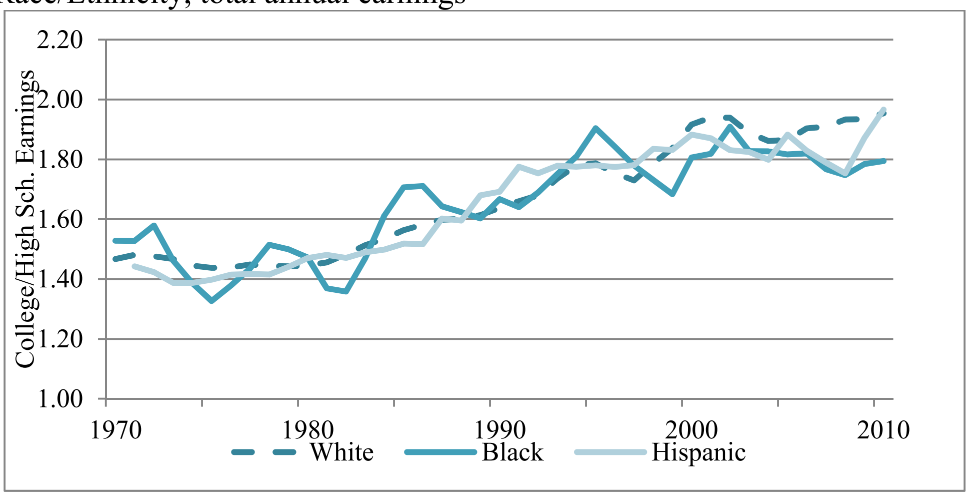
Source: October CPS with difference in lifetime earnings calculated from March CPS. Notes: Coefficients reported are the average marginal effects. Bootstrapped standard errors in parentheses.

Figure 3.1: Differences in Annual Earnings by Education

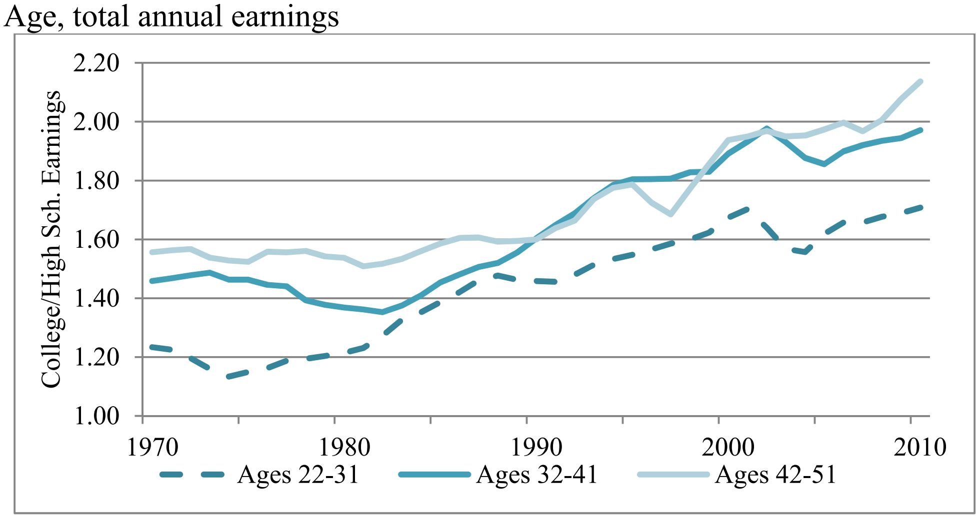
A. All workers



B. By Race/Ethnicity, total annual earnings

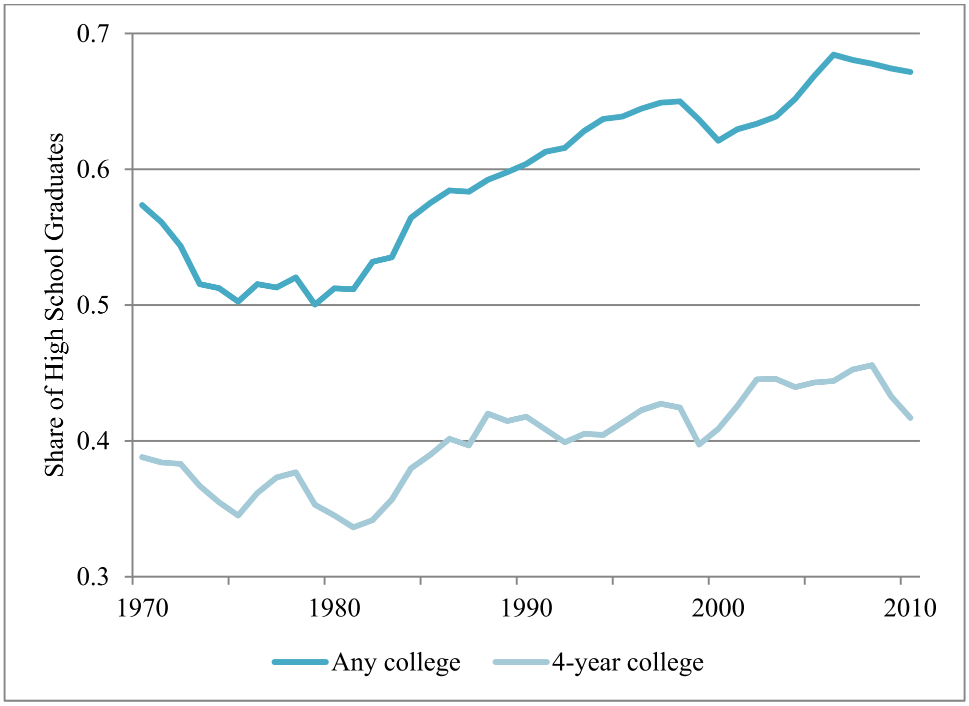


C. By Age, total annual earnings



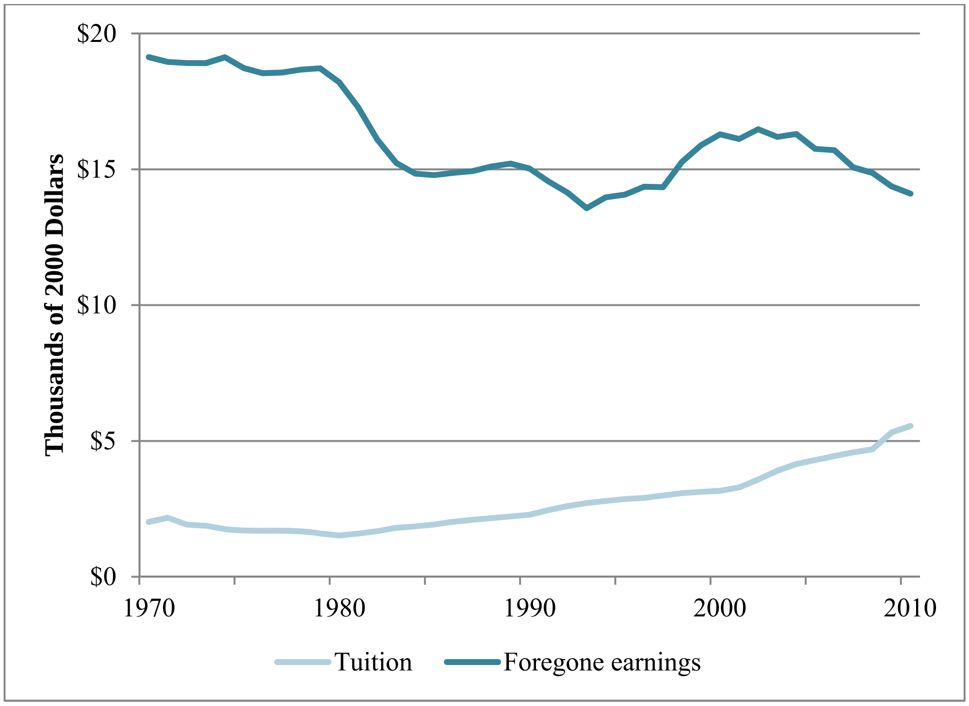
The graphs plot the ratio of CPI-deflated annual earnings workers with exactly 4 years of college and those with exactly a high school diploma. Data are from the March CPS Annual Demographic Supplement 1970-2010.

Figure 3.2: Share of New High School Graduates Enrolled in College



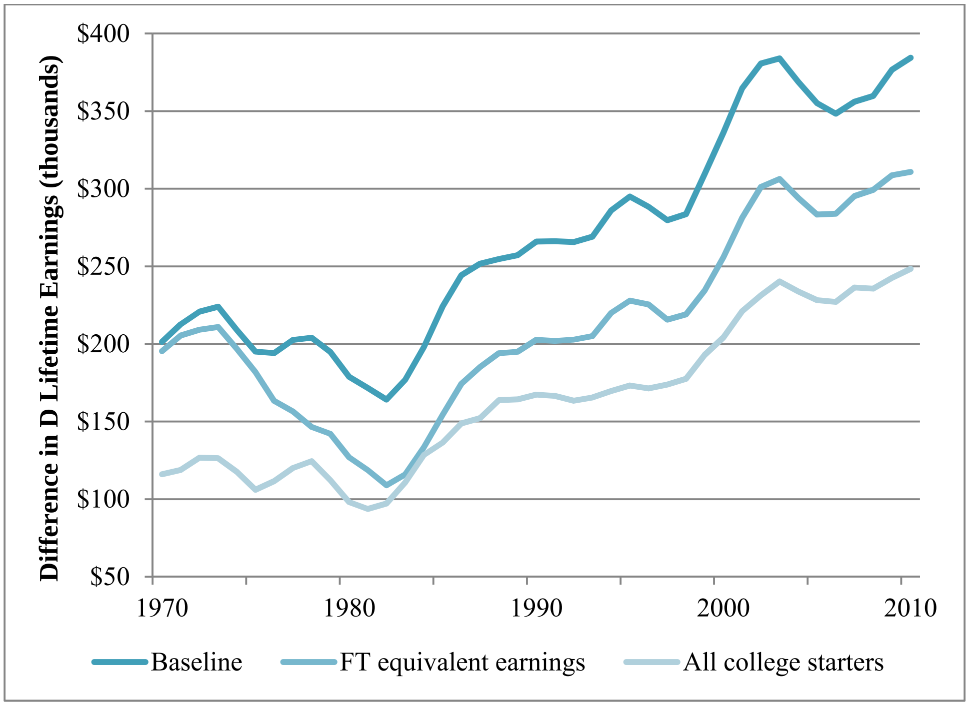
Graphs show the share of 17 to 19 year olds who graduated high school in each year and enrolled in college the same fall. Data from the October CPS Schooling supplement.

Figure 3.3: The Annual Direct and Indirect Costs of College



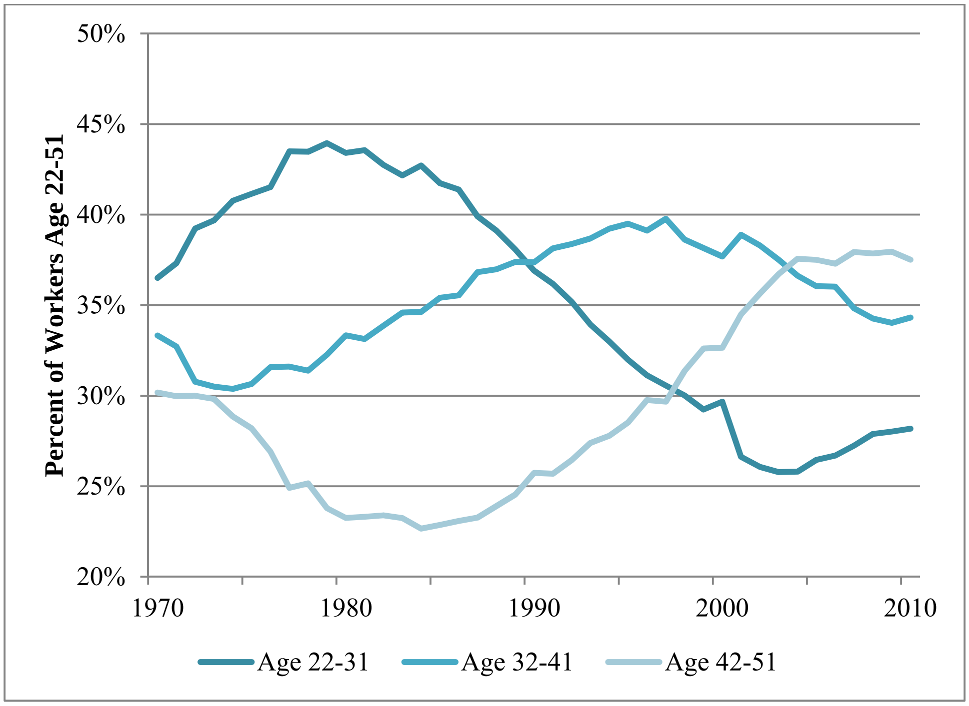
CPI-deflated in-state public tuition for public universities, averaged across states, from the National Association of State Universities and Land Grant Colleges (1970-71) , the Washington Higher Education Coordinating Board "Tuition and Fee Rates" Annual Report (1972-2008), and IPEDS (2009-10). Earnings are the CPI-deflated average annual earnings of 19 to 21 year-old workers with exactly a high school degree from the March CPS.

Figure 3.4: Differences in Lifetime Earnings



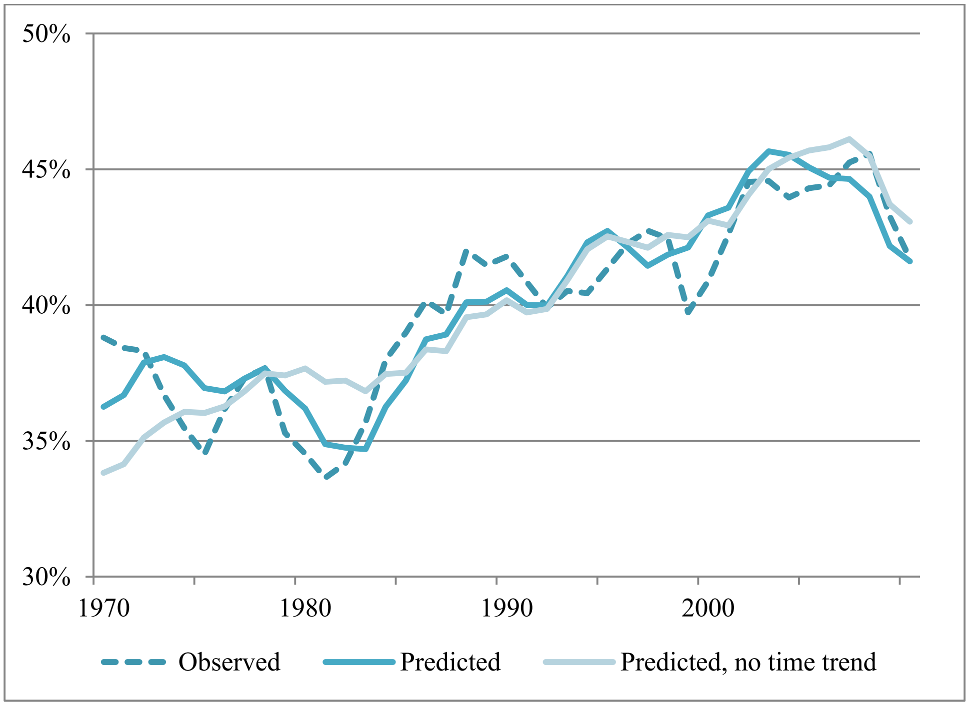
Calculated from March CPS as described in text. Gaps in discounted lifetime earnings are calculated separately by race/ethnicity. This figure plots the gaps for white workers. The patterns for other groups are similar.

Figure 3.5: Age Distribution in Labor Force



Fraction of 22-51 year old workers in each age range from March CPS.

Figure 3.6: Observed and Predicted Four-year College Enrollment



Observed enrollment rate among 17-19 year old high school graduates from October CPS. Predicted rates described in text.

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Chapter 4

The Determinants of Mismatch Between Students and Colleges

I. Introduction

Students graduating high school in the U.S. can choose to apply to and enroll in a wide variety of colleges. The quality of the college they attend may affect their probability of graduating, how long they take to complete their degree, their future income, and the types of future jobs available to them. Black and Smith (2004, 2006) find that higher college quality has a positive effect on future earnings, though Dale and Krueger (2002, 2011) find this effect is only significant for women, black and Hispanic students, and students from lower-income families. Bound, Lovenheim, and Turner (2007) and Bowen, Chingos, and McPherson (2009) add that students at higher quality colleges are more likely to graduate from those colleges and take less time to complete their degree. However, students may struggle at colleges that are too challenging relative to their ability. Light and Strayer (2000) find some evidence that mismatch in either direction lowers a student's probability of graduation. Arcidiacono et al (2011) summarize the contentious literature focused on affirmative action programs. In addition to affecting the students' private outcomes, the match between student and college characteristics also affects how efficiently the substantial investments made by federal and state governments work to grow the supply of workers with college degrees.

We ask how students of different abilities sort into colleges of different qualities, with a focus on the determinants of apparent mismatch between students and colleges. We look separately at students who appear under-qualified (weak students at relatively strong schools) and over-qualified (strong students at relatively weak schools). We address these questions using multivariate analysis of the National Longitudinal Survey of Youth 1997 cohort, supplemented with the School Survey data from the high schools these students attended. We define the match between students and schools as the gap between the percentile of the student's ability and the percentile of the college's quality. Taking advantage of the rich set of covariates available in the NLSY97 survey, we are able to consider separately the student's family financial resources, the education of their parents and other adults in their neighborhood, the share of their high school classmates that go on to college, high school advising, and the range of colleges nearby and within their state university system. We expect that financial constraints will have a larger effect on over-qualification, students attending relatively low quality schools, while differences in guidance about college and the availability of local colleges could affect mismatch in either direction.

While affirmative action-motivated papers have focused on the consequences of relatively low-ability students at high-quality schools, a growing body of work studies the determinants of the opposite type of mismatch: relatively high-ability students at lower quality schools, which we define as over-qualification but is often labeled under-match. Bowen, Chingos, and McPherson (2009), Avery and Turner (2009), and Howell (2010) find that students from lower-income families and black and Hispanic students are significantly less likely to apply to more selective colleges. Griffith and Rothstein (2009) find that geography also plays a role: holding test scores constant students are more likely to apply to a selective college if they live near one.

In addition to including an unusually broad set of characteristics that may affect mismatch, we add to this literature by considering both types of mismatch side by side. A theme in our findings is that many factors appear to affect the quality of college a student attends regardless of that student's ability, rather than affecting mismatch. Students from the wealthiest families, from neighborhoods where many adults have college degrees, and from high schools where many students go on to college are less likely to be over-qualified for their college but also more likely to be under-qualified. One exception is the public university system; students are less likely to end up mismatched in either direction if they have a school with which they are well-matched within their home state university system. We do not find clear evidence that minority students are more likely to be under-qualified for their college, which we would see if affirmative action played a meaningful role in determining enrollment patterns.

For a subset of the sample, we observe the full set of schools that students applied to and their admission decisions at each, allowing us to investigate at what step of the application process they were guaranteed to end up mismatched. This application data reveals that mismatch is overwhelmingly a result of choices made by students and their families, not of choices made by college admissions offices. 91% of over-qualified students and 97% of under-qualified students either did not apply to any colleges with which they were well-matched or applied and were accepted but chose not to go. Very few applied to well-matched schools and were rejected.

Our definition of mismatch does not presume or require that mismatch has negative consequences. Over-qualified students may receive extra coaching and opportunities by virtue of being among the strongest students at their school. Under-qualified students can benefit from the additional resources at higher quality schools. Our result that the students with the most financial resources and information about college are more likely to end up under-qualified with their college may indicate that they believe the greater resources at these schools outweigh the additional effort needed to succeed there.

In the next section we discuss an informal model of how students and their families decide which colleges to attend. In Section III we discuss our data and our methods of estimating ability and college quality. Sections IV and V present our results and Section VI concludes.

II. The College Choice and Mismatched Outcomes

The process by which students are sorted into schools has several stages and involves choices by both the student and the school. The student first decides which colleges to apply to, then the colleges decide which students to admit, and finally the student chooses among her offers of admission. Our framework collapses this multi-stage process into a single choice by the students. As we will discuss in the next section, this simplification is relatively benign. While the most selective schools reject a large share of their applications, we find that the vast majority of mismatched students in our sample end up that way because they did not apply to any schools for which they were well-matched or were accepted to a well-matched school and turned it down. Very few mismatched students were rejected from all the well-matched schools to which they applied.

We assume that college applicants are rational and forward-looking. Nevertheless, during this process there are several influences that could cause a student to end up at a school that does not match her abilities, including information constraints, financial constraints, and social considerations. Lack of information on the part of either the student or the school could result in college mismatch. The student may not have complete information about the quality of different colleges, or about how her abilities compare with other college applicants. Both misunderstandings could cause her to apply to a mismatched set of schools. Lack of information about college qualities could also cause her to choose a poorly matched school out of the set of schools to which she is accepted. The student's application may also be a poor indication of her true ability, for example if she over- or under-performed on the SAT or ACT. If a college misinterprets the student's ability it may admit her to a school for which she is ill-prepared or reject her from a school that would suit her. We expect that less well-informed students will be more likely to be mismatched with their college in either direction.

In a basic framework where students make the best college match they can subject to their budget, financial constraints will tend to push students toward schools for which they are over-qualified, since more elite schools tend to be more expensive. In practice, for strong students from low-income families the extra cost of a top school is largely offset by financial aid, but students do not know their aid offers with certainty when they are applying for schools (Avery and Turner 2009). Financially constrained students may also choose a nearby college to reduce travel costs or avoid the cost of boarding away from home. Again, this will tend to increase over-qualification more than under-qualification since the students have an incentive to attend a nearer school even if they are over-qualified for it, but schools generally have no incentive to accept weaker students just because they live nearby.

The state school system can generate mismatch in either direction. Most state schools offer discounted tuition to state residents, making them more affordable than other options. In addition, some state schools have requirements about admitting state residents and may have a lower, or no, admission threshold for local students. Students may choose this less expensive option even if the state does not offer a well-matched college and may be able to attend those schools even if they are under-qualified for them.

Finally, students may appear mismatched with their college because they based their choice on other factors. Students may choose a college that is good for their major, for example engineering or art, even if it appears to be a poor match on overall quality. Students may be recruited to colleges based on skills, such as athletics, that are not included in our measure of ability. Students may choose to go to the same schools that their friends are going to, the school that their parents attended, or any school where they feel they will fit in with the student body, even if that school is not a match for them academically. In these cases we will observe positive or negative measured mismatch, but students may still be at the school that is best for them in a broader sense.

III. The Data

We use the National Longitudinal Survey of Youth 1997 Cohort (NLSY97) data, which allows us to study a very recent cohort of college students. This survey, a second generation of the extensively used NLSY79, covers a group of American youth born between 1980 and 1984. The first interview was in 1997, with follow-up interviews each year since. The majority of the sample graduated high school and made their college choice between 1999 and 2002. 84 percent of the un-weighted sample graduated high school or got a GED. Of these high school graduates, 42 percent attended a four-year college. We focus on the 2,771 respondents who started at a four-year college in the United States. We also run our analysis pooling these students with the 2,467 respondents who started at a 2-year college.[32] Some students are excluded from the multivariate analysis because we do not have measures of their ability or of the quality of college they attended. We discuss the construction of our sample in more detail in the data appendix. We are left with 1,977 observations in our main analysis.

The NLSY97 sample includes both a representative cross-section of this generation of Americans and an over-sample of black and Hispanic students. We combine these samples in our analyses. We use probability of inclusion weights to combine the two samples, and also to control for differing sampling and response rates in different regions and across age, gender, race, and ethnicity groups.

Our primary measure of student ability is the Armed Forces Vocational Aptitude Battery (ASVAB) test, which is designed for applicants to the U.S. military and was also administered to almost all of the NLSY97 respondents. This test has twelve components, covering both the sorts of skills measured by the SAT such as arithmetic, vocabulary, and

[32] See Reynolds (2009) for an analysis of the choice between starting at a two-year of four-year college.

reading comprehension and other skills such as electronics knowledge and spatial reasoning. The ASVAB test score offers a somewhat richer measure of ability in high school than the SAT or ACT score, and should be less influenced by variation in studying effort and preparation, since there was nothing riding on this test for the NLSY participants.[33] The ASVAB score is also useful because it gives us a measure of ability that is potentially relevant to college performance but not observed by colleges while they are making their admissions decisions. We can therefore capture some of the college mismatch generated by incomplete information on the part of the colleges.

All survey participants who took the ASVAB did so between 1997 and 1998, so their age when they took the test varies and most participants were younger than the larger population taking the test. In addition, the ASVAB is a computer adaptive test, meaning that test takers are asked different questions over the course of each section based on their responses to early questions. The score for each section reported by the NLSY is calculated based on both the number of questions answered correctly and the difficulty of those questions estimated from an earlier sample of test takers. We take the first principal component factor across the 12 section scores as our raw measure of ability.[34] We then calculate each respondent's percentile within the sample of college-bound NLSY97 respondents who took the test at the same age, weighted by probability of inclusion in the sample.

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[33] The ASVAB test is not a straightforward measure of "innate" ability because it includes the influences and training that the student has had up to the point she takes the test. See Neal and Johnson (1996) for a more thorough discussion of what the ASVAB test is measuring. We consider demonstrated ability in high school to be the relevant variable because it captures what students bring to the college application process, without the variation in college preparation that influences the SAT. Neal and Johnson also summarize evidence that, unlike the SAT, the ASVAB test show no signs of racial bias.

[34] Cawley, Heckman, and Vytacil (2001) and Black and Smith (2006) found that the second principal components of the ASVAB score is also relevant in determining later earnings in the NLSY 1979 sample. For our purposes, we need a single measure of ability. The first factor is by far the most important and explains 62 percent of the variation in scores.

We construct a multifaceted index of college quality that combines student characteristics, college and faculty characteristics, and measures of students' revealed preferences over schools. For college quality we merge data from the U.S. Department of Education's Integrated Post-Secondary Data System (IPEDS) and U.S. News and World Report with the colleges listed in the NLSY97 dataset. The components of our college quality index are mean SAT score of entering students, percent of applicants rejected, average faculty salary, and the faculty-student ratio. We use the first principal component factor across these four measures of quality as our quality index, following Black and Smith (2004). We then calculate the school's quality percentile across all four-year institutions in the United States included in the IPEDS, weighted by student body size.

There are several sources of potential measurement error in our estimates of ability, quality, and college match. An important limitation is that we observe college quality at the school level. In practice, individual departments within a college may be better or worse than the average quality of that college. If a strong student who plans to be a physicist attends a school of medium quality as we measure it, but that school has a top-rate physics program, then we will errantly consider that student over-qualified for her school. Likewise, if an aspiring English major enrolls at a top engineering school we will observe her as well-matched or even under-qualified, when in fact that school may not offer strong training in her area of interest. Additionally, while an index across several dimensions of college quality improves on a single measure of quality there is still some measurement error in college quality (Black and Smith 2006).

Finally, the ASVAB score is an imperfect measure of ability. While the ASVAB includes a richer variety of tests than most standardized tests it still does not capture all the abilities that make for a strong college student. Even if it did measure all relevant abilities, the score from a single ASVAB test would be an imperfect measure of ability because some students will perform above or below their usual level on any given day. These sources of error will make our results less precise, biasing our estimated relationships toward zero.

Because we weight the quality percentile by student body size, a school in the nth percentile is the school that a student in the nth percentile would attend if you ranked students by quality of school attended. Therefore, if students sorted into schools based purely on ability and school quality, a student in the nth ability percentile would attend a school in the nth quality percentile and mismatch, defined as the difference in ability percentile and quality percentile, would be equal to zero for all students.

Gaps in this type of a priori match are quite common. Table 4.1 gives the joint distribution of student ability and college quality. Students are concentrated along the diagonal, which indicates a good match, but there are also many mismatched students. Previous discussions of mismatch have often been framed by a discussion of affirmative action, and have therefore focused on students who seem under-qualified for their schools, but we find that strong students at weak schools are at least as common. The gap between the ability percentage of students and the quality percentile of the college they attend has a roughly normal distribution, shown in Figure 4.1. In much of the following analysis we categorize students as under-qualified, well-matched, or over-qualified for their college. We consider students to be very over or under qualified if there is a greater than 20 percentile point gap between their ability percentile and the quality percentile of the first school they attend. These cutoffs assign about a quarter of the sample to each mismatch category.

IV. Understanding the College Choice

The youngest members of the NLSY97 cohort, those born in 1983 and 1984, were asked an additional battery of questions around the time they finished high school about the set of colleges to which they applied and the admission decision from each school. The top panel of Table 4.2 shows that just over 30% of students who ended up mismatched with their college had applied to at least one college with which they would have been well matched, that is a college whose quality percentile fell within 20 percentage points of their ability percentile. Most of those students who applied were also accepted to one of those well-matched schools.

Mismatch is overwhelmingly a result of the choices made by students and their families, not of the choices made by college admission departments. Of students who ended up over-qualified, 69% did not apply to any colleges with which they were well-matched. Only 9% applied to at least one well-matched school and were rejected. The remaining 22% of over-qualified students were accepted to at least one school with which they were well-matched but chose to attend a college for which they were over-qualified. 9% is the upper bound of over-qualified students who ended up in that situation because of college admission decisions rather than their own choices; students who were rejected by all the well-matched schools to which they applied may have chosen a mismatched school even if they had been accepted elsewhere. At least 97% of under-qualified students ended up under-qualified because of their own choices.

Tables 4.3 and 4.4 describe the characteristics of students and their families by the quality of college they attend and by their match category. The patterns across the two tables are often the same. For example, in Table 4.3, students attending the highest college quality quartile have more educated mothers on average than those attending lower quality colleges. In Table 4.4, students who are under-qualified for their college have more educated mothers on average than students who are well-matched to their college, who in turn have more educated mothers than students who end up over-qualified for their college. These linear patterns in Table 4.4 indicate that these characteristics seem to be influencing college quality rather than mismatch per-se. If more educated parents tend to send their children to higher quality colleges, independent of the child's ability, than those children will be more likely to end up under-qualified for their college and less likely to end up over-qualified.

Family wealth has the same linear pattern as parents' education in Table 4.4, consistent with a budget constraint story where students from less wealthy households are more likely to end up over-qualified because they cannot afford to attend a higher quality college. We measure wealth in 1997, somewhat before most students finished high school, because we have the most complete financial information in that first year of the survey. Wealth is a good, but incomplete, measure of the family's ability to pay for college. If additional information about the family's permanent income is captured in parent's education and neighborhood characteristics then these variables will pick up some of the student's financial constraint as well as information constraints.

We use the surveys answered by the high schools of NLSY97 respondents to find measures of how much information and guidance these students had about college that are less tightly correlated with financial resources. We consider the share of teachers at their high school with advanced degrees and the share of graduates from their high school (in the years ahead of them) who went on to attend a 2- or 4-year college.[35] We also consider whether the student lived in a rural area (outside a Metropolitan Statistical area) during high school, since students from sparsely populated areas may know fewer students who have attended different colleges and are less likely to be targeted by recruitment programs (Hoxby 2009). However, in Tables 4.3 and 4.4 these variables also seem to affect college quality more than match.

The one exception to this pattern is the structure of the public university system in the student's home state. Students who are well-matched to one of the colleges in their home state's public university system are more likely to be well-matched with the college they attend and less likely to be either over- or under-qualified. In-state tuition policies give students a strong financial incentive to attend a local state school, which may push them towards mismatch if none of those schools are a good match.

V. Multivariate Analysis

In this section, we estimate the probability that a student will be substantially over- or under-qualified for the first college she attends using probit models, which allow us to consider the effect of budget constraints, information constraints, and demographics holding the remaining factors constant. In our baseline specification, presented in the

[35] We looked at whether the student's high school offered college counseling as well. However, virtually every high school answered "yes" to this question, which was not informative, and there were no follow-up questions that could distinguish how extensive and available this counseling was.

first two columns of Table 4.5a, we continue to consider students mismatched with their school if there is a greater than 20 percentile point gap between their ability percentile and their school's quality percentile. In estimates using other cutoffs, available on request, we found our main findings to be robust to the choice of cutoff.

Ability has a mechanical effect on the probability of mismatch. Very able students will have few schools for which they are under-qualified and many schools for which they are over-qualified. The first principal component of ASVAB scores, the measure of ability we use to define mismatch, has this mechanical effect. Increasing a student's ASVAB percentile by 10 points decreases her probability of being under-qualified by 9 percentage points. Once we control for this first ability measure, however, the other ability measures have the opposite effect; higher high school grades and SAT percentiles raise a student's probability of ending up under-qualified, as defined by her ASVAB score, and lower her probability of being over-qualified. These results suggest that the incomplete information colleges have about their applicants' abilities contributes to college mismatch. Controlling for ASVAB-measure ability, which colleges do not see, students with lower grade point averages, which colleges do see, are more likely to end up at a school for which they are over-qualified, based on the performance on the ASVAB. These results are also a reminder that our ability measures, and therefore our mismatch measures, are subject to measurement error. A student with good grades and SAT scores may truly be a good match for a high-quality school, but we will consider her under-qualified if she scored poorly on the ASVAB.

The remaining results of the multivariate analysis are consistent with the binary statistics presented in Table 4.4. In general, students with more educated or wealthier parents are more likely to be under-qualified for the college they attend and less likely to be over-qualified. Being in the top wealth quartile instead of the 3rd or having a mother with a college degree instead of a high school diploma each lower the probability that a student will be over-qualified by about 4%, the equivalent of raising her SAT percentile by 26 percentage points. Interestingly, the relationship between wealth and under-qualification is non-linear in this specification. Students from the lowest wealth quartile are more likely to be under-qualified for their college than students from the 3rd wealth quartile, the omitted category. This may be a feature of selection. Students from the bottom wealth quartile are less likely to attend college at all (in Table 4.2 the average college attendee is in the 3rd quartile), but those who do may be particularly motivated or subject to some affirmative action by higher quality schools. Starting college more than 12 months after graduating high school, which may be another indication of financial constraints, does raise the probability of being over-qualified by 5 percentage points on average.

Even controlling for parents' wealth and education, the variables that we think capture information and guidance about college, such as the share of the student's high school graduates that go on to college, still lower the probability of over-qualification but raise the probability of under-qualification. Raising the share of adults in the student's neighborhood who have at least four years of college by 5 percentage points—the mean across college enrollees is 21%--has the same effect on the probability of over-qualification as moving from the 3rd to 4th wealth quartile.

Race-based affirmative action programs should lead to minority students being more likely to be under-qualified for their schools, based on their measured ability. We do not find evidence of this effect in our baseline results. In Table 4.5a, Hispanic and black students are somewhat less likely to be under-qualified for their college relative to white students, the omitted category, although other non-white students, who are mostly Asian in this sample, are slightly more likely to be under-qualified.

The second two columns of Table 4.5a repeat our analysis using an expanded measure of college quality that includes dummies indicating a school that does not report SAT scores for its entering students and that admits all students who apply. We set the mean SAT percentile to zero for schools that do not report scores. We designed this measure to better measure college quality across both 2-year and 4-year colleges, but it also allows us to include students starting at 4-year schools that do not report SAT scores. Failure to report SAT scores and open admission policies both have negative weights in our college quality factor analysis, so these new schools are mostly in the lower part of the quality distribution. The determinants of mismatch using this 6-factor measure of college quality are generally quite similar to our baseline results. The positive effects on the probability of being under-qualified of being in the top wealth quartile and having a computer at home are slightly larger than in the baseline specification and are now statistically significantly different from zero.

We also consider an alternative specification of match quality based only on the student's SAT score relative to the average SAT score of the incoming class at her college, presented in Table 4.5b. This definition of mismatch relies on measures of ability observed by the colleges, so it does not capture all the mismatch that arises because colleges have imperfect information about the true ability of applicants or because students misestimate their own abilities relative to other college applicants.

Additionally, the SAT score already embodies some of the guidance students have about applying for college if this information leads them to put extra effort into preparing for the SAT or ACT exams. On the other hand, SAT scores measure ability closer to the time students applied to college. Because of the high stakes of the SAT, there is less risk than in the ASVAB test of under-measuring ability because students have not taken the test seriously.

Using this measure of mismatch, both higher ASVAB scores and higher GPAs make students more likely to be over-qualified and less likely to be under-qualified, the mechanical relationship between ability and match that we would expect. The share of adults in the neighborhood with at least four years of college has a smaller effect using this specification, suggesting that the neighborhood may influence mismatch partially through the student's preparation for college, including their preparation for taking the SAT.

In addition to attending a lower-quality 4-year college, students can also end up over-qualified for their college by starting at a 2-year college. When we use the 6-factor measure of college quality and construct percentiles of college quality across a pooled sample of 2- and 4-year schools, 70% of the 2-year schools are in the lowest quality quartile and almost none are in the top half of the quality distribution. Table 4.5c estimates the determinants of over- and under-qualification using this pooled sample of 2-year and 4-year college starters. In this pooled sample, black students are now more likely to be under-qualified for their college, not less, and both black and Hispanic students are less likely to be over-qualified, a pattern that could be consistent with affirmative action programs. The surprising finding in Table 4.5a that students from the lowest and highest wealth quartiles were both more likely to be under-qualified is not true in this broader sample. Students from less wealthy families are now slightly less likely to be under-qualified for their colleges while students from the wealthiest families are less likely to be over-qualified.

High school GPA and SAT scores have strong effects in this pooled sample; higher GPA and test scores raise the probability of under-qualification and lower the probability of over-qualification. Because 2-year colleges are mostly in the lower end of the quality distribution, this result implies that higher grades and test scores affect both the quality of college students attend, as shown in our baseline results, and the probability of attending a 4-year rather than a 2-year college. By adding more schools we naturally raise the cutoffs for mismatch, 4-year schools that would be more than 20 points from a student's ability percentile in the narrower sample are now considered a good match, so we might expect to see larger effects across the board. However, these stronger effects for grades and test scores persist in both the first two columns of Table 4.5c, where we keep the definition of mismatch consistent with the second two columns of Table 4.5a, and in the second two columns where we recalculate the college quality percentiles using the new, broader set of schools.

VI. Conclusions

In a sample of recent cohort of college entrants, many students appear poorly matched with the college they attend. This mismatch is equally common in both directions; there are about as many high-ability students at relatively low-quality schools as there relatively low-ability students at high-quality schools. In both cases, this mismatch is generally the result of choices made by the student and their families, not by college admissions offices. The vast majority of students who end up mismatched with their college either did not apply to any schools with which they would be well-matched or were accepted to at least one well-matched school and chose to attend a mismatched school instead.

One plausible explanation for over-qualification, when strong students attend relatively low-quality colleges, is that students are financially constrained and cannot afford to attend the higher-quality colleges that would be a better match. We find some evidence to support this theory; students from the wealthiest families are less likely to be over-qualified. However, many factors that we predicted would reduce both types of mismatch instead lower the probability of over-qualification but raise the probability of under-qualification. Students with wealthier and more educated parents are more likely to be under-qualified for their colleges. Factors that we think should lead students to be better informed about their college options, such as the share of graduates from their high school that go on to college, also reduce the probability that students will be over-qualified for their college and raise the probability that students will be under-qualified for their college. Students with a well-matched college within their home state university system are less likely to end up mismatched in either direction. In-state tuition policies often make attending a home state college much less expensive than other options.

Our definitions of over- and under-qualification do not presume that these forms of mismatch are bad for students. Under-qualification in particular may be beneficial since it means that students are attending higher quality colleges than they would be if they were well-matched. In preliminary work (Dillon and Smith 2012), we find that student ability and college quality both raise the probability of graduation and that there is little evidence of a further interaction effect between ability and quality. In general, students seem to benefit from attending a higher-quality college, even if they will be under-qualified there. Students with good information about college and many role models of college attendance appear to recognize this effect and choose their college accordingly.

A1. Data Appendix

We use data from the National Longitudinal Survey of Youth 1997 (NLSY97). This data set contains a very rich set of variables collected in annual interviews with 8,984 American youths. The first survey was conducted in 1997, when the respondents were 13 to 17 years old, and follow-up interviews have been conducted every year since then. The NLSY97 sample contains a representative sample of American youth and an over-sample of black and Hispanic youths. We use observations from both groups of respondents, using the inverse-probability weights developed by the survey collectors to control for the over-sampling.

We consider the college choices of respondents who ultimately attended a 4-year college, about a third of the full sample. Our estimates are based on the 2,385 respondents who went to a college for which we have all the components of our college quality measure and who took the ASVAB test, which we use to measure ability. Appendix Table A4.1 gives details on the construction of our sample.

78% of the youths in the sample took the Armed Services Vocational Aptitude Battery (ASVAB) test, including 85% of the youths who went on to a four-year college (our potential sample). This test has 12 sections, covering the same topics as the SAT, arithmetic, vocabulary, and reading comp, but also other abilities such as electronics knowledge and special reasoning. We estimate common factor loadings across the respondents' scores on all 12 sections. Following Black and Smith (2004) we use the first principal component as our raw measure of student ability. To calculate an ability percentile for each respondent who went to college we rank their raw ability measure among the other respondents who reported attending a 4-year college and who were the same age when they took the ASVAB test. Therefore, this percentile indicates their rank *among college goers*, not among the population as a whole.

We calculate a multi-faceted measure of college quality using the same process as in Black and Smith 2006. We use data from the 2007 Integrated Postsecondary Education Data System (IPEDS) and from the 2009 US News and World Report College Rankings (data collected in 2008). The variables combine measures of peer quality, average SAT score of the incoming class, the resources of the school, faculty/student ratios and average faculty salaries, and a "voting with your feet" measure of how students and their families assess the school, the share of applicants that are rejected. SAT scores are from US News where available and IPEDS otherwise. All other variables are from IPEDS. We calculate factor loadings across these four quality measures and construct the first component factor. Our final measure of college quality is that school's percentile among all 4-year colleges in the IPEDS database, weighted by student body size.

We weight by college size so that if students sorted into schools perfectly by ability and quality the students in the *x* ability percentile would be at schools in the *x* quality percentile. In this case, our measure of mismatch, student ability percentile minus college quality percentile, would be zero for all students.

Appendix Tables A4.2 and A4.3 give details for the construction of the other independent variables in our estimation.

Table 4.1: Joint Distribution of College Quality and Ability, Four-year Starters

Ability Quartiles	College Quality Quartiles: 1st Quartile	2nd Quartile	3rd Quartile	4th Quartile	Total
1st Quartile	9.0	6.4	3.9	2.4	
	(41.5)	(29.2)	(18.0)	(11.2)	(100.0)
	[37.2]	[24.2]	[15.2]	[10.4]	(N=489.1)
2nd Quartile	6.4	7.2	6.8	4.6	
	(25.7)	(28.9)	(27.2)	(18.3)	(100.0)
	[26.5]	[27.4]	[26.4]	[19.4]	(N=562.1)
3rd Quartile	5.8	6.9	7.9	5.9	
	(22.0)	(26.0)	(29.9)	(22.2)	(100.0)
	[24.0]	[26.2]	[30.8]	[24.9]	(N=596.3)
4th Quartile	3.0	5.8	7.1	10.7	
	(11.1)	(21.9)	(26.7)	(40.3)	(100.0)
	[12.2]	[22.2]	[27.6]	[45.4]	(N=598.6)
Total	[100.0]	[100.0]	[100.0]	[100.0]	100.0
	[N=544.9]	[N=590.9]	[N=579.2]	[N=531.1]	N=2246.1

Source: NLSY 1997 cohort. Each cell contains the overall percentage, (the row percentage), and [the column percentage].

Table 4.2: College Applications and Mismatch

N	Ended up under-qualified 211	Ended up well-matched 373	Ended up over-qualified 193
% applied to under	100.0%	22.1%	7.5%
% applied to well	32.0%	100.0%	31.1%
% applied to over	4.0%	14.3%	100.0%
% accepted to under	100.0%	14.3%	4.5%
% accepted to well	29.2%	100.0%	22.1%
% accepted to over	4.0%	14.3%	100.0%

Share of mismatched who:	Underqualified	Overqualified
Didn't apply to a good match	68.0%	68.9%
Applied to a good match but didn't get in	2.8%	9.0%
Were accepted to a good match but didn't attend	29.2%	22.1%

Note: Only the younger NLSY97 respondents were asked questions about college applications. Of the 2,106 respondents who started at a 4-year college and for whom we have a measure of match with their college, 777 are included in these tables. Of the rest, 1,255 (94% of the missing) are excluded because they were born in 1980, 1981, or 1982. Another 38 (3% of missing) are ineligible for the application section for other reasons. 2% are missing because they were eligible but didn't answer any application questions and 1% answered questions, but we could match any of the schools they applied to with match measures. Both tables use inverse probability weights.

Table 4.3: Average Characteristics of Students by College Choice, Four-year Starters

	College Attendees	College quality quartile 1, lowest	2	3	4, highest
N	1,977	517	520	499	441
Male	45%	44%	43%	43%	49%
Black	11%	18%	12%	8%	6%
Hispanic	6%	6%	5%	8%	6%
Other (not white)	7%	2%	5%	8%	12%
Household members age 18 or under	2.2	2.3	2.2	2.1	2.2
Started college late	9%	15%	8%	5%	7%
ASVAB 1 percentile*	52%	39%	49%	55%	64%
ASVAB 2 percentile*	51%	46%	48%	52%	57%
High school GPA percentile*	53%	44%	50%	56%	61%
SAT percentile*	53%	36%	47%	58%	69%
Northeast region	21%	11%	20%	18%	34%
South region	30%	39%	23%	32%	27%
Midwest region	31%	32%	40%	29%	21%
West region	18%	18%	18%	21%	17%
Wealth quartile	3.1	2.8	3.1	3.2	3.3
Mother's highest completed grade	14.4	13.9	14.1	14.5	14.9
Took classes outside of school	40%	34%	35%	42%	48%
Had computer at home	80%	69%	79%	84%	88%
Had dictionary at home	99%	99%	100%	97%	99%
Avg. 4-year in-state tuition	$3,138.2	$2,950.8	$3,143.6	$3,106.0	$3,359.8
Matched public 4-year in state	92%	92%	91%	91%	95%
Matched private 4-year in 50 mi	64%	52%	62%	66%	77%
% adults in district with BA	21%	18%	20%	23%	24%
% of HS teachers with adv degree	57%	51%	58%	57%	61%
% of HS class to 2-year	18%	15%	19%	19%	18%
% of HS class to 4-year	56%	52%	54%	57%	61%
Rural	18%	29%	19%	14%	10%

Notes: This table describes the characteristics of students at each college quartile. For example, the third row shows the percent of students attending each college type that are male. Numbers calculated with probability weights to control for sample selection. ASVAB percentile is among 4-year college starters, adjusted by age.

Table 4.4: Average Characteristics of Students by Match Quality, Four-year Starters

	College Attendees	Very Over-qualified	Well-matched	Very Under-qualified
N	1,977	530	936	511
Male	45%	36%	45%	51%
Black	11%	15%	13%	4%
Hispanic	6%	9%	6%	4%
Other (not white)	7%	12%	6%	3%
Household members age 18 or under	2.216	2.215	2.217	2.214
Started college late	0.088	0.087	0.089	0.090
ASVAB 1 percentile*	52%	31%	51%	71%
ASVAB 2 percentile*	51%	55%	53%	43%
High school GPA percentile*	53%	47%	51%	60%
SAT percentile*	53%	43%	52%	64%
Northeast region	21%	28%	22%	12%
South region	30%	30%	32%	28%
Midwest region	31%	24%	29%	40%
West region	18%	18%	18%	20%
Wealth quartile	3.1	2.9	3.1	3.2
Mother's highest completed grade	14.4	14.0	14.5	14.5
Took classes outside of school	40%	34%	42%	41%
Had computer at home	80%	77%	80%	82%
Had dictionary at home	99%	98%	99%	100%
Avg. 4-year in-state tuition	$3,138.2	$3,184.9	$3,170.7	$3,045.2
Matched public 4-year in state	92%	92%	97%	83%
Matched private 4-year in 50 mi	64%	82%	70%	40%
% adults in district with BA	21%	23%	22%	19%
% of HS teachers with adv degree	57%	58%	57%	54%
% of HS class to 2-year	18%	20%	17%	17%
% of HS class to 4-year	55.92%	56.43%	55.83%	55.64%
Rural	18%	12%	18%	23%

Notes: This table describes the characteristics of students at each college quartile. For example, the third row shows the percent of students attending each college type that are male. Numbers calculated with probability weights to control for sample selection. ASVAB percentile is among 4-year college starters, adjusted by age.

Table 4.5A: Determinants of Mismatch, CQ Index and ASVAB Ability

	4-factor quality index		6-factor quality index	
	Under-qualified	Over-qualified	Under-qualified	Over-qualified
Male	-0.009 (0.011)	-0.045** (0.008)	-0.000 (0.010)	-0.022** (0.008)
Black	-0.042** (0.014)	-0.038** (0.012)	0.006 (0.014)	-0.036** (0.012)
Hispanic	0.031 (0.018)	-0.028 (0.015)	-0.012 (0.017)	-0.037** (0.013)
Other (not white)	0.150** (0.020)	-0.080** (0.017)	0.069** (0.019)	-0.065** (0.016)
Age 18 or younger in hh	-0.009* (0.004)	-0.000 (0.004)	-0.009* (0.004)	0.007* (0.004)
Started college late	-0.035* (0.017)	0.045** (0.016)	-0.082** (0.014)	0.091** (0.015)
ASVAB 1 percentile	-0.901** (0.027)	0.680** (0.018)	-0.872** (0.026)	0.718** (0.018)
ASVAB 2 percentile	0.029 (0.019)	-0.122** (0.016)	0.062** (0.018)	-0.098** (0.015)
High school GPA prctl	0.123** (0.024)	-0.069** (0.020)	0.128** (0.022)	-0.045* (0.018)
SAT percentile	0.249** (0.029)	-0.151** (0.023)	0.110** (0.028)	-0.129** (0.022)
Northeast region	0.085** (0.016)	-0.090** (0.010)	0.124** (0.016)	-0.090** (0.009)
South region	-0.015 (0.015)	-0.030** (0.011)	0.041** (0.015)	-0.035** (0.010)
West region	-0.053** (0.018)	-0.027 (0.014)	0.035 (0.019)	-0.037** (0.012)
1^{st} wealth quartile	0.046* (0.020)	0.004 (0.017)	0.046* (0.019)	0.023 (0.017)
2^{nd} wealth quartile	-0.028 (0.017)	-0.030* (0.013)	0.027 (0.017)	0.006 (0.013)
4^{th} wealth quartile	0.015 (0.014)	-0.037** (0.011)	0.053** (0.014)	-0.023* (0.010)
Mother is HS dropout	0.083** (0.023)	-0.086** (0.017)	0.013 (0.020)	-0.037* (0.017)
Mother has some college	0.014 (0.013)	-0.031** (0.010)	0.045** (0.013)	-0.017 (0.010)
Mother is college graduate	0.013 (0.013)	-0.043** (0.010)	0.021 (0.013)	-0.036** (0.009)
Took classes out of sch.	-0.009 (0.013)	-0.015 (0.011)	-0.006 (0.012)	-0.001 (0.010)
Had computer at home	0.019 (0.015)	-0.016 (0.013)	0.046** (0.015)	-0.033** (0.012)
Avg. 4-year in-state tuition	-0.010 (0.008)	-0.031** (0.007)	-0.011 (0.007)	-0.014* (0.007)
Matched public 4-year in state*	-0.011 (0.019)	-0.139** (0.009)	0.000 (0.013)	-0.058** (0.009)
Matched private 4-year in 50 mi*	0.102** (0.031)	-0.239** (0.008)	0.118** (0.014)	-0.071** (0.007)
% adults in district with BA	0.416** (0.062)	-0.670** (0.061)	0.434** (0.060)	-0.479** (0.057)
% of HS teachers with adv degr	0.013 (0.027)	-0.036 (0.023)	0.053* (0.026)	-0.014 (0.021)
% of HS class to 2-year	0.316** (0.051)	-0.103* (0.041)	0.170** (0.048)	-0.048 (0.038)
% of HS class to 4-year	0.135** (0.032)	0.010 (0.027)	0.044 (0.031)	0.007 (0.025)
Rural	-0.039** (0.015)	-0.009 (0.011)	0.000 (0.015)	-0.018 (0.010)
N	1,977	1,977	2,161	2,161
Pseudo R2	0.272	0.293	0.289	0.320

Note: ** indicates ~statistically significant with 1% confidence, * with 5% confidence. Tuition in thousands of 1997 dollars. Mean marginal effects reported. This table includes four-year college starters.
* Having a well-matched public and private school nearby is determined based on the dependent variable's definition of match for each pair of regressions.

Table 4.5B: Determinants of Mismatch, SAT Mismatch

	Under-qualified	Over-qualified
Male	0.067** (0.007)	-0.053** (0.005)
Black	-0.110** (0.008)	0.081** (0.010)
Hispanic	-0.060** (0.010)	-0.026** (0.010)
Other (not white)	0.122** (0.013)	0.004 (0.011)
Age 18 or younger in household	-0.021** (0.003)	-0.004 (0.003)
Started college late	-0.081** (0.012)	-0.052** (0.010)
ASVAB 1 percentile	0.012 (0.018)	0.055** (0.015)
ASVAB 2 percentile	0.072** (0.011)	-0.033** (0.010)
High school GPA percentile	0.187** (0.013)	-0.008 (0.011)
SAT percentile	-0.844** (0.018)	0.458** (0.016)
Northeast region	-0.008 (0.009)	-0.021** (0.007)
South region	-0.069** (0.007)	-0.028** (0.007)
West region	-0.101** (0.009)	0.016 (0.009)
1st wealth quartile	0.160** (0.014)	0.023 (0.012)
2nd wealth quartile	0.048** (0.010)	-0.028** (0.008)
4th wealth quartile	0.070** (0.009)	-0.040** (0.006)
Mother is HS dropout	0.049** (0.013)	-0.094** (0.008)
Mother has some college	0.034** (0.008)	-0.083** (0.005)
Mother is college graduate	0.042** (0.008)	-0.104** (0.005)
Took classes outside of school	0.042** (0.008)	-0.039** (0.006)
Had computer at home	-0.003 (0.010)	0.053** (0.010)
Avg. 4-year in-state tuition	-0.008 (0.005)	0.005 (0.004)
Matched public 4-year in state*	-0.006 (0.022)	-0.116** (0.008)
Matched private 4-year in 50 mi*	-0.236** (0.009)	0.165** (0.037)
% adults in district with BA	0.293** (0.039)	-0.144** (0.034)
% of HS teachers with adv degr	-0.163** (0.015)	-0.004 (0.013)
% of HS class to 2-year	0.175** (0.030)	0.271** (0.025)
% of HS class to 4-year	0.093** (0.018)	0.028 (0.016)
Rural	-0.030** (0.007)	0.047** (0.007)
N	1,177	1,177
Pseudo R2	0.240	0.194

Note: ** indicates ~statistically significant with 1% confidence, * with 5% confidence. Tuition in thousands of 1997 dollars. Mean marginal effects reported. This table includes four-year college starters.
* Having a well-matched public and private school nearby is determined based on the dependent variable's definition of match for each pair of regressions.

Table 4.5C: Determinants of Mismatch, CQ Index and ASVAB Ability, All Starters

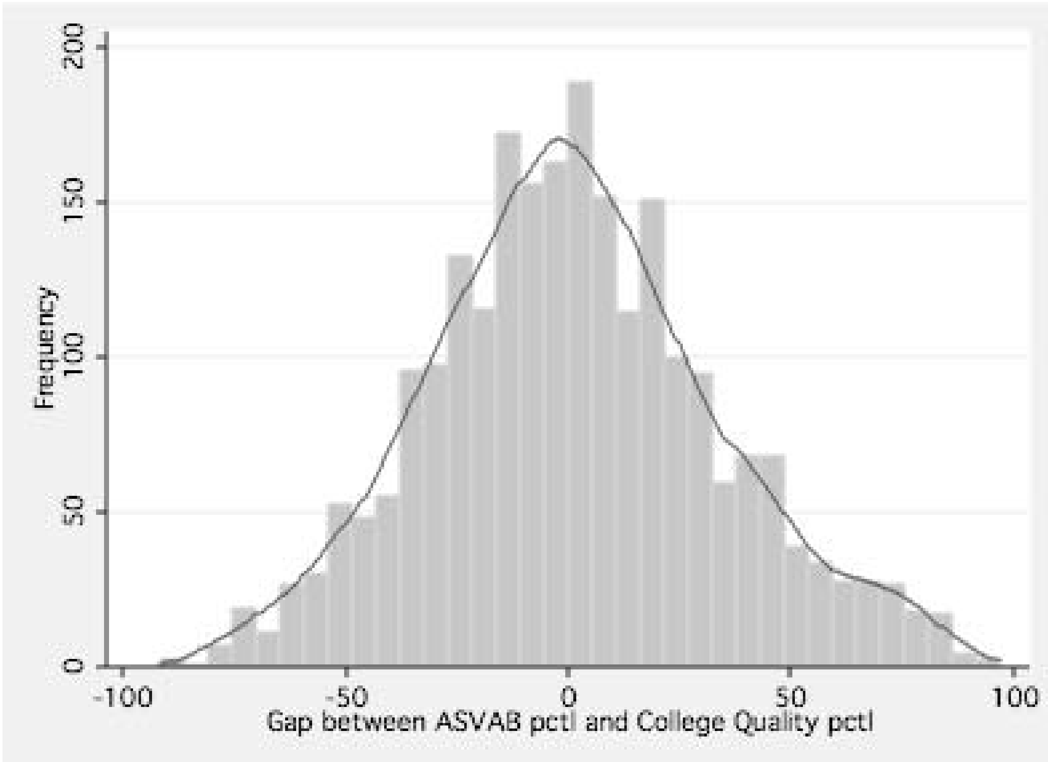
	Ptiles based on 4-year		Ptiles based on all	
	Under-qualified	Over-qualified	Under-qualified	Over-qualified
Male	0.000 (0.007)	-0.021** (0.008)	-0.003 (0.007)	-0.019* (0.008)
Black	0.038** (0.010)	-0.124** (0.010)	0.057** (0.010)	-0.117** (0.010)
Hispanic	-0.026** (0.009)	-0.027* (0.011)	-0.001 (0.010)	-0.036** (0.011)
Other (not white)	0.081** (0.015)	-0.085** (0.015)	0.089** (0.015)	-0.066** (0.015)
Age 18 or younger in hh	-0.010** (0.003)	0.003 (0.003)	-0.004 (0.003)	0.000 (0.003)
Started college late	-0.069** (0.007)	0.086** (0.011)	-0.076** (0.008)	0.094** (0.011)
ASVAB 1 percentile**	-0.448** (0.018)	0.869** (0.018)	-0.708** (0.020)	0.826** (0.016)
ASVAB 2 percentile**	0.089** (0.012)	-0.125** (0.015)	0.095** (0.013)	-0.141** (0.014)
High school GPA prcntl**	0.141** (0.016)	-0.163** (0.017)	0.140** (0.016)	-0.195** (0.017)
SAT percentile**	0.062** (0.020)	-0.247** (0.022)	0.085** (0.021)	-0.216** (0.022)
Northeast region	0.112** (0.015)	-0.126** (0.011)	0.131** (0.014)	-0.091** (0.011)
South region	-0.009 (0.010)	0.011 (0.012)	-0.036** (0.010)	0.042** (0.012)
West region	-0.037** (0.011)	-0.016 (0.014)	-0.061** (0.011)	-0.014 (0.013)
1st wealth quartile	0.004 (0.012)	-0.016 (0.013)	-0.019 (0.012)	-0.000 (0.013)
2nd wealth quartile	-0.013 (0.011)	0.007 (0.012)	-0.029** (0.011)	0.046** (0.012)
4th wealth quartile	0.029** (0.010)	-0.040** (0.010)	-0.002 (0.010)	-0.026** (0.010)
Mother is HS dropout	0.005 (0.012)	-0.066** (0.014)	0.011 (0.012)	-0.041** (0.014)
Mother has some college	0.039** (0.009)	0.005 (0.010)	0.033** (0.009)	-0.003 (0.009)
Mother is college graduate	0.042** (0.010)	-0.076** (0.010)	0.045** (0.010)	-0.084** (0.009)
Took classes out of sch.	-0.001 (0.009)	0.004 (0.010)	-0.005 (0.009)	-0.027** (0.009)
Had computer at home	0.034** (0.010)	-0.024* (0.011)	0.034** (0.010)	-0.023* (0.011)
Avg. 4-year in-state tuition	0.010 (0.006)	-0.045** (0.007)	-0.006 (0.006)	-0.039** (0.007)
Avg. 2-year in-state tuition	-0.053** (0.009)	0.052** (0.010)	-0.059** (0.009)	0.031** (0.009)
Matched public 2- or 4-year in state*	-0.061** (0.015)	-0.130** (0.017)	-0.081** (0.014)	-0.131** (0.014)
Matched private 2- or 4-year in 50 mi*	0.054** (0.009)	0.004 (0.009)	0.064** (0.009)	-0.012 (0.009)
% adults in district with BA	0.357** (0.040)	-0.502** (0.051)	0.412** (0.044)	-0.489** (0.049)
% of HS teachers with adv degr	0.040* (0.017)	0.022 (0.020)	0.041* (0.018)	-0.000 (0.019)
% of HS class to 2-year	-0.180** (0.034)	0.280** (0.035)	-0.181** (0.036)	0.212** (0.033)
% of HS class to 4-year	0.012 (0.021)	-0.023 (0.024)	0.042 (0.022)	-0.049* (0.023)
Rural	0.005 (0.010)	-0.001 (0.011)	0.022* (0.011)	0.013 (0.011)
N	3,388	3,388	3,388	3,388
Pseudo R2	0.199	0.285	0.278	0.283

Note: ** indicates ~statistically significant with 1% confidence, * with 5% confidence. Tuition in thousands of 1997 dollars. Mean marginal effects reported.

* Having a well-matched public and private school nearby is determined based on the dependent variable's definition of match for each pair of regressions.

** Percentiles are set by 4-year starters in first two columns and 2- and 4-year starters in last two columns.

Figure 4.1: Distribution of College Mismatch



Mismatch defined as student ability percentile - college quality percentile. Histogram includes estimated kernel density distribution.

Table A4.1: The Sample

Total Observations	8,984
Graduated HS	7,052
Did not graduate HS but got GED	757
Started at a 2-year college*	2,467
Started at a 4-year college	2,771
Starting college qualities	
Of quality quartile 1	*1,444*
Of quality quartile 2	*1,347*
Of quality quartile 3	*917*
Of quality quartile 4	*900*
Missing quality	*630*
Has quality, but missing ability	*780*
Starting college qualities, 4-year only	
Of quality quartile 1	*614*
Of quality quartile 2	*602*
Of quality quartile 3	*582*
Of quality quartile 4	*507*
Missing quality	*466*
Has quality, but missing ability	*328*

* The 2-year starters include 17 respondents who have no record of either graduating high school or getting a GED.
College quality for first college attended. For 4-year schools only this is based on the 4-factor college quality measure. For all colleges it's based on the 6-factor college quality. Of the 203 respondents who started at a 2-year school for which we don't have a quality measure, 65 are missing quality because I could not identify the college and 237 are missing quality because the school was not in IPEDS or did not have enough information to make the quality measure. For 310 4-year schools without quality, 39 had no identifier and 271 did not have all the quality measures.

Table A4.2: Description of Independent Variables

Variable	Description
Male	Dummy variable equal to 1 if the respondent is male, 0 otherwise
Black	Dummy variable equal to 1 if the respondent lists black as a racial category, 0 otherwise
Hispanic	Dummy variable equal to 1 if the respondent lists Hispanic as an ethnic category and doesn't list black as a racial category
Started college late	Equal to one if the respondent started college more than 12 months after graduating high school.
Region of the U.S.	Region where the respondent lived in the fall before they graduated high school.
ASVAB percentile	Described in the data appendix
High School GPA	Collected from the respondent's high school transcript and standardized to a 4-point scale weighted by Carnegie credits. GPA percentile is calculated within our [weighted] sample of college-goers in the same way as the ASVAB percentile.
SAT score	The combined score on the math and verbal section of the SAT (max score 1600), collected from the respondent's high school transcript. SAT percentile is calculated within our [weighted] sample of college-goers in the same way as the ASVAB percentile.
Mother's Education	The respondent's mother's self-reported highest grade completed. This measure is taken from the NLS-constructed household roster for the fall before the respondent graduated high school (or earlier if that year is unavailable). Mother refers to the mother figure that the respondent lived with. When there was more than one mother figure included in the household, we considered only one, using the following prioritization: biological, adopted, step, or foster.
Took classes outside of school	From the 1997 youth survey. Equal to one if he or she answered yes to "In a typical week, did you spend any time taking extra classes or lessons for example, music, dance, or foreign language lessons?"
Had computer at home	From the 1997 youth survey. Equal to one if he or she answered yes to "In the past month, has your home usually had a computer?"
Had dictionary at home	From the 1997 youth survey. Equal to one if he or she answered yes to "In the past month, has your home usually had a dictionary?"
Quality percentile of state flagship	Quality percentile, as described in the data appendix, of the flagship state university in the state where the respondent lived in the fall before they graduated high school.
% in census district with BA	The share of the adult (over 25) population that has at least 4 years of college (from 1990 census) in the census district where the respondent lived *in 1997*.
Household income	Total 1996 household income for the household where the respondent lived *in 1997*. This number is taken from the parent survey where available and from the youth survey when the parent response is missing (98.6% from parent survey). We use total income across everyone living in the same household as the respondent (whether or not respondent is independent from parents in 1997). Income quintile cutoffs are taken from the 1996 Current Population Survey Annual Social and Economic Supplement Income Percent Distribution for Families.
Household wealth	Total 1997 household net worth for the household where the respondent lived *in 1997*. This number is taken from the parent survey where available and from the youth survey when the parent response is missing (98.6% from parent survey). We use total wealth across everyone living in the same household as the respondent (whether or not respondent is independent from parents in 1997). 1997 wealth quartiles are calculated within the (weighted) sample.

Table A4.3: Description of Independent Variables, Continued

% of HS teachers with advanced degrees	From the restricted NLSY97 School Survey. The response from the respondent's last high school to the survey question "what percent of your teachers have more than a bachelor degree?"
% of HS class to 4-year	From the restricted NLSY97 School Survey. The response from the respondent's last high school to the survey question "by the fall following graduation, about what percent of your 1999 graduating class enrolled in a 4-year college?"
In-state tuition at flagship	In-state tuition, by year, for the flagship university of each state is from the State of Washington Higher Education Coordinating Board. "In-state" tuition for District of Columbia residents is calculated as max(national average in-state tuition, national average out-of-state tuition - $10,000) in accordance with DC Tuition Assistance Grant Program. For each respondent, in-state tuition is the in-state tuition in the fall before they graduated high school in the state where they lived that fall. All tuition is CPI-deflated to 1997 dollars.
Rural	Indicates that the respondent did not live within a Metropolitan Statistical Area (MSA) in fall before they graduated high school.
Nearby universities	Distance is calculated from the zipcode of the respondent's residence in the fall before they graduated from high school. In the 352 cases where the zipcode that fall was missing, the zipcode from the last available year prior to graduation is used.

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