

ABSTRACT

Title of Document: ESSAYS ON THE ECONOMICS OF
EDUCATION

Alexander Thomas Whalley,
Doctor of Philosophy, 2006

Directed By: Professor, Jeffrey Smith, Economics

In the first chapter I study racial differences in the impact of education on labor income volatility. Using panel data on black and white males from the Panel Study of Income Dynamics I find that education reduces labor income volatility more for blacks than for whites. The central specifications indicate that college graduation reduces transitory labor income volatility by more than 65 percent relative to high school dropouts for blacks, whereas whites receive no statistically significant reduction. I also find that more risk averse blacks obtain more education while more risk averse whites do not. I argue that these results imply: (1) that precautionary demand for human capital is quantitatively important; and (2) the black differential investment puzzle can be explained by accounting for racial differences in the impact of education on exposure to labor income volatility. The results can be explained by the precision of employer's beliefs about a worker's productivity increasing more with education for blacks, so that more-skilled blacks face less labor income volatility.

Participants, like econometricians, may have difficulty in constructing the counterfactual outcome required to estimate the impact of a program. In the second chapter, this question is directly assessed by examining the extent to which program participants are able to estimate their individual program impacts ex-post. Utilizing experimental data from the National Job Training Partnership Act (JTPA) Study (NJS) experimentally estimated program impacts are compared to individual self-reports of program effectiveness after the completion of the program. Two methods are implemented to estimate the individual experimental impacts based on: (1) subgroup variation; (2) the assumption of perfect rank correlation in impacts. Little evidence of a relationship between the experimentally estimated program impacts and self-reported program effectiveness is found. There is evidence found that cognitively inexpensive potential proxies for program impacts such as before-after differences in earnings, the type of training received, and labor market outcomes are correlated with self-reported program effectiveness.

ESSAYS ON THE ECONOMICS OF EDUCATION

By

Alexander Thomas Whalley

Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park, in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2006

Advisory Committee:
Professor Jeffrey Smith, Chair
Mark Duggan
Judith Hellerstein
John Shea
John Iceland

© Copyright by
Alexander Thomas Whalley
2006

Foreword

Economists consider education decisions primarily as an investment. Individuals acquire education until the benefit of obtaining additional education is less than the cost. The benefit of education is that the acquired skills allow individuals to be able to sell their labor at a higher price. Education requires time and effort to acquire, however, making it a costly investment. This simple framework based on deterministic costs and benefits of education forms the basis for the standard model of educational investment. I build upon recent findings of the determinants of financial investments, and the psychology of assessing the impact of interventions to provide a richer view of the incentives to invest in education. The two essays that form this dissertation consider the role of uncertainty in determining incentives to invest in education. They differ in the timing of the measurement of the uncertainty concerning when the benefits of education. The first essay examines ex-ante uncertainty, whereas the second measures ex-post uncertainty.

In the first essay I investigate the idea that individual education decisions are influenced by how uncertain the benefits from education might be. Existing evidence shows that the amount of labor income uncertainty that individuals expect to face is an important determinant of financial investment decisions. Thus, a key motive for financial wealth holding is to reduce exposure to labor income uncertainty. If education investment decisions are made in a similar fashion then this motive should also affect educational investment incentives. Thus, education will provide additional

benefits if labor income uncertainty falls with education acquisition. I examine this idea by estimating whether there are differences across race in the effect of education on labor income volatility. I find that education significantly reduces labor income volatility for blacks, but not for whites. This implies that education is more valuable for blacks than is implied by the simple rate of return to education for blacks. In addition, I find that more risk averse blacks obtain more education, but that this is not true for whites. This finding offers a possible explanation for the puzzle that blacks obtain more education (conditional on test scores) than whites, even though the rate of return to education does not differ by race. Thus, the incorporation of uncertainty into the standard model of the investment in education provides an explanation for a previously unexplained puzzle in the economics of education.

The second essay examines to what extent individuals are able to assess whether a job training program they have just completed had a positive impact on their labor income. The central question here is whether individuals can accurately construct the counterfactual experiment of what would have happened to them if they had or had not received an intervention. In this case, the intervention is the receipt of job training. If an individual is able to accurately construct this counterfactual then they are able to estimate their true benefit of a participating in the program. However, constructing a counterfactual for a job training program is a cognitively difficult task. The individual has to be able to assess exactly what outcome they would have had if they did not participate. The question of how well individuals are able to evaluate a job training program is addressed by examining how ex-post participant self-

evaluations of the programs' effectiveness are correlated with experimentally estimated predicted program impacts. If individuals are able to accurately construct the counterfactual outcome, the experimentally estimated predicted impacts will be strongly related to the participants' self-assessment of the program. The results show that there is little evidence for a correlation between the predicted impact and the self-assessment of the individual. This suggests that individuals either have different assessments of what is valuable about a program than administrators, or they are unable to construct counterfactuals well. In addition, evidence that participant self-assessments are correlated with easy-to-observe potential program impact proxies is found. This suggests that individuals do have trouble constructing counterfactual outcomes. In sum, this essay shows that program participants face substantial uncertainty about the benefits of job training programs even after the training has occurred.

Dedication

To my wife Oksana, who contributed to this research in so many ways it is impossible to count. This thesis quite simply would never have been completed without your exceptional support and understanding.

To my mother and father who brought me up to be curious about the world and whose example encouraged me to pursue that curiosity. It is your many years of encouragement and dedication that have enabled me to reach this point.

To my parents-in-law who have been very supportive throughout this endeavor.

Acknowledgements

I owe a huge debt to my advisor Jeffrey Smith for encouragement, and for his many ideas and questions which pushed me to think more deeply about this topic. My other committee members, Judy Hellerstein, John Shea, and Mark Duggan, have also contributed many significant insights that have shaped the direction of this thesis. I would like to thank all of my committee members for their careful reading of multiple drafts of my thesis, and for providing me with frank and helpful feedback at numerous points along the way. I would also like to acknowledge that the second chapter is joint work with Jeffrey Smith and Nathaniel Wilcox.

In addition, I wish to thank Martha Bailey, Dan Black, Sarah Bohn, Mick Coelli, Juan Contreras, Gordon Dahl, Juan-Jose Diaz, Nicola Fuchs-Schundeln, Jonah Gelbach, William Johnson, Beomsoo Kim, Kevin Lang, Sumon Majumdar, Silvio Rendon, Isabel Rodriguez, Seth Sanders, Sarah Simmons, Ignez Tristao, Annette Vissing-Jorgensen, and workshop participants at a number of conferences and universities for offering helpful comments and suggestions on this research. I thank Enrico Moretti for providing the data for compulsory schooling laws on his website. I am very grateful to the Washington Economic Club Dissertation Research Fellowship and the W.E. Upjohn Institute for providing financial support to fund this research. All errors are my own.

Table of Contents

Foreword.....	ii
Dedication.....	v
Acknowledgements.....	vi
Table of Contents.....	vii
List of Tables.....	ix
List of Figures.....	xi
Chapter 1: Racial Differences in the Insurance Value of Education.....	1
1.1 Introduction.....	1
1.2 Labor Income Uncertainty and Investment in Education.....	5
1.2.1 An Illustrative Model.....	6
1.2.2 Racial Differences In the Impact of Education on Labor Income Volatility.....	10
1.3 Data.....	13
1.4 Racial Differences In Education and Labor Income Volatility.....	18
1.4.1 Measuring Labor Income Volatility.....	19
1.4.2 The Impact of Education on Labor Income Volatility.....	20
1.5 Results.....	24
1.5.1 Basic Results.....	25
1.5.2 Sensitivity Analysis: Instrumental Variables Estimates.....	29
1.5.3 Sensitivity Analysis: Alternative Measures of Labor Income.....	31
1.6 Preferences For Risk and Educational Attainment.....	36
1.6 Conclusion.....	41
Chapter 2: Are Participants Good Evaluators?.....	44
2.1 Introduction.....	44
2.2 Data and Institutions.....	48
2.2.1 The JTPA program.....	48
2.2.2 The National JTPA Study data.....	50
2.2.3 The self-evaluation questions.....	52
2.3 Conceptual framework.....	53
2.3.1 A simple model of participants' self-reported evaluations.....	53
2.3.2 Econometric specification using econometric impact estimates.....	55
2.3.3. Econometric specification: impact proxies and performance measures... ..	58
2.4 Econometric impact estimators.....	59
2.4.1 Experimental impacts at the subgroup level.....	59
2.4.2 Quantile treatment effects.....	62
2.5 The relationship between econometric impact estimates and participant evaluations.....	63
2.5.1 Bivariate relationships.....	63
2.5.2 Regression results for experimental subgroup estimates.....	66
2.5.3 Results based on quantile treatment effect estimates.....	67
2.6 Relationship between positive self-evaluation and proxies for impacts.....	69
2.6.1 Motivation.....	69
2.6.2 Results with input and outcome measures.....	70

2.6.3. Results with before-after comparisons of labor market outcomes.....	77
2.7 Results with performance measures	79
2.8 Conclusions.....	82
Appendices.....	84

List of Tables

Table 1.1:	Transitory Variance of Log Labor plus Social Insurance Income, by Race and Education96
Table 1.2:	The Effect of Education on Labor Income Risk, By Race97
Table 1.3:	The Effect of High School Graduation on Labor Income Risk, By Race98
Table 1.4:	The Effect of Education on Labor Income Risk, By Race: Recoded Zero Values Included100
Table 1.5:	The Effect of Education on Labor Income Risk, By Race: Including Federal Income Tax101
Table 1.6:	The Effect of Education on Labor Income Risk, By Race: Log Labor Income Only102
Table 1.7:	Sources of Labor Income Risk, by Race and Education103
Table 1.8:	The Effect of Education on Wage Risk, By Race104
Table 1.9:	The Effect of Education on Unemployment Risk, By Race105
Table 1.10:	The Effect of Preferences for Risk on Educational Attainment, By Race:106
Table 1.11:	The Effect of Preferences for Risk on Educational Attainment, By Race:107
Table 1.12:	The Effect of Preferences for Risk on Educational Attainment With Wealth Interactions, By Race:108
Table 1.A1:	Sample Statistics109
Table 1.A2:	Tabulation of Annual Sample Size, by Race and Education110
Table 1.A3:	Tabulation of Risk Aversion Measure, by Race and Education111
Exhibit 2.1:	JTPA Self-Evaluation Survey Questions112
Table 2.1:	Bivariate Results for the relationship between Experimental Impacts and Positive Self-Evaluation, By Demographic Group113
Table 2.2A:	Bivariate results for the Correlation between Experimental Impacts and Self-Evaluation for Eight Outcomes, Adult Males115
Table 2.2B:	Bivariate results for the Correlation between Experimental Impacts and Self-Evaluation for Eight Outcomes, Adult Females117
Table 2.2C:	Bivariate results for the Correlation between Experimental Impacts and Self-Evaluation for Eight Outcomes, Male Youths119
Table 2.2D:	Bivariate results for the Correlation between Experimental Impacts and Self-Evaluation for Eight Outcomes, Female Youths121
Table 2.3:	Regression results for the relationship between Predicted Impacts and Positive Self-Evaluation for Eight Outcomes, By Demographic Group123

Table 2.4:	Relationship between Quantile Treatment Effects for 18-Month Earnings and the Percent with Positive Self-Evaluation, By Demographic Group125
Table 2.5:	Logit Estimates of the Determinants of Positive Self-Evaluation, By Demographic Group127
Table 2.6:	Test Statistics from Logit Models of the Determinants of Positive Self-Evaluation, By Demographic Group130
Table 2.7:	Logit Estimates of the Relationship between Outcomes and Positive Self-Evaluation: Four Outcomes, By Demographic Group131
Table 2.8:	Test Statistics from Logit Models of the Relationship between Outcomes and Positive Self-Evaluation, By Demographic Group133
Table 2.9:	Logit Estimates of the Relationship between Before-After Self-Reported Earnings Changes and Positive Self-Evaluation, By Demographic Group135
Table 2.10:	Logit Estimates of the Relationship between Before-After UI Earnings Changes and Positive Self-Evaluation, By Demographic Group136
Table 2.11:	Logit Estimates of the Relationship between Before-After Employment Status Changes and Positive Self-Evaluation, By Demographic Group138
Table 2.12:	Test Statistics from Logit Models of the Relationship between Before-After Estimates and Positive Self-Evaluation, By Demographic Group139
Table 2.13:	Logit Estimates of the Relationship between Positive Self-Evaluation and Performance Standards, By Demographic Group140

List of Figures

Figure 2.1A: Quantile Treatment Effects and Percentage Reporting Positive Self-Evaluation, Adult Males142
Figure 2.1B: Quantile Treatment Effects and Percentage Reporting Positive Self-Evaluation, Adult Females143
Figure 2.1C: Quantile Treatment Effects and Percentage Reporting Positive Self-Evaluation, Male Youth144
Figure 2.1D: Quantile Treatment Effects and Percentage Reporting Positive Self-Evaluation, Female Youth145

Chapter 1: Racial Differences in the Insurance Value of Education

1.1 Introduction

For labor income uncertainty to alter incentives to invest in education, two conditions must hold. First, labor income uncertainty must not be completely insurable. If this is the case fluctuations in labor income will result in fluctuations in consumption, leading to a loss of welfare for risk averse individuals. Cochrane (1991) and Attanasio and Davis (1996) are two highly influential empirical papers showing that idiosyncratic labor income uncertainty is not fully insurable.¹ Second, exposure to labor income risk must be affected by the accumulation of education. If acquiring education reduces exposure to labor income uncertainty then education will be more valuable than its impact on mean income alone would indicate. Some evidence suggests that education has a stronger negative relationship with unemployment for blacks than for whites, suggesting that accounting for labor income uncertainty may make education differentially more valuable for blacks if these employment patterns affect uncertainty about labor income.²

This chapter uses data on black and white males from the Panel Study of Income Dynamics (PSID) to measure whether the education-labor income volatility gradient is

¹ While racial differences in the impact of idiosyncratic labor income risk on consumption have not been studied, it seems likely that it is more difficult for blacks to insure consumption than whites given the stark racial differences in wealth.

² Neal (2005) finds that the slope of the relationship between education and unemployment is more negative for blacks than for whites. Bound and Freedman (1992) find that during the 1980s black high school dropouts experienced more severe employment shocks than more-skilled blacks did. In terms of incarceration the estimates presented in Morretti and Lochner (2004) find that the point estimates of the impact of education on incarceration are larger for blacks. But because the underlying average levels of incarceration are much higher for blacks it does not seem like blacks experience larger impacts of education on the probability of being incarcerated.

different by race. I measure the impact of education on the variance of transitory shocks to total labor and social insurance income for both whites and blacks. The central finding is that education reduces labor income volatility more for blacks than for whites. Black college graduates reduce labor income volatility by more than 65 percent relative to high school dropouts, whereas whites receive no statistically significant reduction. I show that these results are robust to addressing the endogeneity of education with respect to both preferences for risk and ability, and including measuring labor income in ways that take account of labor force participation, social insurance and taxation. In a separate analysis of racial differences in the demand for education I find that risk aversion is positively related to education for blacks, but not for whites. Taken together these results are consistent with the notion that education reduces labor income volatility more for blacks than for whites and precautionary motives are quantitatively important in determining educational investments.

The central motivation for this study is to understand whether the riskiness of the returns to educational investments influences individual educational investment decisions. Recently, a number of empirical papers have carefully estimated the riskiness of returns to education.³ The results presented show that education is at least a somewhat risky investment. That is, education does affect an individual's labor income uncertainty. This reduction in labor income uncertainty will influence educational investment decisions if precautionary demand for education is important. There has been a large and influential macro literature showing that precautionary motives are important in

³ See Heckman, Smith and Clements (1997), Lochner and Monge-Naranjo (2002), Charles and Louh (2003), Carneiro, Hansen and Heckman (2003), Palacios-Huerta (2003), Chen (2004), Heckman, Lochner and Todd (2005), Sykt Nielsen and Vissing-Jorgensen (2005) and, Gallipoli, Meghir, and Violante (2005).

explaining financial wealth holding.⁴ Those who expect more labor income risk hold more financial wealth. However, there is no evidence of a precautionary motive in the demand for education at this point.

Previous work on the riskiness of educational investments has not tested for precautionary motives because they have not had the power to complete the test with the data sources that they use. This lack of power comes from two sources. First, these papers do not generally consider cross-section variation across individuals in the riskiness of human capital.⁵ In other words they do not measure who is supplied with educational labor income insurance. Second, they are unable estimate the relationship between preferences for risk and educational attainment. That is, they do not test whether individual demand for education is sensitive to educational labor income insurance.

To conduct a powerful test for precautionary demand for education we must know: (1) whose exposure to labor income volatility is affected by education (the supply of labor income insurance arising from education) and (2) whether risk averse individuals who can reduce their labor income volatility by acquiring more education in fact do so (that is, whether the demand for education is sensitive to labor income insurance). In my case both requirements are satisfied. Race is an individual endowment that is fixed over time, so I am able to estimate if there are racial differences in the supply of educational labor income insurance. Because I have access to a survey-based measure of preferences for risk for individuals in the PSID, I am able to examine if risk averse individuals who

⁴ See Carroll and Samwick (1997), Engen and Gruber (2001), Dynan, Skinner, Zeldes (2004) and Fuchs-Schundeln and Schundeln (2005).

⁵ Palacios-Huerta (2003), and Sykt Nielsen and Vissing-Jorgensen (2005) do think carefully about the cross-sectional distribution of human capital risk. However, they lack access to a risk preference measures needed to test to see if human capital risk actually influences the demand for education.

are likely to reduce their exposure to labor income risk by acquiring education (based on their race) do in fact obtain more education.

Education may affect two sources of labor income uncertainty differentially by race. Uncertainty about shocks to an individual's actual productivity in a given period may be affected by education. Education could have a causal effect on the probability that an individual becomes disabled for example. The tasks that less-skilled individuals do in their jobs are more physically demanding, and more likely to result in a significant injury. The benefits of education in reducing the labor supplied to jobs with significant injury risk may well differ by race. Other sources of these shocks could be incarceration or shocks to productivity in sectors where employment is disproportionately concentrated in a certain race or skill group. I discuss the evidence on the relationship between education and productivity shocks in more detail in what follows.

The other source of labor income uncertainty is due to imperfect information in the labor market. When a worker is initially hired at job they will be paid according to their expected marginal product based upon their observable characteristics. As employers observe the output of a given worker over time they are able to learn about the true ability of the worker and change their wage accordingly. When all workers are also exposed to productivity shocks the employer faces a signal extraction process in determining whether a observed change in the output of a worker provides information about the workers productivity or only reflects the realization of a productivity shock. The optimal signal extraction process that the employer will utilize depends on how much variance there is in terms of ability and the shock process in the observable group. The more heterogeneous a group is in terms of ability, the more valuable the information

contained in an output realization is. If heterogeneity in ability falls with education for blacks, revisions to their wages will be less related to output realizations. Thus their wages will be less related to actual productivity shocks and their labor income will be less volatile.

The chapter unfolds as follows. The next section introduces a simple model of the demand for education when labor income volatility is uninsurable. Section 1.3 outlines the data to be used. In Section 1.4 racial differences in the relationship between education and labor income volatility are examined. Section 1.5 studies whether the relationship between risk preferences and education differs by race. Section 1.6 concludes.

1.2 Labor Income Uncertainty and Investment in Education

To fix ideas, I present a stylized model of investment in education where markets for labor income risk sharing are incomplete in this section. When markets for labor income risk sharing are incomplete, education will have additional benefits beyond the effect on permanent income if education reduces the volatility of the labor income process. The model shows that the insurance elasticity of demand is positively related to risk aversion.

The model is not strictly comparable to Mincerian model of education but does maintain some of the standard elements of this framework. Education increases the mean level of permanent income individuals receive each period. Education is also costly to acquire. Individuals from more disadvantaged family backgrounds and those with lower levels of previously acquired skills face larger non-pecuniary costs of acquiring education. The model makes a number of assumptions necessary for analytic tractability,

which force departure from the Mincerian framework. Individuals have no access to capital markets or storage technologies.⁶ I also assume that education takes no time to acquire, so that there is no opportunity cost of educational investment, and that investments in education are irreversible.

1.2.1 An Illustrative Model

I now consider how this insurance value of education for blacks will influence the demand for education when markets for risk sharing are incomplete. Consider an individual making an irreversible decision about which of two income streams to select.⁷ The expected utility of a given income stream depends on the level of permanent income, the volatility of the income stream, and the individual's preferences for risk. Each income stream has a variance and a level of permanent income that are constant over time.

The individual chooses between two different discrete levels of education, H and L , in order to maximize his expected utility at time zero. Thus, the value of education is

$$V_{ED} = E_0V_H - E_0V_L - NPC, \quad (1.1)$$

where NPC is the non-pecuniary cost of acquiring education. Individuals accumulate education level H if $E_0V_H - E_0V_L \geq NPC$. There is no discounting, there are two periods.

The period utility function is of the Constant Absolute Risk Aversion (CARA) family and

⁶ This stark assumption is useful to model the implications of uninsurable labor income risk. The price of this assumption is that the link between the interest rate and marginal investments in education, which is a prominent feature of the Mincerian model, is lost. This is however a common assumption used to maintain tractability in models of uninsurable labor income risk (see Benabou (2002), Carnerio, Hansen and Heckman (2003) and Bertola (2004)).

⁷ The basic structure of the model is based largely on Cabellero (1991). The model is also similar in spirit to that in Charles and Louh (2003).

is given by: $\frac{-1}{\theta} e^{-\alpha c_t}$, where θ is the coefficient of absolute risk aversion. The individual faces an i.i.d. stochastic stream of labor income y_t that is log-normally distributed with a mean Y and a variance σ^2 . Because there is no storage technology the individual sets c_t equal to y_t in each period. Thus, the expected utility of the income stream (exploiting the log-normality of the stochastic labor income process) is given by:

$$E_0 V = 2E_0 u(y_t) = 2 \left(\frac{-1}{\theta} \right) \left(-\theta Y + \frac{\theta^2 \sigma^2}{2} \right) = 2Y - \theta \sigma^2. \quad (1.2)$$

Substituting (1.2) into (1.1) for each educational level the expected annual value of education is

$$\frac{V_{ED}}{2} = \underbrace{(Y_H - Y_L)}_{\Delta_{ED}} - \frac{NPC}{2} + \frac{\theta}{2} \underbrace{(\sigma_L^2 - \sigma_H^2)}_{\Psi_{ED}}. \quad (1.3)$$

This equation decomposes the annualized value of education into three terms. The first term contains the gain in expected permanent labor income due to education. The second term reflects the annualized non-pecuniary cost of education. The third term in (1.3) reflects the insurance value of education. If Ψ_{ED} is *positive*, education reduces exposure to labor income volatility. However, if Ψ_{ED} is *negative*, then education increases exposure to labor income volatility. Ψ_{ED} enters multiplicatively in θ , the degree of risk aversion, which affects the value of this insurance for a given individual. If education reduces exposure to labor income volatility, then the most risk averse individuals will place the highest value on this component. These individuals will demand the most education if it provides labor income insurance.

In addition, equation (1.3) shows why it is important to consider an individual's risk preferences when estimating the relationship between education and the observed

volatility of labor income.⁸ Individuals who really dislike risk will, on the margin, be willing to pay more to obtain education if it provides labor income insurance. These individuals may also make other labor market decisions to reduce labor income volatility (perhaps based on the compensation structure of a job or the likelihood of involuntary unemployment) that are unobservable to the econometrician. Thus, any test for the impact of education on labor income volatility must control for risk preferences.⁹ This is a key concern in testing for precautionary savings motives for the holding of financial wealth, as noted by Fuchs-Schundeln and Schundeln (2005).

Since the model does not allow individuals either to self-insure against volatility by the accumulation of financial wealth or to borrow in bad times it is important to be clear about what this implies for how the structure of the labor income process will be related to educational decisions. With this set up the distinction between permanent and transitory shocks to labor income in terms of the welfare lost due to volatility is not as important. The demand for education will not be a function of the composition of the shock process in terms of persistent or transitory shocks as long as the total amount of volatility is not affected. However, if individuals are able to accumulate buffer stocks or borrow this would not be the case. In this case transitory shocks to labor income are

⁸ Consider another application of (3). Instead of an individual choosing between two levels of education, the individual chooses between two occupations. Each occupation has a different expected income level and volatility of earnings, and acquiring the skills for occupational qualification is costly. Rearranging (1.3), an individual will change occupations from occupation θ to occupation l iff,

$$\sigma_0^2 \leq \left(\frac{2}{\theta}\right) \left(\Delta Y_{10} - \frac{C_{10}}{2}\right) + \sigma_0^2. \quad (1.4)$$

In equation (1.4) variance σ_j^2 is the volatility of earnings in occupation j , and ΔY_{10} is the gain in permanent income, and C_{10} is the cost of the occupation change from occupation θ to occupation l . The more an individual dislikes risk the more likely he will select into occupations with lower earnings volatility (job 1 here) all else equal.

⁹ If education and preferences for risk were perfectly correlated however it would be impossible to separately identify the impact of education and preferences for risk on labor income volatility.

much easier to insure than permanent shocks, so that the composition of the shock process will affect education demand. Labor income volatility arising from permanent shocks to labor income would affect the demand for education more than uncertainty about transitory shocks. Because the model does not allow for asset accumulation or borrowing I will focus on the composition of the shock process, and instead focus only on transitory shocks to labor income.

While this highly stylized model is primarily designed to illustrate how racial differences in the insurance value of education will affect education demand it can still match the basic facts concerning racial differences in education. When the cost of acquiring education is much larger for blacks, perhaps due to coming from a more disadvantaged background on average (as previous work by Neal and Johnson (1996) and Cameron and Heckman (2001) has found), the model can explain why blacks acquire less education on average. In addition, the model can explain why conditional on family background blacks acquire more education than whites, even though the measured average internal rate of return to education is roughly the same by race (as previous work by Rivkin (1995), Black and Sufi (2002), and Lang and Manove (2005) has found). Because blacks receive an insurance benefit of education, they will acquire more education holding costs and the internal rate of return fixed. The interpretation of this fact as a puzzle depends on whether the average measured internal rate of return to education is the relevant return to education for black and white individuals conditional on their test scores. Some of the non-parametric estimates of the rate of return to high school completion presented in Heckman, Lochner and Todd (2005) indicate that black

human capital is more valuable than white human capital, which would also resolve this puzzle.

1.2.2 Racial Differences In the Impact of Education on Labor Income Volatility

If we consider individuals of different races as facing race-specific insurance values of education (i.e. Ψ_{ED} differs by race) equation (1.3) formalizes the intuition noted in the introduction. For members of a race who are supplied with more educational labor income insurance, risk aversion will be more positively related to the demand for education. So if education reduces labor income uncertainty more for blacks than for whites, relationship between education and risk aversion will be more positive for blacks.

Thus far I have discussed how racial differences in the insurance value of education would affect the demand for education. This begs the question, why there would be racial differences in the relationship between education and labor income volatility at all? As noted in the introduction there are two potential sources for racial differences in the effect of education on labor income volatility. Education may affect uncertainty about shocks to a perceived productivity or to a workers actual productivity.

When there is imperfect information in the labor market about the productivity of workers employer learning about the ability of heterogeneous workers will generate labor income volatility. This informational problem would generate racial differences in the insurance value of education if employers faced more uncertainty about the ability of less-skilled black relative to more-skilled black workers more so than for whites. If the underlying distribution of ability does not differ by race the cutoff ability level in the distribution to obtain a given level of education will be higher for blacks because they

face a larger fixed cost of acquiring education. The dispersion in abilities will then be greatest for blacks with the lowest level of education, so that employers will face the greatest uncertainty about the ability of blacks with the lowest level of education. In a model of statistical discrimination with learning as discussed in Altonji and Pierret (2001) this would lead employers to place more weight on observed productivity (and less on their prior expected productivity of workers of that group) for less-skilled compared to more-skilled black workers in their optimal compensation decisions.¹⁰ For white workers however, employers would adopt more similar weights for less- and more-skilled workers. When the output of all workers is stochastic, the wages of less-skilled black workers will be more closely tied to the stochastic component of productivity, exposing them to more labor income risk. Thus, black workers reduce the uncertainty that they face about labor income through skill acquisition because their wages are less closely tied to the stochastic component of their productivity. Whites, on the other hand, would face compensation schemes that depend less on skill.

Ethnographic evidence indicates that the uncertainty that employers face about the productivity of blacks falls with education. Kirchenman and Neckerman (1991) interviewed 165 employers in Chicago in the late 1980's. They find the most important criteria that employers give for hiring low-skilled blacks is dependability. In contrast, when hiring blacks for clerical jobs (the highest skill category they examine) the most important criteria employers give is the ability to deal with the public. Kirchenman and Neckerman go on to describe employer perceptions of the lack of dependability of less-skilled blacks in the following way:

¹⁰ This is a somewhat different implication than we would typically obtain in a more standard statistical discrimination model without learning (see Aigner and Cain (1977), Coate and Loury (1993), Autor and Scarborough (2005), and Lang and Manove (2005)).

"What is crucial in these jobs is dependability: "Every day coming to work on time." Common complaints about low skilled workers focused on those who were hired and never showed up, or quit without warning. Respondents tended to use such terms as "stability", "dependability", "good work history" and "attendance record" interchangeably, ..."

Kirshenman and Neckerman (1991), pp. 225.

Lang and Manove (2005) find that education is more valuable signal of ability for blacks than for whites. They show that in the National Longitudinal Survey of Youth data that blacks with intermediate ability in terms of AFQT obtain more education than either whites with the same ability, or than blacks with higher or lower ability levels. These results are consistent with the idea that employers are more uncertain about the productivity of less skilled blacks.

The other class of mechanism to consider that generate racial differences in the education-labor income volatility gradient involve uncertainty about true productivity shocks. Less-skilled blacks could be more uncertain about employment if for example the demand for their labor is concentrated in industries that face more uncertainty about product demand or they are subject to more uncertainty about disability or incarceration shocks. There is some evidence in support of this view. Altonji and Blank (1999) show that black employment is concentrated in industries that have volatile employment such as manufacturing. In addition Bound and Freedman (1992) show that racial differences in the regional and industrial concentrations of employment was one of the key features to lead to the decline of in the employment of less-skilled blacks in the 1980s. Shocks to non-employment such as, incarceration and disability, are very different across education

and racial groups. Bound, Schoenbaum and Waldman (1995) find that both blacks and the less educated are the most likely to be disabled. This suggests that uncertainty about disability shocks falls with skill level more for blacks than whites. The evidence in Lochner and Moretti (2004) indicates the education-probability of incarceration gradient is not larger for blacks. This suggests that incarceration would not lead to any racial differences in the insurance value of education. Thus, there is some evidence that there could be important racial differences in the relationship between education and uncertainty about employment and disability shocks.

1.3 Data

This study is conducted using sixteen years (from 1977 to 1992) of data from the Panel Study of Income Dynamics (PSID).¹¹ The sample I use initially contains annual observations on a panel of five thousand individuals. A large number of black individuals are in this data set because it has a poverty oversample (the Survey of Economic Opportunity (SEO) sub-sample). This sub-sample has two thousand observations and a large fraction of black respondents. I use the PSID individual sampling weights for all estimates to make the estimates representative of the U.S. population.¹² The variables I take from the PSID are race, education, annual labor income, annual social insurance income (unemployment insurance and workers' compensation for the household head only, and total dollars of food stamp receipt, SSI

¹¹ I use these years of data because of the major change in the data cleaning procedures that the PSID implemented in the 1994 survey year. Note that I index my sample years not by the survey year, but by the year which the labor income report corresponds to, which is one year prior to the survey year.

¹² The individual sampling weights for each year of data are used to generate the labor income volatility estimates. The cross-sectional analysis looking at the relationship between labor income volatility and education, as well as, preferences for risk and education uses only the individual sample weights in the cross-sectional year to estimate the models.

income, and AFDC income for the household), annual hours worked, number of weeks unemployed, household head's marital status, the number of household members, parents education wealth, preferences for risk, and the region of the country the household is located in.

I include social insurance income in my measures of total labor income for two reasons. I wish to consider labor market decisions in the context of the social insurance mechanisms that are currently in place. Dyanarski and Gruber (1997) find these mechanisms to be a significant source of insurance for labor income volatility. In addition, I am able to include observations with some social insurance income but zero labor income in my sample because the log of labor plus social insurance income variable is defined for these observations. The low labor force participation of less-skilled blacks may well reflect an important component of labor income uncertainty. Note that when I discuss labor income from this point forward I will be referring to labor plus social insurance income. I also use a measure of disposable labor income by subtracting federal income tax liabilities. I use TAXSIM to simulate the federal tax liability for each household head in each year using total annual labor income as wage income and declaring all households to be single filers.¹³ I measure education using categorical variables that divide individuals into one of four groups: high school drop out, high school graduate, some college attendance and college graduate. I use these categories because in some years of the data years of education is only available as a bracketed variable.

In addition, survey-based measures of risk preferences were collected for individuals in the PSID in 1996. Because of the panel structure of the dataset, I can link

¹³ The exact definitions of each of the key dependent variables are given in the data appendix.

these measures to the sample from earlier years. These types of measures were first used in the Health and Retirement Study and first analyzed in Barsky et al. (1997). Similar measures have been found to be correlated with wealth accumulation (see Charles and Hurst (2003)) and savings (see Mazzocco (2004)). To construct these measures a total of five questions concerning lotteries of labor income were asked of employed respondents.¹⁴ Depending on which gambles were accepted and which were rejected the respondent was assigned a coefficient of relative risk aversion. The PSID creates two risk aversion measures based on the responses to these questions. The second of these two adjusts the responses using the procedure in Barsky et al. (1997) to minimize measurement error due to incomplete responses and this is the risk aversion measure I use throughout.

Unfortunately, there is an important concern with the timing of the PSID risk preference measure which makes testing for precautionary demand for education problematic. Risk preference is measured after individuals have made their education decisions and experienced labor income volatility. If risk-aversion is a time-invariant individual characteristic then we can consider this measure of risk aversion as indicating the individual's preferences for risk at the time educational decisions are made. However, recent work by Brunneimeier (2004) indicates that individuals will have a decreasing sensitivity to losses as they experience more losses. This would imply that as individuals experience more negative labor income shocks they become less risk averse. If education reduces the probability of negative labor income shocks, and this reduction leads individuals to become less risk averse then we would observe that risk aversion and education are negatively correlated. This negative correlation would solely be due

¹⁴ See the data appendix for an example question.

changes in risk preferences over the sample period, but would be observationally equivalent to precautionary demand for education. There is little direct evidence on the diminishing sensitivity to losses and the impact on risk aversion. Brunnermeier and Nagel (2005) examine how individuals reallocate their portfolio in response to wealth shocks. To test for diminishing sensitivity they look at whether an individual reallocates their portfolio so that they are subject to more risk after experiencing a wealth shock. They find no evidence that this is the case.

Another issue which I face is that individuals make education based upon their unobserved ability which may also be related to labor income volatility. To address the correlation of education with unobserved ability I utilize data on compulsory schooling laws as used by Lochner and Moretti (2004). These laws were originally coded by Acemoglu and Angrist (2000) and reflect the maximum of: (1) the minimum number of years that the child is required to stay in school, and (2) the difference between the earliest and latest age that the child is required to enroll.¹⁵

The sample exclusion rules I impose are the following.¹⁶ I only include household heads present in the dataset for at least nine years, since many of the required variables are only measured for these individuals and to obtain more precise estimates of labor income volatility for households I require a reasonable amount of time-series data. I drop female household heads because gender differences in labor supply behavior are beyond the scope of this chapter. I drop those who are not between 22 and 45 years of

¹⁵ There has been some concern that this coding of laws do not reflect the actual constraints imposed by compulsory schooling laws (see the discussion in Goldin and Katz (2003)). However, the alternative compulsory schooling laws utilized in Goldin and Katz do not cover the individuals in my sample period (since they are born between the years 1932 and 1955, and are age 14 in 1946 to 1969) so I use the Acemoglu and Angrist (2000) coding here.

¹⁶ See the data appendix for the exact number of observations lost from each sample restriction.

age in 1977, so that the entire sample is of prime working age for the length of the panel. Also, I treat top-coded labor income as a missing value.¹⁷ It is important to note that I do not condition on labor force status in my sample selection because being in a state of non-employment is likely to represent a significant source of labor income volatility.

The means of the key variables for this study in the first and last years of the sample are displayed in Table 1.A1. There are two important concerns to note about the sample for the analysis that will be conducted. First, there are a number of observations with zero annual labor income and zero annual social insurance income in a given year. For these observations the log of annual labor and social insurance income will be undefined. Overall, 2.7 percent of the person-year observations in the sample have a zero labor and social insurance income. For high school dropout blacks, 10.7 percent of the person-year observations have a zero value. Since the PSID data cleaning procedures assign a value for labor income for all members of the sample, a zero observation may reflect either a year with no labor or social insurance income, or a year where labor and social insurance income are not recorded. In this first case, the zero values will reflect true labor income and should be included in the sample when measuring the degree of labor income uncertainty individuals have experienced. In the second case, however, including the zeros in the sample treats missing values as significant shocks to labor and social insurance income. In this case, including the zero observations will add noise to my measure of labor income volatility which is unrelated to labor income uncertainty implying that my labor income volatility estimates will be biased away from zero. I

¹⁷ I lose only 0.11 percent of the person-year observations from this restriction, all of whom are highly educated whites. Those with continuously top coded labor income values would have no measured variance in their labor income even though they may in fact experience significant labor income volatility.

address this concern in the analysis by estimating the models with and without these zero observations. The results are not sensitive to their inclusion.

The second important concern is that there is differential attrition from the panel by race and education. As can be seen in Table 1.A2, high school dropout blacks are the most likely to attrit from the sample. Since those who attrit from panels are likely those with the least stable life circumstances this would imply that I will understate the true degree of labor income volatility particularly for black high school dropouts.¹⁸

Systematically understating the true degree labor income volatility faced by less-skilled blacks may bias my estimates of the insurance value of education. The direction of the bias depends on whether the education-labor volatility gradient for those who attrit the sample is greater or less than those who do not. I attempt to address this issue by controlling for the number of years (minus nine) that an individual appears in the sample. The fact that the results are not sensitive to the inclusion of the number of years in the sample dummy variables as covariates implies that differential attrition is not likely to be driving the results.

1.4 Racial Differences In Education and Labor Income Volatility

In this section I describe the method used to measure labor income volatility and the impact of education on labor income volatility. The idea is that deviations from the lifecycle permanent profile represent transitory shocks to labor income. The measure used is the variance of transitory labor income shocks. This measure of labor income volatility is similar to that used in Dynarski and Gruber (1997). In a world without

¹⁸ See Fitzgerald, Gotteschalk and Moffitt (1998) for a study of attrition in the PSID.

financial wealth holding, transitory labor income volatility will accurately reflect the welfare loss due to labor income volatility.¹⁹

The analysis follows two steps. First, for each individual in the sample the variance of transitory labor income volatility is estimated using the panel of 16 observations from 1977 to 1992. Then I relate these estimated labor income volatilities to the education and race of the individual in the 1977 cross-section (the first year of the sample). The central parameter I seek to estimate is the impact of education on exposure to labor income volatility for blacks and whites.

1.4.1 Measuring Labor Income Volatility

Following Dynarski and Gruber (1997) I use a simple procedure to measure transitory labor income volatility. I take the third difference of log labor income²⁰ and regress this on a quartic in age and a full set of interactions of the age quartic with race and the three

¹⁹ I have conducted an alternative variance decomposition of the type utilized by Carroll and Samwick (1997) to look at the impact of education on uncertainty about both permanent and transitory shocks by race. The results of these estimates are that permanent labor income volatility is not related to education for either blacks or whites, and that transitory labor income volatility falls more with education for blacks than for whites. Unfortunately, this decomposition procedure does not restrict the permanent and transitory estimates to be positive and results in a large number of negative variance estimates. Over sixty percent of the sample has negative permanent or transitory variance estimates. The large number of negative variance estimates seems to indicate that the variance decomposition specification is rejected by the data (perhaps due to the affect of measurement error in labor income being correlated with the number of observations used for the differences or a correlation between permanent and transitory shocks). For this reason I do not report these results as the central estimates even though they are qualitatively the same as the results that I do report.

²⁰ This method does explicitly allow for autocorrelation in the transitory shocks to current labor income. A number of researchers (McCurdy (1982), Abowd and Card (1989) and, Meghir and Pistaferri (2004)) have found this autocorrelation to follow a second order moving average process in the PSID. Thus, I allow transitory labor income shocks to follow an MA(2) process, which means that I estimate the model for the third difference in labor income. I have also estimated all of the models below using the first difference, as in Dynarski and Gruber (1997), and the fifth difference, as in Gottschalk and Moffitt (1994). The results are not sensitive to these different assumptions about the order of the autocorrelation.

educational categories greater than high school dropout, as well as year fixed effects.²¹

This allows lifecycle variation in permanent income to differ by race and education, and allows for year-specific shocks to permanent income. I take the square of the residuals from the estimation of this equation and divide by two to get an individual specific estimate of the amount of transitory labor income volatility. The exact details of the estimation procedure are provided in the data appendix.

1.4.2 The Impact of Education on Labor Income Volatility

With estimates of transitory labor income volatility for each individual in the sample in hand I test for racial differences in the education-labor income volatility gradient in the following fashion. I begin by using the 1977 cross-section to define the population (using labor income volatility estimated using data from 1977 to 1992). I first look at the differences in the mean of estimated labor income volatility by race and by education within race. I then regress the estimated labor income volatility on individual characteristics separately by race, so that I can control for other determinants of the observed labor income volatility which may be correlated with education.

More formally, I estimate the equation

$$\sigma_i^2 = \beta_1 hsgrad_i + \beta_2 somecollege_i + \beta_3 collegegrad_i + \beta_4 RA_i + X_i \Lambda + u_i, \quad (1.5)$$

by race in the 1977 cross-section. The estimate of β_1 is the conditional effect of being a high school graduate relative to a high school dropout on transitory labor income

²¹ This is similar to the specification used in Gottschalk and Moffitt (1994) with race and education interactions added. I find that there is no differential income growth relationship with age by race but that the lifecycle profile of earnings does differ by education group. I have also estimated the models with only an age quartic (no interactions) and year fixed effects, and obtained very similar results.

volatility. The parameters β_1 , β_2 , and β_3 are the central estimates of interest. A negative estimate for any of these coefficients implies that higher levels of education reduce labor income volatility. The coefficient β_4 is the estimate of the impact of risk aversion category on the labor income volatility. It is expected that β_4 will be negative, i.e. that those who are most risk averse will experience the least labor income volatility.

In my first specification the matrix X_i contains covariates such as age, age squared, a missing risk preference dummy variable and seven dummies indicating whether the individual spends more than nine years in the sample.²² As noted in the model above risk averse individuals may self-select into education if it reduces labor income volatility. Risk aversion may also have an independent effect on the volatility of labor income because risk averse individuals are able to make other labor market decisions which can reduce labor income volatility. By including risk aversion as a covariate I am able to examine the relationship between education and labor income volatility treating the risk preference composition of the education group as fixed. Risk aversion and education are not perfectly correlated as long as there are other determinants of education which vary across individuals within race, such as discount rates. In my second specification the matrix X_i also contains two dummies for marital status, the size of the household and region. The advantage of adding these covariates is that I am able to control for a number of characteristics of individuals which are likely related to both education and labor income volatility in different ways by race. For example, because less-skilled black males typically marry black females with more stable labor income processes marriage may represent an important source of diversification. However, these

²² The results are not sensitive to the inclusion of this set of dummy variables in the specification.

additional covariates may also be endogenous with respect to education, making the resulting parameter estimates of β_1 , β_2 , and β_3 biased.

A major issue to confront in the estimation of the transitory variance of labor income is measurement error in labor income. If there is a significant measurement error in the first difference of labor income this will increase the estimated standard errors in equation (1.5). Measurement error in labor income could make it difficult to find a significant impact of education on risk even if such an impact exists. In the case of the PSID we have a good idea of how much measurement error there is and what form it takes. Bound et al. (1994) examine the PSID validation study data which matches survey reports to employer reports of annual earnings and hours for two waves data in the PSID. They are able to examine how changes in annual earnings, and hourly earnings, differ between employer and self-reported measures. Approximately twenty-five percent of the cross-sectional variance in the first difference of annual labor earnings is due to measurement error. Because the measure that I employ to measure the volatility is the third difference in earnings I see this as a reasonable approximation for how much measurement error exists in my measure. Then is some concern that my standard errors will be inflated by the additional noise in labor income. In addition, Bound et al. (1994) also find that hourly earnings, as measured by annual labor income divided by annual hours worked, is measured more poorly than annual income. Because I utilize annual labor income per hour to measure an individual's annual average wage, wage uncertainty will be measured less precisely than uncertainty annual labor income uncertainty in my analysis.

A larger concern with measurement error in this context is that it is non-classical and correlated with an individual's education or race. Fortunately the evidence in Bound and Krueger (1991) indicates that measurement error in annual labor income appears to be uncorrelated with race and education. They examine a cross-sectional sample from Current Population Survey which is matched to employer-reported social security earnings data. While they do find a significant amount of measurement error in terms of the mismatch between the annual earnings reported in the survey and by the employer, it appears to be uncorrelated with individual observable characteristics. If this were not true we would have to be concerned that the non-classical measurement error in labor income would bias the estimates of β_1 , β_2 , and β_3 differently by race.

The correlation between unmeasured ability and schooling may also create bias in my estimates of β_1 , β_2 , and β_3 . The direction of the OLS bias could plausibly go in either direction.²³ It could be true that those with higher levels of ability would be less subject to labor income volatility as they are less likely to have their employment relationships end. Conversely, it could be true that those with higher ability self-select into jobs with high-powered incentives, and so their wages are more exposed to productivity risk than the wages of those with lower ability. To address this issue I follow Lochner and Moretti (2004) and use compulsory schooling laws at age 14 in an individual's state of birth as excluded instruments for high school graduation to estimate (1.5) (with education defined as a binary categorical variable of high school graduation or more) by instrumental variables (IV).

²³ See Cunha, Heckman and Navarro (2005) and Chen (2004) for discussions of this issue in a similar context.

This IV specification will identify the Local Average Treatment Effect (LATE) of high school graduation on labor income volatility. In a world where the effects of education on labor income volatility are heterogeneous, the LATE estimate is an average of the effect of high school graduation on labor income volatility among those who are induced to change high school graduation status because of the instrument. If those who change their educational attainment in response to this instrument are representative of the overall population in terms of the education-labor income volatility gradient then this LATE parameter will be close to the population average treatment effect (ATE). If this is not true, then the LATE estimate could be different than the population ATE.

1.5 Results

In this section I discuss the results of the estimation procedure described above. This procedure yields estimates of the transitory labor income volatility for each individual in the sample. Table 1.1 provides a first look at how labor income volatility, estimated using data from 1977 to 1992, is related to race and education. The values presented in the table are the means of the estimated labor income volatility for each subgroup in the 1977 cross-section. Also presented in the table are standard errors and F-statistics for the hypothesis test that there are no differences in labor income volatility by subgroup.

1.5.1 Basic Results

At the top of Table 1.1 we see that blacks experience about 70 percent more labor income volatility than whites.²⁴ This difference is statistically significantly different from zero at the five percent level. This result that blacks experience more labor income volatility than whites is consistent with previous work. The tabulations reported in Altonji and Blank (1999) show that blacks are much more likely to be non-employed than whites and the results reported in Farber (1993) and Fairlie and Kletzer (1998) indicate that blacks are more likely to be displaced. This result is somewhat different than that found by Datcher-Loury (1986), who finds that blacks experience only slightly more labor income volatility.²⁵

The next two sets of results in Table 1.1 examine whether there are differences in the amount of labor income volatility experienced by individuals with different levels of education within race. For whites we can see that average labor income volatility is higher for dropouts than the other categories. The difference across education categories

²⁴ These results do not include those observations with zero labor and social insurance in the sample. The results including these observations are presented in the sensitivity analysis.

²⁵ Using PSID data on black and white household heads aged 24-35 from 1974 to 1977 Datcher-Loury finds only a small difference in the stability of high income levels between blacks and whites. Her central results indicate that 44 percent of blacks with annual labor income above \$20,000 in every year from 1974 to 1976 are above this threshold in 1977. For whites, her results indicate 55 percent have annual labor income above this threshold conditional on being above the threshold in all of the previous years. Her work suggests that blacks experience more labor income volatility than whites but the differences are modest. She does not provide a statistical test for the differences in this parameter by race, so it is difficult to say whether her estimates indicate whether or not this difference is statistically different from zero. There are numerous differences between her study and the present analysis. I use more years of data which allows me to obtain more precise estimates. In addition, I am using a different sample which includes low income blacks. By examining only high income blacks she is likely to understate the true degree of labor income volatility that blacks face if black individuals often experience large shocks which lead them to not be in her high-income sample. If the fixed component of labor income is negatively correlated with the variance she will obtain estimates of labor income volatility which are biased downward for blacks. Most importantly her chosen measure reflects only one particular component of the volatility in labor income, whereas the measure employed here reflects all of the transitory volatility in labor income.

is not statistically different from zero at the five percent level however. For blacks, however, education is associated with a statistically significant decrease in labor income volatility. These results indicate that blacks are receiving a larger amount of income insurance from education than whites. However, as noted above these correlations may not be causal if high school dropout blacks are less risk averse than more educated blacks and risk aversion has an effect on labor income volatility. If more risk averse blacks chose to acquire more education, and risk aversion is negatively correlated with labor income volatility these results could be attributable to the composition of the education groups in terms of their risk preferences. The results in Table 1.1 could be explained because less-skilled blacks are less risk averse than more-skilled blacks, without education actually having a causal impact on labor income volatility at all.

Table 1.2 presents the estimates from the OLS estimation of equation (5). This equation is estimated separately by race for the 1977 cross-section. Each column in the table presents the estimates from one specification. Model (1) includes only age, age squared and seven dummies for spending more than 9 years in the sample as additional covariates.²⁶ Model (2) adds three dummies for marital status, the number of household members and dummies for census regions. The omitted educational category is high school dropout. Because these specifications control for the risk preferences they address the concern that the basic correlations are not causal due to selection into education based on preferences for risk.

The point estimates in the first two columns of Table 1.2 indicate that while high school graduation, some college, and college graduation are negatively associated with

²⁶ These dummies are intended to address the fact that the less-skilled, and less-skilled blacks especially, are more likely to leave the sample. The results are not sensitive to their inclusion as covariates.

labor income volatility for whites, the coefficient estimates are not significantly different from zero at the 5 percent level. This result is consistent with the findings of Carroll and Samwick (1997) who find that education is not related to transitory labor income volatility in a statistically significant way looking at a population of white males in the PSID.

Another interesting result in the first two columns of Table 1.2 is that the point estimate of the impact of risk aversion is negative and statistically different from zero at the 5 percent level. This suggests that individuals are making decisions about which jobs to select based on their preferences for risk and expectations about which jobs are likely to be risky. We would expect that the most risk averse will choose jobs with less volatile labor income paths. This result validates the key concern underlying the precautionary savings test implemented by Fuchs-Schundeln and Schundeln (2005) that those who are more risk averse will choose less volatile income paths.

The central result in this chapter emerges in columns three and four of Table 1.2. The estimates show that high school and college graduation both reduce labor income volatility for blacks. The negative point estimates are significantly different from zero at the five percent level. The large magnitude of these estimates relative to whites is especially notable. They indicate that by becoming college graduates blacks reduce their exposure to labor income volatility by more than 90 percent (column 3) or more than 65 percent (column 4) relative to high school dropout blacks. These are large reductions in labor income volatility. The specifications in column four also control for marital status, household size and region. It is important to note that the coefficient estimates of the effect of high school graduation change very little when these controls are added.

The central results in the above tables are difficult to reconcile with those of Palacios-Huerta (2003). He finds that education is a more valuable asset for whites than for blacks. Using time-series wage data from the CPS for full-time employed individuals he applies the method of mean-variance spanning to value education assets. For racial differences, the method is based on taking the mean and variance of the return to education for an individual of a given race and asking whether adding the education of an individual of the other race would improve the mean return per unit risk of the original portfolio. He finds that a white individual would not want to add black education to their portfolio, and a black individual would want to add white education. This implies that black education is less valuable than white education.

There are a number of concerns with the method employed in Palacios-Huerta (2003). First, because he looks at time-series variation in the rate of return to education within experience-education-race cells, he is not measuring most of the uncertainty about labor income that individuals actually face. Individual-level uncertainty will be averaged away by his procedure. Second, on a conceptual level it is not possible for black individuals to ever hold white education. General education can only be owned by the individual who invested in it. Thus, while this framework is conceptually correct for decisions about the holding of various financial assets it breaks down when considering the holding of different individual's education. The mean-variance spanning procedure he uses is inconsistent with the fact the general human capital is non-tradable. Lastly, he only includes full-time employed individuals in his sample. Thus, he is measuring mostly wage risk, disregarding a potentially important source of labor income volatility: involuntary unemployment.

1.5.2 Sensitivity Analysis: Instrumental Variables Estimates

A concern with the results presented thus far is that the estimates showing the insurance value of education for blacks could be biased due to omitted ability. The direction of the bias could go in either direction. Individuals with higher levels of unobserved ability may be less likely to have their employment relationships end and experience less labor income risk. This would imply that the OLS estimates are upward biased. Conversely those with higher ability may select into jobs with high-powered incentives and be exposed to more labor income volatility. In this case the OLS estimates will be biased downward.

I address this concern by comparing OLS and IV estimates of the impact of high school graduation or more on labor income volatility in Table 1.3.²⁷ Panel A in Table 1.3 displays OLS estimates of the relationship between high school graduation or more (this variable subsumes all of the educational categories used in the previous analysis) and labor income volatility by race.²⁸ The results are qualitatively the same as in Table 1.2; blacks receive a statistically significant insurance benefit from education, whereas whites do not. In panel B of Table 1.3, I present IV estimates where high school graduation or more is instrumented with compulsory schooling laws as of age of 14 in the individual's state of birth.

²⁷ Note that the sample size in the Panel B of Table 3 is slightly smaller than in Panel A. This is due to the fact that a small number of individuals in the sample are foreign born or have missing values for state of birth. It is not possible match these individuals to the appropriate compulsory schooling law so they must be dropped from the sample for the Instrumental Variables specification.

²⁸ The reason to pool the education categories here is that compulsory school laws have the largest effect in terms of moving individuals out of the high school dropout education level into the high school graduate category. The first stage will be not be as strong for higher education levels.

The results in columns one and two show that although the point estimates of the impact of education on labor income volatility are positive for whites, they are not statistically different from zero at the 5 percent level. In columns three and four we see that the coefficient estimates are negative and statistically different from zero for blacks. These point estimates are very large, but they are not statistically different from the estimates presented in panel A. This suggests that correlation between education and ability does not lead to substantial bias in the OLS estimates in this context.

It is important to be precise in the interpretation of the parameter estimated in the IV specifications in panel B of Table 1.3. The IV estimates are LATE parameters, they measure the relationship between education and labor income volatility for those who are induced to change education status by the compulsory schooling law. When different individuals have different education-labor income volatility gradients some of the differences in between the OLS and IV estimates reflect the fact that they are based on different populations. The fact the IV point estimates for blacks are larger than the corresponding OLS estimates could be because the blacks for whom the compulsory schooling law binds have particularly large education-labor income volatility gradients.

That the F-statistics for joint test that the excluded instruments in the first-stage are zero indicate that there is a weak instrument concern here for whites. In the first specification the excluded instruments are jointly significantly different from zero in the first stage at the 5 percent level with an F-statistic of 3.07. When the additional controls are added the F-statistic drops to a value of 1.31 and indicates that the excluded instruments are statistically different from zero. This weak instrument problem is likely to be related to the fact that whites are more likely to attend and complete college, so that

the compulsory schooling laws may not be especially binding for whites. The IV estimates for whites are arguably inferior to the OLS estimates for this reason. The results in this table imply that the OLS estimates are preferred, so I now only present OLS estimates in what follows.

1.5.3 Sensitivity Analysis: Alternative Measures of Labor Income

Another concern with measuring labor income volatility is whether zero values for labor and social insurance income are due to measurement error or really measure true uncertainty. In the analysis thus far we have excluded these observations with zero income from the measure of transitory labor income volatility reported in Tables 1.1, 1.2 and 1.3. Because labor force participation rates are very low for less-skilled blacks, and labor force non-participation may represent a significant source of uncertainty, it is important to check the sensitivity of the results to excluding zero labor income observations.

In Table 1.4, I include observations with zero labor income by recoding zero labor and social insurance income to be equal to one, so that log of income is now defined for these observations. The estimates in columns one and two indicate there is no statistically significant relationship between education and labor income volatility for whites. In columns three and four, however, we see that the coefficient on college graduation is negative and statistically significantly for blacks.²⁹

²⁹ I have also estimated a probit model of whether income is zero or not with the same covariates as in specification (1) in Table 2 by race. I find that for individuals of either race both high school graduation and college graduation reduce the probability of a zero labor and social insurance income observation relative to a high school dropout. The marginal effect estimates on high school and college graduation are

Dynarski and Gruber (1997) show that one of the most important mechanisms for smoothing variable labor income is the progressive nature of the income tax schedule. This is true because the volatility in disposable income is less closely tied to volatility in earned labor income at higher marginal tax rates. Because the marginal tax rate falls as earned labor income falls, a negative shock to earned labor income may result in a fall in the marginal tax rate which provides some additional insurance for disposable income. In Table 1.5, I examine whether accounting for progressive income taxation significantly affects my results. I use TAXSIM to estimate the federal tax liability for each individual in my sample for each year based on their total labor income. I subtract this tax liability from the total of labor and social insurance income to construct the disposable income-based measure of risk reported in Table 1.5.³⁰ In contrast to the results above the estimates do indicate that white individuals receive a statistically significant reduction in labor income volatility from high school graduation, though only in the specification with limited covariates. For blacks we observe that college graduates receive a statistically significant decrease in labor income volatility in both specifications, and blacks with a high school diploma or some college are exposed to less volatility in the last specification. These results indicate that racial differences of the impact of high school graduation are somewhat sensitive to the inclusion of taxes in the measurement of labor income volatility. However it is important to note two points about the results in Table 1.5. First, college graduate blacks still receive a larger reduction in exposure to labor income volatility than college graduate whites. Second, all of the point estimates for the

more than four times larger for blacks than for whites indicating that role of education in reducing the probability of a zero labor plus social insurance income observation is larger for blacks.

³⁰ This measure does not contain recoded observations for zero total labor income as in Table 4. It is the same measure as reported in the specifications in Tables 1, 2, and 3 but net of federal tax liabilities.

impact of education on labor income volatility in Table 1.5 are more negative for blacks than for whites.

In Table 1.6 I examine whether the results are sensitive to the exclusion of social insurance income from the labor income measure. Redefining labor income in this way redefines the zero labor income observations. The sample is now smaller as those individuals who with zero labor income but some social insurance income are excluded from the sample. The results in Table 1.6 indicate that, for whites, acquiring any education level beyond dropping out of high school results in a statistically significant decrease in the amount of labor income volatility experienced. For blacks however, only college graduates experience a large statistically significant reduction in labor income volatility. The impact of college graduation on exposure to labor income volatility for blacks is statistically different and larger than the corresponding estimate for whites when we compare the results in columns one and three. The estimates in column two and four for the relationship between college graduation and labor income volatility are not statistically different, but the point estimate is larger for blacks.

1.5.4 Education and Sources of Labor Income Risk

I now look more closely at the determinants of labor income volatility and how education impacts two sources of labor income uncertainty: wage volatility, and volatility in weeks unemployed. For both sources of volatility I implement the same methodology as I conducted above for labor income volatility.³¹ I measure transitory wage volatility using

³¹ The procedure I follow is to first regress the third difference of wages or weeks unemployed on the age and, age*education. I then take the residuals from this regression, square them and divide by two to obtain my measure either wage or weeks unemployed volatility. See the data appendix for the precise details of this procedure.

the full panel from 1977 to 1992, and then examine how wage volatility differs by education for blacks and whites in the 1977 cross-section. Wages are measured as the ratio of annual labor income (excluding all social insurance income) to total annual hours worked. I also use the same procedure of taking out lifecycle variation and examining the variance of the third difference of weeks unemployed.³² This measure of employment uncertainty is used because there is likely be less measurement error than for hours worked.

As noted above, studying the impact of education on wage and employment volatility is a worthwhile exercise to better understand the mechanisms underlying the relationship between education and labor income risk. If the racial differences in the impact of education on labor income volatility are mostly driven by employment shock uncertainty then we would expect that volatility of weeks worked should fall more with education for blacks than for whites. Conversely if racial differences in the impact of education on labor income volatility stem mostly from employers being very uncertain of the productivity of black high school dropouts then we should see that the volatility of wages should fall more with education for blacks.

The results in Table 1.7 show that there are differences by race in volatility of wages and weeks unemployed. Blacks face significantly more uncertainty about weeks unemployed than whites. These differences by race are significant at the five percent level. While the point estimates indicate that blacks are exposed to more wage volatility than whites, these differences are not statistically significant. Thus, the results in Table 1.1 that blacks face more labor income risk seem to be driven largely by racial

³² This measure includes the total number of weeks that the respondent reports being unemployed or temporarily laid off, it does not include time out of the labor force.

differences in employment volatility. Interestingly, when we look at the relationship between education and the volatility of wages by race we see that only blacks have statistically significant differences across educational categories. The point estimates also indicate that the magnitude of the reduction in the volatility of wages is larger for blacks than for whites. White college graduates experience a statistically insignificant 20 percent reduction of the wage volatility relative to white high school dropouts. In contrast, black college graduates experience a more than 75 percent reduction the wage volatility compared to black high school dropouts. In addition, the results in Table 1.7 show that both black and white individuals receive large and statistically significant reductions in the volatility of weeks unemployed from education, with blacks again experiencing the larger reduction.

In Table 1.8, I display OLS estimates for the effect of education on wage volatility. These are the same specifications as reported above in Table 1.2, but now with wage risk as the dependent variable. There are two interesting results to note in column three. First, college graduate blacks experience a statistically significant reduction in wage volatility compared to black high school dropouts. However, whites receive no statistically significant reduction in wage volatility from education. This suggests that some of the racial difference in the impact of education on labor income volatility is due to a reduction in wage volatility. In column four, we see that conditioning on the demographic controls the relationship between education and wage volatility for blacks is no longer statistically significant. That the point estimates on college graduation are very similar across columns three and four indicates that this result could be due to measurement error in this measure of wages.

Table 1.9 displays the results of OLS regressions of education on employment volatility. In the first two columns the results clearly show that education beyond the high school dropout level leads to a large statistically significant reduction in employment volatility for whites. The estimates in column one and two are very similar, indicating that adding the additional demographic controls does little to alter this relationship. Turning now to columns three and four the estimates indicate that blacks also receive a large and statistically significant reduction in employment volatility. The estimated impact of education on employment volatility for blacks is larger than that for whites. This suggests that racial differences of the impact of education on employment volatility can also provide some explanation for the racial differences in the impact of education on labor income volatility found above.³³

1.6 Preferences For Risk and Educational Attainment

The results thus far indicate that blacks receive more labor income insurance from education than whites. In this next section I study whether these racial differences in the insurance value of education lead to racial differences in the impact of risk aversion on the demand for education as the theoretical model in section 2 would predict. If the estimates above are unbiased and uncertainty about labor income influences behavior then we should observe that more risk averse blacks acquire more education while risk averse whites do not. This is conditional of course, on black and white individuals being

³³ I have also examined the impact of education on the variance of current labor force status by race (without taking out lifecycle variation). The results are qualitatively similar to those reported in Table 9. Blacks and whites reduce their uncertainty about being either unemployed or non-employed by acquiring education. Moreover, the reduction in labor force status uncertainty due to education is larger for blacks than for whites.

aware of the insurance value of education that they face. This serves as an indirect test of the hypothesis that markets for labor income volatility sharing are incomplete in an important way. If individuals can perfectly smooth consumption in response to labor income shocks then the measure of labor income volatility should not affect decision regarding educational attainment.

The model I estimate can be expressed as

$$Education_i = \delta_1 RA_i + X_i \Gamma + \eta_i, \quad (1.6)$$

where RA_i is the survey-based risk aversion measure, X_i is a matrix of additional covariates and η_i is the error term. I estimate this equation using two different measures of education. I first use a dummy for high school graduate or more as a binary education measure and estimate a probit model. I then use a categorical measure for education and estimate an ordered probit model.

The coefficient of interest is δ_1 which measures the relationship between risk aversion and educational attainment. If δ_1 is positive it indicates that more risk averse individuals obtain more education. The central question is whether the estimate of δ_1 is different for blacks and whites. Because the results above show that blacks receive more labor income insurance from education than whites we would expect δ_1 to be larger for blacks than whites, as long as precautionary motives affect the demand for education.

I estimate several specifications of (1.6) which include different sets of covariates in X_i . In model (1) I include only age, age squared and region dummy controls. In the second model I add covariates for parent's education levels. In the third and fourth models I add linear and categorical covariates for own wealth (with a dummy variable for missing wealth also included) respectively. It is important to note that conditioning on

wealth is imperfect in this context because it will be endogenously affected by the amount of labor income volatility an individual has experienced. However, because individuals are able to self-insure against labor income volatility by accumulating financial wealth it is important to see if any relation between education and preferences for risk holds up when we condition on wealth. A preferable variable would be an arguably exogenous measure of wealth such as parental wealth. Unfortunately, parental wealth (as collected in 1988) is missing for most of the sample due to non-response and attrition so the individual's wealth in 1994 is the best available substitute. I estimate this model on the 1992 cross-section, which is closest in time to the 1996 survey date within my sample. This allows me to minimize the number of missing values for risk preferences, though there are still a significant number.³⁴ The tabulation of the risk aversion measure by race and education is reported in Table 1.A3.

Table 1.10 presents estimates of the derivatives evaluated at the mean of the independent variables and their associated standard errors for the probit models where the independent variable is high school graduate or more. The first column indicates that risk aversion is negatively and statistically significantly associated with high school graduation for whites. The fifth column shows that risk aversion for blacks is positively and statistically significantly associated with high school graduation. The results for blacks are consistent with high school graduation providing a reduction in exposure to labor income volatility, as we found above. In contrast, the results for whites are not consistent with the previous results, since we did not find that exposure to labor income volatility increased with high school graduation for whites.

³⁴ Those who are not currently employed in 1996 are not asked this question, which explains some of the missing values.

When the controls for parents' education (columns two and six in Table 1.10) are added, the estimates of the derivative evaluated at the mean of risk aversion on high school graduation for whites drop in magnitude by nearly a factor of two and become statistically indistinguishable from zero. For blacks the estimated derivative of risk aversion on high school graduation remains positive and statistically significantly different from zero. These are important specifications for two reasons. First, Charles and Hurst (2003) have shown that there is significant intergenerational correlation in risk preferences. If there is a long-run relationship between risk preferences and education, omitting parents' education is a significant problem as it is potentially correlated both with the child's educational attainment and risk preferences. Second, it is important to note that the results are robust to including both linear and categorical wealth controls (columns three and four, and seven and eight). We would expect that those who are more risk averse would hold more wealth to self-insure against labor income volatility which would allow individuals to insure consumption through wealth alone. The results here show that risk aversion is positively associated with high school graduation for blacks, but not robustly associated with high school graduation for whites.

The binary education category probit model estimates above do not use all of the available information on education. The ordered probit results in Table 1.8 do use all of this information. The education category dependent variable here takes on one of four values: one for high school dropout, two for high school graduate only, three for some college and four for college graduate. Because the dependent variable is a categorical variable (i.e. the mean does not have a sensible interpretation) I do not present marginal effect estimates for the ordered probit specifications. I present the ordered probit

coefficient estimates and standard errors, which are useful for statistical inference about the sign of the relationship but do not have any clear interpretation in terms of magnitudes.

The results from the ordered probit model estimation reported in Table 1.11 for blacks confirm the main findings of Table 1.10. More risk averse blacks acquire more education. This result is robust to the inclusion of parents' education and wealth controls. The results for whites in Table 1.11 are more puzzling. They indicate that whites who are more risk averse acquire less education, and this coefficient is statistically significantly different from zero. This would be consistent with the model if education increased labor income volatility for whites. In general I do not find this to be true. However, the results in Table 1.5, where tax liabilities are subtracted from total labor and social insurance income, suggest a possible answer. In this set of results the relationship between education and exposure to labor income volatility is not monotonic for whites. Whites who move from any other level of education into the high school graduate only category receive some labor income insurance. It is possible that risk averse whites who are on the margin between some college and high school graduation choose to become high school graduates because of the fact that they can reduce their uncertainty about labor income by making this choice. This could explain the results for whites in Table 1.11. However, this somewhat unsatisfying explanation means that results for whites remain puzzling.

In Table 1.12 I present ordered probit model estimates similar to those in columns four and eight in Table 1.11, but with risk aversion interacted with the wealth categorical variable.³⁵ In this specification I am interested in testing if the positive relationship between education and risk aversion for blacks is strongest amongst those least self-

³⁵ The wealth categorical variable is defined by race-specific quartiles of the wealth distribution.

insured against labor income volatility -- those with the lowest wealth holding. The omitted group (and interaction) is those with the most wealth. The results in the first two columns of the table indicate that there is no differential relationship between risk aversion and education by wealth for whites. Interestingly, in columns three and four we notice that the positive relationship between risk aversion and education found for blacks in Tables 1.10 and 1.11 is strongest for those with moderately low levels of wealth holdings. Because those individuals with less wealth are exposed to more consumption volatility for a given amount of labor income volatility this is exactly the result we would expect. The fact that those blacks with the lowest levels of wealth do not have the strongest relationship between risk aversion and education may reflect the fact that they do not have the previously acquired skills to be near the margin for high school completion and other higher levels of educational attainment. Neal and Johnson (1996) and Cameron and Heckman (2001) argue that the lack of skill development early in the lifecycle for blacks is one of the main determinants of racial differences in education. The results from this table show that blacks who are most exposed to labor income volatility have demand for education that is most sensitive to risk-aversion. This fact offers further support for the argument that the precautionary demand for education is quantitatively important.

1.6 Conclusion

The central result of this chapter is that blacks receive more labor income insurance from the accumulation of education than whites do. The fact that more risk averse blacks choose to accumulate more education is consistent with labor income insurance having

value. Precautionary motives do operate for human wealth, much as previous research has shown they do for financial wealth.

That the results seem to be driven partly by racial differences in the impact of education on wage volatility is consistent with a statistical discrimination model where employers are less certain initially about the productivity of less-skilled blacks than more highly-skilled blacks. While the evidence on the impact of education on wage volatility is indeed suggestive of this mechanism, further work to directly test for this precise mechanism would be valuable. A test of the sort introduced by Altonji and Pierret (2001) could be implemented to test whether employers learn more about less-skilled blacks than more-skilled blacks over their careers. This would suggest that employers have less initial information about the productivity of less-skilled blacks. In addition, efforts to calibrate more realistic models where individuals can insure consumption both by accumulating a buffer stock of assets and acquiring education to attempt to match racial differences in human and financial wealth holding would be valuable.

More broadly, this research implies that the determinants of the cross-sectional distribution of human wealth and the cross-sectional distribution of financial wealth holdings are not as different as the previous literature would lead us to believe. Individual investment decisions about how much wealth to hold in either human or financial forms are both related to individuals' desire to self-insure against labor income uncertainty. The same prudence motive operates in both contexts, driving precautionary savings in financial wealth as well as decisions about how much human wealth to accumulate.

Chapter 2: Are Participants Good Evaluators?

2.1 Introduction

Systematic and rigorous program evaluation represents an important component of evidence-based policy. Recent developments in econometric evaluation methods, summarized in, e.g., Heckman, LaLonde and Smith (1999) and Abbring, Heckman and Vytlacil (2005), have been rapid and substantively important. Social experiments, virtually unknown before 1970, are now frequently used to evaluate a wide variety of economic, social and criminal justice policies; see the exhaustive list in Greenberg and Shroder (2004).

At the same time, participant evaluations have gained attention as a complement to, or substitute for, experimental or econometric evaluation of such programs.

Participant evaluation builds on responses by program participants to survey questions about whether or not the program helped them. While the specific question wording, as well as the number and specificity of the questions, varies substantially among programs, the data from many, if not most, econometric and experimental evaluations that rely on survey data for their outcome measures (rather than, or in addition to, administrative data) include participant evaluations.

This chapter compares econometric estimates of program impacts at the individual or subgroup level with individual participant evaluations using the rich data from the U.S. National Job Training Partnership Act (JTPA) Study (NJS). JTPA was the major employment and training program for the disadvantaged in the U.S. during the

1980s and 1990s. Section 2.2 describes the JTPA experimental evaluation and the data it generated in detail, including the specific structure and wording of the participant evaluation survey question.

We consider two big picture interpretations of our comparisons between econometric impact estimates and participant evaluations. The first interpretation assumes the consistency of both estimators, but views them as estimating different parameters. In this view, the econometric impact estimates consistently measure the treatment effect of a program on a specific outcome, such as earnings or employment, over a specific time period. The participant evaluation, in contrast, consistently estimates the treatment effect of the program on participant utility, with the reference period depending on the question wording (and perhaps on participant interpretation conditional on the wording). In this context, the relationship between the two measures provides information on the relative importance of impacts on earnings and employment in the period covered by the econometric impact estimate to participants' overall benefit (or lack thereof) from program participation.

The second interpretation presumes that participants, like econometricians, have difficulty constructing the counterfactual outcome required to estimate the impact of a program. In this view, because constructing counterfactuals constitutes a cognitively difficult task, participants may (implicitly) rely on crude, cognitively inexpensive methods of impact estimation in constructing their self-reported impacts. The cognitively inexpensive alternatives to consistent estimation of the counterfactual we consider include simple before-after comparisons, and the use of inputs (type of training received)

and outcomes (what happened in the labor market without reference to a counterfactual) as proxies for impacts.

The second interpretation of the relationship between econometric impact estimates (including proxy measures) and participant evaluations has a number of empirical implications. First, consistent econometric estimates may have only a weak relationship with participant evaluations, even if participants care a lot about the outcome in question over the period covered by the econometric estimates. Second, comparisons of the strength of the relationship between participant evaluations and estimates produced by alternative impact estimators may shed light on the particular estimator implicitly employed by program participants in responding to survey questions on program benefits. Third, participant evaluations may exhibit a strong relationship with econometric impacts constructed using the same crude estimators that the participants implicitly use.

We also consider the relationship between the impact proxies commonly used in administrative performance standards systems for employment and training programs and participant self-evaluations. These measures, which consist of simple but poor proxies for long run impacts on earnings and employment, represent a bureaucratic solution to the difficulties associated with constructing proper impact estimates quickly and at low cost. Their simplicity and low cost suggest that participants may (implicitly) rely on them as well in constructing their survey responses. In addition, this analysis has independent interest in that it suggests the extent to which participant evaluations might substitute for these measures in administrative performance systems.

In addition to informing decisions about how best to evaluate policies, our research has broader implications. First, whether or not individuals can accurately assess

their program impacts, and how they go wrong if they cannot, has implications for the interpretation of instrumental variables estimates in the context of the correlated random coefficient model, as in Angrist (2004), Heckman (1997a), Heckman and Vytlacil (2005) and Carniero, Heckman and Vytlacil (2005). In that model, complications arise when using instruments that are correlated with the individual-specific component of impacts. Those problems go away if individuals do not know their impacts (that is, if they make decisions based on “noise”). Of course, if individuals use biased estimates of their impacts in making decisions, the problems may return in a different form depending on how the bias relates to the instrument. Second, and more broadly, the ability to individuals to accurately envision the outcomes associated with alternative choices lies at the heart of rational models of human behavior. We return to this broader issue in the conclusion and offer some thoughts regarding the meaning of our results in terms of this broader question.

We have identified little in the way of existing literature that tries to link objective impact estimates with subjective participant evaluations. The most directly related analyses are those of Heckman and Smith (1998) and Philipson and Hedges (1998), both of which use treatment group dropout as a crude indicator of participants’ evaluations. More broadly, Jacob and Lefgren (2005) compare principals’ subjective evaluations of teachers to econometric estimates of teacher value added, but do not consider the teachers’ evaluations of their own value-added. Prendergast (1999) reviews the literature on subjective performance evaluation, but that literature primarily views subjective evaluations as a way to deal with situations in which agents have multiple tasks (the outputs from some but not all of which allow objective measurement), not as a potentially

cost-saving alternative to objective evaluation. That literature is also focused mainly on performance evaluation of workers within firms, not evaluation of the effects of programs on participant labor market outcomes.

To foreshadow our main findings, the data indicate that the impact estimates we prefer – the ones using subgroup variation in experimental impacts and the ones based on quantile differences – have little relationship with self-reported impacts. At the same time, inputs, outcomes and simple before-after estimates all do predict self-reported impacts, often strongly so.

We organize the remainder of the chapter as follows. Section 2.2 describes the data from the JTPA experiment and the basic structure of the JTPA program. Section 2.3 presents the conceptual framework that guides our econometric analysis and our interpretation of our results. Section 4 discusses the construction and interpretation of the alternative econometric estimates of program impact on employment and earnings that we construct using the experimental data. Section 2.5 presents our results on the relationship between participants' self-reported impacts and impacts estimated using the experimental data. Section 2.6 examines the relationship between self-reported impacts and before-after employment and earnings changes, as well as proxies such as inputs and outcomes while Section 2.7 examines the relationship between self-reported evaluations and performance measures. Finally, Section 2.8 lays out the conclusions that we draw from our analysis.

2.2 Data and Institutions

2.2.1 The JTPA program

The U.S. Job Training Partnership Act program was the primary federal program providing employment and training services to the disadvantaged from 1982, when it replaced the Comprehensive Employment and Training Act (CETA) program, to 1998, when it was replaced by the Workforce Investment Act (WIA) program. All of these programs share more or less the same set of services (though the latter two omit the public sector jobs that led to scandal under CETA) and serve the same basic groups. They differ primarily in their organizational details (i.e. do cities or counties play the primary role) and in the emphasis on, and ordering of, the various services provided. Nonetheless, the commonalities dominate with the implication that our results for JTPA likely generalize to WIA (and CETA).³⁶

The JTPA eligibility rules included categorical eligibility for individuals receiving means tested transfers such as Aid to Families with Dependent Children (AFDC) or its successor Temporary Aid to Needy Families (TANF) as well as food stamps. In addition, individuals were eligible if their family income in the preceding six months fell below a specific cutoff value. There were also special eligibility rules for a number of small groups and a 10 percent “audit” window that basically allowed local sites to enroll individuals at their own discretion. See Devine and Heckman (1996) for more details on the JTPA eligibility rules and Kemple, Doolittle and Wallace (1993) for detailed descriptive statistics on the experimental sample in the NJS. Heckman and Smith (1999, 2004) provide thorough analyses of the determinants of participation in JTPA conditional on eligibility.

³⁶ One possible caveat is that potential participants may have better information to guide them in making participation decisions about a relatively old program, as JTPA was at the time of the experiment, than about a relatively new program. This reasoning suggests greater selection on impacts over time as a program matures.

The JTPA program provided five major services: classroom training in occupational skills (CT-OS), subsidized on-the-job training (OJT), job search assistance (JSA), adult basic education (ABE) and subsidized work experience (WE). Local sites had the flexibility to emphasize or de-emphasize particular services in response to the needs of the local population and the availability of local service providers. In general, CT-OS was the most expensive service, followed by OJT, ABE and WE. JSA costs a lot less. See Heinrich, Marschke and Zhang (1998) for a detailed study of costs in JTPA and Wood (1995) for information on costs at the NJS study sites.

Services get assigned to individuals by caseworkers, typically as the result of a decision process that incorporates the participant's abilities and desires. This process leads to clear patterns in terms of the observable characteristics of participants assigned to each service. The most job ready individuals typically get assigned to JSA or OJT, while less job ready individuals typically get assigned to CT-OS, ABE or WE, where CT-OS often gets followed by JSA. See Kemple, Doolittle and Wallace (1993) for more about the service assignment process. This strongly non-random assignment process has implications for our analyses below in which we examine the relationship between the participant evaluations and types of services they receive.

2.2.2 The National JTPA Study data

The National JTPA Study (NJS) evaluated the JTPA program using a random assignment design. It was the first major social experiment to evaluate an ongoing program rather than a demonstration program brought into existence solely for the purposes of the experiment. Random assignment in the NJS took place at a non-random sample of 16 of

the more than 600 JTPA Service Delivery Areas (SDAs). Each SDA had a local geographic monopoly on the provision of employment and training services funded under the JTPA. The exact period of random assignment varied among the sites, but in most cases random assignment ran from late 1987 or early 1988 until sometime in spring or summer of 1989. A total of 20,601 individuals were random assigned, usually but not always with the probability of assignment to the treatment group set at 0.67.

The NJS data come from multiple sources. First, respondents completed a Background Information Form (BIF) at the time of random assignment. The BIF collected basic demographic information along with information on past schooling and training and on labor market outcomes at the time of random assignment and earlier. Second, all experimental sample members were asked to complete the first follow-up survey around 18 months after random assignment. This survey collected information on employment and training services (and any formal schooling) received in the period since random assignment, as well as monthly information on employment, hours and wages, from which a monthly earnings measure was constructed. Third, a random subset (for budgetary reasons) of the experimental sample members was asked to complete a second follow-up survey around 32 months after random assignment. This survey collected similar information for the period since the completion of the first follow-up survey or, in the case of individuals who did not complete the first follow-up survey, over the period since random assignment. Response rates to both follow-up surveys were around 80 percent. Finally, administrative data on quarterly earnings and unemployment from state

UI records in the states corresponding to the 16 NJS states were collected.³⁷ See Doolittle and Traeger (1990) on the design of the NJS, Orr et al. (1996) and Bloom et al. (1997) for the official impact reports and Heckman and Smith (2000) and Heckman, Hohmann, Smith and Khoo (2000) for further interpretation. Appendix 1 describes the data used in this study in greater detail.

2.2.3 The self-evaluation questions

Exhibit 2.1 presents the two survey questions that, taken together, define the participant evaluation measure we use in this chapter. The question appears on both the first follow-up survey and the second follow-up survey. In both cases, the skip pattern in the survey excluded control group members from both questions. Respondents in the treatment group were asked these questions in the second follow-up survey only if they did not complete the first follow-up survey.

The first question asks treatment group members whether or not they participated in JTPA. The question assumes application because it is implied by the respondent having been randomly assigned. The JTPA program had different names in the various sites participating in the evaluation; the interviewer included the appropriate local name in each site as indicated in the question.

In the second question, respondents who self-report having participated in the program get asked whether the program helped them get a job or perform better on the job. This is not the ideal question from our point of view, as it focuses more on a specific

³⁷ These data were collected twice, once for 12 of the 16 sites by Abt Associates, one of the prime contractors on the original experiment, and then for all 16 sites later on by Westat under a separate contract. We use the latter dataset in our analysis.

outcome than on an overall impact, but it is what we have to work with in the JTPA evaluation. However, to the extent that it focuses respondents' attention specifically on the effect of program participation on labor market outcomes, it should increase the strength of the relationship between the participant evaluations and the econometric estimates of labor market impacts, relative to a broader question that asked about generic program benefits.

We code the responses to both questions as dummy variables. The participant evaluation measure employed in our empirical work consists of the product of the two dummy variables. Put differently, our self-reported evaluation measure equals one if the respondent replies "YES" to question (D7), and "YES" to question (D9). Otherwise, it equals zero.

2.3 Conceptual framework

2.3.1 A simple model of participants' self-reported evaluations

In this section, we lay out a model of how individual participants might respond to a question regarding whether or not they benefited from a program. The discussion here is inspired by those in Manski (1990) and Dominitz and Manski (1994), who provide careful economic (and econometric) analyses of responses to questions about fertility intentions and returns to schooling, respectively. Our (very) simple model helps to structure the design and interpretation of our empirical work.

To begin, we suppose that respondents compare their observed utility given participation with the utility they would have experienced had they not participated. Let U_1 denote utility given participation, U_0 denote utility given non-participation and let

$\Delta_{SR} \in \{0,1\}$ denote the response to the self-evaluation question. Then if respondents generate their answer by comparing the two utilities, we have

$$\Delta_{SR} = 1(U_1 > U_0).$$

This formulation ignores the timing of any affects of participation on utility relative to the survey response. Depending on the wording of the survey question and the respondents' interpretation thereof, respondents may focus on impacts during the period up to the survey response, after the survey response, or some combination of the two. In the JTPA context, we expect them to focus primarily on the effects of the program in the period leading up to the survey response. Expanding our notation, let the subscript "b" denote the period before the survey response and the subscript "a" denote the period following the survey response. We can then write

$$\Delta_{SR} = 1(U_1 > U_0) = 1(f(U_{1b}, E(U_{1a})) > f(U_{0b}, E(U_{0a}))),$$

where $f(\square)$ is an increasing function of both its arguments that maps the utility associated with participation or non-participation, both before and after the self-reported evaluation, into an overall valuation.

Next we consider what aspects of participation affect the utility levels of individuals. In particular, we can decompose the impacts that individuals experience into components related to earnings or employment and a residual component that includes other direct costs and benefits as well as psychic costs and benefits. Denote labor market impacts in the standard notation of the evaluation literature as

$$\Delta_Y = Y_1 - Y_0,$$

where Y_1 denotes the labor market outcome in the treated state and Y_0 denotes the labor market outcome in the untreated state. Similarly, denote the impact on all other determinants of participant utility by

$$\Delta_B = B_1 - B_0,$$

where B_1 and B_0 parallel Y_1 and Y_0 in their interpretation. In what follows, we will further distinguish between impacts realized before and after the survey response.

This decomposition into impacts on labor market outcomes and on all other outcomes that individuals care about corresponds to the components of the impacts that we can and cannot estimate econometrically using our data. The outcomes we (and hopefully the respondents) have in mind other than labor market outcomes include direct costs of participating in training, such as transportation and childcare expenses, leisure time as in Greenberg (1997), as well as any psychic costs and benefits from participating. Rewriting the survey response function in terms of this additional notation yields

$$\Delta_{SR} = 1(U_1 > U_0) = 1(U(Y_{1b}, B_{1b}, E(Y_{1a}), E(B_{1a})) > U(Y_{0b}, B_{0b}, E(Y_{0a}), E(B_{0a}))),$$

or, alternatively

$$(2.1) \quad \Delta_{SR} = 1(U_1 > U_0) = 1(g(Y_{1b} - Y_{0b}, B_{1b} - B_{0b}, E(Y_{1a} - Y_{0a}), E(B_{1a} - B_{0a}))).$$

We estimate two variants of equation (2.1), one in cases where we have econometric estimates of $(Y_{1b} - Y_{0b})$ and another in cases where we examine simple proxies for $(Y_{1b} - Y_{0b})$. The next two subsections define these variants.

2.3.2 Econometric specification using econometric impact estimates

In the case of the econometric impact estimates, we begin by assuming additive separability of the $g(\square)$ function into components related to the labor market impact in the period prior to the survey and the remainder of the function.³⁸ Assuming that the utility function is monotonic in its arguments, we can then rewrite the relationship to put the labor market impact on the left hand side, yielding

$$(2.2) \quad Y_{1b} - Y_{0b} = h(\Delta_{SR}, B_{1b} - B_{0b}, E(Y_{1a} - Y_{0a}), E(B_{1a} - B_{0a})).$$

We actually estimate linear versions of (2.2), given by

$$(2.3) \quad \hat{Y}_{1b} - Y_{0b} = \beta_0 + \beta_1 \Delta_{SR} + \varepsilon,$$

where the hat on the impact denotes an estimate and where ε includes the idiosyncratic pieces of $B_{1b} - B_{0b}$, $E(Y_{1a} - Y_{0a})$ and $E(B_{1a} - B_{0a})$ (the means are captured in the intercept) as well as the estimation error in the impact estimate and any approximation error due to inappropriate linearization.

We adopt this formulation in the case of the econometric impact estimates for two reasons. First, because the econometric impact estimate includes estimation error, we want to put it on the left hand side for the usual reasons associated with measurement error. In contrast, the participant evaluation has no measurement error; the variable is defined as the response to the survey question.³⁹ We include no additional covariates on the right hand side because one of our two econometric estimates (described in detail in Section 2.4.1) consists of predicted subgroup experimental impact estimates. To include both these predicted impacts and a set of observables would require excluding at least one

³⁸ Additive separability is not innocuous here; it implies no complementarities between the component of the impacts we estimate and the other components of the impacts.

³⁹ The counter-argument in favor of making the participant evaluation the dependent variable despite the estimation error in our impact estimates relies on the econometric impacts having the larger variance of the two variables.

observable from this equation, but including it among the observables used to construct the subgroup impacts. The observables available to us lack an obvious candidate for exclusion.

Under the first interpretation of our analysis, a weak estimated relationship in equation (2.3) indicates that participants care primarily about something other than earnings or employment impacts in the period prior to the survey. This conclusion requires the qualification that we should not forget what lies in the error term. Among the items in the error term, we would expect long term impacts to correlate positively with short term impacts; in contrast, impacts on leisure likely correlate negatively with impacts on labor market outcomes prior to the survey. A weak relationship in (2.3) could thus also result from a combination of a positive direct effect of impacts on employment or earnings and a negative indirect effect on leisure, working through the correlation between the omitted impact on leisure and the included impact on employment or earnings. Finally, in a common effect world in which the program has the same impact on everyone, or in which the impact varies but the idiosyncratic component is unknown even to participants ex-post, the true coefficient on the econometric estimate in (2.3) equals zero, since the consumer term would absorb the constant true impact.

Under the second interpretation of our analysis, the absence of a relationship between the participant impacts and our econometric estimates has an additional possible meaning, namely that the respondents have used some other, less cognitively taxing, estimator to (implicitly) construct their own impact estimates. In this case, our econometric estimates will have a weak relationship with the participants' survey responses, but variables related to the chosen alternative estimator, such as crude proxy

variables that respondents might rely on, should display a strong relationship with the participants' self-reported evaluations.

Finally, under either interpretation, large estimated standard errors suggest that our econometric impact estimates contain substantial estimation error.

2.3.3. Econometric specification: impact proxies and performance measures

In the case of the proxy variables and the simple performance measures that act as proxies for impacts, we adopt a more direct analog to equation (2.1) as our econometric specification. In particular, we assume that

$$(2.4) \quad \Delta_{SR} = 1(\beta_0 + \beta_1(proxy(Y_{1b} - Y_{0b})) + \beta_X X + \varepsilon > 0),$$

where ε has a logistic distribution and X is a vector of observable characteristics with corresponding coefficients β_X . This is, of course, a standard logit model, which means that we can identify the coefficients only up to scale; we report estimates of mean derivatives below.

We employ (2.4) rather than (2.3) in this case because the proxies for impacts that we examine, such as labor market outcomes and the types of services received, unlike our econometric impact estimates, do not contain any measurement or estimation error. In addition, because we measure these variables directly, rather than predicting them as a linear combination of the X , we can include conditioning variables X . These conditioning variables soak up residual variance and thus make our estimates more precise. They may also proxy, in part, for $B_{1b} - B_{0b}$, $E(Y_{1a} - Y_{0a})$ and $E(B_{1a} - B_{0a})$, thus clarifying the interpretation of our estimates.

2.4 Econometric impact estimators

This section describes the two econometric estimators that we apply to the experimental data to obtain impact estimates that vary among participants.

2.4.1 Experimental impacts at the subgroup level

The first method we employ for generating impact estimates that vary among participants takes advantage of the experimental data and the fact that random assignment remains valid for subgroups defined based on characteristics measured at or before random assignment, as discussed in, e.g. Heckman (1997b).

We estimate regressions of the form

$$(2.5) \quad Y_i = \beta_0 + \beta_D D_i + \beta_X X_i + \beta_I D_i X_i + \varepsilon_i,$$

where Y_i is some outcome measure, D_i denotes assignment to the experimental treatment group, X_i denotes a vector of characteristics measured at or before random assignment and $D_i X_i$ represent interactions between the characteristics and the treatment indicator.

It is these terms that yield variation in predicted impacts among individuals at the subgroup level. The predicted impacts based on (2.5) for the treatment group members are given by

$$(2.6) \quad \overline{Y}_i - Y_{0i} = \hat{\beta}_D + \hat{\beta}_I X_i.$$

Though quite straightforward conceptually, our experimental subgroup impact estimates do raise a few important issues, which we now discuss. The first issue concerns the choice of variables to interact with the treatment indicator. We address this issue by presenting two sets of estimates based on vectors of characteristics selected in

very different ways. One set of estimates simply borrows the vector of characteristics employed by Heckman, Heinrich and Smith (2002) in their analysis of the JTPA data. The notes to Table 3 list these variables. The second set of estimates utilizes a set of characteristics selected using the somewhat unsavory method of stepwise regression. While economists typically shun stepwise procedures as atheoretic, for our purposes here that bug becomes a feature, as it makes the variable selection procedure completely mechanical. Thus, we can be assured of not having stacked the deck in one direction or another. In both cases, we restrict our attention to main effects in order to keep the problem manageable.

We implement the stepwise procedure using essentially all of the variables from the BIF including variables measuring participant demographics, site, receipt of means-tested monetary and in-kind transfers, labor force status and work history. We include a missing indicator for each variable (to avoid losing a large fraction of the sample due to item non-response), and interact both the variables and the missing indicators with the treatment group indicator. The stepwise procedure is restricted to keep or drop each variable along with the missing indicator and interactions with the treatment indicator as a group. The stepwise procedure, which we perform separately for each of the four demographic groups, iteratively drops variables with coefficients not statistically different from zero in a regression with self-reported earnings in the 18 months after random assignment as the dependent variable.⁴⁰

The second issue concerns the amount of subgroup variation in impacts in the NJS data within the four demographic groups – adult males and females ages 22 and

⁴⁰ We employ the “step up” stepwise procedure as it has more power than the “step down” and “single step” procedures. See Dunnett and Tamhane (1992) and Lui (1997) for details. We set the p-value for choosing variables in the final specification at 0.05.

older and male and female out-of-school youth ages 16-21 – for which both we and the official reports conduct separate analyses. Although the NJS impact estimates differ substantially between youth and adults (and between male and female youth when considering the full samples), the experimental evaluation reports – see Exhibits 4.15, 5.14, 6.6 and 6.5 in Bloom et al. (1993) for the 18 month impacts and Exhibits 5.8, 5.9, 5.19 and 5.20 in Orr, et al. (1994) for the 30 month impacts – do not reveal a huge amount of statistically significant variation in impacts among subgroups defined by the observables available in the BIF. If the impact does in fact vary a lot among individuals, but not in a way that is correlated with the characteristics we use in our model, then we may reach the wrong conclusions about participants’ ability to construct consistent estimates of earnings impacts. This case has more than academic interest given that Heckman, Smith and Clements (1997) bound the variance of the impacts in the JTPA data away from zero for adult women; their lower bound on the standard deviation of the impacts equals \$674.50 with a standard error of \$137.53 (see their Table 3).⁴¹ In addition to simply keeping it in mind, we attempt to address this concern in part by examining the quantile treatment effect estimates described in the next section, which do vary a lot among participants, and by looking, in other work, at data from other experimental evaluations with more in the way of subgroup variation in impacts.

The third issue concerns an additional assumption that we must make in order to interpret our results in the way that we have described here. A simple example illustrates the need for this assumption. Consider two subgroups and suppose that participants care only about earnings impacts, and give a positive survey evaluation when they have an

⁴¹ Our subgroup impacts have standard deviations that range from \$840 to \$2600 depending on the demographic group and set of covariates. The quantile treatment effects have lower standard deviations; they range between \$257 and \$477.

earnings impact greater than zero. In group one, suppose that 10 percent of the individuals have an impact of 1000 while 90 percent have an impact of zero. The mean impact for subgroup one thus equals 100, while the fraction of positive participant evaluations equals 0.1. In contrast, in group two, 20 percent of the individuals have an impact of 400 while 80 percent have an impact of zero. The mean impact for subgroup two thus equals 80 while the fraction of positive participant evaluations equals 0.2. In this example, when comparing across the two subgroups the mean impact varies inversely with the fraction with a positive impact. In interpreting our results below, we assume that this case does not hold in the data. Put differently, we assume that mean impacts and the fraction with a positive impact positively co-vary at the subgroup level.

2.4.2 Quantile treatment effects

The second econometric method we use to derive individual level treatment effect estimates relies on an additional non-experimental assumption. In particular, we make the assumption of a perfect positive rank correlation between the outcomes in the treated and untreated states as described in Heckman, Smith and Clements (1997). Intuitively, we assume that the expected counterfactual for an individual at a given percentile of the treatment group outcome distribution consists of the mean outcome at the same percentile of the control group outcome distribution. One way to think about this assumption is that expected labor market outcomes depend on a single factor, so that individuals who do well in the treatment state also do well in the control state. This represents a very different view of the world than, for example, the classic model of Roy (1951), but may represent a reasonable approximation for treatments, such as those offered by JTPA, that have small average impacts relative to the mean of the outcomes in question.

Using this method, we estimate the impact for treated individual “ i ” with an outcome at percentile “ j ” of the treatment group outcome distribution as

$$(2.7) \quad \hat{Y}_{i1} - Y_{i0} = \hat{Y}_1^{(j)} - \hat{Y}_0^{(j)},$$

where the superscript “ (j) ” denotes the percentile. This estimator underlies the literature on quantile treatment effects, as in Abadie, Angrist and Imbens (2002) and Bitler, Gelbach and Hoynes (2004), with the difference that rather than interpreting the estimates as the effect of treatment on the quantiles of the outcome distribution, we make the additional rank correlation assumption. As discussed in Heckman, Smith and Clements (1997), the rank correlation assumption pins down the joint distribution of outcomes, which in turn pins down which quantile of the control outcome distribution provides the counterfactual for each quantile of the treatment outcome distribution, and allows us to assign impact estimates to specific individuals.

2.5 The relationship between econometric impact estimates and participant evaluations

2.5.1 Bivariate relationships

We begin our analysis of the data from the NJS with simple bivariate relationships between mean experimental impacts for a variety of labor market outcomes and the fraction of participants with a positive self-reported evaluation for the four demographic groups in the experiment. This analysis, presented in Table 2.1, extends that presented in Table 8.11 of Heckman and Smith (1998). It represents a very basic application of the methodology outlined in Section 2.4.1.

The first four rows in Table 2.1 correspond to the four demographic groups described above. The first column presents the fraction of the experimental treatment group with a positive self-reported evaluation. The remaining columns report mean

impacts on eight different earnings and employment outcomes. The first two measures consist of self-reported earnings and any self-reported employment in the first 18 months after random assignment, which roughly corresponds to the period prior to the survey response for most sample members. The second two measures consist of self-reported earnings and employment in month 18 after random assignment, thus focusing on the respondent's status right around the time of the survey, rather than over the entire period since random assignment. The remaining four measures repeat the first four, but now using the quarterly data from the matched UI earnings records rather than the self-reported outcome data. We include both sets because they appear substantially different at both the individual and aggregate levels – see the discussions in Smith (1997a,b) and Kornfeld and Bloom (1999). The final row of Table 2.1 reports the correlation between the percent with a positive self-reported evaluation and the impact estimates in each column, as well as the p-value from a test of the null hypothesis that the population correlation coefficient equals zero.

Table 2.1 reveals that the subgroup with the worst impact on all eight of the outcome measures, namely male youth, has the second highest fraction with a positive self-reported evaluation. The group with the highest fraction with a positive self-reported evaluation, namely female youth, often has the second-worst impact estimate. Consistent with this basic pattern, the correlations reported in the last row end up negative six out of eight times, though we can never reject the null of a zero correlation (which is not surprising given that we have only four groups). Thus, at this crude level, we find very little in the way of an association between the participant evaluations and the econometric estimates that rely on subgroup variation; as noted above, this may mean that earnings

and employment do not figure much in respondents' evaluations of JTPA, or it may mean that respondents do not do a very good job of constructing the relevant counterfactual.

Looking specifically at the employment impacts, we might expect improved performance given the focus of the actual question on help in finding a job. However, the signs of the correlation differ between employment measures for both data sets and between data sets for each measure. More broadly, if we make the rank correlation assumption described in Section 2.4.2, the experimental impacts show that the program improved the employment situation of at most a few percent of the respondents, yet well over half self-report a positive impact on the survey.

Tables 2.2A, 2.2B, 2.2C and 2.2D report the results of a similar bivariate analysis using variation in the experimental impacts among subgroups of the four demographic groups. Each table corresponds to one of the four demographic groups. Within each table, the rows correspond to the variables used to define the subgroups and the columns refer to the same eight labor market outcome variables considered in Table 2.1. The variables we use are race/ethnicity, years of schooling categories, marital status, time since last employment categories, site and age categories, where we omit the age variable for youth as the group is limited to individuals from 16 to 21 in any case. Each entry in the table gives the estimated correlation coefficient between the fraction with a positive self-reported evaluation and the experimental impact estimate for the outcome variable for the column and for the subgroups defined by the row variable.

The bottom of each table also presents some summary statistics. In particular, we present the number of positive and negative correlations in the table and, within each of these categories, the fraction statistically significant at the five and ten percent levels.

For the adults, we would expect random variation to lead to 4 or 5 estimates statistically significant at the 10 percent level and 2 or 3 at the five percent level. These vote counts provide a useful but imperfect summary of the 48 (or 40 for the youth) entries in each table. In particular, the vote counts ignore the lack of independence among the estimates in each table and do not make any attempt to weight or value the estimates based on their precision.

The results in Table 2.2 paint a picture that looks a lot like the one from Table 2.1. No clear patterns emerge in terms of coefficient signs and the number of statistically significant correlations is roughly what one would expect if the true population coefficients all equaled zero. So far, our findings strongly suggest either that participants weight labor market outcomes over the period prior to the survey very little in evaluating JTPA, or that participants do care about impacts but do a very bad job of estimating them, or that the experimental impact estimates based on subgroup variation have only a weak correlation with actual individual impacts.

2.5.2 Regression results for experimental subgroup estimates

we now to turn evidence from regressions of estimated impacts on each of the labor market outcomes considered in Tables 2.1 and 2.2 on the indicator variable for a positive self-reported evaluation. In terms of our earlier discussion, we report estimates of β_1 from equation (2.3), where the dependent variable consists of an experimental impact estimate based on subgroup variation in impacts as in equations (2.5) and (2.6). These estimates appear in Table 2.3, where each entry in the table represents a separate regression. The rows correspond to particular labor market outcomes. Each of the four

demographic groups has two columns of estimates, one for each of the two sets of covariates used in estimating equation (2.5). The columns headed by (2.1) contain the estimates using the covariates from Heckman, Heinrich and Smith (2002), while the columns headed by (2.2) contain the estimates using the covariate set chosen by the stepwise procedure. The final two rows of the table summarize the evidence in each column; in particular, they give the numbers of positive and negative estimates and, within each category, the number of statistically significant estimates at the five and ten percent levels.

The evidence in Table 2.3 suggests little, if any, relationship between the experimental impact estimates based on subgroup variation and the self-reported evaluations. While the estimates of β_1 lean negative in the aggregate, only a handful of the estimates reach conventional levels of statistical significance (and not all of those fall on the negative side of the ledger). The regression evidence thus compounds the evidence from the simple bivariate relationships examined in Tables 2.1 and 2.2. Either the participants do not weigh labor market impacts very heavily in their response, or else their impact estimates (or ours) do not do a very good job of capturing the actual impacts.

2.5.3 Results based on quantile treatment effect estimates

This section presents evidence on the relationship between self-reported effects and impact estimates constructed under the perfect positive rank correlation assumption described in Section 2.4.2. We present these results in both graphical and tabular form. Figures 1A to 1D present the evidence in graphical form, with one figure for each demographic group. The horizontal axis in each figure corresponds to percentiles of the untreated outcome distribution. The solid line in each graph presents impact estimates at

every fifth percentile (5, 10, 15, ..., 95) constructed as in equation (2.7). The broken line in each graph represents an estimate of the fraction with a positive self-reported evaluation at every fifth percentile. For percentile “j”, this estimate consists of the fraction of the treatment group sample members in the interval between percentile “j-2.5” and percentile “j+2.5” with a positive self-reported evaluation. If the assumptions underlying the percentile difference estimator hold, if participants care about labor market outcomes in answering the survey question, and if participants consistently estimate their own impacts, then the two lines should move together in the figures.

Several features of the figures merit notice. First, in the lower percentiles in each figure the econometric impact estimate equals zero. This results from the fact that the lowest percentiles in both the treated and untreated outcome distributions have zero earnings in the 18 months after random assignment; the difference between the two then equals zero as well. Surprisingly, a substantial fraction (over half in all four demographic groups) of treatment group members at these percentiles respond positively to the survey evaluation question, even though they have zero earnings in the 18 months after random assignment. This suggests that respondents view the question as asking about longer term labor market impacts and not solely as a narrow question about finding a job immediately after participation in the program.

Second, the fraction with a positive self-reported evaluation has remarkably little variation across percentiles of the outcome distribution. For all four demographic groups, it remains within a band from 0.6 to 0.8. For the adults, the mean increases with the percentile; for the youth, the data fail to reveal a clear pattern.

Third, no obvious relationship between the two variables emerges from the figures for three of the four demographic groups. Adult women constitute the exception; for them, both variables increase with the percentile of the outcome distribution. More specifically, for adult women, both variables have a higher level for percentiles where the impact estimate exceeds zero. Within the two intervals defined by this point, both variables remain more or less constant.

Table 2.4 presents some of the numbers underlying the figures. In particular, the first five rows present the values for the 5th, 25th, 50th, 75th and 95th percentiles. The last two rows of the table give the correlation between the quantile treatment effects and the fraction with a positive self-reported evaluation for each group (and the corresponding p-value from a test of the null that the correlation equals zero) along with the estimated coefficient from a regression of the quantile treatment effects on the fraction with a positive self-reported evaluation (and the corresponding standard error). The correlation and regression estimates quantify and confirm what the figures indicate: a strong positive relationship for adult women, a weak and statistically insignificant positive relationship for adult men, and moderately strong and negative relationship for male youth and a similar, but not statistically significant, relationship for female youth. Although we find a bit more here than in the estimates that rely on subgroup variation, once again the data do not suggest a strong, consistent relationship between the econometric impact estimates and the self-reported evaluations.

2.6 Relationship between positive self-evaluation and proxies for impacts

2.6.1 Motivation

In this section, we present evidence on the extent to which simple proxies that respondents might use in constructing their impact estimates predict a positive self-reported evaluation. The proxies we examine include input (training type) measures, labor market outcome (employment and earnings) measures and simple before-after differences in labor market outcomes. For each of these proxies, we present estimates of equation (2.4) in Section 2.3.3. If the proxy variables drive the self-reported evaluations, this suggests that participants rely on these readily accessible variables in answering the survey evaluation question instead of thinking hard about their counterfactual outcome. If respondents really do know the impacts, then proxies poorly correlated with the actual impacts should have little explanatory power.

Two important caveats weaken this argument. First, if we have imprecise (or inconsistent, in the case of the quantile treatment effects) econometric estimates, then we have no way of knowing the extent to which the proxies correlate with actual impacts; our view that they do not relies solely on our priors. Second, particularly in the case of the input measures, the proxies may correlate with both the psychic and the direct costs and benefits of participation not captured in the labor market outcomes we examine. For example, classroom training may be more fun than, say, job search assistance. Alternatively, classroom training may have higher direct costs if it takes place at a distant community college while job search assistance takes place at a local neighborhood organization. With both the big picture and these caveats in mind, we now turn to our results.

2.6.2 Results with input and outcome measures

Table 2.5 presents logit estimates of equation (2.4) that include not one but two measures of the training received by JTPA treatment group members. We interpret these inputs as potential proxies for impacts. Respondents receiving only inexpensive services (or no services at all) might reason that as a result the program can have had little if any impact, while participants who receive expensive services such as CT-OS or OJT may draw the opposite conclusion.

The two measures of service receipt derive from self-reports collected in the NJS follow-up surveys and from administrative data from the individual sites participating in the experiment.⁴² As shown in Smith and Whalley (2005), these two measures differ substantially; as a result we do not run into collinearity problems when including them both. The two data sources code the service types somewhat differently; for comparability and ease of interpretation, we employ just five service types: CT-OS, OJT/WE (which is almost all OJT), JSA, ABE and “other”. We code a dummy variable for each service type in each data source indicating whether or not the respondent received it; a respondent who received more than one service type in a given data source gets coded based on the training type they receive in their first spell.

The logit models presented in Table 2.5 also include a variety of background variables. These variables play two roles. First, we expect them to pick up parts of the overall impact of participation unrelated to the labor market outcomes we examine. For example, the site dummies will pick up differences in the friendliness and efficiency of site operation as perceived by the respondents. The variable “work for pay”, which is an

⁴² In fact, two versions of the administrative data on service receipt exist, one created by MDRC and one created by Abt Associates. Both rely on the original MIS files from the 16 sites in the experiment. Our experience with both files, described in detail in Smith and Whalley (2005), leads us to employ the Abt version in this paper.

indicator variable for whether or not the respondent has ever worked for pay, relates to the opportunity cost of participation, as does the variable for having a young child. The AFDC receipt at time of random assignment variable captures variation in the cost of classroom training due to the availability of an income source not tied to employment. In order to avoid losing a large fraction of the sample due to item non-response, we recode missing values to zero and include indicator variables for missing values of each variable.

Each column in Table 2.5 corresponds to one of the four demographic groups. The table presents mean derivatives, estimated standard errors for the mean derivatives in parentheses and the p-value of a test of the null hypothesis that the mean derivative equals zero in square brackets. Table 2.6 summarizes the results in Table 2.5 by presenting test statistics and p-values from tests of the joint null that the mean derivatives for groups of related covariates (e.g. all of the self-reported training type variables) equal zero.

Consider the variables other than the training type variables first. Although they are not shown in the table, the site variables have a strong effect on the probability of a positive self-reported evaluation. The magnitudes vary a lot as well; for example, for adult males the coefficients on the site dummies range from -0.257 to 0.093. Moreover, Table 2.6 shows that these variables are strongly statistically significant as a group. We interpret this as indicating that respondents take account of non-pecuniary aspects of their JTPA experience, such as the friendliness and efficiency of the staff and the attractiveness and ease of access of the JTPA office and the local service providers. However, these variables may also proxy for site differences in program impacts (though this seems

unlikely given the findings in Section 2.5.2) or for other features of the local environment, such as the state of the economy, that affect respondent's experiences.

With the exception of age for adults, race for youth, and age and education for female youth, the other demographic variables play surprisingly little role in determining the probability of a positive self-reported evaluation. Among adults, age has a strong negative effect on the probability of a positive self-evaluation, while black male youth and Hispanic male and female youth have higher probabilities of a positive response. The limited role played by background characteristics in the analysis surprised us.

In contrast to the background characteristics, the training type variables play a major role in determining individual self-reported evaluations. Table 2.6 shows that, taken together, both the self-reported and administrative training type variables achieve high levels of statistical significance for adults, and the administrative measures do so for youth.

Smith and Whalley (2005) show that the self-reported and administrative measures of receipt of classroom training in occupational skill tend to agree; as such, we can (as a crude approximation) simply add their coefficients. Doing so reveals that CT-OS has a large positive effect on the probability of a positive self-reported evaluation for all four groups. Subsidized on-the-job training reports often do not coincide in the two data sources; here we find that self-reported OJT has a strong (and usually statistically significant) positive effect, as does administratively reported OJT! CT-OS and OJT generally represent the largest resource investment in the JTPA participant; the participants appear to recognize this and use it as a proxy for impacts in responding to the self-reported evaluation questions.

Job search assistance, the cheapest of the services, elicits less of a positive effect. This training type also tends to get reported differently in the two data sources. Here, except for self-reported JSA for adult males and administratively reported JSA for adult females, we find modest and statistically insignificant effects. Adult Basic Education (ABE) and “other” training do not yield precise estimates, except for “other” training in the administrative data, which has, somewhat puzzlingly, a negative and statistically significant effect for adult women and a positive and statistically effect for female youth. These two training types have, in general, smaller sample sizes than the others, which may account for the imprecision of the estimates.

Overall, the training type variables matter in predicting a positive self-reported evaluation in a manner consistent with the view that respondents use the intensity of the services they receive – the inputs – as a proxy for the impact the services have upon them.

Tables 2.7 and 2.8 describe a set of logit estimates of equation (2.4) in which we include the same background variables as in the models of Table 2.5 but add various versions of Y_1 , the labor market outcome in the treated state. Respondents may reason that if they have done well in the labor market over the period between random assignment and the survey or if they are doing well around the time of the survey, then the program must have benefited them. Respondents who have not done well may draw the opposite conclusion. As with the training type variables, the outcomes represent an easily observable proxy for impacts that respondents may rely in when determining their responses to the survey evaluation question.

The top panel of Table 2.7 reports estimates from a specification in which we divide self-reported earnings in the 18 months after random assignment into five categories: zero, and four quartiles of the distribution of positive earnings, and then include dummy variables for four of the five categories, with the highest category as the excluded category. The second panel of Table 2.7 corresponds to a similar specification but using earnings in the six calendar quarters after random assignment from the UI administrative data. The last two lines report estimates from a specification that includes a dummy variable for any employment in the 18 months after random assignment, again measured first based on the survey data and then based on the UI administrative data. Table 2.8 summarizes the evidence in Table 2.7 as well as evidence from a series of alternative specifications not reported here for reasons of space. As in Table 2.6, the summary takes the form of p-values from tests of the null hypothesis that the coefficients or coefficients corresponding to a specific labor market outcome measure equal zero.

The broad picture from Table 2.7 is that labor market outcomes appear to predict self-reported evaluations. This is particularly true for adults. For both adult males and adult females and for both earnings measures, all of the estimated coefficients are negative (as expected when compared to the highest earnings quintile) and most are statistically significant. Also, broadly speaking, the estimated coefficients decrease as earnings increase, as expected. For youth, the coefficients also turn out largely negative (indicating a positive relationship) but rather imprecisely estimated. The self-reported employment measure also has a strong positive and statistically significant relationship to the self-reported evaluations, but the UI employment outcome measure does not. This

latter finding may result from measurement error in UI employment due to its omission of government jobs and informal jobs.

Turning to Table 2.8, which summarizes a large number of specifications via chi-square tests and associated p-values (including both the specifications reported in Table 2.7 and many not reported there for reasons of space), we find similar patterns. The relationships tend to be statistically stronger for adults than for youth, and stronger for the measures based on the self-reported data than on the UI data. Also, the earnings measures tend to yield more statistically significant relationships than the employment measures, especially for measures that consider outcomes just around the time of the survey.

Overall, this section presents compelling evidence that simple proxies for impacts in the form of inputs and outcomes predict self-reported evaluations. This pattern of findings lends support to the view that respondents adopt cognitively simple alternatives to the difficult task of trying to construct a counterfactual outcome when answering the self-evaluation question.

At the same time, the role of the input measures may also reflect, in part, various non-pecuniary aspects of the services received. For example, in addition to representing a larger financial investment, respondents who receive classroom training may have more fun in their JTPA experience than those who receive, say, job search assistance. Also, outcomes may proxy in part for impacts (and with less measurement error than the impact measures we consider earlier in the chapter); in a world where the counterfactual is zero earnings – the correct world for some fraction of the treatment group – outcomes and impacts coincide, which muddies the interpretation of our findings.

2.6.3. Results with before-after comparisons of labor market outcomes

In addition to inputs and outputs, survey respondents may employ simple before-after comparisons of outcomes as a (cognitively as well as conceptually) simple estimator. Of course, before-after comparisons, despite their simplicity, provide consistent estimates of program impacts under the condition that the “before” period outcome consistently estimates the outcome that would have been realized in the absence of treatment. Heckman and Smith (1999) show that this condition fails rather dramatically in the NJS data, with the result that, due to “Ashenfelter’s dip” in earnings in the pre-program period, before-after impact estimates tend to have a strong upward bias. In this section, we relate several before-after impact estimates on employment and earnings to our self-reported evaluation measure.

Tables 2.9 and 2.10 present estimates of logit models with the self-reported evaluation measure as the dependent variable and three different measures of before-after earnings changes as independent variables, along with all of the variables in Column 1 of Table 2.5. The first measure, for which the estimates appear in Table 2.9, consists of the difference in average monthly earnings between the 12 months before random assignment and the 18 months after random assignment. We can use only the 12 months before random assignment due to the limitations of the survey data on pre-random assignment earnings for the treatment group.⁴³ The second and third measures, for which the estimates appear in Table 2.10, rely on the UI earnings data. The first measure, in the top panel of Table 2.10, consists of mean monthly earnings in the six calendar quarters after random assignment minus mean monthly earnings in the six calendar

⁴³ In particular, we only have the response to a question about earnings in the previous year from the BIF.

quarters after random assignment. The second measure, denoted UI(2) in the lower panel of Table 2.10, consists of the difference in mean monthly earnings between just the sixth quarter before and the sixth quarter before random assignment. In each case, we let the data speak to the functional form by including indicator variables for quintiles of the before-after difference.

We find strong evidence that before-after differences in labor market outcomes predict self-reported impacts. For the self-reported measure, the relationship is clearest for the adult females and the male youth, where the estimated coefficients increase monotonically (or almost so) and are statistically and substantively significant for the upper quintiles. Even stronger findings appear for the UI earnings difference measure in the top panel of Table 2.10, with large, and almost always statistically significant, coefficients for all four groups for the two upper quintiles. For male youth, the key difference seems to be between the lowest quintile and the other four; the four coefficients are all relatively large, all about equal and all statistically significant. For the other three groups, there is a general pattern of increasing coefficients as you move down the table. The results for the UI(2) measure in the bottom panel of Table 2.10 are weaker, in both a substantive and a statistical sense, than those in the top panel; this suggests that respondents use outcomes over the entire pre- and post-random assignment periods in constructing their implicit before-after estimates of program impact.

Given that the second of the two survey questions that compose our self-reported evaluation measure asks directly about finding a job, in Table 2.11 we consider its relationship to before-after employment changes. We coded an employment status difference variable based on employment at the date of random assignment and 18

months after random assignment. This yields four patterns. We include dummy variables for three of the four patterns, with employed at both points in time as the omitted pattern. The findings here are, perhaps, less strong than expected. In general, relative to the always employed, those who are never employed or who lose a job tend to have less positive self-reported evaluations. For the adults, those who gain a job tend to be somewhat more positive. However, only a handful of the differences achieve statistical significance. Measurement error in the “after” employment status may account for our weak results. By looking at employment around the time of the survey, we have given the respondents plenty of time to lose jobs that JTPA helped them find and, in the case of dropouts, to find jobs without the help of JTPA.

Finally, Table 2.12 presents the results of chi-squared tests for the joint significance of the before-after difference variables considered in Tables 2.9, 2.10 and 2.11. The test statistics and p-values in this table confirm that respondents’ self-reported evaluations depend (in a statistical sense) on these before-after differences. Indeed, the joint tests for the employment change variable look stronger than the individual t-tests, suggesting that our omitted group lies in the middle of the categories in terms of its effect on the self-evaluation measure. Overall, the findings in this section lend support to the view that respondents implicitly or explicitly use natural and cognitively simple (but nonetheless quite biased) before-after comparisons in constructing their self-reported evaluations.

2.7 Results with performance measures

In this section we present results on the relationship between participant self-reported evaluations and performance measures based on program outcomes commonly used in employment and training programs both in the U.S. and elsewhere. Performance standards systems attempt to provide information on the impacts of programs quickly and at low cost by relying on crude proxies. In this sense, as the reader will quickly discern, some of the performance measures are fairly closely related to the outcome proxies examined in Section 2.6.2. In the JTPA program, the performance measures had real effects on site budgets; sites that did well on them received budgetary rewards while sites that did exceptionally poorly could receive “technical assistance” and also experienced the threat of formal reorganization. See, e.g., Heckman, Heinrich and Smith (2002), Heckman and Heinrich (2005) and Barnow and Smith (2004) for more detailed descriptions of the performance standards systems in JTPA, WIA and other programs, for evidence that the usual performance measures have little, if any, relationship to the actual impacts of the program, for evidence of strategic behavior by program staff in response to the incentives provided by the JTPA performance standards system, and for additional pointers to the literature.

The performance measures we examine here are subsets of those included in the JTPA and WIA performance standards systems. From the JTPA system we consider employment status at termination from the program, wages at termination from the program (which is defined only for those employed at termination), employment at “follow-up”, which is 13 weeks after termination, and weekly earnings (not including zeros) at follow-up. We use self-reported information to construct the JTPA performance measures, as was done in that program. From the WIA system we consider employment

at termination, employment at six months after termination conditional on employment at termination (this measure aims to count “retention”, although it does not require the individual to stay at the same job), and the difference in quarterly earnings between the two calendar quarters after termination and the two quarters prior to random assignment. Note that the earnings gain measure, which was an innovation in the WIA system relative to JTPA, exploits the pre-program dip in mean earnings discussed in, e.g. Heckman and Smith (1999), to obtain a measure that will invariably suggest positive earnings impacts whether the program works or not. We follow the WIA program in relying mainly on the UI earnings records in constructing the WIA performance measures for our sample.

The top panel of Table 2.13 presents results based on estimating logit models with self-reported evaluations as the dependent variable and one of the performance measures, again including all of the variables in Table 2.5 as covariates. The bottom panel reports results from similar models using the WIA performance measures.

Three patterns emerge from the findings in Table 2.13. First, among the JTPA measures, employment at termination and employment at follow-up are significantly related, in both senses, to self-reported evaluations. The estimated mean change in the probability of a positive self-evaluation due to employment at termination ranges from 0.08 for male youth to 0.13 for adult females. Employment at follow-up shows a similarly strong relationship. Second, the WIA measures, other than the earnings change measure, show little in the way of a consistent relationship with the self-reported evaluation measure. In particular, using the UI earnings data to measure employment at termination rather than survey data on employment spells adds enough measurement error to yield a much weaker relationship for all four groups and one that is statistically

significant only for adult males. Third, the relationship between self-reported evaluations and the performance measures appears stronger for women than for men.

Overall, Table 2.13 yields a mixed picture. Some performance measures based on labor market outcomes have substantively and statistically significant relationships with self-reported evaluations but even these account for only a modest fraction of the variance. Thus, the self-reported evaluations capture something related to, but very different from, the performance measures.

2.8 Conclusions

Broadly speaking, and putting aside the material in Section 2.7 regarding the performance standards, we have two main findings. The first is that self-reported evaluations by treatment group members from the JTPA experimental evaluation have, in general, little if any relationship to either experimental impact estimates at the subgroup level or to what we regard as relatively plausible econometric impact estimates based on percentile differences. The second is that the self-reported evaluation measures do have consistent relationships with crude proxies for impacts, such as measures of service type (a proxy for the amount of money spent), labor market outcome levels (which measure impacts only if the counterfactual state consists of no employment or earnings, which it does not for the vast majority of our sample), and before-after comparisons.

Taken together, these two findings provide strong support for the view that respondents avoid the cognitive burden associated with trying to construct (implicitly or explicitly) the counterfactual outcome they would have experienced had they been in the

control group and thus excluded from JTPA. Instead, they appear to use readily available proxies and simple heuristics to conclude, for example, that if they are employed at the time of the survey or if their earnings have risen relative to the period prior to random assignment, that the program probably helped them find a job or get a better job. At the same time, our evidence does not rule out the view that respondents consider factors in their answers not captured in our experimental and econometric impact estimates, such as expected impacts in later periods. The proxy variables still leave much variation in the self-evaluation measure to be explained by other factors.

Overall, we conclude that participant self-evaluation measures of the type analyzed here represent a very poor substitute for rigorous experimental or non-experimental estimates of program impact. At the same time, to our knowledge this chapter represents the first attempt to seriously study what these questions actually measure. The literature on using surveys to measure expectations, as discussed in Manski (2004), provides some reason for thinking that more sophisticated survey questions might do a better job of measuring the underlying objects of interest. Without additional research, we hesitate at this point to make any claims about the broadest of the questions that motivated this study, namely whether or not individuals can effectively construct the counterfactual outcomes required for them to make economically rational decisions.

Appendices

1. Data Appendix for Chapter 1

1.1 Sample Selection

The PSID full-sample (both the main and SEO oversample) contains 63453 individuals ever in the sample from 1977 to 1992. I drop those who are not household heads for at least 9 years between 1977 and 1992 (this also drops the 1990 Latino subsample) and I am left with 5015 individuals in the 1977 cross-section. I then drop female individuals and those who are not aged 22 to 45 in 1977, so that the sample only includes those aged 22 to 60 in every year. I am then left with 1894 individuals in the 1977 cross-section. Dropping those who are not white or black, those ever a student and those with missing education leaves 1830 individuals in the 1977 cross-section. This final sample also contains 1601 individuals in the 1992 cross-section.

1.2 Risk Preference Measures

PSID Risk Preference Question (M1):

Now I have another kind of question for you. Suppose that 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?

0 INAP

1 YES

5 NO

8 DK

9 RF

The response 'INAP' indicates that the question is inappropriate, the response 'DK' indicates that the respondent does not know, and the response 'RF' indicates that the respondent refused to answer. The next questions ask the respondent about different losses in spending of either a 50 percent, 20 percent, 10 percent and 75 percent cut in income. Which questions the respondent answers depends on their responses to the previous questions. By using the responses to all the questions asked a measure of an individual's risk preference is constructed based on the method in Barsky et al. (1997). This method assumes a CES utility function, corrects for measurement error, and classifies individuals into one of four risk aversion categories: "very high", "high", "moderate" and "low" each with an associated coefficient of relative risk aversion.

1.3 Definitions of Key Variables

log (annual labor and social insurance income)
 = log(annual labor income + annual workers compensation income + annual unemployment insurance income + annual foodstamp income + annual AFDC income + annual SSI income)

Recoded log (annual labor and social insurance income)
 = log(annual labor income + annual workers compensation income + annual unemployment insurance income + annual foodstamp income + annual AFDC income + annual SSI income) if log (annual labor and social insurance income) > 0
 = 1 otherwise

log (annual labor income)
 = log(annual labor income)

log(wage)
 = log(annual labor income/annual hours worked)

weeks unemployed
 = annual number of weeks respondent reports being unemployed or laid off

1.4 Labor Income Volatility Estimation Details

In this section I provide the estimation details of the individual volatility measures which I utilize. I first estimate the equation for all individuals in the panel from 1977 to 1992,

$$y_{it} - y_{it-3} = age_{it} \Phi_1 + age_{it} * X_{it} \Phi_2 + \gamma_t + \varepsilon_{it},$$

Where:

y_{it} is the labor income variable for individual i in period t

age_{it} is a matrix containing the variables age , age^2 , age^3 , and age^4 for individual i in period t

X_{it} is a matrix containing the dummy variables *high school graduate only*, *some college*, *college graduate*, and *black* for individual i in period t

γ_t are year fixed effects

The next step is to use the coefficient estimates to obtain an estimate of the residual for individual i in period t as $\hat{\varepsilon}_{it}$, square them to obtain $\hat{\varepsilon}_{it}^2$, I take the average for each individual and divide by two to obtain $\hat{\sigma}_i^2$, the measure of labor income volatility which I use in the analysis.

2. Data Appendix for Chapter 2

2.1 Sample Selection Criteria for the Samples Used

Our data set combines self-reported information from the Background Information Form, completed at or near the time of random assignment and the First Follow-Up Survey, collected around 18 months after random assignment with administrative data on quarterly earnings from matched UI wage records.

The full experimental sample contains 6639 observations in the control group and 13972 observations in the treatment group. If I restrict our sample to only those with valid self-reported earnings for the 18 months after random assignment I lose 2080 observations from the control group and 4329 observations from the treatment group. If I instead restrict the sample to only those with valid UI earnings over the six quarters after random assignment I lose 122 observations from the control group and 232 observations from the treatment group. I only require sample members to have valid values for earnings for the analysis in question; that is, I use all available observations for a given dependent variable. The analyses presented in Tables 5 to 13 require only the data from the experimental treatment group.

Our self-reported earnings data consists of the self-reported data used in Bloom et al. (1993), the official 18-month impact report. The data I use include the recoded values for outliers (which were examined individually and by hand by staff of Abt Associates) but do not include the imputed values based on the matched UI earnings records that they employed in some of their analyses. This earnings variable is not available on the public use CD but is available from the authors by request.

The matched administrative data from UI records consists of earnings in each calendar quarter. As a result, for some sample members, the six calendar quarters after the calendar quarter of random assignment (the period used in some of our dependent variables from the UI data) will cover a slightly different set of months than the 18 months after the month after random assignment (the period covered in some of our dependent variables from the self-reported data).

I do not drop observations with missing values of covariates from the sample for any of our analyses; instead I include dummy variables for those with missing values of the covariates used in each analysis. If I had instead listwise deleted observations from the sample having any missing value for the covariates I would lose 18327 observations out of the 20601 observations in the full experimental sample.

2.2 Variable Definitions

Predicted impact: This consists of the experimentally estimated predicted impact of the program for an individual based on either the individual's measured characteristics or the individual's quantile in the untreated outcome distribution.

Percent positive self-evaluation: This is the mean of a binary indicator for a positive participant self-evaluation. It is defined only for individuals in the treatment group.

Earnings one: This is total earnings over the 18 months after random assignment based on the self-reported earnings data.

Employment one: This is a binary variable indicating any employment over the 18 months after random assignment using self-reported earnings data. The variable equals one if self-reported earnings over the 18 months after random assignment are positive and zero otherwise.

Earnings two: This is total earnings in the 18th month after random assignment based on the self-reported earnings data.

Employment two: This is a binary variable indicating employment in month 18 after random assignment based on the self-reported earnings data. The variable equals one if self-reported earnings in the 18th month after random assignment are positive and zero otherwise.

Earnings three: This is total earnings in the six calendar quarters after the calendar quarter of random assignment based on the matched UI administrative earnings data.

Employment three: This is a binary variable indicating any employment over the six calendar quarters after the calendar quarter of random assignment based on the matched UI administrative earnings data. This variable equals one if UI earnings over the six calendar quarters after the calendar quarter of random assignment are positive and zero otherwise.

Earnings four: This is total earnings in month 18 after random assignment based on the matched UI administrative earnings data.

Employment four: This is a binary variable indicating any employment in the sixth calendar quarter after the calendar quarter of random assignment based on the matched UI administrative earnings data. This variable equals one if UI earnings in the sixth calendar quarter after random assignment are positive and zero otherwise.

**Table 1.1: Transitory Variance of Log Labor plus Social Insurance Income,
by Race and Education**

	Transitory Variance
White	21.10 (1.10)
Black	35.92 (4.28)
<i>F- test: Difference</i>	11.25 [0.0008]
White High School Dropout	28.34 (3.22)
White High School Graduate Only	19.95 (1.97)
White Some College	19.29 (1.84)
White College Graduate	19.84 (2.05)
<i>F- test: Difference</i>	2.17 [0.0901]
Black High School Dropout	45.46 (6.09)
Black High School Graduate Only	33.10 (7.53)
Black Some College	28.41 (11.19)
Black College Graduate	9.45 (2.85)
<i>F- test: Difference</i>	11.31 [0.0000]
<i>N</i>	1821

Notes: Source: Authors calculations using the PSID data. The entries in the tables are the means and standard errors (in parentheses) multiplied by 100. The sample contains 1259 white individuals and 562 black individuals. Log annual labor and social insurance income is measured in 1992 dollars using the GDP consumption deflator.

Table 1.2: The Effect of Education on Labor Income Risk, By Race
 Dependent Variable: Transitory Variance of Log Labor plus Social Insurance Income
 OLS Estimates (Standard Errors)

Race: Model:	White		Black	
	(1)	(2)	(1)	(2)
High School Graduate Only	-5.71 (3.75)	-6.08 (3.74)	-17.91** (8.88)	-20.75** (7.85)
Some College	-4.96 (3.57)	-5.48 (3.61)	-15.34 (13.66)	-26.12** (12.07)
College Graduate	-4.85 (3.76)	-5.28 (3.87)	-42.21** (9.13)	-30.67** (7.86)
Risk Aversion	-1.52** (0.50)	-1.47** (0.51)	-2.86 (2.37)	-1.40 (2.63)
Age	-1.50 (1.43)	-1.30 (1.53)	-2.21 (1.59)	-1.00 (1.71)
Age Squared	0.03 (0.02)	0.02 (0.02)	0.02 (0.02)	0.00 (0.02)
Demographic and Regional controls	N	Y	N	Y
<i>N</i>	1259	1259	562	562

Notes: Source: Authors calculations using the PSID data. Estimates and standards errors (in parentheses) presented are multiplied by 100. * indicates that the coefficient is significantly different from zero at the 10% level. ** indicates that the coefficient is significantly different from zero at the 5% level. All specifications also include a constant, age, age squared, a missing risk tolerance measure dummy, and a seven dummies for the years in included in the sample. Demographic and regional controls include: dummies for marital status, a variable for the number of members in the household, three dummy variables for the census regions, and a missing census region dummy. All estimates weight observations by sampling weights. Log annual labor income is measured in 1992 dollars using the GDP consumption deflator.

Table 1.3: The Effect of High School Graduation on Labor Income Risk, By Race
 Dependent Variable: Transitory Variance of Log Labor plus Social Insurance Income
 OLS and IV Estimates (Standard Errors)

Race:	White		Black	
Model:	(1)	(2)	(1)	(2)
<i>PANEL A: OLS</i>				
High School Graduate Plus	-5.17 (3.38)	-5.62 (3.39)	-19.80** (8.90)	-23.08** (7.25)
Risk Aversion	-1.54** (0.51)	-1.48** (0.51)	-3.19 (2.48)	-1.62 (2.79)
Age	-1.45 (1.39)	-1.24 (1.48)	-2.13 (1.69)	-1.04 (1.71)
Age Squared	0.02 (0.02)	0.02 (0.02)	0.02 (0.02)	0.01 (0.02)
Demographic and Regional Controls	N	Y	N	Y
<i>N</i>	1259	1259	562	562
<i>PANEL B: IV</i>				
High School Graduate Plus	7.14 (31.78)	31.25 (50.60)	-58.78** (24.82)	-51.17** (24.17)
Risk Aversion	-1.13 (0.68)	-0.90 (0.70)	-2.48 (2.61)	-0.77 (2.82)
Age	-0.51 (0.72)	-0.95 (1.41)	-2.88 (1.84)	-1.18 (1.77)
Age Squared	0.01 (0.01)	0.02 (0.02)	0.02 (0.02)	0.00 (0.02)
Demographic and Regional controls	N	Y	N	Y
<i>N</i>	1225	1225	548	548
<i>F-Test: First-Stage Excluded Instruments</i>	3.07 [0.0270]	1.31 [0.2705]	8.17 [0.0000]	6.33 [0.0003]

Notes: Source: Authors calculations using the PSID data. Estimates and standards errors (in parentheses) presented are multiplied by 100. * indicates that the coefficient is significantly different from zero at the 10% level. ** indicates that the coefficient is significantly different from zero at the 5% level. All specifications also include a constant, age, age squared, a missing risk tolerance measure dummy, and a seven dummies for the years included in the sample. Demographic and regional controls include: dummies for marital status, a variable for the number of members in the household, three dummy variables for the census regions, and a missing census region dummy. All estimates weight observations by

sampling weights. Log annual labor income is measured in 1992 dollars using the GDP consumption deflator.

Table 1.4: The Effect of Education on Labor Income Risk, By Race
 Dependent Variable: Transitory Variance of Log Labor plus Social Insurance Income
 with Recoded Zero Values Included
 OLS Estimates (Standard Errors)

Race: Model:	White		Black	
	(1)	(2)	(1)	(2)
High School Graduate Only	-9.31 (36.07)	13.22 (35.74)	-39.57 (84.25)	-92.21 (69.74)
Some College	15.80 (37.40)	19.58 (38.17)	-52.74 (106.02)	-103.52 (93.31)
College Graduate	3.01 (37.54)	7.91 (39.51)	-272.71** (81.94)	-237.14** (71.61)
Risk Aversion	0.24 (5.89)	-0.43 (6.17)	7.53 (14.98)	11.32 (14.13)
Age	-10.77 (12.48)	-11.67 (13.11)	-28.45** (15.65)	-40.42 (13.27)
Age Squared	0.26 (0.17)	0.27 (0.18)	0.41** (0.18)	0.52 (0.15)
Demographic and Regional controls	N	Y	N	Y
<i>N</i>	1263	1263	567	567
Mean of Labor Income Risk	164.89		246.49	

Notes: Source: Authors calculations using the PSID data. Estimates and standards errors (in parentheses) presented are multiplied by 100. The labor and social insurance income variable has zero values recoded as one so that the log of labor and social insurance income is defined for those with zero for this variable. * indicates that the coefficient is significantly different from zero at the 10% level. ** indicates that the coefficient is significantly different from zero at the 5% level. All specifications also include a constant, age, age squared, a missing risk tolerance measure dummy, and a seven dummies for the years in included in the sample. Demographic and regional controls include: dummies for marital status, a variable for the number of members in the household, three dummy variables for the census regions, and a missing census region dummy. All estimates weight observations by sampling weights. Log annual labor income is measured in 1992 dollars using the GDP consumption deflator.

Table 1.5: The Effect of Education on Labor Income Risk, By Race
 Dependent Variable: Transitory Variance of Log Labor plus Social Insurance Income
 Minus Federal Income Tax
 OLS Estimates (Standard Errors)

Race: Model:	White		Black	
	(1)	(2)	(1)	(2)
High School Graduate Only	-8.48** (3.75)	-8.79 (3.83)	-13.01 (8.16)	-15.95** (7.19)
Some College	-6.43* (3.87)	-6.94 (3.94)	-9.60 (11.97)	-17.62* (10.95)
College Graduate	-5.77 (4.16)	-6.23 (4.26)	-40.45** (9.23)	-30.90** (7.49)
Risk Aversion	-1.52** (0.54)	-1.46** (0.55)	-3.86* (2.13)	-2.48 (2.44)
Age	-1.53 (1.32)	-1.34 (1.41)	-2.38 (1.45)	-1.96 (1.44)
Age Squared	0.03 (0.02)	0.02 (0.02)	0.02 (0.02)	0.02 (0.01)
Demographic and Regional controls	N	Y	N	Y
<i>N</i>	1259	1259	562	562

Notes: Source: Authors calculations using the PSID data. Estimates and standards errors (in parentheses) presented are multiplied by 100 * indicates that the coefficient is significantly different from zero at the 10% level. ** indicates that the coefficient is significantly different from zero at the 5% level. All specifications also include a constant, age, age squared, a missing risk tolerance measure dummy, and a seven dummies for the years in included in the sample. Demographic and regional controls include: dummies for marital status, a variable for the number of members in the household, three dummy variables for the census regions, and a missing census region dummy. All estimates weight observations by sampling weights. Log annual labor income is measured in 1992 dollars using the GDP consumption deflator.

Table 1.6: The Effect of Education on Labor Income Risk, By Race
 Dependent Variable: Transitory Variance of Log Labor Income Only
 OLS Estimates (Standard Errors)

Race: Model:	White		Black	
	(1)	(2)	(1)	(2)
High School Graduate Only	-9.75** (4.88)	-10.33** (4.89)	-3.00 (10.37)	-2.75 (10.71)
Some College	-10.11** (4.64)	-10.99** (4.77)	-8.33 (13.81)	-13.20 (13.68)
College Graduate	-10.13** (4.79)	-10.75** (4.87)	-37.43** (7.76)	-20.99** (9.83)
Risk Aversion	-2.18** (0.61)	-2.11** (0.62)	-2.12 (2.18)	-1.65 (2.49)
Age	-2.14 (1.83)	-1.94 (1.98)	-2.67 (1.74)	-1.86 (1.85)
Age Squared	0.03 (0.03)	0.03 (0.03)	0.02 (0.02)	0.02 (0.02)
Demographic and Regional controls	N	Y	N	Y
<i>N</i>	1255	1255	550	550

Notes: Source: Authors calculations using the PSID data. Estimates and standards errors (in parentheses) presented are multiplied by 100 * indicates that the coefficient is significantly different from zero at the 10% level. ** indicates that the coefficient is significantly different from zero at the 5% level. All specifications also include a constant, age, age squared, a missing risk tolerance measure dummy, and a seven dummies for the years in included in the sample. Demographic and regional controls include: dummies for marital status, a variable for the number of members in the household, three dummy variables for the census regions, and a missing census region dummy. All estimates weight observations by sampling weights. Log annual labor income is measured in 1992 dollars using the GDP consumption deflator.

Table 1.7: Sources of Labor Income Risk, by Race and Education

	Transitory Variance of Log Wage	Variance of Weeks Unemployed
White	15.88 (0.75)	30.48 (1.92)
Black	19.74 (2.39)	58.92 (5.52)
<i>F- test: Difference</i>	2.38 [0.1228]	23.72 [0.0000]
White High School Dropout	18.30 (2.03)	64.01 (7.21)
White High School Graduate Only	15.20 (1.27)	32.62 (3.46)
White Some College	15.87 (1.44)	22.79 (2.68)
White College Graduate	15.43 (1.45)	14.36 (2.14)
<i>F- test: Difference</i>	0.60 [0.6156]	18.69 [0.0000]
Black High School Dropout	23.76 (4.79)	84.17 (9.40)
Black High School Graduate Only	19.40 (3.35)	43.58 (7.45)
Black Some College	13.38 (2.95)	53.08 (14.29)
Black College Graduate	8.81 (2.52)	4.20 (2.18)
<i>F- test: Difference</i>	3.64 [0.0127]	30.00 [0.0000]
<i>N</i>	1805	1830

Notes: Source: Authors calculations using the PSID data. The entries in the tables are the means and standard errors in parentheses (both multiplied by 100 for log wage). The estimated presented for the variance of log wages are based on a sample of 1255 white and 550 black individuals, and the estimated presented for the variance of weeks unemployed are based on a sample of 1263 whites and 567 blacks. All estimates weight observations by sampling weights. Log wages are measured in 1992 dollars using the GDP consumption deflator.

Table 1.8: The Effect of Education on Wage Risk, By Race
 Dependent Variable: Transitory Variance of Log Wage
 OLS Estimates (Standard Errors)

Race: Model:	White		Black	
	(1)	(2)	(1)	(2)
High School Graduate Only	-2.49 (2.59)	-2.23 (2.64)	-9.12 (7.78)	-10.23 (9.16)
Some College	-1.30 (2.67)	-0.96 (2.71)	-11.84 (7.47)	-12.79 (8.13)
College Graduate	-1.63 (2.75)	-1.16 (2.79)	-18.58** (8.59)	-15.53** (10.89)
Risk Aversion	-0.94** (0.44)	-0.99** (0.44)	-1.20 (1.13)	-0.80 (1.18)
Age	-1.38 (1.25)	-1.53 (1.34)	-1.32** (1.01)	-2.10* (1.15)
Age Squared	0.02 (0.02)	0.02 (0.02)	0.01 (0.01)	0.02 (0.01)
Demographic and Regional controls	N	Y	N	Y
<i>N</i>	1255	1255	550	550

Notes: Source: Authors calculations using the PSID data. Estimates and standards errors (in parentheses) presented are multiplied by 100. * indicates that the coefficient is significantly different from zero at the 10% level. ** indicates that the coefficient is significantly different from zero at the 5% level. All specifications also include a constant, age, age squared, a missing risk tolerance measure dummy, and a seven dummies for the years in included in the sample. Demographic and regional controls include: dummies for marital status, a variable for the number of members in the household, three dummy variables for the census regions, and a missing census region dummy. All estimates weight observations by sampling weights. Log annual labor income is measured in 1992 dollars using the GDP consumption deflator.

Table 1.9: The Effect of Education on Unemployment Risk, By Race
 Dependent Variable: Variance of Weeks Unemployed
 OLS Estimates (Standard Errors)

Race: Model:	White		Black	
	(1)	(2)	(1)	(2)
High School Graduate Only	-32.95** (7.87)	-32.49** (7.92)	-48.22** (13.45)	-49.81** (13.99)
Some College	-42.64** (7.69)	-41.97** (7.71)	-28.09 (17.81)	-32.18 (19.86)
College Graduate	-49.21** (7.47)	-48.24** (7.47)	-77.91** (12.07)	-57.60** (12.87)
Risk Aversion	-0.51 (0.97)	-0.40 (0.99)	-4.69 (2.93)	-3.77 (3.53)
Age	-2.37* (1.27)	-3.04** (1.39)	-7.26** (2.67)	-7.19** (2.64)
Age Squared	0.02 (0.02)	0.03 (0.02)	0.08** (0.03)	0.07** (0.03)
Demographic and Regional controls	N	Y	N	Y
<i>N</i>	1263	1263	567	567

Notes: Source: Authors calculations using the PSID data. * indicates that the coefficient is significantly different from zero at the 10% level. ** indicates that the coefficient is significantly different from zero at the 5% level. All specifications also include a constant, age, age squared, a missing risk tolerance measure dummy, and a seven dummies for the years in included in the sample. Demographic and regional controls include: dummies for marital status, a variable for the number of members in the household, three dummy variables for the census regions, and a missing census region dummy. All estimates weight observations by sampling weights. Log annual labor income is measured in 1992 dollars using the GDP consumption deflator.

Table 1.10: The Effect of Preferences for Risk on Educational Attainment, By Race

Dependent Variable: High School Graduate or More
 Probit Model Marginal Effect Estimates (Standard Errors)

Race: Model:	White				Black			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Risk Aversion	-0.010* (0.006)	-0.006 (0.005)	-0.006 (0.005)	-0.006 (0.005)	0.032* (0.018)	0.034** (0.017)	0.031* (0.017)	0.027* (0.016)
Father Some High School	--	0.024 (0.024)	0.020 (0.024)	0.017 (0.024)	--	0.020 (0.071)	0.032 (0.060)	0.044 (0.056)
Father High School Graduate	--	0.126** (0.024)	0.119** (0.025)	0.110** (0.024)	--	0.036 (0.090)	0.037 (0.080)	0.040 (0.074)
Mother Some High School	--	0.036 (0.023)	0.037* (0.022)	0.040** (0.021)	--	0.069 (0.060)	0.050 (0.058)	0.036 (0.056)
Mother High School Graduate	--	0.117** (0.028)	0.113** (0.028)	0.104** (0.027)	--	0.279** (0.079)	0.258** (0.065)	0.237** (0.060)
Wealth Controls	N	N	Linear	Categorical	N	N	Linear	Categorical
<i>N</i>	1138	1138	1138	1138	449	449	449	449
Mean of High School Graduate +		0.85				0.74		

Notes: Source: Authors calculations using the PSID data. * indicates that the marginal effect is significantly different from zero at the 10% level. ** indicates that the marginal effect is significantly different from zero at the 5% level. The sample is the 1992 cross-section. All specifications also include age, age squared, three regional dummies and a missing region indicator dummy variable. The omitted parent's education categories are no high school attendance. The specifications (2), (3) and (4) also include dummy variables for missing mothers and fathers' education. The specifications in (3) and (4) also include a missing wealth indicator dummy variable. For these specifications the 14 observations (12 whites and 2 blacks) with a missing regional dummy predict success perfectly and are not used. All estimates weight observations by sampling weights.

Table 1.11: The Effect of Preferences for Risk on Educational Attainment, By Race

Dependent Variable: Educational Attainment Category
 Ordered Probit Model Coefficient Estimates (Standard Errors)

Race: Model:	White				Black			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Risk Aversion	-0.077** (0.019)	-0.067** (0.019)	-0.064** (0.019)	-0.065** (0.019)	0.091* (0.052)	0.102** (0.050)	0.089* (0.200)	0.085* (0.050)
Father Some High School	--	0.152 (0.118)	0.140 (0.117)	0.141* (0.118)	--	0.160 (0.212)	0.132 (0.020)	0.161 (0.201)
Father High School Graduate	--	0.558** (0.086)	0.538** (0.086)	0.502** (0.087)	--	0.342 (0.208)	0.334 (0.211)	0.338 (0.216)
Mother Some High School	--	0.279** (0.127)	0.296** (0.209)	0.307** (0.129)	--	-0.115 (0.227)	-0.125 (0.218)	0.183 (0.211)
Mother High School Graduate	--	0.675** (0.100)	0.672** (0.100)	0.643** (0.099)	--	0.680** (0.265)	0.638** (0.261)	0.574** (0.273)
Wealth Controls	N	N	Linear	Categorical	N	N	Linear	Categorical
<i>N</i>	1150	1150	1150	1150	451	451	451	451

Notes: Source: Authors calculations using the PSID data. * indicates that the coefficient is significantly different from zero at the 10% level. ** indicates that the coefficient is significantly different from zero at the 5% level. All specifications also include age, age squared, three regional dummies and a missing region indicator dummy variable. The sample is the 1992 cross-section. The omitted parent's education categories are no high school attendance. The specifications (2), (3) and (4) also include dummy variables for missing mother and father's education. The specifications in (3) and (4) also include a missing wealth indicator dummy variable. All estimates weight observations by sampling weights.

**Table 1.12: The Effect of Preferences for Risk on Educational Attainment
With Wealth Interactions, By Race**

Dependent Variable: Educational Attainment Category
Ordered Probit Model Coefficient Estimates (Standard Errors)

Race:	White		Black	
Model:	(3)	(3)	(3)	(3)
Risk Aversion	-0.053 (0.037)	-0.035 (0.038)	-0.057 (0.074)	-0.058 (0.081)
Risk Aversion*	-0.030 (0.053)	-0.028 (0.053)	-0.179 (0.119)	-0.113 (0.118)
Very Low Wealth				
Risk Aversion*	-0.031 (0.053)	-0.030 (0.054)	0.444** (0.118)	0.423** (0.123)
Low Wealth				
Risk Aversion *	-0.023 (0.050)	-0.061 (0.051)	0.160 (0.124)	0.192 (0.123)
Moderate Wealth				
Very Low Wealth	-0.713** (0.267)	-0.547** (0.266)	-0.396 (0.648)	-0.543 (0.581)
Low Wealth	-0.465* (0.275)	-0.325 (0.218)	-2.84** (0.642)	-2.56** (0.667)
Moderate Wealth	-0.150 (0.254)	0.047 (0.260)	-1.129** (0.635)	-1.135** (0.612)
Parent's Education	N	Y	N	Y
Controls				
<i>N</i>	1150	1150	451	451

Notes: Source: Authors calculations using the PSID data. * indicates that the coefficient is significantly different from zero at the 10% level. ** indicates that the coefficient is significantly different from zero at the 5% level. All specifications also include age, age squared, three regional dummies, a missing region indicator dummy, a missing wealth indicator variable and an interaction between missing wealth and risk aversion variable. The sample is the 1992 cross-section. The wealth category is high wealth. The specifications (4) also include dummy variables for missing mother and father's education. All estimates weight observations by sampling weights.

Table 1.A1: Sample Statistics

Year:	1977	1992
Log (Annual Labor Income)	10.218 (0.018)	10.357 (0.026)
Log (Annual Labor + Social Insurance Income)	10.236 (0.018)	10.312 (0.028)
Log (Annual Labor + Social Insurance Income – Income Tax)	10.072 (0.017)	10.170 (0.026)
Log (Labor Income/Hours Worked)	2.588 (0.015)	2.763 (0.026)
Annual Weeks Unemployed	1.913 (0.174)	1.619 (0.196)
Risk Aversion	4.606 (0.071)	4.586 (0.070)
Black	0.071 (0.005)	0.086 (0.007)
High School Graduate Only	0.343 (0.013)	0.270 (0.013)
Some College	0.211 (0.011)	0.236 (0.012)
College Graduate	0.267 (0.013)	0.338 (0.014)
Age	35.047 (0.255)	44.561 (0.208)
Married	0.803 (0.011)	0.807 (0.011)
Single	0.096 (0.008)	0.056 (0.007)
Divorced or Widowed or Separated	0.101 (0.008)	0.137 (0.010)
Household Size	3.450 (0.044)	3.078 (0.042)
Census Region 1: Northeast	0.239 (0.012)	0.212 (0.012)
Census Region 2: North Central	0.308 (0.013)	0.279 (0.013)
Census Region 3: South	0.288 (0.012)	0.308 (0.013)
Census Region 4: West	0.158 (0.010)	0.190 (0.011)
<i>N</i>	1830	1601

Notes: Source: Authors calculations using the PSID data. The entries in the table are means of the variables with the standard errors in parentheses. All statistics are weighted by sampling weights. All monetary variables are measured in 1992 dollars using the GDP consumption deflator

Table 1.A2: Tabulation of Annual Sample Size, by Race and Education

Race:	White		Black	
Education:	HS Dropout	HS Graduate	HS Dropout	HS Graduate
1977	227	1036	296	271
1978	214	1048	281	285
1979	213	1051	268	296
1980	210	1056	253	313
1981	202	1066	248	322
1982	197	1072	244	325
1983	197	1072	235	336
1984	214	1081	208	358
1985	213	1079	208	359
1986	209	1062	202	348
1987	201	1048	197	338
1988	197	1031	192	331
1989	193	1025	184	327
1990	187	1006	182	312
1991	186	1002	175	308
1992	181	969	164	287
% change in observation counts between 1977 and 1992	-20.3 %	-6.5 %	-44.5 %	5.9 %

Source: Author's Calculations using PSID final sample from 1977 to 1992.

Table 1.A3: Tabulation of Risk Aversion Measure, by Race and Education

Race: Education:	White		Black	
	HS Dropout	HS Graduate +	HS Dropout	HS Graduate +
1.75	18	163	14	35
2.85	9	120	16	18
3.57	12	125	7	28
6.66	65	369	39	104
Missing	77	192	88	102

Notes: Source: Authors calculations using the PSID data for the 1992 cross-section.

Exhibit 2.1: JTPA Self-Evaluation Survey Questions

(D7)

According to (LOCAL JTPA PROGRAM NAME) records, you applied to enter (LOCAL JTPA PROGRAM NAME) in (MONTH/YEAR OF RANDOM ASSIGNMENT). Did you participate in the program after you applied?

YES (SKIP TO D9)

NO (GO TO D8)

(D9)

Do you think that the training or other assistance that you got from the program helped you get a job or perform better on the job?

YES

NO

Source: JTPA First Follow-Up Study Survey Instrument

Table 2.1: Bivariate Results for the relationship between Experimental Impacts and Positive Self-Evaluation, By Demographic Group

	Percentage Positive Self- Evaluation	Earnings One	Employ One	Earnings Two	Employ Two	Earnings Three	Employ Three	Earnings Four	Employ Four
Adult Males	0.63 (0.01)	538.20 (379.22)	0.03 (0.01)	23.58 (28.55)	0.02 (0.02)	-36.42 (293.50)	0.00 (0.01)	-24.10 (65.69)	-0.03 (0.02)
Adult Females	0.65 (0.01)	750.87 (236.17)	0.03 (0.01)	56.79 (18.34)	0.04 (0.14)	594.08 (195.48)	0.04 (0.01)	131.24 (44.18)	0.03 (0.01)
Male Youth	0.67 (0.02)	-777.33 (463.33)	0.01 (0.01)	-82.93 (37.00)	-0.03 (0.02)	-381.03 (328.19)	-0.02 (0.02)	-128.07 (73.54)	-0.03 (0.02)
Female Youth	0.72 (0.01)	-44.89 (295.12)	0.04 (0.02)	8.38 (29.87)	-0.00 (0.02)	-233.74 (227.97)	0.01 (0.02)	-13.84 (50.74)	0.00 (0.02)
Correlation with Positive Self-Evaluation	--	-0.4620 [0.538]	0.5510 [0.449]	-0.2239 [0.776]	-0.4553 [0.545]	-0.4381 [0.562]	-0.1486 [0.851]	-0.1858 [0.814]	0.1426 [0.857]

Notes: Source: Authors' calculations using the NJS data. Values in the table are means for Positive Self-Evaluation, and experimental impacts for the eight outcomes. The values in parentheses are standard errors and the values in square brackets are p-values. Percentage Positive Self-Assessment is calculated as the mean of the binary indicator positive self-assessment variable for those who self-report participating and are in the treatment group. Earnings one and employment one are earnings and any employment over the 18-months after random assignment using self-reported earnings data. Earnings two and employment two are earnings and employment in month 18 after random assignment using self-reported earnings data. Earnings three and employment three are earnings and any employment over the 18-months after random assignment using UI-reported earnings data. Earnings four and employment four are earnings and employment in month 18 after random assignment using UI-reported earnings data. Those with missing outcomes are dropped from the estimate for that

outcome only.

Table 2.2A: Bivariate results for the Correlation between Experimental Impacts and Self-Evaluation for Eight Outcomes, Adult Males

	Earnings One	Employment One	Earnings Two	Employment Two	Earnings Three	Employment Three	Earnings Four	Employment Four
Race	0.1742 [0.826]	-0.3556 [0.644]	-0.2184 [0.782]	-0.0346 [0.965]	0.7508 [0.249]	0.6956 [0.304]	0.7023 [0.298]	0.6447 [0.355]
Age Category	0.9974 [0.046]	-0.9989 [0.030]	0.9870 [0.103]	-0.9137 [0.266]	0.8198 [0.388]	-0.0455 [0.971]	0.7573 [0.453]	0.0169 [0.989]
Education Category	0.4984 [0.393]	-0.7003 [0.188]	0.7613 [0.135]	-0.3996 [0.505]	0.9077 [0.033]	0.1717 [0.783]	0.9798 [0.003]	-0.8810 [0.048]
Marital Status	-0.5476 [0.631]	-0.9999 [0.007]	-0.9946 [0.066]	-0.9404 [0.229]	0.9939 [0.070]	-0.2063 [0.868]	0.7701 [0.440]	0.6103 [0.582]
Employ Category	-0.1606 [0.897]	-0.8638 [0.336]	-0.1177 [0.925]	-0.5880 [0.600]	-0.6909 [0.514]	0.3855 [0.748]	-0.3794 [0.752]	-0.9451 [0.212]
Site	0.3380 [0.200]	0.1495 [0.581]	0.1682 [0.533]	0.4170 [0.108]	-0.2132 [0.428]	-0.1497 [0.580]	-0.1015 [0.709]	0.0265 [0.923]

Positive Correlations

Overall: 24 of 48 (50 %); significant at 0.10: 4 of 48 (8 %); significant at 0.05: 3 of 48 (6 %)

Negative Correlations

Overall: 24 of 48 (50 %); significant at 0.10: 4 of 48 (8 %); significant at 0.05: 3 of 48 (6 %)

Notes: Source: Authors' calculations using the NJS data. Values in the table are the correlation between the mean of Positive Self-Evaluation, and the experimental impacts by subgroup. The values in square brackets are p-values. Percentage Positive Self-Evaluation is calculated as the mean of the binary indicator positive self-evaluation variable for those who self-report participating and are in the treatment group. Earnings one and employment one are earnings and any employment over the 18-months after random assignment using self-reported earnings data. Earnings two and employment two are earnings and employment in month 18 after random assignment using self-reported earnings data. Earnings three and employment three are earnings and any employment over the 18-months after random assignment using UI-reported earnings data. Earnings four and employment four are earnings and employment in month 18 after random assignment using UI-reported earnings data. Those with missing outcomes are dropped from the estimate for that outcome only. The categories are defined as the following. Race: White, Black, Hispanic and Other. Age: less than 19 years, 19-21 years, 22-25 years, 26-34 years and 35+ years. Education: under 10 years, 10-11 years, 12 years, 13-15 years and 16+ years. Marital Status: single, married, and divorced/widowed/separated. Employment Status: out of labor force, unemployed, and employed. Site: sixteen site categories.

Table 2.2B: Bivariate results for the Correlation between Experimental Impacts and Self-Evaluation for Eight Outcomes, Adult Females

	Earnings One	Employment One	Earnings Two	Employment Two	Earnings Three	Employment Three	Earnings Four	Employment Four
Race	0.3681 [0.632]	0.4127 [0.587]	-0.2490 [0.751]	-0.2490 [0.751]	0.3618 [0.638]	0.2877 [0.712]	0.4188 [0.581]	0.6203 [0.380]
Age Category	-0.8947 [0.295]	0.1282 [0.918]	-0.9136 [0.267]	-0.9681 [0.161]	0.5726 [0.612]	-0.4926 [0.672]	-0.9490 [0.204]	-0.9597 [0.181]
Education Category	-0.6549 [0.230]	-0.6872 [0.200]	-0.5260 [0.363]	0.1167 [0.852]	-0.1673 [0.788]	0.6492 [0.236]	-0.6820 [0.205]	-0.2442 [0.692]
Marital Status	0.0802 [0.949]	0.8084 [0.401]	0.4329 [0.715]	0.5232 [0.650]	0.4284 [0.718]	0.3944 [0.742]	-0.7971 [0.413]	0.5074 [0.661]
Employ Category	0.9296 [0.240]	0.7264 [0.482]	0.6294 [0.567]	0.2549 [0.836]	0.9978 [0.042]	0.8663 [0.333]	0.2949 [0.809]	0.8199 [0.388]
Site	-0.0745 [0.784]	-0.2296 [0.392]	-0.0628 [0.817]	0.1812 [0.502]	-0.0753 [0.782]	0.1143 [0.674]	-0.0250 [0.927]	0.1923 [0.476]
Positive Correlations								
Overall: 28 of 48 (58 %); significant at 0.10: 0 of 48 (0 %); significant at 0.05: 0 of 48 (0 %)								
Negative Correlations								
Overall: 20 of 48 (42 %); significant at 0.10: 0 of 48 (0 %); significant at 0.05: 0 of 48 (0 %)								

Notes: Source: Authors' calculations using the NJS data. Values in the table are the correlation between the mean of Positive Self-Evaluation, and the experimental impacts by subgroup. The values in square brackets are p-values. Percentage Positive Self-Evaluation is calculated as the mean of the binary indicator positive self-evaluation variable for those who self-report participating and are in the treatment group. Earnings one and employment one are earnings and any employment over the 18-months after random assignment using self-reported earnings data. Earnings two and employment two are earnings and employment in month 18 after random assignment using self-reported earnings data. Earnings three and employment three are earnings and any employment over the 18-months after random assignment using UI-reported earnings data. Earnings four and employment four are earnings and employment in month 18 after random assignment using UI-reported earnings data. Those with missing outcomes are dropped from the estimate for that outcome only. The categories are defined as the following. Race: White, Black, Hispanic and Other. Age: less than 19 years, 19-21 years, 22-25 years, 26-34 years and 35+ years. Education: under 10 years, 10-11 years, 12 years, 13-15 years and 16+ years. Marital Status: single, married, and divorced/widowed/separated. Employment Status: out of labor force, unemployed, and employed. Site: sixteen site categories.

Table 2.2C: Bivariate results for the Correlation between Experimental Impacts and Self-Evaluation for Eight Outcomes, Male Youths

	Earnings One	Employment One	Earnings Two	Employment Two	Earnings Three	Employment Three	Earnings Four	Employment Four
Race	0.471 [0.528]	0.2314 [0.769]	-0.0844 [0.916]	-0.3902 [0.610]	0.2621 [0.738]	0.1283 [0.872]	0.2866 [0.713]	0.2097 [0.790]
Education Category	-0.4412 [0.559]	0.2749 [0.725]	-0.1031 [0.897]	0.8667 [0.133]	0.4519 [0.548]	-0.8642 [0.136]	0.6511 [0.349]	0.9301 [0.070]
Marital Status	-0.8985 [0.289]	-0.8039 [0.406]	-0.9816 [0.122]	-0.2344 [0.849]	0.9262 [0.246]	-0.9926 [0.077]	0.9954 [0.061]	0.9671 [0.164]
Employ Category	0.8770 [0.319]	0.9606 [0.179]	0.7889 [0.421]	0.8352 [0.371]	0.9985 [0.035]	0.8704 [0.328]	0.9865 [0.105]	0.7949 [0.415]
Site	0.3258 [0.236]	-0.1278 [0.637]	0.1120 [0.680]	0.6023 [0.014]	0.2063 [0.443]	0.1062 [0.696]	-0.1386 [0.609]	-0.4438 [0.085]

Positive Correlations

Overall: 27 of 40 (68 %); significant at 0.10: 4 of 40 (10 %); significant at 0.05: 2 of 40 (5 %)

Negative Correlations

Overall: 13 of 40 (32 %); significant at 0.10: 2 of 40 (5 %); significant at 0.05: 0 of 40 (0 %)

Notes: Source: Authors' calculations using the NJS data. Values in the table are the correlation between the mean of Positive Self-Evaluation, and the experimental impacts by subgroup. The values in square brackets are p-values. Percentage Positive Self-Evaluation is calculated as the mean of the binary indicator positive self-evaluation variable for those who self-report participating and are in the treatment group. Earnings one and employment one are earnings and any employment over the 18-months after random assignment using self-reported earnings data. Earnings two and employment two are earnings and employment in month 18 after random assignment using self-reported earnings data. Earnings three and employment three are earnings and any employment

over the 18-months after random assignment using UI-reported earnings data. Earnings four and employment four are earnings and employment in month 18 after random assignment using UI-reported earnings data. Those with missing outcomes are dropped from the estimate for that outcome only. The categories are defined as the following. Race: White, Black, Hispanic and Other. Education: under 10 years, 10-11 years, 12 years, 13-15 years and 16+ years. Marital Status: single, married, and divorced/widowed/separated. Employment Status: out of labor force, unemployed, and employed. Site: sixteen site categories.

Table 2.2D: Bivariate results for the Correlation between Experimental Impacts and Self-Evaluation for Eight Outcomes, Female Youths

	Earnings One	Employment One	Earnings Two	Employment Two	Earnings Three	Employment Three	Earnings Four	Employment Four
Race	-0.7560 [0.244]	-0.2111 [0.789]	-0.7309 [0.269]	-0.5835 [0.417]	-0.5849 [0.415]	0.4595 [0.541]	-0.6132 [0.387]	0.7253 [0.274]
Education Category	-0.9988 [0.000]	-0.9995 [0.000]	-0.9914 [0.001]	-0.9896 [0.001]	-0.9459 [0.015]	-0.9950 [0.000]	-0.9616 [0.009]	-0.9702 [0.006]
Marital Status	-0.4687 [0.690]	-0.1860 [0.881]	0.6774 [0.526]	0.3977 [0.740]	0.8910 [0.300]	-0.1495 [0.905]	0.9998 [0.012]	0.3252 [0.789]
Employ Category	0.8395 [0.366]	-0.5065 [0.662]	-0.8400 [0.365]	-0.9703 [0.156]	0.2660 [0.829]	0.9998 [0.012]	-0.3951 [0.741]	0.1339 [0.915]
Site	0.2620 [0.346]	0.3277 [0.233]	0.2164 [0.439]	0.3086 [0.263]	0.2643 [0.341]	0.1690 [0.547]	0.2554 [0.358]	0.2112 [0.450]

Positive Correlations

Overall: 19 of 40 (47 %); significant at 0.10: 2 of 40 (5 %); significant at 0.05: 2 of 40 (5 %)

Negative Correlations

Overall: 21 of 40 (53 %); significant at 0.10: 8 of 40 (20 %); significant at 0.05: 8 of 40 (20 %)

Notes: Source: Authors' calculations using the NJS data. Values in the table are the correlation between the mean of Positive Self-Evaluation, and the experimental impacts by subgroup. The values in square brackets are p-values. Percentage Positive Self-Evaluation is calculated as the mean of the binary indicator positive self-evaluation variable for those who self-report participating and are in the treatment group. Earnings one and employment one are earnings and any employment over the 18-months after random assignment using self-reported earnings data. Earnings two and employment two are earnings and employment in month 18 after random assignment using self-reported earnings data. Earnings three and employment three are earnings and any employment over the 18-months after random assignment using UI-reported earnings data. Earnings four and employment four are earnings and employment in month 18

after random assignment using UI-reported earnings data. Those with missing outcomes are dropped from the estimate for that outcome only. The categories are defined as the following. Race: White, Black, Hispanic and Other. Education: under 10 years, 10-11 years, 12 years, 13-15 years and 16+ years. Marital Status: single, married, and divorced/widowed/separated. Employment Status: out of labor force, unemployed, and employed. Site: sixteen site categories.

Table 2.3: Regression results for the relationship between Predicted Impacts and Positive Self-Evaluation for Eight Outcomes, By Demographic Group

	Adult Males		Adult Females		Male Youths		Female Youths	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Earnings over 18 Months	-121.04 (134.85)	48.66 (83.41)	45.71 (85.10)	-16.86 (57.75)	-21.95 (244.01)	273.06 (214.97)	-208.51 (89.36)	24.82 (97.87)
Any Employment During 18 Months	-0.009 (0.003)	-0.002 (0.04)	0.001 (0.023)	0.005 (0.004)	-0.003 (0.003)	0.010 (0.006)	-0.005 (0.002)	-0.003 (0.008)
Earnings in Month 18	-21.20 (20.65)	-6.05 (7.73)	2.37 (4.95)	0.97 (3.80)	35.53 (31.26)	-6.67 (16.58)	-2.32 (9.49)	1.54 (10.37)
Employment in Month 18	-0.005 (0.003)	-0.002 (0.004)	-0.004 (0.004)	0.002 (0.003)	-0.003 (0.014)	-0.001 (0.011)	-0.002 (0.001)	-0.003 (0.011)
Earnings (UI) over 6 Quarters	-63.67 (94.48)	-61.17 (85.90)	-85.43 (50.92)	14.51 (35.79)	-71.24 (133.03)	271.47 (134.34)	-103.53 (68.76)	78.86 (68.61)
Any Employment (UI) During 6 Quarters	-0.003 (0.002)	-0.002 (0.003)	0.001 (0.003)	0.002 (0.003)	-0.007 (0.007)	0.000 (0.006)	0.003 (0.013)	0.007 (0.006)
Earnings (UI) in Quarter 6	-22.56 (23.25)	-14.52 (17.18)	0.73 (10.96)	-10.17 (7.65)	-0.16 (18.90)	80.59 (31.78)	2.60 (13.53)	-13.14 (12.60)
Employment (UI) in Quarter 6	-0.004 (0.003)	0.000 (0.004)	0.000 (0.003)	-0.004 (0.003)	0.008 (0.005)	0.019 (0.010)	-0.001 (0.002)	0.007 (0.009)
Positive (overall / 0.10 / 0.05)	0/0/0	1/0/0	5/0/0	5/0/0	2/0/0	5/3/2	2/0/0	5/0/0
Negative (overall / 0.10 / 0.05)	8/1/1	6/0/0	2/1/0	3/0/0	6/0/0	2/0/0	5/2/2	3/0/0

Notes: Source: Authors' calculations using the NJS data. Each cell in the table is a coefficient estimate from the regression of the estimated impacts for an individual (based on their X) as the dependent variable and self-evaluation as the independent variable. The population used is the treatment sample. The values

in parentheses are the heteroskedastic-consistent standard errors. The values in the bottom two rows are the counts of the number of cells in the column above which are positive or negative, and counts of those that are significantly different from zero at the 10% and 5% levels. Specification (1) selects the set of X's used to predict the impacts for each individual by a stepwise procedure. Specification (2) uses the specification of X's used in Heckman, Heinrich and Smith (2003) (HHS) to predict the impacts for each individual. The HHS set of X's contains: are race, age, education, marital status, employment status, AFDC receipt, receipt of food stamps and site.

Table 2.4: Relationship between Quantile Treatment Effects for 18-Month Earnings and the Percent with Positive Self-Evaluation, By Demographic Group

	Adult Males		Adult Females		Male Youths		Female Youths	
	Quantile Treatment Effects	Percentage Positive Self-Evaluation	Quantile Treatment Effects	Percentage Positive Self-Evaluation	Quantile Treatment Effects	Percentage Positive Self-Evaluation	Quantile Treatment Effects	Percentage Positive Self-Evaluation
5 th	0 (0.90)	0.51 (0.03)	0 (0.38)	0.57 (0.02)	0 (1.15)	0.56 (0.07)	0 (1.07)	0.68 (0.04)
25 th	1233 (452)	0.66 (0.05)	501 (193)	0.63 (0.04)	-516 (515)	0.62 (0.08)	402 (193)	0.72 (0.06)
50 th	825 (608)	0.56 (0.05)	747 (416)	0.63 (0.04)	-1161 (681)	0.83 (0.06)	-39 (371)	0.71 (0.07)
75 th	8 (590)	0.72 (0.05)	938 (383)	0.78 (0.04)	-1261 (701)	0.68 (0.08)	-479 (566)	0.83 (0.06)
95 th	1589 (1323)	0.65 (0.05)	1910 (740)	0.70 (0.04)	-887 (1959)	0.81 (0.07)	-53 (1012)	0.64 (0.07)
Correlation with Percentage Positive Self-Evaluation	0.0760 [0.750]	--	0.7652 [0.000]	--	-0.4527 [0.045]	--	-0.4209 [0.065]	--
Coefficient on Percentage Positive Self-Evaluation	511 (1686)	--	5489 (1204)	--	-2232 (909)	--	-1576 (931)	--

Notes: Source: Authors' calculations using the NJS data. The values in the left column of the upper panel for each demographic group are quantile treatment effects estimates with standard errors in parentheses for five quantiles. The values in the right column of the upper panel for each demographic group are the means of the binary positive self-evaluation indicator variable for each quantile of the outcome distribution for those in the treatment group. The first row of the lower panel contains the correlation between the treatment effect estimates and the percentage positive self-evaluation by quantile (where one observation is one of the 20 quantiles) and the p-value for the correlation is in square brackets. The second row of the lower panel contains the coefficient of the regression with percentage positive self-evaluation as the independent variable and the treatment effect estimate as the dependant variable (where one observation is one of the 20 quantiles). The hetero-skedastic consistent standard errors for these estimates appear in parentheses.

**Table 2.5: Logit Estimates of the Determinants of Positive Self-Evaluation,
By Demographic Group**

	Adult Males	Adult Females	Male Youths	Female Youths
Age: 19-21 Years	--	--	0.026 (0.039) [0.505]	-0.036 (0.031) [0.255]
Age: 26-34 Years	-0.020 (0.029) [0.500]	-0.023 (0.026) [0.383]	--	--
Age: 35+ years	-0.071 (0.034) [0.036]	-0.122 (0.032) [0.000]	--	--
Marital Status: Married	-0.035 (0.032) [0.283]	0.039 (0.030) [0.188]	-0.009 (0.065) [0.889]	-0.013 (0.048) [0.794]
Marital Status: Divorced/Widowed/ Separated	-0.032 (0.032) [0.327]	-0.009 (0.025) [0.706]	0.204 (0.119) [0.086]	-0.145 (0.053) [0.006]
Education: 10-11 Years	0.049 (0.035) [0.155]	-0.022 (0.032) [0.487]	0.040 (0.044) [0.364]	0.083 (0.035) [0.018]
Education: 12 Years	0.048 (0.032) [0.137]	-0.051 (0.029) [0.081]	-0.003 (0.049) [0.944]	0.082 (0.038) [0.030]
Education: 13-15 Years	-0.005 (0.041) [0.900]	-0.035 (0.037) [0.337]	-0.075 (0.090) [0.406]	0.102 (0.059) [0.066]
Education: 16+ Years	0.022 (0.057) [0.705]	0.065 (0.058) [0.266]	--	--
Race: Black	0.020 (0.034) [0.543]	-0.016 (0.030) [0.598]	0.122 (0.045) [0.007]	-0.035 (0.043) [0.425]
Race: Hispanic	0.048 (0.047) [0.313]	-0.001 (0.042) [0.979]	0.159 (0.052) [0.002]	0.103 (0.048) [0.031]
Race: Other	0.035 (0.075) [0.646]	-0.054 (0.072) [0.458]	0.039 (0.130) [0.764]	0.017 (0.106) [0.872]
English Language	0.051	0.106	0.135	-0.058

	(0.070)	(0.059)	(0.107)	(0.158)
	[0.471]	[0.073]	[0.210]	[0.715]
AFDC Receipt	0.039	-0.015	0.085	-0.007
	(0.042)	(0.024)	(0.052)	(0.037)
	[0.352]	[0.521]	[0.097]	[0.850]
Work for Pay	0.037	0.039	-0.059	0.015
	(0.045)	(0.028)	(0.053)	(0.036)
	[0.415]	[0.164]	[0.264]	[0.697]
Child less than Six	-0.019	0.003	-0.146	0.003
	(0.033)	(0.023)	(0.073)	(0.035)
	[0.571]	[0.900]	[0.044]	[0.940]
Self-Report	0.135	0.122	0.084	0.010
Training: CT-OS	(0.028)	(0.023)	(0.040)	(0.034)
	[0.000]	[0.000]	[0.038]	[0.764]
Self-Report	0.153	0.115	0.109	0.081
Training: OJT/WE	(0.040)	(0.034)	(0.062)	(0.055)
	[0.000]	[0.001]	[0.076]	[0.138]
Self-Report	0.085	0.022	0.000	0.029
Training: JSA	(0.042)	(0.038)	(0.088)	(0.066)
	[0.044]	[0.570]	[0.998]	[0.661]
Self-Report	0.046	0.019	0.075	0.095
Training: ABE	(0.049)	(0.038)	(0.049)	(0.040)
	[0.347]	[0.614]	[0.129]	[0.018]
Self-Report	0.153	0.048	0.079	0.090
Training: Other	(0.053)	(0.048)	(0.082)	(0.061)
	[0.004]	[0.313]	[0.334]	[0.140]
Administrative-	0.039	-0.005	-0.023	0.154
Report Training:	(0.058)	(0.046)	(0.104)	(0.052)
CT-OS	[0.493]	[0.916]	[0.824]	[0.003]
Administrative-	0.055	0.088	-0.084	0.067
Report Training:	(0.058)	(0.048)	(0.113)	(0.067)
OJT/WE	[0.340]	[0.066]	[0.459]	[0.313]
Administrative-	0.036	-0.045	0.077	0.062
Report Training:	(0.058)	(0.052)	(0.099)	(0.063)
JSA	[0.532]	[0.387]	[0.433]	[0.326]
Administrative-	0.108	-0.122	-0.184	-0.064
Report Training:	(0.066)	(0.068)	(0.108)	(0.078)
ABE	[0.102]	[0.073]	[0.089]	[0.409]
Administrative-	0.050	-0.121	-0.148	0.123
Report Training:	(0.063)	(0.057)	(0.110)	(0.060)
Other	[0.425]	[0.033]	[0.178]	[0.040]

Notes: Source: Authors' calculations using the NJS data. Columns two through five of the table report the results from a logit model where the binary positive self-evaluation variable is the dependant variable and the categorical variables listed in column one are the independent variables. The values in the table are mean numerical derivatives, with the standard errors in parentheses and p-values in square brackets. The population for these regressions is the treatment sample. Indicator variables for missing values for the independent variables are also included in the regression. The omitted age category for adults is age 22-25

years and is age less than 19 for youths. The omitted marital status is single, the omitted education category is less than 10 years, the omitted racial group is white, and the omitted training type for both self-report and administrative report is no training for all demographic groups.

Table 2.6: Test Statistics from Logit Models of the Determinants of Positive Self-Evaluation, By Demographic Group

	Adult Males	Adult Females	Male Youths	Female Youths
Site	65.30 [0.000]	73.06 [0.000]	29.66 [0.009]	61.12 [0.000]
Age Category	5.09 [0.078]	21.71 [0.000]	0.45 [0.504]	1.26 [0.262]
Marital Status	1.44 [0.487]	3.68 [0.159]	1.44 [0.488]	8.55 [0.014]
Education Category	4.50 [0.343]	6.47 [0.167]	2.46 [0.482]	6.37 [0.095]
Race	1.20 [0.753]	0.80 [0.849]	10.49 [0.015]	6.36 [0.095]
English Language	0.91 [0.633]	3.16 [0.206]	1.12 [0.290]	0.15 [0.703]
Other Individual Characteristics	6.13 [0.294]	3.16 [0.675]	6.72 [0.242]	1.23 [0.942]
Self-Reported Training Type	30.21 [0.000]	30.66 [0.000]	6.23 [0.284]	7.05 [0.217]
Administrative Reported Training Type	30.67 [0.000]	53.40 [0.000]	21.55 [0.002]	25.61 [0.000]

Notes: Source: Authors' calculations using the NJS data. Columns two through five of the table report the results from a logit model where the binary positive self-evaluation variable is the dependent variable and the categorical variables summarized in column one are the independent variables. The values in the table are χ^2 -statistics for joint tests that all of the coefficients equal zero for a given group of variables, with the p-values in square brackets. The population for these regressions is the treatment sample. The variables in 'Other Individual Characteristics' are AFDC receipt, child less than six indicator, and worked for pay indicator. Indicator variables for missing values for the independent variables are also included in the regressions.

Table 2.7: Logit Estimates of the Relationship between Outcomes and Positive Self-Evaluation: Four Outcomes, By Demographic Group

	Adult Males	Adult Females	Male Youths	Female Youths
Earnings over 18 Months = 0	-0.171 (0.042) [0.000]	-0.184 (0.038) [0.000]	-0.151 (0.084) [0.073]	-0.065 (0.064) [0.312]
Earnings over 18 Months Bottom Quartile	-0.096 (0.040) [0.015]	-0.094 (0.035) [0.007]	-0.158 (0.062) [0.011]	-0.028 (0.053) [0.593]
Earnings over 18 Months Lower Middle Quartile	-0.038 (0.035) [0.269]	-0.133 (0.034) [0.000]	-0.001 (0.056) [0.992]	0.068 (0.049) [0.172]
Earnings over 18 Months Upper Middle Quartile	-0.083 (0.033) [0.013]	-0.033 (0.034) [0.337]	-0.052 (0.056) [0.358]	0.053 (0.053) [0.319]
Earnings over 18 Months = 0 (UI)	-0.076 (0.041) [0.062]	-0.027 (0.034) [0.423]	-0.063 (0.073) [0.387]	-0.068 (0.064) [0.290]
Earnings over 18 Months Bottom Quartile (UI)	-0.080 (0.039) [0.038]	-0.112 (0.034) [0.001]	-0.120 (0.061) [0.049]	-0.081 (0.057) [0.153]
Earnings over 18 Months Lower Middle Quartile (UI)	-0.068 (0.035) [0.049]	-0.067 (0.032) [0.035]	-0.050 (0.056) [0.368]	-0.089 (0.057) [0.113]
Earnings over 18 Months Upper Middle Quartile (UI)	-0.026 (0.032) [0.420]	-0.035 (0.030) [0.249]	-0.030 (0.059) [0.605]	0.014 (0.054) [0.795]
Any Employment Over 18 Months	0.122 (0.038) [0.001]	0.105 (0.027) [0.000]	0.084 (0.071) [0.237]	0.082 (0.044) [0.060]
Any Employment over 18 Months (UI)	0.038 (0.035) [0.289]	-0.029 (0.025) [0.253]	0.000 (0.054) [1.000]	0.006 (0.039) [0.886]

Notes: Source: Authors' calculations using the NJS data. Columns two through five of this table report the results from logit regressions where the binary positive self-evaluation variable is the dependant variable and the categorical variables listed in column one of Table 5 are the independent variables, in addition an outcome variable is included in each regression. The values in the table are mean numerical derivatives,

with the standard errors in parentheses and p-values in square brackets. For earnings outcomes the continuous variables are entered as four categorical variables: zero earnings, an indicator for being in the lowest quartile of the non-zero earnings distribution, lower middle quartile of the non-zero earnings distribution, upper middle quartile of the non-zero earnings distribution. The omitted category is for those with earnings in the highest quartile of the non-zero earnings distribution. For the employment outcomes a binary variable is included indicating whether the respondent was employed or not. Each set of cells in the table is the result for a different specification where the outcome to be included as an independent variable is different. The sets of cells are defined as two groups of four and two groups of two depending on how the outcome enters the regression. The population for these regressions is the treatment sample. Indicator variables for missing values for the independent variables are also included in the regression.

**Table 2.8: Test Statistics from Logit Models of the Relationship between Outcomes and Positive Self-Evaluation,
By Demographic Group**

	Adult Males	Adult Females	Male Youths	Female Youths
Earnings over 18 Months	20.37 [0.001]	34.33 [0.000]	15.96 [0.007]	12.18 [0.032]
Any Employment during 18 Months	11.38 [0.003]	15.60 [0.000]	5.58 [0.062]	4.51 [0.105]
Earnings in Month 18	15.75 [0.008]	30.55 [0.000]	8.02 [0.155]	1.32 [0.933]
Employment in Month 18	6.69 [0.035]	12.36 [0.002]	5.67 [0.059]	0.99 [0.609]
Earnings over 6 Quarters (UI)	7.37 [0.195]	13.92 [0.016]	4.98 [0.418]	10.06 [0.074]
Any Employment During 6 Quarters (UI)	1.17 [0.556]	1.27 [0.529]	0.39 [0.825]	2.41 [0.300]
Earnings in Quarter 6 (UI)	12.23 [0.032]	12.49 [0.029]	3.53 [0.473]	7.14 [0.211]
Employment in Quarter 6 (UI)	0.98 [0.612]	0.51 [0.776]	0.28 [0.595]	0.68 [0.710]
Earnings in the Month of the Survey	9.85 [0.080]	21.78 [0.001]	15.57 [0.008]	4.65 [0.460]
Employment in the Month of the Survey	8.78 [0.012]	5.59 [0.061]	8.33 [0.016]	1.50 [0.473]
Earnings in the Quarter of the Survey (UI)	10.46 [0.063]	9.00 [0.109]	11.74 [0.039]	3.53 [0.618]
Employment in the Quarter of the Survey	3.45	0.24	10.59	0.82

(UI) [0.178] [0.889] [0.005] [0.664]

Notes: Source: Authors' calculations using the NJS data. Columns two through five of this table report the results from logit models where the binary positive self-evaluation variable is the dependant variable and the categorical variables listed in column one of Table 5 are the independent variables, in addition an outcome variable is included in each regression. Each cell in the table is the result for a different specification where the outcome to be included as an independent variable is different. The values in the table are χ^2 -Statistics for joint tests that all of the coefficients are zero for a given outcome, with the p-values in square brackets. For earnings outcomes the continuous variables are entered as four categorical variables: zero earnings, an indicator for being in the lowest quartile of the non-zero earnings distribution, lower middle quartile of the non-zero earnings distribution, upper middle quartile of the non-zero earnings distribution. The omitted category is for those with earnings in the highest quartile of the non-zero earnings distribution. For the employment outcomes a binary variable is included indicating whether the respondent was employed or not. The population for these regressions is the treatment sample. Indicator variables for missing values for the independent variables are also included in the regression.

Table 2.9: Logit Estimates of the Relationship between Before-After Self-Reported Earnings Changes and Positive Self-Evaluation, By Demographic Group

	Adult Males	Adult Females	Male Youths	Female Youths
Before-After Self Reported Earnings 2 nd Quintile	-0.025 (0.041) [0.540]	-0.008 (0.035) [0.825]	0.030 (0.057) [0.600]	-0.077 (0.059) [0.190]
Before-After Self Reported Earnings 3 rd Quintile	0.033 (0.037) [0.375]	0.055 (0.031) [0.077]	0.107 (0.050) [0.031]	0.015 (0.047) [0.752]
Before-After Self Reported Earnings 4 th Quintile	0.044 (0.038) [0.250]	0.067 (0.031) [0.029]	0.115 (0.049) [0.019]	0.053 (0.045) [0.239]
Before-After Self Reported Earnings 5 th Quintile	0.020 (0.034) [0.547]	0.109 (0.027) [0.000]	0.132 (0.046) [0.004]	0.035 (0.040) [0.379]

Notes: Source: Authors' calculations using the NJS data. "Before-After Self-Reported Earnings" consists of monthly self-reported earnings over the 18 months after random assignment minus monthly self-reported earnings in the 12 months prior to random assignment. The estimates come from logit models with an indicator for a positive self-evaluation as the dependent variable and the before-after earnings change variable and the categorical variables listed in column one of Table 5 as independent variables. The values in the table are mean numerical derivatives, with standard errors in parentheses and p-values in square brackets. The before-after earnings changes enter in the form of indicator variables for being in the 2nd, 3rd, 4th, and 5th quintiles of the before-after earnings change distribution. The omitted category is the 1st quintile of the distribution. The population for these regressions is the treatment group. Indicator variables for missing values for the independent variables are also included in the regression.

Table 2.10: Logit Estimates of the Relationship between Before-After UI Earnings Changes and Positive Self-Evaluation, By Demographic Group

	Adult Males	Adult Females	Male Youths	Female Youths
Before-After UI Reported Earnings 2 nd Quintile	0.008 (0.036) [0.825]	0.006 (0.031) [0.853]	0.164 (0.044) [0.000]	0.029 (0.044) [0.494]
Before-After UI Reported Earnings 3 rd Quintile	0.053 (0.035) [0.124]	0.032 (0.030) [0.275]	0.127 (0.044) [0.004]	-0.021 (0.043) [0.623]
Before-After UI Reported Earnings 4 th Quintile	0.071 (0.034) [0.037]	0.083 (0.028) [0.003]	0.135 (0.044) [0.002]	0.076 (0.039) [0.050]
Before-After UI Reported Earnings 5 th Quintile	0.093 (0.033) [0.005]	0.107 (0.027) [0.000]	0.143 (0.044) [0.001]	0.135 (0.035) [0.000]
Before-After UI (2) Reported Earnings 2 nd Quintile	0.015 (0.051) [0.765]	-0.075 (0.051) [0.143]	0.015 (0.059) [0.792]	0.156 (0.112) [0.163]
Before-After UI (2) Reported Earnings 3 rd Quintile	-0.032 (0.038) [0.407]	-0.060 (0.035) [0.083]	0.021 (0.054) [0.697]	0.024 (0.044) [0.579]
Before-After UI (2) Reported Earnings 4 th Quintile	0.022 (0.030) [0.473]	-0.001 (0.026) [0.960]	0.045 (0.045) [0.326]	0.012 (0.035) [0.723]
Before-After UI (2) Reported Earnings 5 th Quintile	0.065 (0.030) [0.027]	0.085 (0.025) [0.001]	0.073 (0.045) [0.102]	0.092 (0.034) [0.006]

Notes: Source: Authors' Calculations using the NJS data. "Before-After UI Reported Earnings" consist of monthly UI earnings in the six quarters after random assignment minus monthly UI earnings in the 18 months before random assignment. "Before-After UI (2) Reported Earnings" consist of monthly UI earnings in the 6th quarter after random assignment minus monthly UI earnings in the 6th quarter before random assignment. The estimates come from logit models with an indicator for a

positive self-evaluation as the dependent variable and the before-after earnings change variable and the categorical variables listed in column one of Table 5 as independent variables. The values in the table are mean numerical derivatives, with standard errors in parentheses and p-values in square brackets. The before-after earnings changes enter in the form of indicator variables for being in the 2nd, 3rd, 4th, and 5th quintiles of the before-after earnings change distribution. The omitted category is the 1st quintile of the distribution. The population for these regressions is the treatment group. Indicator variables for missing values for the independent variables are also included in the regression.

Table 2.11: Logit Estimates of the Relationship between Before-After Employment Status Changes and Positive Self-Evaluation, By Demographic Group

	Adult Males	Adult Females	Male Youths	Female Youths
Employed Before & Not Employed After	-0.059 (0.102) [0.567]	-0.077 (0.093) [0.408]	-0.502 (0.230) [0.029]	-0.282 (0.139) [0.043]
Not Employed Before & Employed After	0.040 (0.028) [0.161]	0.014 (0.024) [0.570]	-0.112 (0.045) [0.013]	-0.013 (0.034) [0.696]
Always Not Employed	-0.123 (0.060) [0.038]	-0.042 (0.042) [0.319]	0.071 (0.142) [0.616]	-0.073 (0.064) [0.249]

Notes: Source: Authors' calculations using the NJS data. Employment status changes are based on changes in self-reported employment status measured at the date of random assignment and 18 months after random assignment. The omitted category is always employed. The estimates come from logit models with an indicator for a positive self-evaluation as the dependent variable and the before-after employment change variable and the categorical variables listed in column one of Table 5 as independent variables. The values in the table are mean numerical derivatives, with standard errors in parentheses and p-values in square brackets. The population for these regressions is the treatment group. Indicator variables for missing values for the independent variables are also included in the regression.

Table 2.12: Test Statistics from Logit Models of the Relationship between Before-After Estimates and Positive Self-Evaluation, By Demographic Group

	Adult Males	Adult Females	Male Youths	Female Youths
Before-After Self Reported Earnings	3.95 [0.4130]	25.18 [0.0000]	12.18 [0.0160]	7.54 [0.1100]
Before-After UI Reported Earnings	11.25 [0.0239]	23.20 [0.0001]	18.66 [0.0009]	23.06 [0.0001]
Before-After UI (2) Reported Earnings	7.14 [0.1286]	23.04 [0.0001]	2.95 [0.5659]	9.74 [0.0451]
Before-After Employment Status Changes	10.46 [0.0150]	2.90 [0.4070]	11.15 [0.0109]	5.23 [0.1555]

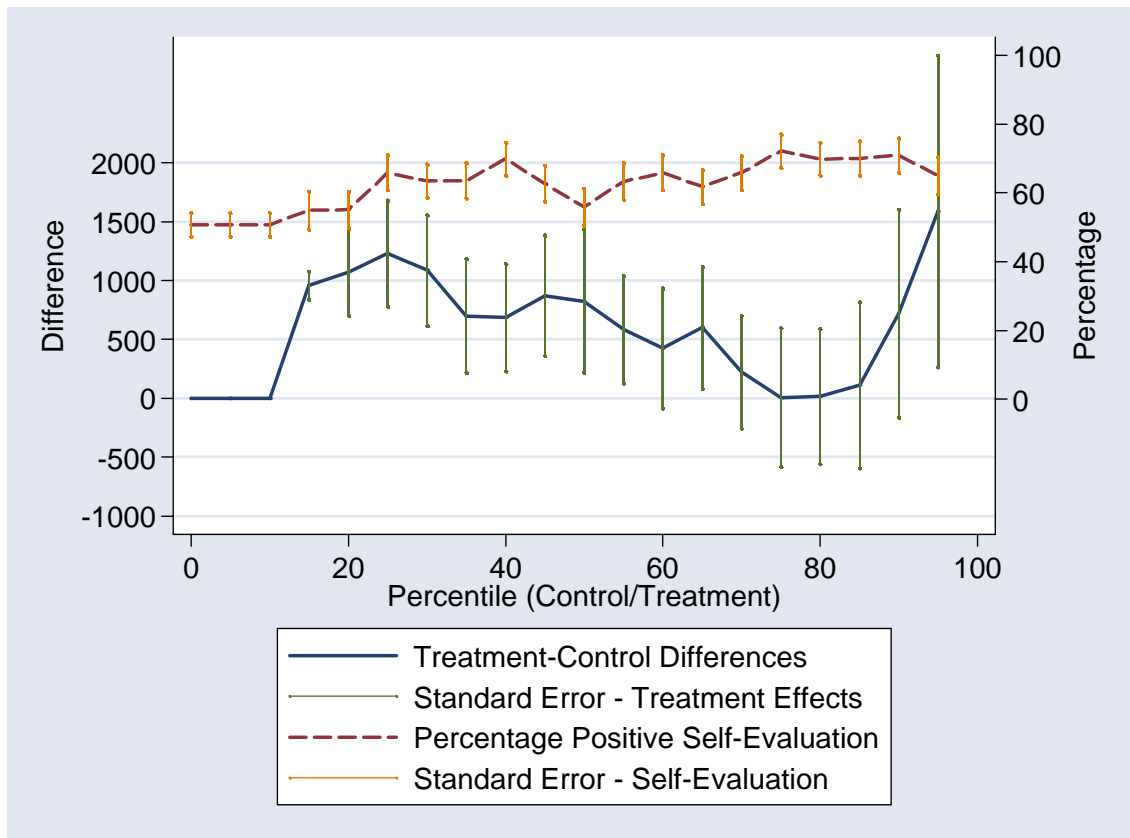
Notes: Source: Authors' Calculations using the NJS data. The values in the table are χ^2 -Statistics for joint tests of the null hypothesis that all of the coefficients equal zero for a given outcome, with the p-values in square brackets. The tests correspond to the estimates presented in Tables 9, 10 and 11. See the notes for those tables for further details.

Table 2.13: Logit Estimates of the Relationship between Positive Self-Evaluation and Performance Standards, By Demographic Group

	Adult Males	Adult Females	Male Youths	Female Youths
<i>A. JTPA:</i>				
Employment at Termination	0.115 (0.034) [0.001] n=1507	0.126 (0.029) [0.000] n=1882	0.083 (0.047) [0.078] n=699	0.091 (0.039) [0.020] n= 890
Wages at Termination	0.012 (0.009) [0.173] n=617	0.028 (0.010) [0.003] n=873	0.026 (0.022) [0.232] n=280	0.021 (0.021) [0.316] n=319
Employment at Follow-up	0.097 (0.035) [0.005] n=1507	0.137 (0.029) [0.000] n=1882	0.043 (0.049) [0.380] n=699	0.076 (0.040) [0.056] n=890
Weekly Earnings at Follow-up	0.007 (0.015) [0.623] n=617	0.040 (0.015) [0.007] n=883	0.051 (0.033) [0.120] n=302	0.065 (0.030) [0.028] n=336
<i>B. WIA:</i>				
Employment at Termination	0.052 (0.036) [0.151] n=1155	0.048 (0.032) [0.122] n=1536	-0.073 (0.054) [0.179] n=528	0.035 (0.051) [0.484] n=705
Employment at 6-Months	0.030 (0.046) [0.511] n=659	-0.069 (0.044) [0.123] n=778	0.131 (0.065) [0.045] n=291	0.143 (0.060) [0.018] n=293
Earnings Gain at 6-Months	0.000 (0.000) [0.340] n=566	0.001 (0.000) [0.022] n=634	0.002 (0.001) [0.079] n=240	0.004 (0.001) [0.003] n=204

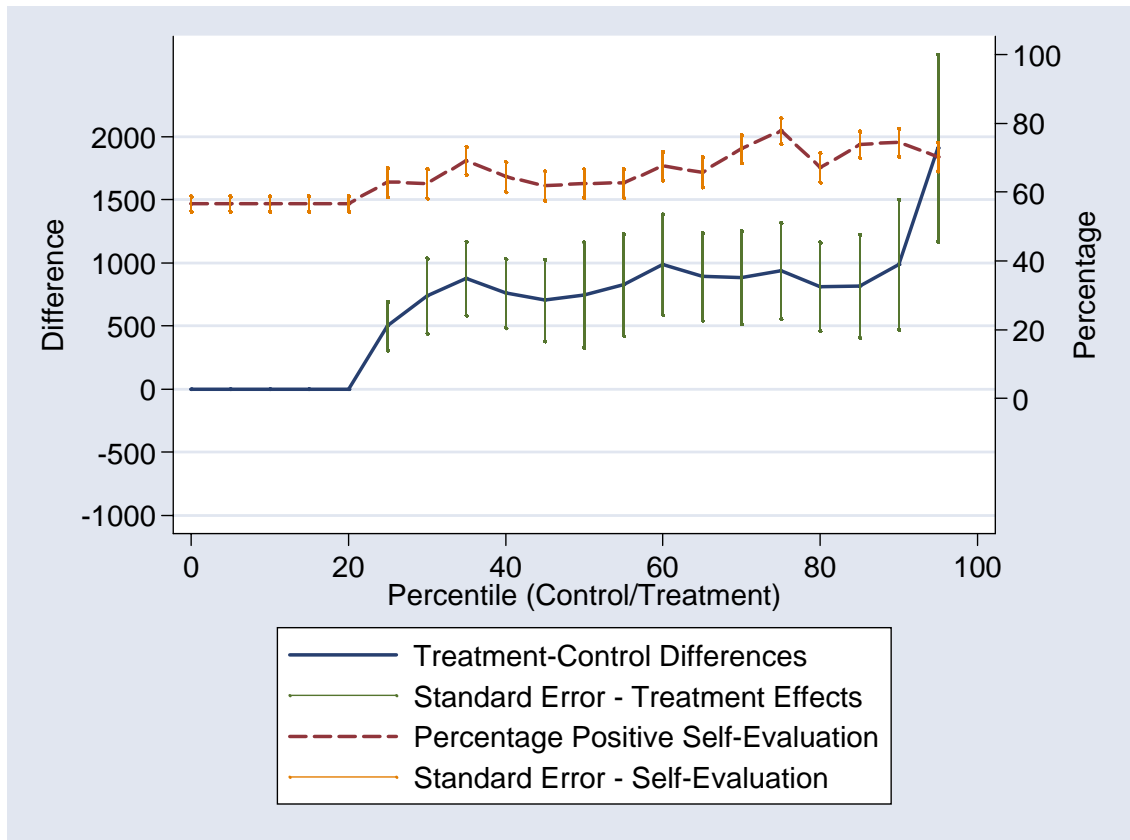
Notes: Source: Authors' calculations using the NJS data. The JTPA performance measures consist of (1) employment at JTPA termination date; (2) employment at follow-up, which is 13 weeks after termination in JTPA; (3) wage per hour at termination date (conditional on employment) in dollars; and (4) the average total weekly earnings at follow up (conditional on employment). Our construction of all of the JTPA performance measures relies on self-reported data. The WIA performance measures consist of (1) employment at exit, which we calculate as non-zero UI earnings in the calendar quarter of termination (conditional on non-employment at the date of random assignment based on self-reported labor force status); (2) employment at six months after termination, which we calculate as non-zero UI earnings in the third calendar quarter after termination (conditional on employment in the first quarter after termination); (3) earnings differences (conditional on employment in the first quarter after termination), which we calculate as the sum of UI earnings in the second and third calendar quarters after program termination minus the sum of earnings in the two calendar quarters prior to random assignment. The estimates in the table correspond to logit models with an indicator for a positive self-evaluation as the dependent variable and one of the performance measures as the only independent variable. The models also include all of the variables listed in Table 5 as additional covariates. The values in the table are mean numerical derivatives, with standard errors in parentheses and p-values in square brackets. We multiply the values for the earnings-based performance measures by 100 for ease of presentation. The final row in each cell gives the sample size for the sample used to produce each estimate. Before deleting observations with missing values of the performance measures, the treatment group samples contain 3067 adult males, 3922 adult females, 1308 male youths, and 1711 female youths. The population for these regressions is the treatment group.

Figure 2.1A: Quantile Treatment Effects and Percentage Reporting Positive Self-Evaluation, Adult Males



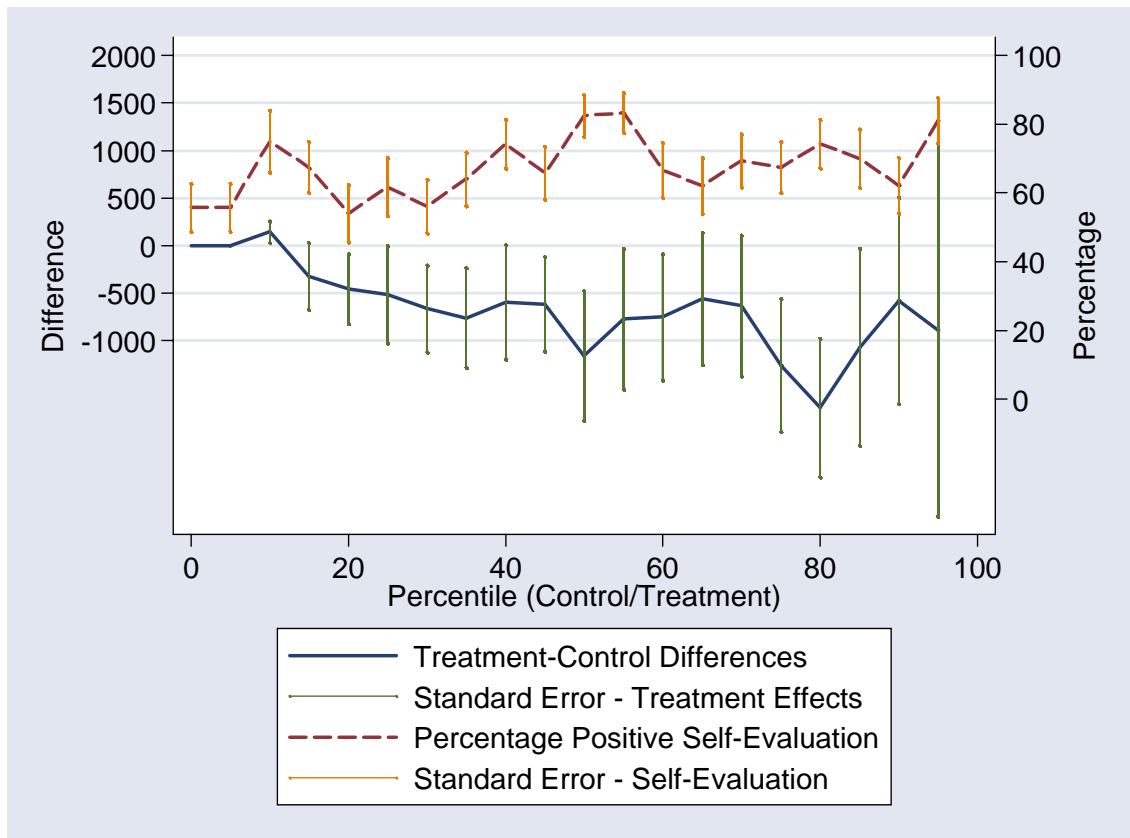
Notes: Source: Authors' Calculations using the JTPA data. The outcome used here is self-reported earnings over the 18months after random assignment.

Figure 2.1B: Quantile Treatment Effects and Percentage Reporting Positive Self-Evaluation, Adult Females



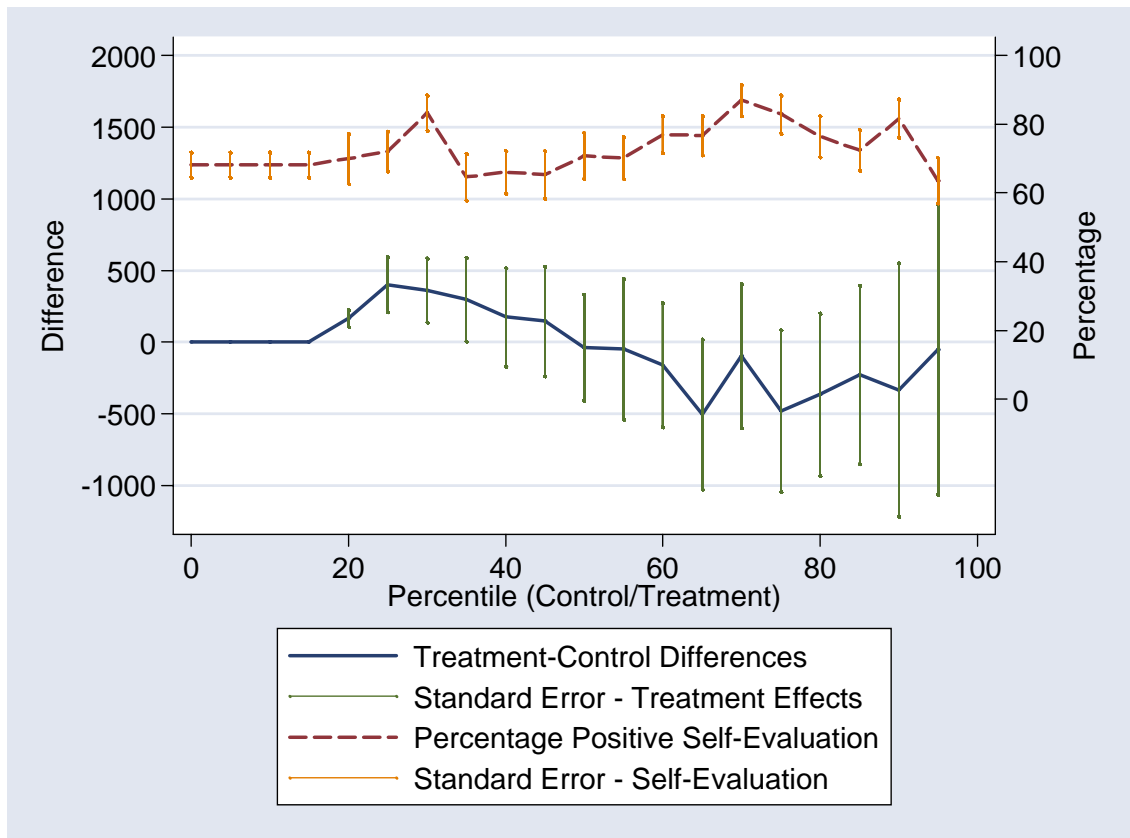
Notes: Source: Authors' Calculations using the JTPA data. The outcome used here is self-reported earnings over the 18months after random assignment.

Figure 2.1C: Quantile Treatment Effects and Percentage Reporting Positive Self-Evaluation, Male Youth



Notes: Source: Authors' Calculations using the JTPA data. The outcome used here is self-reported earnings over the 18months after random assignment.

Figure 2.1D: Quantile Treatment Effects and Percentage Reporting Positive Self-Evaluation, Female Youth



Notes: Source: Authors' Calculations using the JTPA data. The outcome used here is self-reported earnings over the 18months after random assignment.

Bibliography

Abadie, Alberto, Joshua Angrist and Guido Imbens (2002). "Instrumental Variables Estimates of the Effect of Subsidized Training on the Quantiles of Trainee Earnings." *Econometrica*. 70(1): 91-117.

Abbring, Japp, James Heckman and Edward Vytlacil (2005) "Econometric Evaluation of Social Programs" in James Heckman and Edward Leamer (eds.) *Handbook of Econometrics, Volume 6*. Amsterdam: North-Holland: forthcoming.

Abowd, John and David Card (1989) "On The Covariance Structure of Earnings and Hours Changes." *Econometrica*, 57(2), 411-445.

Acemoglu, Daron and Joshua Angrist (2000) "How Large are Human-Capital Externalities? Evidence from Compulsory Schooling Laws." *NBER Macroeconomics Annual*, 15, 9-59.

Angrist, Joshua (2004) "Treatment Effect Heterogeneity in Theory and Practice." *Economic Journal* 114(494): C52-C83.

Ainger, Dennis and Glen Cain (1977) "Statistical Theories of Discrimination in Labor Markets." *Industrial and Labor Relations Review*, 30(2), 175-187.

Altonji, Joseph and Rebecca Black (1999) "Race and Gender in the Labor Market." in O. Ashenfelter and D. Card eds. *Handbook of Labor Economics, Volume 3*, 3143-3251.

Altonji, Joseph and Charles R. Pierret (2001) "Employer Learning and Statistical Discrimination." *Quarterly Journal of Economics*, 116(1), 383-341.

Atanasio, Orazio and Stephen Davis (1996) "Relative Wage Movements and the Distribution of Consumption." *Journal of Political Economy*, 104(6), 1227-1262.

Autor, David and David Scarborough (2005) "Will Job Testing Harm Minority Workers? Evidence From the Retail Sector." *Working Paper*, Massachusetts Institute of Technology, Cambridge, MA.

Barnow, Burt and Jeffrey Smith (2004) "Performance Management of U.S. Job Training Programs: Lessons from the Job Training Partnership Act." *Public Finance and Management* 4(3): 247-287.

Barsky, Robert, Thomas Juster and Miles Kimball (1997) "Preference Parameters and Behavioral Heterogeneity: An Experimental Approach in the Health and Retirement Study." *Quarterly Journal of Economics*, 112(2), 537-579.

- Benabou, Roland (2002) "Tax and Education Policy in a Heterogeneous Agent Economy: What Levels of Redistribution Maximize Growth and Efficiency?" *Econometrica*, 70(Mar), 481-518.
- Bertola, Guiseppe (2004) "A Pure Theory of Job Security and Labor Income Risk." *Review of Economic Studies*, 71, 43-61.
- Bitler, Marianne, Jonah Gelbach and Hilary Hoynes (2004) "What Mean Impacts Miss: Distributional Effects of Welfare Reform Experiments." Unpublished manuscript, University of Maryland.
- Black, Sandra E. and Amir Sufi (2002) "Who Goes to College? Differential Enrollment by Race and Family Background." *Working Paper 9310*, National Bureau of Economic Research, Cambridge, MA.
- Bloom, Howard, Larry Orr, George Cave, Stephen Bell and Fred Doolittle (1993) *The National JTPA Study: Title II-A Impacts on Earnings and Employment at 18 Months*. Bethesda, MD: Abt Associates.
- Bloom, Howard, Larry Orr, Stephen Bell, George Cave, Fred Doolittle, Winston Lin, and Johannes Bos (1997) "The Benefits and Costs of JTPA Title II-A Programs: Findings from the National Job Training Partnership Act Study." *Journal of Human Resources* 32: 549-576.
- Bound, John and Richard B. Freedman (1992) "What Went Wrong? The Erosion of Relative Earnings and Employment Among Young Black Men In The 1980s." *Quarterly Journal of Economics*, February, 201-232.
- Bound, John and Alan Krueger (1991) "Measurement Error in Longitudinal Earnings Data: Do Two Wrongs Make A Right?" *Journal of Labor Economics*, 9(1), 1-24.
- Bound, John, Charles Brown, Greg J. Duncan and William L. Rodgers (1994) "Evidence on the Validity of Cross-Sectional and Longitudinal Labor-Market Data." *Journal of Labor Economics*, 12(3), 345-368.
- Bound, John, Micheal Schoenbaum and Timothy Waidmann (1995) "Disability Status and Racial Differences in Labor Force Attachment." *Journal of Human Resources*, S227-S267.
- Brunnermeier, Markus K. (2004) "Learning to Re-optimize Consumption at New Income Levels: A Rationale for Prospect Theory." *Journal of European Economic Association*, 2(1), 98-114.
- Brunnermeier, Markus K. and Stefan Nagel (2005) "Do Wealth Fluctuations Generate Time-varying Risk Aversion? Micro-Evidence on Individuals' Asset Allocation." *Working Paper*, Princeton University, Princeton, NJ.

- Caballero, Ricardo J. (1991) "Earnings Uncertainty and Aggregate Wealth Accumulation." *American Economic Review*, 81(4), 859-871.
- Cameron, Stephen and James Heckman (2001) "The Dynamics of Educational Attainment for Black, Hispanic and White Males." *Journal of Political Economy*, 109(3), 455-499.
- Carneiro, Pedro, Karsten Hansen and James Heckman (2003) "Estimating Distributions of Treatment Effects with an Application to the Returns to Schooling and Measurement of the Effects of Uncertainty on College Choice." *International Economic Review*, 44(2), 361-423.
- Carniero, Pedro, James Heckman and Edward Vytlačil (2005) "Understanding What Instrumental Variables Estimate: Estimating Marginal and Average Returns to Education." Unpublished manuscript, University of Chicago.
- Carroll, Christopher and Andrew Samwick (1997) "The Nature of Precautionary Wealth." *Journal of Monetary Economics*, 40(1), 41-77.
- Charles, Kerwin and Eric Hurst (2003) "The Correlation of Wealth Across Generations." *Journal of Political Economy*, 111(6), 1155-1182.
- Charles, Kerwin and Ming-Ching Louh (2003) "Gender Differences in Completed Schooling." *The Review of Economics and Statistics*, 85(3), 559-577.
- Chen, Stacey (2004) "Estimating the Variance of Wages in the Presence of Selection and Unobservable Heterogeneity." *Working Paper*, State University of New York at Albany, Albany NY.
- Coate, Stephen and Glenn Loury (1993) "Will Affirmative Action Eliminate Negative Stereotypes?" *American Economic Review*, December, 1220-1240.
- Cochrane, John (1991) "A Simple Test for Consumption Insurance." *Journal of Political Economy*, 99(5), 957-976.
- Cunha, Flavio, James Heckman and Salvador Navarro (2005) "Separating Uncertainty From Heterogeneity in Life Cycle Earnings." *Working Paper 11024*, National Bureau of Economic Research, Cambridge, MA.
- Datcher-Loury (1986) "Racial Differences in the Stability of High Earnings among Young Men." *Journal of Labor Economics*, 4(July), 301-316.
- Devine, Theresa, and Heckman, James (1996) "The Structure and Consequences of Eligibility Rules for a Social Program" in Solomon Polachek (ed.) *Research in Labor Economics Volume 15*. Greenwich, CT: JAI Press. 111-170.

- Dominitz, Jeff and Charles Manski (1996) "Eliciting Student Expectations of the Returns to Schooling." *Journal of Human Resources*. 31(1): 1-26.
- Doolittle, Fred, and Linda Traeger (1990) *Implementing the National JTPA Study*. New York, NY: Manpower Demonstration Research Corporation.
- Dunnett, Charles and Ajit Tamhane (1992) "A Step-Up Multiple Test Procedure." *Journal of the American Statistical Association* 87(417): 162-170.
- Dynan, Karen, Jonathan Skinner and Stephen P. Zeldes (2004) "Why Do the Rich Save More?" *Journal of Political Economy*, 112(2), 397-444.
- Dynarski, Susan and Jonathan Gruber (1997) "Can Families Smooth Variable Earnings?" *Brookings Papers on Economic Activity*, 1997(1), 229-303.
- Engen, Eric and Jonathan Gruber (2001) "Unemployment Insurance and Precautionary Savings." *Journal of Monetary Economics*, 47(3), 545-579.
- Fairlie, Robert and Lori Kletzer (1998) "Jobs Lost, Jobs Regained: An Analysis of Black/White Differences in Job Displacement in the 1980s." *Industrial Relations*, 37(4), 460-475.
- Farber, Henry (1993) "The Incidence and Costs of Job Loss: 1982-1991." *Brookings Papers on Economic Activity: Microeconomics*, 1, 73-119.
- Farber, Henry and Robert Gibbons (1996) "Learning and Wage Dynamics." *Quarterly Journal of Economics*, 111(4), p.1007-47.
- Fitzgerald, John, Peter Gotteschalk and Robert Moffitt (1998) "An Analysis of Sample Attrition in Panel Data: The Michigan Panel Study of Income Dynamics." *Journal of Human Resources*, Spring, 251-299.
- Fuchs-Schundeln, Nicola and Matthias Schundeln (2005) "Precautionary Savings and Self-Selection - Evidence from the German Reunification 'Experiment'." *Quarterly Journal of Economics*, 120(3), 1085-1120.
- Galipolli, Giovanni, Costas Meghir, and Gianluca Violante (2005) "Education Decisions, Equilibrium Policies and Wages Dispersion." *Working Paper*, University College - London, London, UK.
- Greenberg, David (1997) "The Leisure Bias in Cost-Benefit Analyses of Employment and Training Programs." *Journal of Human Resources* 32(2): 413-439.
- Greenberg, David and Daniel Shroder (2004) *Digest of the Social Experiments, Third Edition*. Washington, DC: Urban Institute Press.

Goldin, Claudia and Lawrence Katz (2003) "Mass Secondary Schooling and the State." *Working Paper 10075*, National Bureau of Economic Research, Cambridge, MA.

Gottschalk, Peter and Robert Moffit (1994) "The Growth of Earnings Instability in the U.S. Labor Market." *Brookings Papers on Economic Activity*, 2, 207-250.

Heckman, James (1997a). "Instrumental Variables: A Study of the Implicit Behavioral Assumptions Used in Making Program Evaluations." *Journal of Human Resources* 32(3): 441-452.

Heckman, James (1997b) "Randomization as an Instrumental Variable." *Review of Economics and Statistics* 78(2): 336-341.

Heckman, James and Carolyn Heinrich, eds. (2005) *Performance Standards in a Government Bureaucracy*. W.E. Upjohn Institute for Employment Research.

Heckman, James, Carolyn Heinrich, and Jeffrey Smith (2002) "The Performance of Performance Standards." *Journal of Human Resources* 36(4): 778-811.

Heckman, James, Neil Hohmann, Jeffrey Smith, with the assistance of Michael Khoo. (2000) "Substitution and Drop Out Bias in Social Experiments: A Study of an Influential Social Experiment." *Quarterly Journal of Economics* 115(2): 651-694.

Heckman, James, Lance Lochner and Petra Todd (2005) "Earnings Functions, Rates of Return, and Treatment Effects: The Mincer Equation and Beyond." *Working Paper 11544*, National Bureau of Economic Research, Cambridge, MA.

Heckman, James, Robert LaLonde, and Jeffrey Smith (1999) "The Economics and Econometrics of Active Labor Market Programs" in Orley Ashenfelter and David Card (eds.), *Handbook of Labor Economics, Volume 3A*. Amsterdam: North-Holland, 1865-2097.

Heckman, James and Jeffrey Smith (1998) "Evaluating the Welfare State" in Steiner Strom (ed.), *Econometrics and Economic Theory in the 20th Century: The Ragnar Frisch Centennial*. Cambridge University Press for Econometric Society Monograph Series, 241-318.

Heckman, James, and Jeffrey Smith (1999) "The Pre-Program Earnings Dip and the Determinants of Participation in a Social Program: Implications for Simple Program Evaluation Strategies." *Economic Journal* 109(457): 313-348.

Heckman, James, and Jeffrey Smith (2000) "The Sensitivity of Experimental Impact Estimates: Evidence from the National JTPA Study" in David Blanchflower and

Richard Freeman (eds.), *Youth Employment and Joblessness in Advanced Countries*, Chicago: University of Chicago Press for NBER, 331-356.

Heckman, James, and Jeffrey Smith (2004) "The Determinants of Participation in a Social Program: Evidence from the Job Training Partnership Act." *Journal of Labor Economics* 22(4): 243-298.

Heckman, James, Jeffrey Smith, with the assistance of Nancy Clements (1997) "Making the Most Out of Programme Evaluations and Social Experiments: Accounting for Heterogeneity in Programme Impacts." *Review of Economic Studies* 64(4): 487-535.

Heckman, James and Edward Vytlacil (2005) "Structural Equations, Treatment Effects and Econometric Policy Evaluation." *Econometrica*, forthcoming.

Heinrich, Carolyn, Gerald Marschke and Annie Zhang. (1998) "Using Administrative Data to Estimate the Cost-Effectiveness of Social Program Services." Technical report. The University of Chicago.

Jacob, Brian and Lars Lofgren (2005) "Principals as Agents: Subjective Performance Measurement in Education." NBER Working Paper No. 11463.

Kornfeld, Robert and Howard Bloom (1999) "Measuring Program Impacts on Earnings and Employment: Do Unemployment Insurance Wage Reports From Employers Agree With Surveys of Individuals?" *Journal of Labor Economics* 17(1): 168-197.

Kemple, James, Fred Doolittle, and John Wallace (1993) *The National JTPA Study: Site Characteristics and Participation Patterns*. New York, NY: Manpower Demonstration Research Corporation.

Kirschenman, Joleen and Kathryn Neckerman (1991) "'We'd Love to Hire Them, But': The Meaning of Race for Employers." in Christopher Jencks and Paul Peterson, eds., *The Urban Underclass*, Brookings Institution Press, Washington DC.

Lang, Kevin and Micheal Manove (2005) "Education and Labor-Market Discrimination." *Working Paper*, Boston University, Boston, MA.

Lochner, Lance and Enrico Moretti (2004) "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests and Self-Reports." *American Economic Review*, 94(1), 155-189.

Lochner, Lance and Alexander Monge-Naranjo (2002) "Endogenous Credit Constraints and Education Formation." *Working Paper 8815*, National Bureau of Economic Research, Cambridge, MA.

- Liu, Wei (1997) "Stepwise Tests When the Test Statistics Are Independent." *Australian Journal of Statistics* 39(2): 169-177.
- Manski, Charles (1990) "The Use of Intentions Data to Predict Behavior: A Best-Case Analysis." *Journal of the American Statistical Association* 85(412): 934-940.
- Manski, Charles (2004) "Measuring Expectations." *Econometrica* 72(5): 1329-1376.
- Mazzocco, Maurizio (2004) "Saving, Risk Sharing and Preferences for Risk." *American Economic Review*, 94(4), 1169-1182.
- McCurdy, Thomas E. (1982) "The Use of Time Series Processes to Model the Error Structure of Earnings in a Longitudinal Data Analysis." *Journal of Econometrics*, 83, 83-114.
- Meghir, Costas and Luigi Pistaferri (2004) "Income Variance Dynamics and Heterogeneity." *Econometrica*, 72(1), 1-32.
- Neal, Derek (2005) "Has Black-White Skill Convergence Stopped?" *Working Paper 11090*, National Bureau of Economic Research, Cambridge, MA.
- Neal, Derek and William Johnson (1996) "The Role of Premarket Factors in Black-White Wage Differences." *Journal of Political Economy*, 104(5), 869-895.
- Orr, Larry, Howard Bloom, Stephen Bell, Winston Lin, George Cave and Fred Doolittle (1994) *The National JTPA Study: Impacts, Benefits and Costs of Title II-A*. Bethesda, MD: Abt Associates.
- Orr, Larry, Howard Bloom, Stephen Bell, Fred Doolittle, Winston Lin, and George Cave (1996) *Does Training Work for the Disadvantaged? Evidence from the National JTPA Study*. Washington, DC: Urban Institute Press.
- Palacios-Huerta, Ignacio (2003) "An Empirical Analysis of the Risk Properties of Education Returns." *American Economic Review*, 93(3), 948-964.
- Philipson, Tomas and Larry Hedges (1998) "Subject Evaluation in Social Experiments." *Econometrica* 66(2): 381-408.
- Rivkin, Stephen (1995) "Black/White Differences in Schooling and Employment." *Journal of Human Resources*, XXX(4), 826-852.
- Roy, A. D. (1951) "Some Thoughts on the Distribution of Earnings." *Oxford Economic Papers* 3: 135-146.

Skyt Nielsen, Helena and Annette Vissing-Jorgensen (2005) "The Impact of Labor Income Risk on Educational Choices: Estimates and Implied Risk Aversion." *Working Paper*, Northwestern University, Evanston, IL.

Smith, Jeffrey (1997a) "Measuring Earnings Dynamics Among the Poor: Evidence from Two Samples of JTPA Eligibles." Unpublished manuscript, University of Western Ontario.

Smith, Jeffrey (1997b) "Measuring Earnings Levels Among the Poor: Evidence from Two Samples of JTPA Eligibles." Unpublished manuscript, University of Western Ontario.

Smith, Jeffrey and Alexander Whalley (2005) "How Well Do We Measure Public Job Training?" Unpublished manuscript, University of Maryland.

Wood, Michelle. 2005. "National JTPA Study – SDA Unit Costs." Abt Associates Memo to Jerry Marsky [sic] and Larry Orr.