2013

Three essays in development economics

Murali Kuchibhotla
Iowa State University

Three essays in development economics

by

Murali Kuchibhotla

A thesis submitted to the graduate faculty

in partial fulfillment of the requirements for the degree of

DOCTOR OF PHILOSOPHY

Major: Economics

Program of Study Committee:

Peter Orazem, Major Professor

Rajesh Singh

Wallace Huffman

Brent Kreider

Darin Wohlgemuth

Iowa State University

Ames, Iowa

2013

TABLE OF CONTENTS

Page

CHAPTER 1 GENERAL INTRODUCTION 1

Introduction 1

References 4

CHAPTER 2 SCHOOL TO WORK TRANSITION OF YOUTH IN SRI LANKA 5

Abstract 5

Introduction 5

Literature Review 6

Unemployment Trends and Institutional Background in Sri Lanka 10

Theory of Time Allocation 12

Estimation 17

Data-Description and Trends 20

Data-Construction and Variable Description 22

The Non-employment-Education Puzzle 24

Results for One Part Model-Separating Training from Non-employment 25

Results for ZIB Model-Separating Training from Non-employment 28

Results-School Quality Sub-Sample 30

Simulation Results 30

Conclusion 32

References 33

CHAPTER 3 EARNINGS AND EMPLOYMENT EFFECTS OF YOUTH TRAINING PROGRAMS IN SRI LANKA 60

Abstract 60

Introduction 60

Literature Review 65

Matching Methods and Theoretical Framework 70

Estimating the Effect of Training on Future Employability 73

Bounding the Effects of Training Participation on Wages 75

Data Construction 80

What Determines Training and Employability 83

Specifying the Propensity Score Function 84

Assessing Balance after Estimating the Propensity Score 86

How Does Training Affect the Probability of Obtaining Wage Employment? 87

How Does Training Affect Wages? 88

Conclusion 90

References 91

CHAPTER 4 THE SCARRING EFFECTS OF YOUTH JOBLESSNESS: MICRO-EVIDENCE FROM SRI LANKA 109

Abstract 109

Introduction 109

Literature Review 112

Data 115

The Fractional Logit Model 118

Problems with the Fractional Logit Approach 121

Dealing with Measurement Error in Early Joblessness 122

Methods 123

Covariates and Balancing Score Checks in Matching Methods 129

Matching Results 132

Local Randomized Experiment Results 134

Comparisons with Fractional Logit Results 135

Conclusion 136

References 138

CHAPTER 5 GENERAL CONCLUSIONS 164

General Discussion 164

Recommendations for Future Research 165

CHAPTER 1. INTRODUCTION

The wave of recent unrest in the Middle East and North Africa has recently focused attention on the extent of employment problems that youth face in the Arab world. What is less well known however, is that youth employment difficulties are widespread, ranging from both low-income developing countries to high-income OECD countries. The International Labor Organization (ILO) has collected extensive data on the extent of youth unemployment across the world. In 2011, the ILO estimates that 74.6 million youth were unemployed globally. The ILO has also established that across a range of different countries, youth unemployment rates tend to be considerably higher than adult rates. The global youth to adult unemployment (YTAU) ratio is 2.8. There is, however, substantial variation in this ratio across countries. Two of the worst affected regions of the world are South Asia (YTAU=4.5) and the Middle East (YTAU=4.1).

It is natural for youth to experience higher unemployment rates compared to adults as they have less general and occupation specific work experience. They also have lower opportunity costs for job search and thus may spend more time looking for work. Indeed, research by the World Bank shows this to be true in a number of developing countries.[1] However, this problem has reached epic proportions in some countries. In Armenia, Bosnia and Herzegovina, Egypt and South Africa, nearly 50 percent of youth are now unemployed.

When unemployment reaches such high levels, it is critical that it be resolved quickly. This is because high unemployment has the potential to drastically reduce youth welfare. Such adverse effects may manifest themselves through multiple mechanisms:

First, early unemployment experiences may impose significant scars on youth. There is a large empirical literature that supports this proposition. Consider the case of British youth, as

[1] See World Bank (2007).

documented by Gregg and Tominey (2005). They show that a large pay gap exists between the adult wages of individuals who have similar attributes, but who tend to differ in terms of their early unemployment experiences.

Second, the damage done to youth from the lack of early access to good job opportunities can be severe, particularly in developing countries. Because few youth in developing countries can afford to remain economically idle, they are often forced to accept jobs under poor working conditions and low wages in the informal sector. Many remain stuck in jobs that prevent them from climbing up the economic ladder, leaving them trapped in poverty.

While much is known about the school-to-work transition process and the associated employment difficulties for youth in developed countries[2], little is known about this transition process for developing country youth. This dissertation is a step towards filling that void.

The primary reason for the lack of developing country evidence on this issue is the paucity of data on labor market outcomes for youth. Even the World Bank- an entity charged with funding many labor market interventions in developing countries-does not specifically collect information on how its projects influence youth employment.

Specifically, this thesis aims to provide information on youth employment struggles in a small developing country, Sri Lanka. Youth unemployment rates have consistently exceeded adult unemployment rates for many decades, [3] but the root causes of the poor youth transition from school to work have not been explored. As a result, many important labor market policies that are being adopted to ameliorate this situation are being adopted without a firm purchase of the realities on the ground. This study aims to provide detailed systematic evidence on the

[2] Ryan (2001) surveys this issue for OECD countries.
[3] See World Bank (2005).

school-to-work transition for Sri Lankan youth, which would, in turn, help to improve policy responses that the Government of Sri Lanka has adopted to try to tackle this problem.

The contributions of this thesis are as follows:

1. It lays out the difficulties that Sri Lankan youth face in making the transition from school-to-work. It addresses the issue of whether school leavers have the appropriate skills to thrive in the labor market. It highlights the need to target early school dropouts with various learning opportunities that can help improve their employment prospects. Key interventions here are work-skills training through job training programs and stand-along vocational training programs provided by both the private sector and NGOs.
2. It provides evidence that early out-of-work experiences tend to be damaging to future job prospects. Our study constitutes the first attempt ever to provide rigorous statistical estimates on this issue for Sri Lanka.
3. It provides a strong evidence based framework to evaluate training programs aimed at improving the labor market prospects of Sri Lankan youth by undertaking rigorous evaluation of these programs. By doing so, we improve knowledge about youth employment in a country that has traditionally underemphasized the collection of labor market outcomes data.

Chapter 2 starts with the observation that a strong positive correlation between educational qualifications and unemployment rates among young workers has been documented in previous studies on Sri Lanka. This puzzling finding is at odds with the theory of human capital which predicts that rising levels of educational attainment help improve employment outcomes. Using data from a household level survey administered in Sri Lanka in 2006, we show that the positive correlation between education and unemployment turns out to be spurious in nature. Because previous studies have not had any information on the amount of time spent by young people in training programs, they have erroneously incorporated training into unemployment. We show that once time spent in training is adequately accounted for, the association between education and unemployment no longer exists in the data.

Chapter 3 looks at the performance of one component of active labor market programs (namely training) in improving the employment prospects and wages of Sri Lankan youth. While these programs do not seem to improve the prospects of finding paid employment, they do deliver a substantial wage payoff to training participants, who earn significantly more after training than non-trainees.

Finally, chapter 4 provides evidence on how costly early periods of joblessness can be for young people. We show that being out-of-work in the first year after leaving school can negatively affect the future prospects for finding paid employment. Our estimates imply that differences in early jobless exposure among individuals can contribute to between 6 -33 months of additional joblessness in the future.

References

Ryan, P. 2001. The School-to-Work Transition: A Cross National Perspective. *Journal of Economic Literature*. 39(1): 34-92.

The World Bank. *Treasures of the Education System in Sri Lanka: Restoring Performance, Expanding Opportunities and Enhancing Prospects*. Washington, D.C. 2005.

World Development Report. 2007. New York: Oxford University Press.

CHAPTER 2. SCHOOL-TO-WORK TRANSITION OF YOUTH IN SRI LANKA

A paper to be submitted to the Review of Development Economics

Murali Kuchibhotla

I. Abstract

Previous research on Sri Lanka has documented a strong positive correlation between educational qualifications and unemployment rates among young workers. This finding, however, is at odds with the theory of human capital, according to which rising levels of educational attainment help improve employment outcomes. Using data from a household level survey administered in Sri Lanka in 2006, we show that the above documented relationship between education and unemployment is spurious in nature. Because previous studies have not had any information on the amount of time spent by young people in training programs, they have erroneously incorporated training into unemployment. We show that once time spent in training is adequately accounted for, the association between education and unemployment no longer exists in the data. We also find that school quality plays an important role in easing the transition from school-to-work for Sri Lankan youth.

II. Introduction

Thanks to its heavy investment in public education, Sri Lanka has one of the highest education rates in the developing world (Mayer and Salih 2006). However, the number of unemployed youth has also grown rapidly, pointing to a serious mismatch between the supply and demand for educated youth. In 2004, the overall unemployment rate was 8.3%, while the corresponding rate for young adults (15-29 years of age) was nearly 30%.[4] Unlike the pattern of unemployment in developed countries, the highest unemployment rates are for more educated youth.

This study explores why some youth experience relatively rapid transitions from school to work while others face extended spells of unemployment or inactivity in a developing country setting. In doing so, we aim to resolve the puzzle of why human capital and youth unemployment in Sri Lanka are positively correlated. Addressing that issue requires us to address numerous additional questions. Among them: How do youth allocate their time between training, employment and non-employment after leaving school? What are the characteristics of individuals facing the most difficult transitions? What roles do ability, educational and family background play in easing this transition process? What are the beneficial effects of enrollment in training programs on employment prospects? We tackle these issues using a unique retrospective survey of Sri Lankan youth. The survey, tracks the work, unemployment and training profiles of seven cohorts of school leavers for as many as seven years after leaving school.

We show that the education-unemployment puzzle is actually an artifact of ignoring training that occurs after leaving school. Time allocated to training by better educated and more able youth creates a spurious correlation between education and unemployment when training is erroneously counted as a part of unemployment.

We also find that better schools help contribute to smoother school-to-work transitions, with students from the best schools spending as much as 30% more of their post-schooling time in wage employment relative to students from the worst performing schools.

III. Literature Review

The "school-to-work transition", is defined as, "the period between the end of schooling and the attainment of stable employment"[5]. Research on the passage from school to work covers

[4] Vodopivec and Withanachchi (2006).

employment, schooling and training. Much of this literature is centered on developed countries, although a small body of work examining such issues for developing countries also exists[6].

One of the main themes in the school-to-work transition literature is the usefulness of the unemployment rate as an indicator of youth employment problems. Instead of focusing exclusively on the unemployment rate, Ryan (2001) argues for the importance of joblessness[7] in identifying employment problems among the young. The basis for this conclusion is his finding that, for the seven countries[8] that he surveys, many youth are in fact inactive and not unemployed. Moreover, he finds that changes in inactivity show little relationship to changes in unemployment, suggesting that focusing on either inactivity or unemployment in isolation can lead to misleading conclusions about the extent of youth employment problems.

A second theme in this literature concerns the deterioration of youth labor market outcomes in the post-1970 period for developed economies. As Ryan (2001) documents, youth pay and employment relative to older workers have declined over the past 30 years over the seven developed countries that he surveys. However, there is considerable heterogeneity in these outcomes across countries, with some countries experiencing neither declines in youth employment nor pay (Germany, Japan and Netherlands) while others have seen declines in both of these outcomes (France, Sweden and USA). Ryan identifies skill-biased technological change, coordinated pay setting institutions and national school-to-work transition institutions as the main driving forces behind these trends.

---

[5] Ryan (2001).
[6] Two recent books that devote chapters to this issue with a focus on developing countries include World Development Report 2007 and Growing up Global: The Changing Transitions to Adulthood in Developing Countries.
[7] Joblessness is defined as the sum of unemployment and inactivity.
[8] The seven countries are USA, UK, Sweden, Netherlands, Japan, Germany and France.

Problems in the youth (and adult) labor markets for these countries have led to a push for the use of labor market policies in combating these problems. Labor market programs feature prominently in this pursuit. Labor market programs in developed countries have traditionally targeted disadvantaged workers and have provided such services as job search assistance, work experience, job training as well as access to jobs. Many of these programs are in fact targeted towards youth.

Labor market programs in the US and Europe have been the subject of a large literature in program evaluation, which has used both experimental and non-experimental methods to assess the effectiveness of such programs. There is mixed evidence on how these programs affect young people, with US programs generally failing to improve employment prospects as well as subsequent pay. European programs seem to yield more positive benefits, particularly in terms of improving participants' future employment prospects. Pay effects, on the other hand, are negligible or non-existent for European programs[9].

Developing country youth also face problems transiting from school to work. The most common problems are that youth start work too early in life to develop skills and that youth get stuck in non-employment or else find dead-end jobs that fail to allow future opportunities for career growth[10].

In poor developing countries, some youth are unlikely to even make it to school, while many others are likely to be working while still in school. Working while in school is likely to be damaging to the schooling attainment of these youth, as there is evidence to indicate that working youth are both more likely to fare poorly while in school as well as more likely to drop

[9] The evidence on the pay effects of labor market programs is generally limited to the UK.
[10] The evidence cited here on developing countries draws on the findings from the World Development Report 2007.

out of school altogether, relative to youth who attend school full time. Poor schooling outcomes lead to poorer adult earnings and also contribute to the intergenerational transmission of poverty, given that poorer households are more likely to send their children to work.

Once young workers enter the labor market, they are likely to face significant difficulties in finding employment, as young workers are more likely to be unemployed relative to adults. Across different developing countries, the youth unemployment rate is about two to three times higher than the adult unemployment rate. Moreover, in some countries, such as those in the Middle East and North Africa, youth unemployment is mostly concentrated among the educated youth.

For youth that do end up finding jobs, much of this employment is likely to be in unpaid family jobs or low paying jobs. As long as young people can move to more productive opportunities over time, this should not matter much for their long term prospects. But if job mobility is low, such jobs can end up as cul-de-sacs for young workers. Starting in unpaid and informal work may also deprive these workers of the benefits of further human capital accumulation, as the potential for on-the-job training may be much higher for formal sector jobs. Since the wage returns from such on-the-job training decline as individuals' age, youth facing such labor market difficulties are likely to be greatly disadvantaged.

There are some key issues that have not been previously explored in this literature. First, there has been no systematic attempt to explore the interactions between different labor market activities such as employment, unemployment and training. Young people engage in a number of activities after leaving school; examining each activity in isolation misses the many important interactions that arise between them. In order to address this concern, the approach we take in this paper is to model the fraction of time allocated to these different activities. This allows us to compare the effects of the covariates of interest across different activities, thereby yielding a more complete picture of the interaction among these different activities.

Second, there is a strong link between employment problems and social disadvantage that exists in the literature. While evidence from developed countries indicates that more educated workers are less likely to face employment problems, the evidence from some developing countries seems to suggest the converse, namely, that employment problems are likely to be more severe among the educated members of society. This is inconsistent with the predictions of the traditional human capital model, according to which more education should result in better outcomes relating to employment and pay. This puzzle relating to the positive association between education and unemployment has not been adequately addressed in the literature. Addressing this puzzle is a key concern of this paper.

Our dataset, along with the empirical strategy that we develop below, is well suited to addressing both of these issues. Our focus on modeling the fraction of time that individuals allocate to various labor market activities is unique. To the best of our knowledge this approach has never been used before to study labor market issues.

IV. Unemployment Trends and Institutional Background in Sri Lanka

Table 1 in appendix-C provides details on the unemployment situation in Sri Lanka. Unemployment over the years 1999-2002 averaged 28% for Sri Lankan youth aged 15-19 years. Unemployment problems persist as these youth grow into young adults. Unemployment for the 20-29 year group averages 19% and only falls below 5% from age 30 on. About 80% of the unemployed population is concentrated in the 15-29 year age group, with 60% of these individuals in the 20-29 year age group.

Table 2 in appendix-C provides a breakdown of the unemployment rate by educational qualifications. Unlike the pattern in developed countries, unemployment rates are highest among the most educated. Compared to the average unemployment rate of 8.3% between1999-2002, O-level (ordinary level) graduates had an average unemployment rate of 12.5% and A-level (advanced level) graduates had even higher unemployment rates at 16.2%[11]. The plight of university educated youth is even worse, with the unemployment rate among this group exceeding that of A-level graduates. Vodopivec and Withanachchi (2006) found that only half of the college graduates in their sample had found permanent employment within four years of receiving their degrees. In contrast, those who never attended school at all had an average unemployment rate below 1%.

High unemployment rates among the most educated youth are especially worrisome, given how difficult and costly it is to complete secondary education in Sri Lanka. Only 30% of those who sit for the O-level exam qualify to take the A-level exams. Only 50% of those who sit for the A-level exam qualify for a university education, and only about 15% of qualified A-level applicants gain entry to the universities.[12] Such highly competitive university entry criteria lead potential university prospects to spend on average between 1-2 years preparing for these examinations. This in turn pushes the average age of university graduates to the mid-20s, imposing significant opportunity costs on graduates. Unfortunately, for many of those who make it to university and graduate and for those who complete their O-levels or A-levels but do not attend university, schooling does not seem to generate returns from the labor market.

Over the years a small (but growing) literature on Sri Lanka has explored the positive correlation between education and unemployment. Dickens and Lang (1996) find that controlling for gender and sector of employment eliminates the positive correlation between unemployment and

[11] An O-level education corresponds roughly to a High School education in the US. The A-level certification is equivalent to somewhere between one year of college and an AA degree.
[12] The evidence cited in this paragraph comes from Nanayakkara(2004).

education in the urban sector, but the positive correlation remains significant for rural women. Even this relationship disappears once they account for the fact that median unemployment durations are high and that the more educated have had a shorter time period to search for jobs.

Vodopivec and Withanachchi (2006) document the labor market experiences of college educated youth. They find evidence that university graduates prefer to remain unemployed while waiting for government or formal sector work. Moreover, rural and female graduates and those from lower socio-economic strata experience even greater disadvantages in finding jobs.

While the two studies cited above reveal some interesting patterns in the data, they suffer from a number of drawbacks. First, while it is important to characterize the unemployment experiences of college educated youth, most young people in Sri Lanka never make it to college. Thus, work patterns among the college educated group may be unrepresentative of the activity patterns for non-college bound youth. Therefore, we need to pay careful attention to the work patterns of non-college bound youth. Second, unlike the findings of Dickens and Lang (1996), we find that controlling for both gender and sector fails to remove the positive correlation between education and non-employment observed in our data. Thus, the resolution of the puzzle of the positive association between non-employment and education must lie elsewhere.

V. Theory of Time Allocation

Because the key strategy employed in this paper focuses on modeling individual allocations of time across different uses, we first need to develop a meaningful behavioral theory of time allocation that accounts for these choices. This framework, which is developed below, is essentially a modified version of standard human capital model.

Graphical example

As argued above, we are interested in formulating a theory of time allocation which allows us to characterize the fraction of available time that individuals allocate to the following four labor market activities: wage employment (*E*), self-employment (*O*), non-employment (*N*) and training (*T*). We designate Y^k , *k=E, O, N, T* to represent the fraction of time devoted to each labor market activity. We begin with a graphical illustration of a simplified form of the problem at a specific point in time and then generalize to the problem of allocating time across several periods.

Suppose that an individual is making the initial time allocation decision upon completing school. To keep things simple, we focus on just three choices, *E, N,* and *T*, where we assume that in this instance, the returns from wage-employment dominate the returns from self-employment. The choices are mutually exclusive at any point in time, although individuals may alter their allocations in subsequent periods. The choice is illustrated in Figure 5A.[13] The horizontal axis represents the fraction of post-schooling time the individual plans to spend in additional skill acquisition before devoting full time to work. One can get training on the job by choosing E and devoting Y_P^T time to training part-time. Alternatively, one can choose to specialize in training, devoting Y_F^T time to full-time training. The third choice involves taking the human capital α_N produced by the end of schooling and using it in nonmarket activities, N. The individual will stop producing additional human capital, *h* when the return on additional investments falls below the rate of time preference *r*.

The vertical axis in Figure 5A represents the log of the wage net of training cost that an individual can earn at each possible fraction of time spent training. We assume that after completing the training, this wage is constant for the remaining duration of work life. The formulation for log wage is:

[13] This section is a modified version of the model outlined by Rosen (1977).

$\ln(W_t) = h^k(\alpha_N, Y^k|X)$, k = *E*, *N*, *T*.[14]

When the individual specializes in training, they cannot work, so the value of spending an infinitely small amount of time specializing in training is zero because no human capital is acquired and no time is spent earning. The on-the-job training option has an immediate reward because some of the time is spent earning. Eventually, the wage from specializing in training rises above the wage one can earn working part-time and training part-time.

These earnings streams are functions of various individual level attributes, represented by the vector *X*. Elements of *X* include measures of human capital acquired before entering the labor market: educational achievement, ability, and schooling quality. We expect higher levels of ability, education and school quality to shift these earnings streams upwards, yielding higher wage returns for any given fraction of time spent in skill acquisition. However, these human capital measures may not raise all earnings streams by the same proportion, and so they can change the optimal allocation of time.

In this framework, the time allocation decisions in the school-to-work transition involve choosing *k* so as to maximize the present value of lifetime income, subject to a rate of time preference, r. The optimum is shown as the tangency between log iso-present value lines that have a slope equal to *r* and the log wage function that has a slope equal to $\frac{\partial h^k}{\partial Y^k}$.[15] That is, at the optimum we must have:

---

[14] In our current formulation, we are assuming that the lifetime earnings for wage work dominate self-employment so that $h^E(\alpha_N, Y^E|X) > h^O(\alpha_N, Y^O|X)$. Self-employment is preferred over wage work if this inequality is reversed.

[15] The log iso-present value lines have an intercept equal to the log of the present value of the wage weighted by the interest rate. The continuous discounted present value formula is $V(\alpha_N, Y^k|X) = \frac{1}{r}\exp\{h(\alpha_N, Y^k|X) - rY^k\} = \frac{W}{r}e^{-rY^k}$. Taking logs and rearranging yields the familiar relationship, $\ln W = \ln(rV) + rY^k$, where the logarithm of the wage is linear in the amount of time spent in training.

$$\frac{\partial[\ln(W_k)]}{\partial Y^k} = \frac{\partial[h^k(\alpha_N, Y^k|X)]}{\partial Y^k} = r, \quad k = E, T.$$

The rate of time preference will vary with family income and other family characteristics. If r is viewed as a cost of borrowing, we may presume that poor families face higher borrowing costs relative to richer families[16]. As r rises, the optimum shifts from specializing in training, to on-the-job training, to deciding not to train at all. Figure 5A shows an example where the optimum will be to specialize in training for a length of time, Y_F^T and then working full time. Figure 5B shows an example where the optimum choice is to devote Y_P^T time to on-the-job training after which the individual works fulltime. Eventually, if r rises high enough, the best choice reverts to setting Y^k = 0 for k=*E, O, T*, so that the individual will allocate full time to nonmarket activities and earn a value of timeα_N.

Empirical formulation

While the graphical example shows the optimum at one point in time, the transition from school to work will include many periods which will allow individuals to devote time to more than one activity and potentially all four activities. While one could formally model the sequential decisions of time use in each month after leaving school using dynamic programming,[17] such models are computationally burdensome and impose substantial structure on individual decisions. Instead, we expand the time from one period to multiple periods and then model the fraction of time devoted to each of the four activities over the sample period. That allows us to analyze the school-to-work transition in the context of a single period discrete choice model where all four choices are possible.

Let the present discounted value of the returns from each possible activity be represented by $V(Y^E)$,$V(Y^O)$, $V(Y^N)$ and $V(Y^T)$ with Y^k denoting the fraction of time spent in activity k =*E, O, N* and *T*. We assume that the present value reflects the optimum sequence of time allocated to each

[16] See the discussion in Cameron and Taber (2004).
[17] See, for example, Wolpin (1995).

of the four activities so, for example, at each Y^E,$0 \leq Y^E \leq 1$, the associated $V(Y^E)$ represents the highest present value possible from allocating the remaining $1 - Y^E$ fraction of time across the other three activities. The value functions $V(.)$ will also depend on various individual level characteristics, denoted by the vector$X$.

The time allocation problem for the school-to-work transition period for any given individual is then characterized by:

$$\max_{Y^E, Y^O, Y^N, Y^T, \lambda} V(Y^E) + V(Y^O) + V(Y^N) + V(Y^T) + \lambda\{1 - Y^E + Y^O + Y^N + Y^T\}$$

The individual chooses time allocations so as to maximize the present value of income.[18] The expression in the curly brackets in above is the adding up constraint on time which requires that all time shares add up to 1. Because of the adding-up constraint, the choice of any three time allocations implies the time spent on the fourth labor market activity. The Lagrange multiplier λ represents the shadow value of time which equals the rate of time preference *r* in the graphical example.

First order conditions for an optimum are:

$$V'(Y^E) \leq \lambda$$

$$V'(Y^O) \leq \lambda$$

$$V'(Y^T) \leq \lambda$$

with $Y^N = 1 - Y^E - Y^O - Y^T$. Strict equalities hold at interior optima so that if an individual engages in activities *i* and *j*, it must be the case that

$$V'(Y^i) = V'(Y^j) = \lambda = r$$

If an individual does not allocate time to activity *j*, then $V'(Y^j) < \lambda = r$.

[18] This can also be recast as choosing *E, T, O,* and *N* so as to maximize the present value of utility. The resulting reduced form solution is identical.

Assuming interior solutions, the derived demand functions for each of the four activities will depend on individual characteristics X that are all known at the time of school leaving. Elements of X include ability (A), educational qualifications before entering the labor market (S), household wealth (W) and schooling quality (Q), all of which enter the value function, $V(.)$. Optimal time allocations are thus functions of the following exogenous variables:

$$Y^{E*} = Y^{E}(A, S, Q, W)$$

$$Y^{O*} = Y^{O}(A, S, Q, W)$$

$$Y^{N*} = Y^{N}(A, S, Q, W)$$

$$Y^{T*} = Y^{T}(A, S, Q, W)$$

These reduced forms justify the formulation of the empirical work that follows.

VI. Estimation

In keeping with the theory outlined above, we require an empirical strategy that will allow us to model the proportion of time individuals spend in various labor market activities. Recent years have seen numerous attempts to tackle the problems posed by such fractional data.[19] There are three main issues which need to be addressed for the appropriate modeling of such data. First, proportional data are only observed over the [0, 1] interval, which implies that the conditional expectation of the variable must be a non-linear function of the covariates[20]. Second, the conditional variance must be a function of the conditional mean because the conditional variance must change as the conditional mean approaches either boundary. And finally, account must be taken of the fact that individuals choose to do or not do something for different reasons.

Commonly used methods for dealing with such data are typically subject to specification errors on account of the inappropriate handling of proportional data. For example the Tobit model is

[19] Examples abound in the finance literature. See Cook, Kieschnick and McCullough (2008).
[20] This is required to ensure that the predicted proportions lie within the [0, 1] interval.

a common approach used for such data in econometrics. This model assumes that the data are normally distributed, with the observed data lying within a certain interval. By contrast, proportional data are not censored as they are not defined outside of the [0, 1] interval. Using the Tobit model to represent such data can yield biased and inconsistent results.[21] To make matters worse, the Tobit model restricts the factors that influence whether or not an individual engages in a particular activity to have the exactly the same influence on how much time to allocate to that particular activity.

We present two alternative formulations for modeling such fractional data. These models should produce similar results in large samples, but they may yield different results in small samples. The first approach is to model fractional data by imposing constraints on the conditional mean of the dependent variable, such that this conditional mean is restricted to lie between 0 and 1. The conditional expectation function of interest is given by:

$$E(Y^k|X) = G(\beta X) \quad (1)$$

where Y^k is the fraction of time spent in the k^{th} labor market activity, with k=E, O, N, T and where $\beta$is a set of parameters and X is a covariate vector with the following component variables: $X = (A, S, Q, W)$. Total cumulative time spent in these four activities must exhaust the total time endowment available, so we impose the constraint that $\sum_{i=1}^{4} Y^k$=1. $G(.)$ above is a known cumulative distribution function satisfying $0 < G(z) < 1$ for all z∈R. This restriction on $G(.)$ ensures that the predicted values of Y^k lie within the 0-1 interval.

Four popular choices for $G(.)$ are presented in table 2 and include the logistic, standard normal, the loglog and complementary loglog distributions. The differences between these specifications for $G(.)$ are that while the logistic and standard normal specifications are symmetric about the point 0.5 and thus approach the values 0 and 1 at the same rate, the loglog and

[21] Maddala (1991) discusses this issue in further detail.

complementary loglog specifications are not symmetric. The loglog model increases sharply at small values of $G(.)$ and slowly when is near 1, while the opposite holds for the complementary loglog model.

The conditional mean model of equation (1) can be consistently estimated by either non-linear least squares (NLS) or through the quasi-likelihood method (QML) proposed by Papke and Woolridge (1996). Papke and Woolridge (1996) propose a particular QML method based on the following Bernoulli log-likelihood specification:

$$l(\beta) = Y^k ln[G(\beta X)] + (1 - Y^k) ln[1 - G(\beta X)] \text{ ; k=E, O, N, T}$$

As the Bernoulli distribution is a member of the linear exponential family of distributions, the QML estimator of β is consistent and asymptotically normal, provided that the $E(Y^k|X)$ specified in equation (1) is indeed correctly specified. Since the results of the estimation exercise depend upon the accuracy of the G(.) function, we shall present results for all four model specifications for G(.) to see how sensitive the estimates are to changes in functional form of the G(.) function.

The second model that we use for modeling our data is the zero-inflated beta (ZIB) regression model developed by Kieschnick and McCullough (2003). Both the one-part model described above as well as the ZIB model allow for the clustering of observations at zero. The one-part model allows for a non-linear conditional mean and for the conditional variance to be a function of the conditional mean. The ZIB model relaxes further the sample selection assumption associated with the fractional logit model.

As stated previously, the need to formulate the ZIB model arises from the need to capture the heterogeneity present in the data. The ZIB model belongs to a broader class of mixed discrete-continuous random variable models, and can be represented as follows:

$$f(Y^k = 0; X) = 1 - C(\alpha X) \; for \; Y^k = 0$$

$$f(Y^k : X) = C(\alpha X) \left[\frac{\Gamma(p + q(X))}{\Gamma(p)\Gamma(q(X))} Y^k (1 - Y^k) \right] \; for \; 0 < Y^k < 1$$

where k=E,O,N,T and $X = (A, S, Q, W)$, $q(X) = pe^{-\beta X}$ and p is a parameter of the beta distribution. Also, $C(\alpha X)$ represents the probability that an individual will choose to engage in a particular labor market activity. The cumulative logistic function is used to model this probability and consistent with Cook, Kieschnick and McCullough (2008), this part of the model is referred to as the "selection equation". Since the vectors α and β (which represent the coefficients of the exogenous variables) are allowed to be different, this allows for the effects of these variables on the choice of use to differ from their effects on the quantity of use.

VII. Data – Description and Trends

The data used in this study is obtained from a 2006 survey on school-to-work transition that was administered in Sri Lanka by the University of Colombo, with support from the World Bank. Data was collected on respondents who left school and were between the ages of 15-26 years at the time of the survey. The survey was administered between April and May of 2006 to 1026 individuals from 450 different households who completed formal schooling between 1999 and 2006. Care was taken to ensure that the sample was representative of the nation, with the exception of the conflict ridden provinces. The dataset contains retrospective information on the allocation of time across various activities from the date of leaving school until the time of the survey. Detailed information was obtained on the amount of time spent in wage-employment, self-employment, unemployment, inactivity and training. Information was also obtained on the socio-economic background of respondents as well as their personal characteristics.

The definitions of employment, unemployment and inactivity used in the survey are in accordance with conventional international usage. The population of unemployed consists of those seeking and available for work but who had no employment in the reference period, while those classified as inactive were neither looking nor available for work. The employed population consists of those individuals, who during the reference period either worked as paid employees (referred to as wage-employees) or as employers, own account workers or unpaid family workers (collectively referred to as the self-employed). Finally, those enrolled in a training program at either a public or private training center constitute our sample of trainees.

Figure 1 displays the fraction of school leavers in each of the six cohorts who engaged in various activities over the course of their first year after leaving school. These cohorts seem to have had similar experiences: about 80% of respondents had experienced some form of unemployment or inactivity, 20% engaged in some form of training, 10% were self-employed and between 30%-40% were engaged in wage employment[22].

Figure 2 extends this time window and follows a given cohort for as many years as we have data. The incidence of both forms of employment rises while the incidence of unemployment remains relatively unchanged. Thus, while individuals experience high probabilities of being unemployed immediately after leaving school, over time they are increasingly likely to find some source of employment.

Figure 3 reports the fraction of accumulated time spent in each activity. As length of time out of school increases, the fraction of time in employment rises, and so gradually the cohort successfully transits from school to work. While the 2005 cohort devoted over 70% of available post-school time to either inactivity or unemployment, only half the available time for the 2000 cohort was spent in either inactivity or unemployment.

The puzzle is that the school to work transition appears to be most difficult for the more educated, as shown in Figure 4. For each one of the graduation cohorts from 2000 to 2005, the O/L

[22] Fractions do not add up to one as individuals participate in multiple activities over the period.

and A/L certified respondents spend a significantly smaller fraction of their time in wage-employment relative to those who drop out of school without obtaining either of these two qualifications. Time allocations are especially stark for the 2001 cohort, with the least educated group of respondents spending over 35% of their post-schooling time in wage-employment, while the A/L certified spend less than 20% of their in wage work. The higher fraction of time spent in self-employment by the most educated makes up only a small fraction of this gap. Therefore, the most educated in Sri Lanka appear to have less success in the labor market after leaving school, consistent with Vodopivec and Withanachchi's (2006) findings regarding the poor labor market performance of university educated youth. The rest of the paper is devoted to investigating this puzzle.

VIII. Data –Construction and Variable Description

Instead of working with unemployment and inactivity separately, we combine these two labor market categories to create a single "non-employment" category. We do this to avoid the often arbitrary distinction that is made between the two while retaining all those individuals who possess some attachment to the labor force.[23] Other labor market categories are retained without any modifications.

The choice of variables to include in the empirical specification is drawn from our theory of time allocation outlined above. First, the variable "ability" is constructed from the results of an ability test which was administered to survey respondents. The test had a reasoning ability module as well as an English language skills module. Secondly, since the survey itself does not contain information on the value of assets owned at the household level, we construct a wealth index from the detailed information in the survey on the different types of assets owned by the household.[24]

[23] Details on the unemployment and out of labor force measures are provided in Appendix-A.
[24] Details on the construction of this index are provided in the Appendix-B below.

These are the (A, W) elements of the covariate vector X that we use in the estimation exercise detailed below.

Other explanatory variables used in our analysis include EXPOSURE, educational qualifications and gender. EXPOSURE is the accumulated length of the transition from school until the survey date in the middle of 2006. We allow it to enter in quadratic form. It allows us to measure the expected length of the transition from school to work. The educational achievement variables that we use are dummy variables for O-level and A-level certifications. The dummy variables are cumulative, and so anyone who completed the A-level also completed the O-level.

For a subset of the sample, we were able to match the respondent to their primary school. A separate survey of school attributes conducted by the World Bank allowed us to merge information on school quality with our data on school leavers. We use two measures of school quality, the student-teacher ratio and the proportion of trained teachers. These two variables then constitute the Q element of the covariate vector X. Trained teachers are those who completed a college degree. Our presumption is that graduates of higher quality schools would leave with higher levels of human capital, α_N. It is not clear if higher levels of α_N would raise or lower time at work, as it may both raise the productivity of time spent training while raising the opportunity cost of time in training programs.

Table 1 displays the means and medians for the fraction of time spent in each of the employment, self-employment, non-employment and training categories, along with the number of individuals who spent all or none of their time in each of these four activities. The largest fraction of respondents' time is clearly non-employment. On average, individuals in our sample spend 47% of their working lives in non-employment, followed by wage employment (28.4%), training (15.4%) and self-employment (9%). About 81% of the respondents in our sample have experienced at least one spell of non-employment in the observation period and 15% of all those who have ever been in a non-employment state have spent their entire time since leaving school not working. However, training is an important option for time use. About 64% of the individuals in our sample have engaged in some form of training over the observation period, with the average time spent in training being about 25 months.

IX. The Non-employment–Education Puzzle

Previous data sets have not had information on time spent training. We have shown that many youth in Sri Lanka spend time training after leaving school. Those specializing in training could well be labeled as non-employed in traditional surveys that divide the population into only three states, employed, unemployed and out of the labor force. Previous studies have shown a tendency for educated youth to spend long periods not working and apparently not seeking work, not just in Sri Lanka, but in the Middle East and North Africa as well[25]. We illustrate the issue by estimating equation (1) for each of the wage employment, self-employment and non-employment activities where the training group is incorrectly viewed as non-employed.

Results of this estimation exercise are displayed for each labor market activity, for each of the logit, probit, loglog and complementary loglog specifications in tables 3, 4 and 5. The results from these four specifications are consistent with one other. Even though the magnitude of the effect of the individual covariates differs across these four models, the coefficient signs and significance levels are the same across the various models. Given this and in order to streamline the discussion below, we shall restrict our attention to the results from the one-part model with the logit specification.

[25] Though the literature poses this puzzle in terms of the relationship between education and unemployment, as we argue in appendix B, non-employment is often a better indicator of employment problems. The puzzle can thus be recast in terms of the positive association between education and non-employment, which, as we show above, does exist in our data.

As displayed in table 6, those receiving an A/L qualification spend 9% more time in non-employment, summing the effects of the A/L and O/L qualifications. F-tests for the joint insignificance of the O/L and A/L effects are rejected at conventional significance levels. In addition, more able individuals- as indicated by scores on the ability test- spend more time in non-employment than their equally educated but less able classmates. Consistent with the results of previous studies on Sri Lanka, a strong positive association exists between human capital and non-employment.

X. Results for One Part Model-Separating Training from Non-employment

We are now in a position to document how the results when we separate out training as a distinct activity from non-employment. Instead of working with only three activities, equation 1 is now re-estimated using four different activity categories-wage employment, self-employment, non-employment and training. The results of this estimation exercise are displayed in table 7. Note that by construction of the logit specification, the wage employment and self-employment categories are unchanged from before even after the redefinition of the non-employment category. We first address the issue of how educational qualifications affect the time allocations to these different activities, followed by the effects of other covariates of interest. All results refer to the one-part model with the logit specification.

EDUCATIONAL QUALIFICATIONS

Education no longer has a significant effect on non-employment once we separate out time spent in training. The F- test for the joint insignificance of the O/L and A/L variables in the non-employment equation can no longer be rejected at conventional significance levels. However, education has a strong effect on training. The estimated impact of obtaining an A/L degree is economically large, with those receiving an A/L education allocating an extra 8% of their available time to training. The F- test of joint significance of the O/L and A/L effects on training strongly rejects the null at conventional significance levels. In Sri Lanka the education puzzle is resolved by decomposing non-employment into training and true idleness. The apparent positive correlation between education and non-employment was due entirely to the true correlation between education and training.

Training also explains the puzzling employment effect. A/L certification has a negative and significant correlation with both wage employment and self-employment. Thus, more educated individuals spend smaller fractions of time in both forms of employment while allocating a larger fraction of time to training.

ABILITY

Ability mimics the pattern for schooling. Ability no longer affects time in non-employment once we split out time spent in training. The estimated impact is quite large: a move from the bottom decile to the top decile of the ability distribution increases the fraction of time devoted to training by 10%. More able individuals allocate significantly less time to employment as they allocate more time to training.

HOUSEHOLD WEALTH

Household wealth has a positive and statistically significant impact on time allocated to non-employment. Wealth has a negative and significant effect on training. It is the educated and more able poor who are most likely to enroll in training programs. Family wealth does not affect the probability that youth engage in wage employment or self-employment.

EXPOSURE

EXPOSURE is the length of time the respondent has been out of school. EXPOSURE has a positive but declining effect on the fraction of time allocated to wage employment. A 10 month increase in EXPOSURE leads to an additional 10% increase in time allocated to wage-employment. Because the mean fraction of time spent in wage employment is about 0.3, increasing EXPOSURE by ten months raises the fraction of time spent in employment by about 33%, on average. In contrast to the wage-employment results, increasing levels of EXPOSURE reduces the fraction of time devoted to self-employment. This suggests that those entering the labor force through self-employment move into wage-employment over time, either because work experience in self-employment makes them more attractive to potential employers or because they work in a family business until they succeed in finding better paying jobs on the labor market. EXPOSURE's effect on training is similar with the fraction of time spent training falling significantly as time out of school rises. That is consistent with the presumption that training will concentrate soon after leaving school. Non-employment also declines over time, but the joint effect of the quadratic terms is only marginally significant.

In figure 6, we provide graphical simulations of the effects of EXPOSURE on the cumulative time allocations for all four activities. Time allocations to non-employment and training fall monotonically with increasing levels of EXPOSURE. The fraction of time spent in non-employment falls slowly from a little over 0.5 to about 0.45 over the post schooling period, and so the average transition from school to work is relatively slow for those who do not find work soon after leaving school. On the other hand, the fraction of time spent in wage employment almost doubles over this period from about 0.15 to 0.30, with two-thirds of the transition being from training to work and one-third from non-employment to work. Time spent in self-employment remains fairly constant at 0.1 over this period.

XI. Results for ZIB Model- Separating Training from Non-employment

The ZIB model allows us to correct for nonrandom sorting into the various activities. Results are presented in table 8.

EDUCATIONAL QUALIFICATIONS

For those individuals who allocate a non-zero fraction of their time to training, greater education has a positive impact on time allocated to training while reducing the fraction of time spent in both forms of employment as well as for non-employment. Thus, as was the case for the fractional logit model, more educated individuals spend less time working but more time training.

Regarding the factors influencing participation in various activities, receiving an A/L education significantly increases the probability of participation in both training and non-employment. Schooling does not significantly affect the probability of entering employment.

ABILITY

The effect of ability is different from what we found under the fractional logit model. While the ability effect is still positive for training, it is no longer statistically significant. Moreover, ability reduces the fraction of time spent in non-employment, with this effect being statistically significant. This contrasts with the positive but insignificant effect of ability on non-employment that we obtained in table 7. As before, more able people spend less time in employment, though these results are no longer statistically significant.

Unlike the case for the level equation, ability has a strong positive effect on participation in training. Individuals in the top decile of the ability distribution are about 50% more likely to devote some fraction of their available time to training compared to those in the bottom decile. Ability also has statistically significant effects on participation in both wage employment and non-employment, with those in the top decile about 13% more likely to enter non-employment and 14% less likely to enter wage employment, relative to those in the bottom decile.

HOUSEHOLD WEALTH

Individuals from richer families spend a larger fraction of their available time in both self-employment and non-employment, while reducing time allocations to training. This is in contrast to the fractional logit model, under which wealth had statistically significant impacts on only non-employment and training.

The influence of wealth on participation is only statistically significant for the case of training, with rising levels of wealth making participation in training programs less likely.

EXPOSURE

The effects of EXPOSURE in the level equation are in the same direction as they were under the fractional logit model. The major difference is the precision gained by EXPOSURE, which now has statistically significant effects on both non-employment and training.

EXPOSURE has a positive and statistically significant impact in the selection equation for each labor market activity. This corresponds well with our previous findings displayed in figure 2– the more time individuals have spent out of school, the more likely they are to have participated in multiple labor market activities.

XII. Results-School Quality Sub-Sample

We were able to match only 381 of our original survey participants with information from a World Bank survey providing information on primary school attributes. We replicate our fractional logit specification from table 7 including school quality estimates. Results are presented in table 9. As the effects of the common covariates are similar to those in table 7, we focus only on the effect of school quality on time allocation.

Improving school quality hastens the school to work transition. Increasing student-teacher ratios lowers the fraction of time allocated to wage employment while exposure to better trained teachers increases time in wage work. Consistent with the interpretation that better schools improves the school-to-work transition, the greater time in work comes from lessened time in non-employment. The school quality effects on non-employment are jointly significant at the 5% level. School quality does not significantly affect time spent in self-employment or training. Our results on school quality suggest that school quality plays an important role in easing the transition from school to work.

XIII. Simulation Results

We use the estimated results from tables 7 and 9 to simulate the effects of different covariates of interest on the time allocations to various activities. These simulations are shown in figures 7-11.

Figure 7 shows how educational qualifications affect wage employment allocations. It focuses on how wage employment allocations change over the range of EXPOSURE for three different educational levels: O/L graduates; A/L graduates; and non-graduates: those who drop out before the O/L qualification. Since the simulated time allocations for the O/L and non-graduate groups are virtually indistinguishable from one another, we focus only on comparisons between A/L graduates and non-graduates. Both of these groups experience rising time allocations to wage employment as EXPOSURE rises, with the fraction of allocated time for non-graduates always higher than the corresponding fraction for A/L graduates. The difference between time allocations for these two groups is about 10% of the available endowment of time at a level of EXPOSURE of seventy months.

Figure 8 displays differences in wage time allocations between the top and bottom deciles of the ability distribution. Similar to the results obtained for education, those who are more able are likely to spend a smaller fraction of their time engaged in wage employment.

The results for training contrast sharply with those for wage employment. Figure 9 shows that more educated individuals are likely to devote larger fractions of their time to training, with the difference in time allocations between A/L graduates and non-graduates reaching around 12% of the available time endowment at five months of EXPOSURE, which then declines to about 5% as the level of EXPOSURE reaches seventy months. Likewise, as illustrated in figure 10, more able individuals train more, with training allocations for individuals in the top decile of the ability distribution being around 25% of their time endowment, at five months of EXPOSURE. This contrasts with training allocations of only 15% for those in the bottom decile, for the same level of EXPOSURE. Even though these differences decline over time, there is still a 5% gap in favor of the more able as EXPOSURE reaches seventy months.

Finally, the simulation results for the effects of school quality attributes on wage employment are shown in figures 11 and 12. A rising student-teacher ratio reduces time allocations to wage employment, with individuals enrolled in schools in the bottom decile (with 11 students per class) devoting about 20% of their available time to wage work, as opposed to only 10% for those in the top decile (with 30 students per class), at an EXPOSURE level of six months. These differences in allocations become even more pronounced as EXPOSURE rises, with the differences between the top and bottom deciles reaching about 20% of the time endowment at seventy months of EXPOSURE.

Figure 12 shows large impacts on wage employment of a rising proportion of trained teachers. Individuals enrolled in schools in the top decile of the distribution of trained teachers (corresponding to about 9 out of 10 trained teachers) spend about 30% of their time in wage employment, compared to 23% for those in the bottom decile (corresponding to about 6 out of 10 trained teachers), at seventy months of EXPOSURE.

XIV. Conclusions

One of the main conclusions to come out of many prior studies on Sri Lanka is the finding of a positive association between education and unemployment. This finding constitutes a puzzle, as traditional human capital models argue that higher levels of education should increase the chances of finding and keeping employment. While the raw data used in our study would also seem to support the presence of such a relationship, we find that controlling for the amount of time spent in training eliminates the puzzling positive effect of schooling or ability on time spent out of the labor force. Instead, better educated and more able youth spend a larger fraction of their available time in training, which leaves less time to allocate for work.

We also find that school quality has a large effect on time spent in wage employment. Our simulations indicate that individuals enrolled in the top tier schools in terms of quality-defined as those with the lowest student-teacher ratios and the highest fraction of trained teachers- spend as much as 30% more of their available time in wage employment compared to those enrolled in the bottom tier. These results are consistent with the notion that better schools help smoothen the transition from schooling to stable employment.

XV. References

Cook D, Kieschnick R and McCullough BD. Regression Analysis of Proportions in Finance with Self Selection. *Journal of Empirical Finance*.Vol 15. 860-867.

Cameron S and Taber C. 2004. Estimation of Educational Borrowing Constraints Using Returns to Schooling. Journal of Political Economy. Vol 112(1).

Dickens WT and Lang K. 1995. An Analysis of the Nature of Unemployment in Sri Lanka. *Journal of Development Studies*. Vol 31.

Filmer D and Pritchett L. 1994. Estimating Wealth Effects without Expenditure Data or Tears. World Bank Research Working Paper.

Glewwe P. 1987. Unemployment in Developing Countries: Economists Models in Light of Evidence from Sri Lanka. *International Economic Journal*. Vol 4. 1-17.

Growing Up Global: The Changing Transitions to Adulthood in Developing Countries. National Academies Press.

Jones, S and Riddell, C. 1999. The Measurement of Unemployment: An Empirical Approach. *Econometrica*. 67(1). 147-162.

Maddala, GS. 1991. A Perspective on the Use of Limited-Dependent and Qualitative Variables Models in Accounting Research. *The Accounting Review*. Vol 66. 786-807.

Nanayakkara, A.G.W. Employment and Unemployment in Sri Lanka –Trends, Issues and Options. Department of Census and Statistics Sri Lanka.

Papke, L and Woolridge J. 1996. Econometric Methods for Fractional Response Variables with an Application to 401 (K) Plan Participation Rates. *Journal of Applied Econometrics*. 11: 619-632.

Rama M. 2003. The Sri Lankan Unemployment Problem Revisited. *Review of Development Economics*. 7(3): 510-525.

Ryan, P. 2001. The School-to-Work Transition: A Cross National Perspective. *Journal of Economic Literature*. 39(1): 34-92.

Vodopivec, M and Withanachchi,N. 2006. Reducing Graduate Unemployment in Sri Lanka. World Bank. Mimeo.

Wolpin, K. 1995. Empirical Methods for the Study of Labor Force Dynamics. Taylor and Francis.

World Development Report. 2007. New York: Oxford University Press.

Table 1: Summary Statistics for Time Allocations

	Fraction of Time Allocated to Various Activities			
	0	**1**	**Median**	**Mean**
Employment	369	34	.128	.284
Self-employment	762	29	0	.09
Non-Employment	168	109	.484	.470
Training	329	3	.052	.154
Total Observations	900			

Note: The numbers in columns 1 and 2 above are to be interpreted as follows: out of a total of 900 respondents, 369 respondents are never engaged in wage employment, while 34 respondents allocate all of their time to wage employment, with the remaining 497 individuals (900-369-34=497) spending a part of their labor market time in wage employment. Similar interpretations follow for the self-employment, non-employment and training categories.

Table 2: Alternative Conditional Mean Specifications for Fractional Response Variables

Model Designation	**Distribution Function**	**Conditional Mean:** $\boldsymbol{G(\beta X)}$
Logit	Logistic	$\frac{e^{\beta X}}{1+e^{\beta X}}$
Probit	Standard normal	$\Phi(\beta X)$
Loglog	Extreme maximum	$e^{-e^{-\beta X}}$
Complementary loglog	Extreme minimum	1-$e^{-e^{\beta X}}$

Table 3: Regression Results for One Part Model –Wage Employment

	QML Estimation			
	Logit	**Probit**	**Loglog**	**Cloglog**
Exposure	. 0406***	. 0247***	.0239***	.0336***
	(.0129)	(.007)	(.0069)	(.0110)
Exposure Squared	.- 0003**	-.0002***	-.0002***	-.0002**
	(.0001)	(.00007)	(.00007)	(.0001)
Male	. 458***	.274***	.2610***	.3784***
	(.114)	(.067)	(.0638)	(.0963)
O/L	. 0209	.0119	.0089	.0171
	(.156)	(.0949)	(.0949)	(.1258)
A/L	-.415***	-.248***	-.232***	-.3573***
	(.152)	(.0888)	(.0811)	(.133)
Wealth	.0327	.0200	.0202	.0258
	(.0277)	(.0167)	(.0164)	(.0226)
Ability	-.645**	-.387***	-.367**	-.525**
	(.273)	(.159)	(.143)	(.236)
Observations	900	900	900	900

Notes: (1) Robust Standard errors are reported below the coefficients in parentheses.

(2) Stars represent p-values with *** p<0.01, ** p<0.05, * p<0.1

Table 4: Regression Results for One Part Model –Self-employment

	QML Estimation			
	Logit	**Probit**	**Loglog**	**Cloglog**
Exposure	-.041*	-.0209*	-.0157*	-.0386*
	(.0220)	(.0113)	(.008)	(.020)
Exposure Squared	.0004**	.0002**	.0001**	.0004**
	(.0002)	(.0001)	(.00008)	(.0002)
Male	.818***	.3978***	.2869***	.786***
	(.2133)	(.104)	(.0768)	(.203)
O/L	.3154	.158	.118	.302
	(.303)	(.153)	(.115)	(.289)
A/L	-.510*	-.252*	-.185*	-.485**
	(.260)	(.131)	(.098)	(.245)
Wealth	-.0537	-.030	-.025	-.047
	(.0529)	(.027)	(.020)	(.050)
Ability	-1.016**	-.499**	-.364**	-.971**
	(.453)	(.231)	(.174)	(.425)
Observations	900	900	900	900

Notes: (1) Robust Standard errors are reported below the coefficients in parentheses.

(2) Stars represent p-values with *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5: Regression Results for One Part Model –Training and Non-employment Combined

	QML Estimation			
	Logit	**Probit**	**Loglog**	**Cloglog**
Exposure	-.018	-.011	-.013	-.0124
	(.012)	(.007)	(.009)	(.007)
Exposure Squared	.00009	.00006	.00006	.00007
	(.0001)	(.00007)	(.00009)	(.00007)
Male	-.691***	-.420***	-.546***	-.421***
	(.107)	(.065)	(.084)	(.066)
O/L	-.134	-.079	-.110	-.069
	(.151)	(.093)	(.112)	(.101)
A/L	.524***	.318***	.429***	.314***
	(.140)	(.084)	(.113)	(.085)
Wealth	-.010	-.006	-.007	-.007
	(.026)	(.016)	(.020)	(.017)
Ability	.913***	.553***	.707***	.551***
	(.252)	(.151)	(.199)	(.149)
Observations	900	900	900	900

Notes: (1) Robust Standard errors are reported below the coefficients in parentheses.

(2) Stars represent p-values with *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: Marginal Effects for One Part Model - Training and Non-employment Combined

Variables	Wage Employment	Self-Employment	Non-Employment
Exposure	.008***	-.0029*	-.004
	(.002)	(.0015)	(.002)
Exposure Squared	-.00006**	.00003**	.00002
	(.00003)	(.00002)	(.00003)
Male	.090***	.059***	-.158***
	(.022)	(.015)	(.024)
O/L	.0041	.0208	-.030
	(.030)	(.018)	(.034)
A/L	-.0799***	-.0345**	.118***
	(.028)	(.016)	(.030)
Wealth	.0065	-.0038	-.002
	(.005)	(.0037)	(.006)
Ability	-.128**	-.072**	.211***
	(.054)	(.0317)	(.058)
Observations	900	900	900

Notes: (1) Marginal effects reported.

(2) Standard errors in parentheses.

(3) Stars represent p-values with *** p<0.01, ** p<0.05, * p<0.1

Table 7: Marginal Effects for One Part Model – Training and Non-Employment as Separate Activities

	Wage Employment	**Self-Employment**	**Non-Employment**	**Training**
Exposure	.008***	-.0029*	-.0014	-.001
	(.002)	(.0015)	(.002)	(.0014)
Exposure Squared	-.00006**	.00003**	.000001	.000006
	(.00003)	(.00002)	(.0001)	(.00001)
Male	.090***	.059***	-.150***	-.0048
	(.022)	(.015)	(.023)	(.010)
O/L	.0041	.0208	-.020	.014
	(.030)	(.018)	(.036)	(.021)
A/L	-.0799***	-.0345**	.016	.077***
	(.028)	(.016)	(.030)	(.016)
Household Wealth	.0065	-.0038	.014**	-.018***
	(.005)	(.0037)	(.006)	(.003)
Ability	-.128**	-.072**	.041	.101***
	(.054)	(.0317)	(.052)	(.023)
Observations	900	900	900	900

Notes: (1) Marginal effects reported.

(2) Standard errors in parentheses.

(3) Stars represent p-values with *** p<0.01, ** p<0.05, * p<0.1

Table 8: Zero Inflated Beta Model for Fractional Response Data

Variables	Wage Employment	Self-Employment	Non-Employment	Training
Level Equation				
Exposure	-.006*	-.010**	-.0106***	-.011***
	(.003)	(.004)	(.002)	(.001)
Exposure Squared	. 00004	.00008*	.00007***	.00009***
	(.00003)	(.00004)	(.00002)	(.00002)
Male	.146***	.028	-.045**	.013
	(.026)	(.049)	(.021)	(.015)
O/L	-.019	-.023	-.0389	-.020
	(.035)	(.056)	(.028)	(.027)
A/L	-.038	-.155**	-.030	.103***
	(.035)	(.070)	(.027)	(.018)
Wealth	.009	.040***	.018***	-.016***
	(.007)	(.012)	(.0049)	(.004)
Ability	-.154	-.078	-.081*	.054
	(.062)	(.121)	(.048)	(.033)
Observations	531	138	732	571
Selection Equation				
Exposure	.018***	-.003	.006**	.016***
	(.0037)	(.002)	(.002)	(.0036)
Exposure Squared	-.0001***	.00004*	-.00005**	-.0001***
	(.00004)	(.00003)	(.00003)	(.00004)
Male	.0603*	.086***	-.152***	-.083**
	(.034)	(.023)	(.025)	(.035)
O/L	-.027	.030	.012	.084*
	(.050)	(.028)	(.032)	(.049)
A/L	-.055	-.031	.067**	.123***
	(.045)	(.029)	(.030)	(.043)
Wealth	.004	-.015	.008	-.051***
	(.009)	(.006)	(.006)	(.008)
Ability	-.141*	-.102	.133**	.438***
	(.082)	(.055)	(.061)	(.090)
Observations	900	900	900	900

Notes: (1) Marginal Effects Reported

(2) Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 9: Fractional Logit Estimates-School Quality Sub-Sample

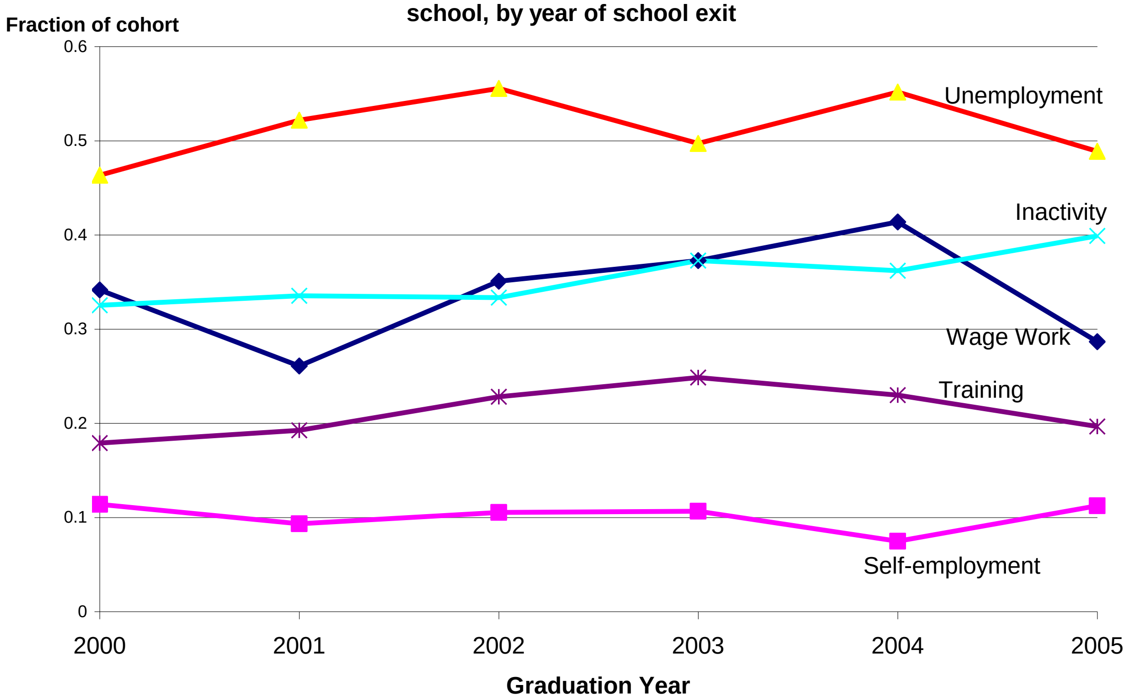
Variables	Wage Employment	Self-Employment	Non-Employment	Training
Exposure	.0107***	-.003	-.003	-.0003
	(.003)	(.002)	(.004)	(.001)
Exposure Squared	-.00009**	.00004*	-.000005	-.000003
	(.00004)	(.00002)	(.00004)	(.00002)
Male	.067*	.065***	-.139***	-.003
	(.036)	(.021)	(.039)	(.016)
O/L	-.002	.019	-.020	.008
	(.050)	(.027)	(.060)	(.030)
A/L	-.016	-.049**	.026	.027
	(.047)	(.022)	(.049)	(.023)
Household Wealth	.008	.001	.011	-.023***
	(.009)	(.005)	(.011)	(.004)
Ability Score	-.119	-.043	-.007	.105***
	(.092)	(.057)	(.090)	(.038)
Student-Teacher Ratio	-.007***	.0007	.006***	.0007
	(.002)	(.001)	(.002)	(.001)
Proportion-Trained Teachers	.268*	-.129	-.173	.057
	(.153)	(.089)	(.170)	(.060)
Observations	381	381	381	381

Notes: (1) Marginal effects reported.

(2) Standard errors in parentheses.

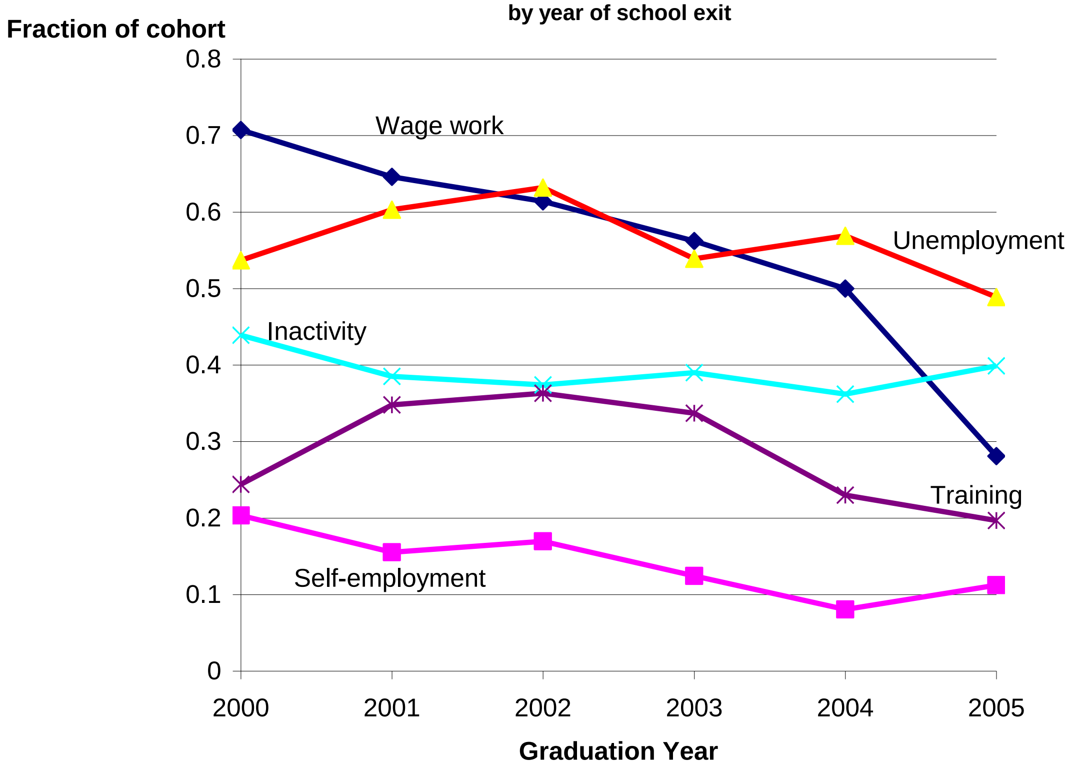
(3) Stars represent p-values with *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Figure 1: Fraction of graduates engaged in each activity one year after leaving school, by year of school exit

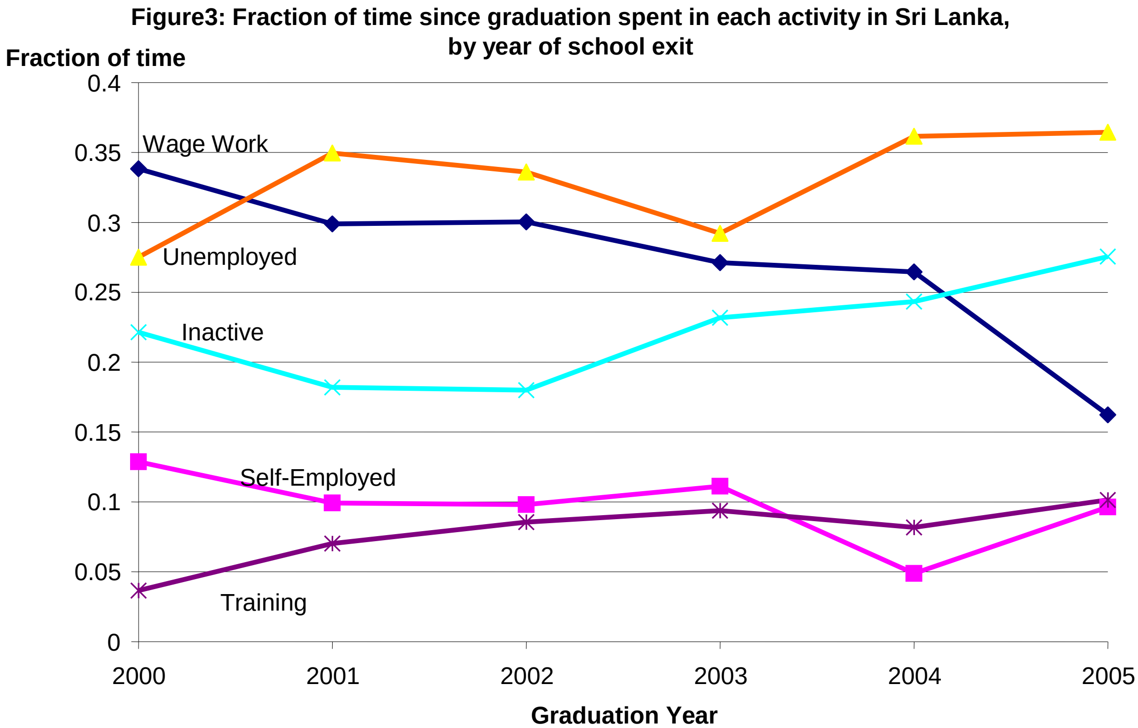


Source: Author's Calculations

Figure 2: Fraction of graduates who have ever engaged in each activity after leaving school, by year of school exit

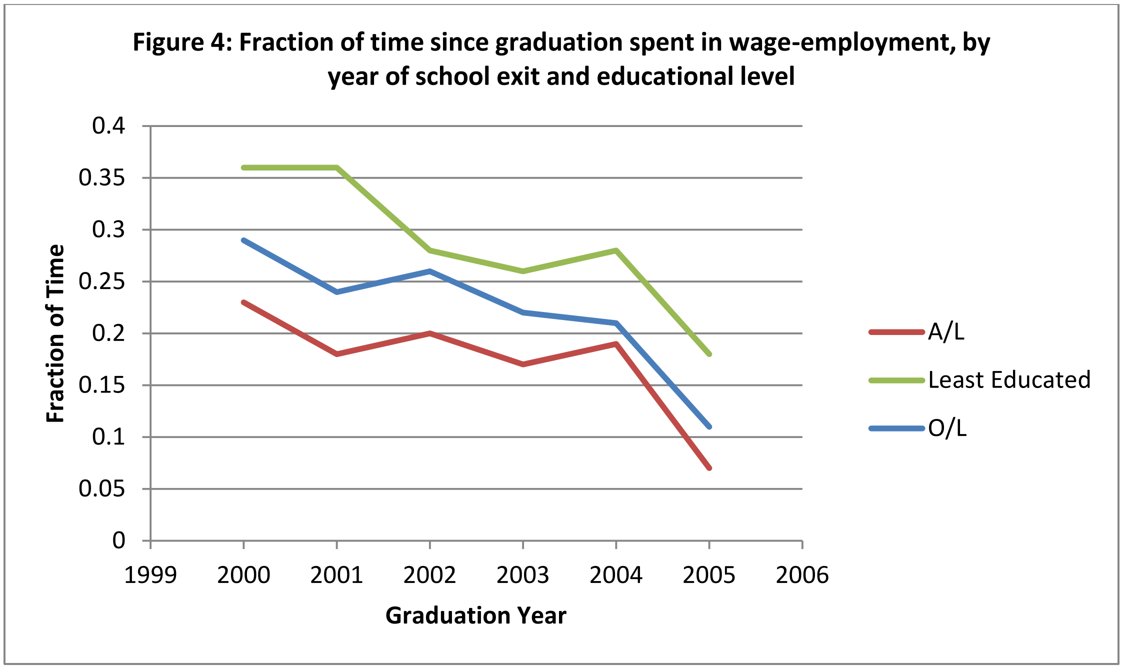


Source: Author's Calculations



Source: Author's Calculations

Figure 4: Fraction of time spent in wage-employment by educational category



Note: The least educated group consists of that group of individuals who did not obtain an O/L certification and therefore, the A/L certification, as a pass grade on the O/L exam is required to sit for the A/L exams.

Source: Author's Calculations

Figure 5: Illustration of altrnative lifetime earnings streams from full-time and part-time training options

Figure 5A: Full-time training dominates

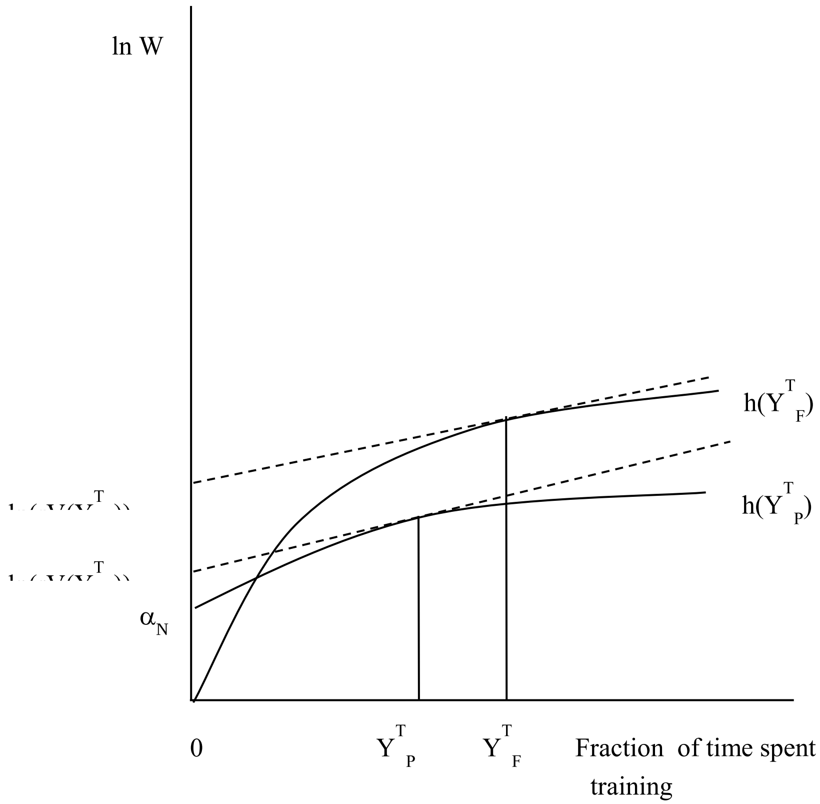
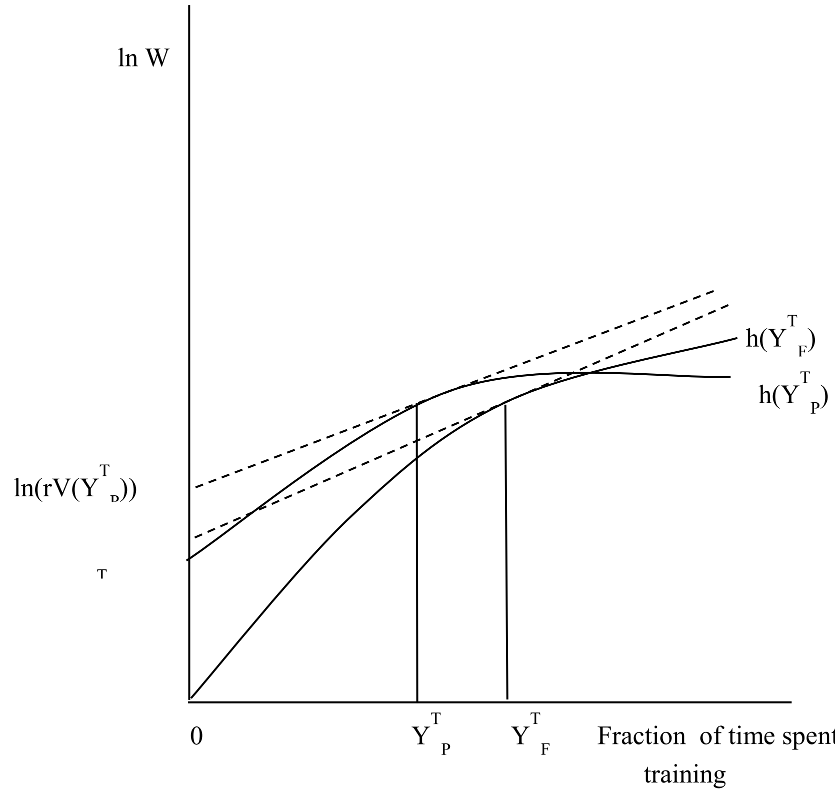
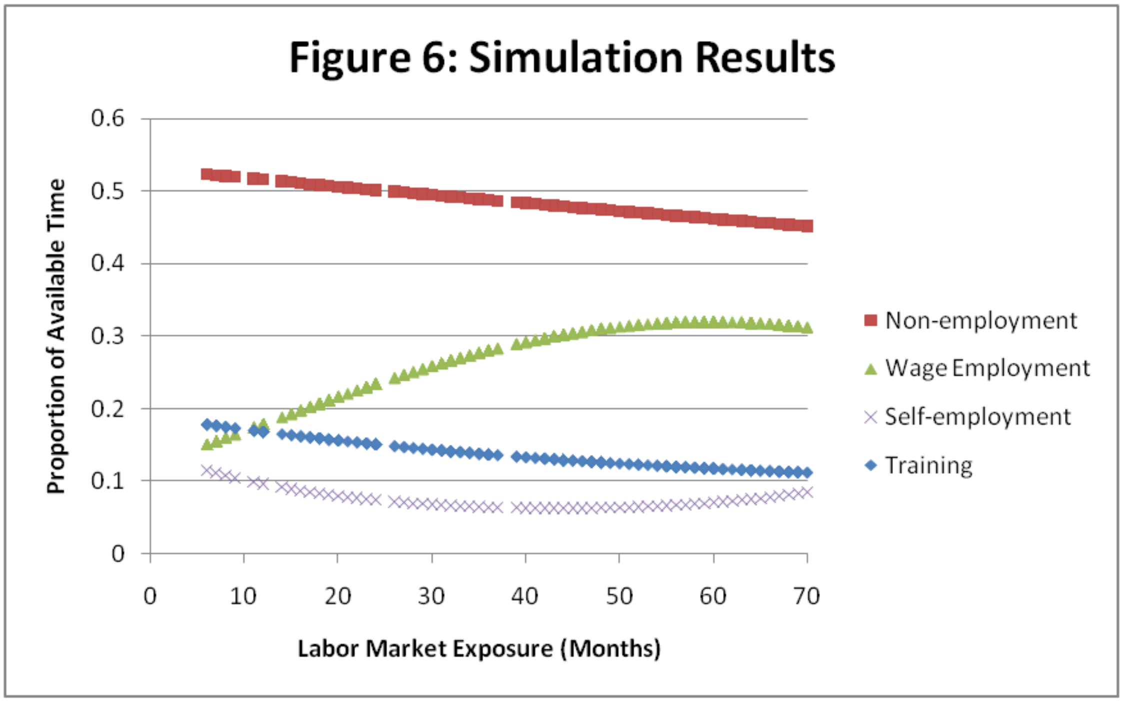


Figure 5B: Part-time training dominates



Notes: The dashed lines are iso-present value lines with slope equal to the interest rate, r. At period zero, the child's earnings capacity indexed by α_N will reflect all accumulations of human capital up to that period.

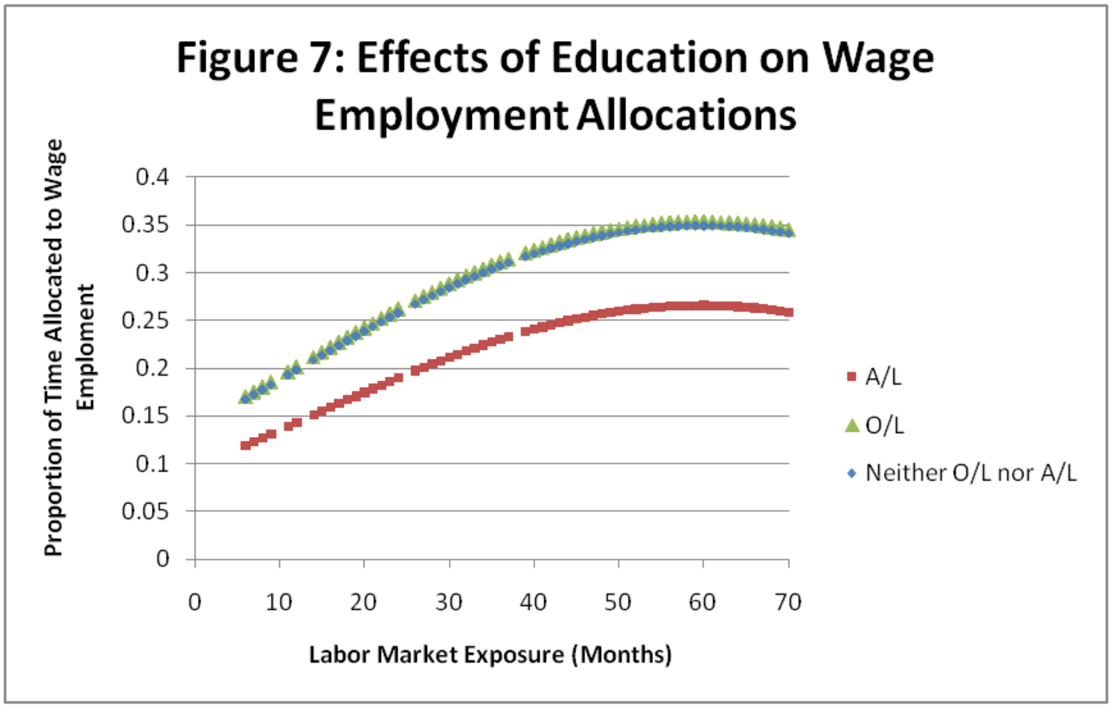
Figure 6: Simulation Results



Source: Author's Calculations

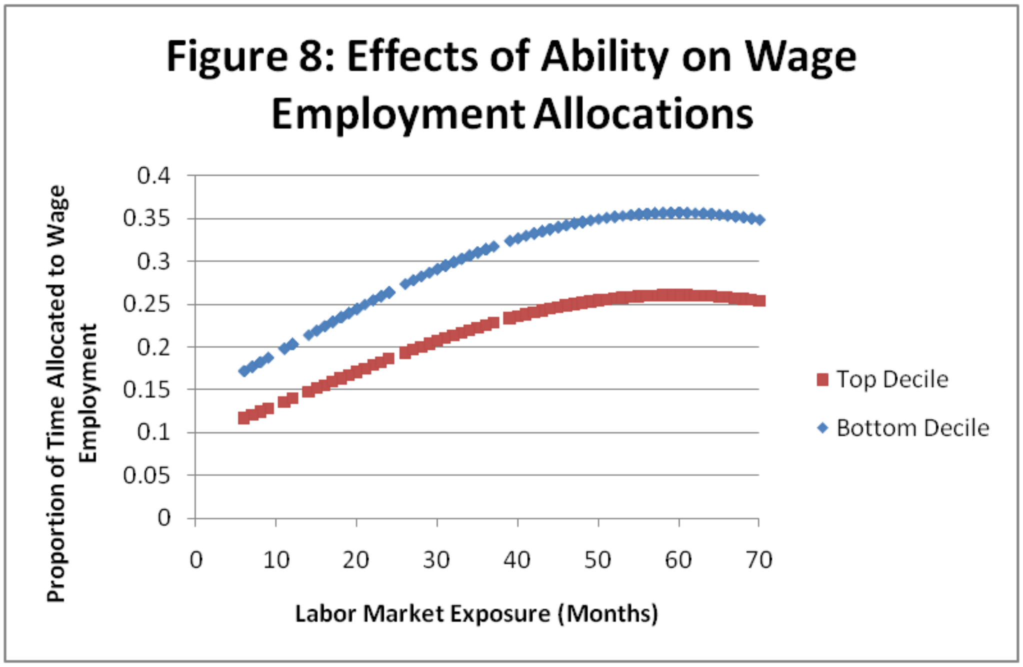
Note: At any given level of exposure, the proportion of time allocated to all activities must sum to one.

Figure 7: Education and Wage Employment Allocations



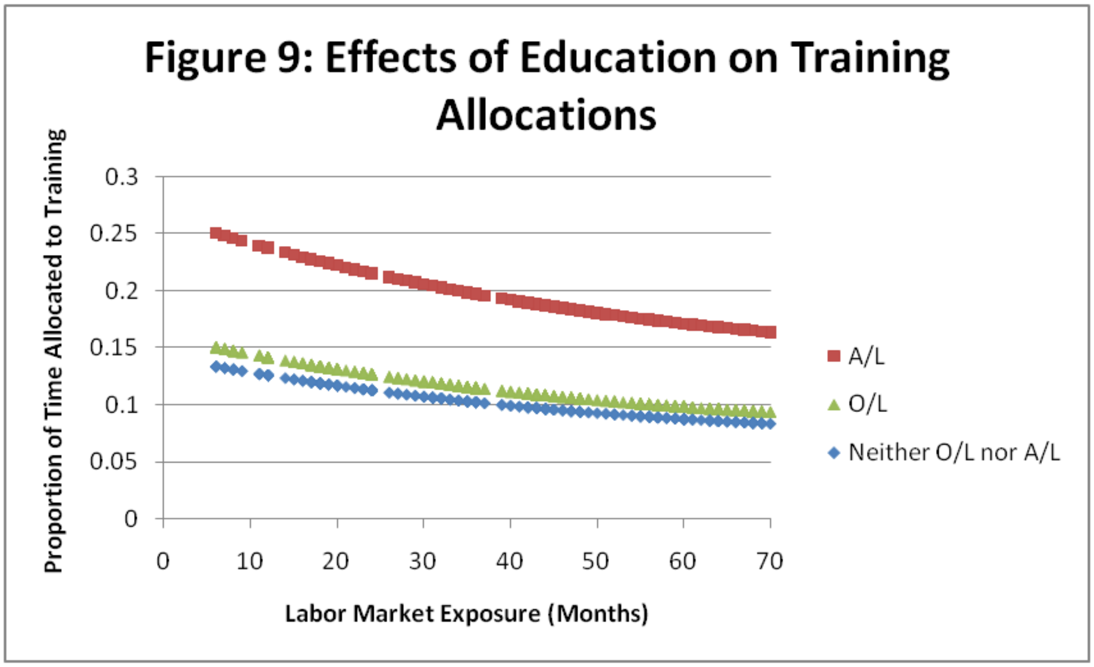
Source: Author's Calculations

Figure 8: Ability and Wage Employment Allocations



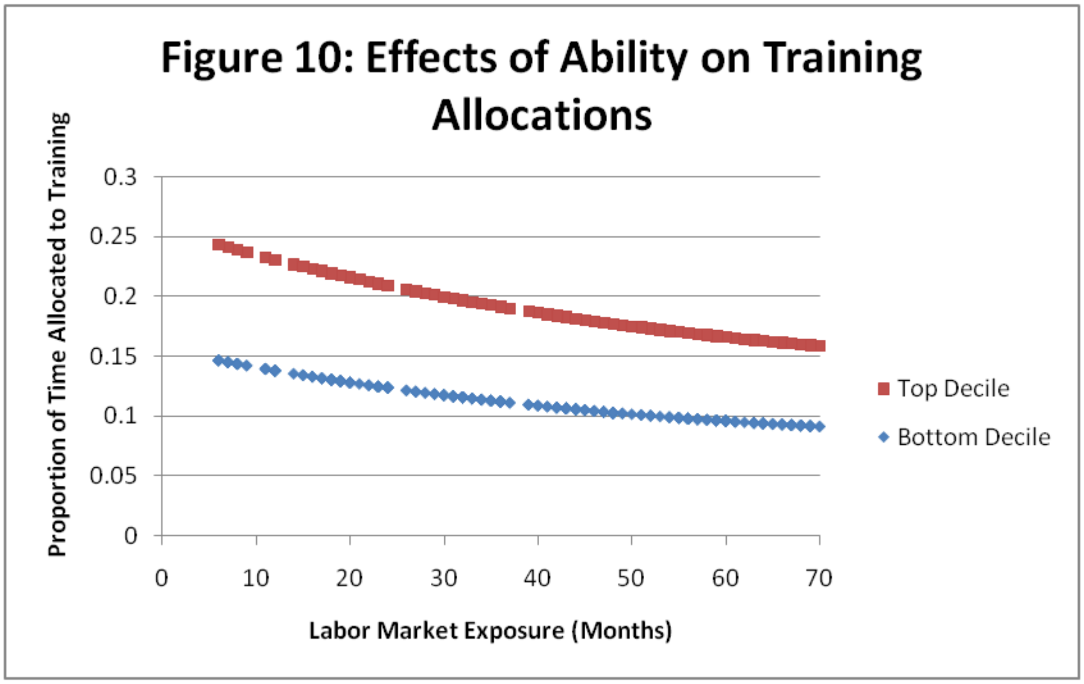
Source: Author’s Calculations

Figure 9: Education and Training Allocations



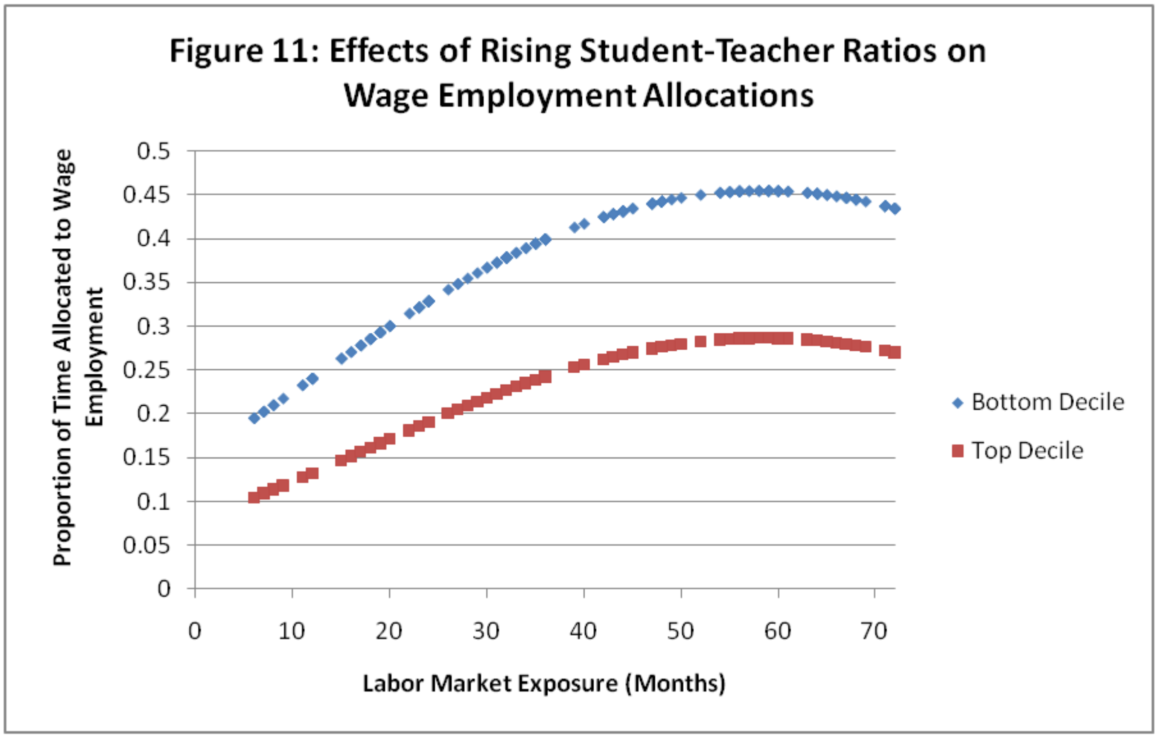
Source: Author’s Calculations

Figure 10: Ability and Training Allocations



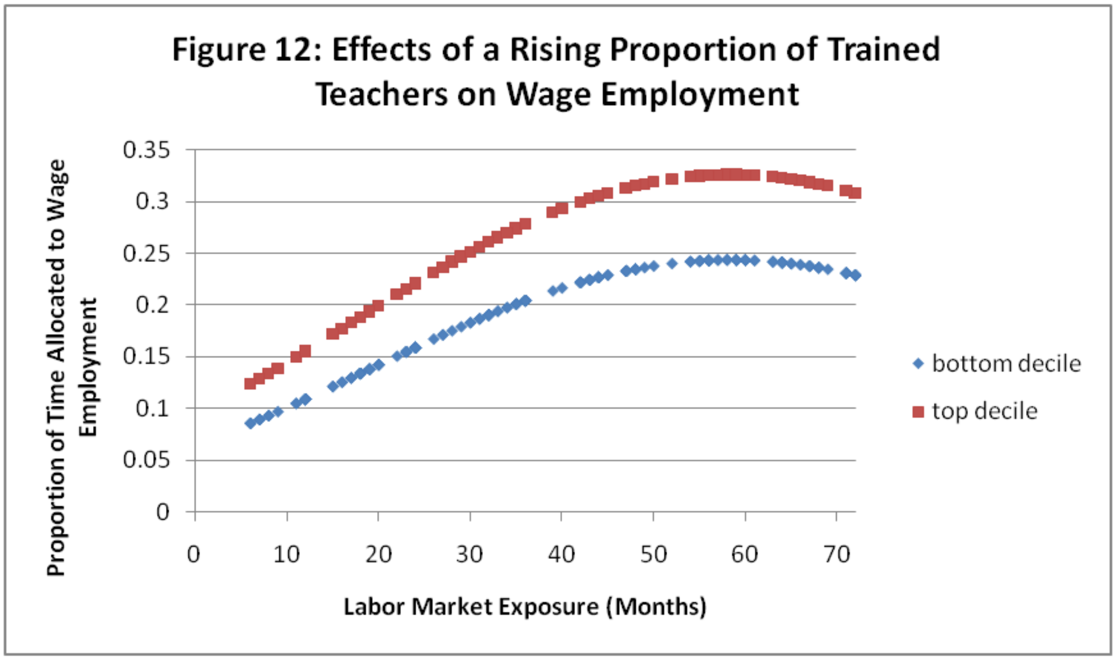
Source: Author's Calculations

Figure 11: School Quality Effects on Wage Employment Allocations



Source: Author's Calculations

Figure 12: Training and Wage Employment



Source: Author's Calculations

Appendix- A

Data Construction

As described in section 6, the data used in this study from a survey of Sri Lankan youth carried out in 2006, which was intended to be the primary source of information on the school-to-work transition process in Sri Lanka. This was a national survey (excluding the two conflict ridden provinces in the North and East) of the employment, unemployment, inactivity, training and post-school educational experiences of 16-25 year old youth who had stopped formal schooling over the years 1999-2006. Information on labor market experiences over this period was collected retrospectively in 2006.

Survey respondents were asked to complete a diary for the entire period from the time they left school to the time of the survey, in which they recall their labor market status in each month. The classification of labor market states was as follows: (1) Wage Employment, (2) Self-employment, (3) unemployment, (4) Inactivity and (5) Training. The survey thus provides information on the amount of time spent in each labor market state, for each survey respondent over the observation period (the observation period is defined in section 7).

We proceeded to aggregate the labor market states further, till we were left with the following four labor market states: (1) Wage Employment (same as above), (2) Self-employment (same as above), (3) Non-employment (obtained by combining unemployment and inactivity), (4) Training (same as above).

The decision to aggregate unemployment and inactivity to form a single non-employment category was based on the often thin distinction between the two that is made in the literature. Many countries base the distinction between unemployment (U) and out-of-the-labor force (O) on the basis of job search behavior, which is supposed to be indicative of differences in the attachment to the

Appendix- A (Continued)

labor force between these two categories of workers. However, problems arise because some non-searchers may have substantial attachment to the labor force, leading to their classification as O workers being inappropriate[26]. To ensure that we are indeed capturing individuals with at least some attachment to the labor market, we eliminate from our sample all of those respondents who spent their entire careers in the state of inactivity. This then reduces our sample from 1026 individuals to 917 individuals.

Two additional sources of information were used for the purposes of this paper. The first additional source of information on our respondents comes from an ability test that was administered by the National Education Research and Evaluation Centre of the University of Colombo to most of the original survey participants. Each question on this test carried ten points, with the maximum score attainable being one hundred points. We merged this data on ability scores with the data on labor market states to obtain a single complete file. As the ability score was missing for some of our respondents, we dropped the individuals with missing ability scores from our sample. This then reduced our final sample to 900 individuals.

[26] See, for example, Jones and Riddell (1999).

Appendix- B

Principal Components Index

This appendix discusses the issue of how to use information on household ownership of assets to construct an index of from these assets that can then act as a proxy for household wealth. Forming a linear index from these assets requires that we impute certain weights to each asset included in the construction of the index. Picking these weights is a potentially problematic issue. Typically, asset prices are used as weights, but this is not feasible in our particular case as the survey does not collect information on asset prices. We explore then, a different approach to constructing these weights- the method of principal component analysis (PCA).

PCA is a method for extracting from a large number of variables those linear combinations of them which provide the largest amount of information common to all of them. For the purposes of this paper we will only work with the first principal component.

Our asset index is then created as follows:

$$A_j = f_1 * \frac{(a_{j1} - a_1)}{s_1} + \cdots + f_N * \frac{(a_{jN} - a_N)}{s_N}$$

Where A_j denotes the asset index for household j, f_1 is the scoring factor for the first asset as determined by the method of principal components, a_{j1} is the value of the first asset for the household and a_1 and s_1 are, respectively, the mean and standard deviation of the first asset value across all households.

Filmer and Pritchett (1994) apply their asset index obtained using PCA to examine the distribution of average asset ownership across poor, middle and rich households in India and find that their index seems to capture well the asset variation across these different groups and is robust to the inclusion of different subsets of variables in the construction of the PCA.

Appendix- C

Table 1: Unemployment Rate by Age Group

Year	All Ages	Age Group				
		15-19	**20-29**	**30-39**	**40-49**	**50+**
1999	8.9	28.4	18.9	4.4	1.6	1.0
2000	7.6	23.4	17.4	3.6	1.4	0.8
2001	7.9	29.8	18.4	3.4	1.4	0.5
2002	8.8	30.1	20.1	4.0	1.5	0.8

Source: Quarterly Labor Force Statistics (QFLS Statistics)

Table 2: Unemployment Rate by Level of Education

Year	Total	No Schooling	Grade 0-4	Grade 5-10	O/L	A/L
1999	8.9	0.4	1.9	8.2	13.6	17.9
2000	7.6	1.2	1.0	7.5	11.3	14.9
2001	7.9	0.5	1.5	7.1	11.8	15.3
2002	8.8	1.0	2.0	7.9	13.3	16.8

Source: Quarterly Labor Force Statistics (QFLS Statistics)

Table 3: Percentage Distribution of Unemployed Population by Education & Sex

Year	Total	No Schooling	Grade 0-4	Grade 5-10	O/L	A/L
Both Sexes						
1999	100.0	0.2	4.2	41.9	29.2	24.5
2000	100.0	0.6	2.6	45.6	26.0	25.3
2001	100.0	0.2	3.3	40.7	27.3	28.4
2002	100.0	0.4	4.3	40.8	25.1	29.4
Male						
1999	100.0	0.2	4.6	48.3	29.1	17.8
2000	100.0	0.8	3.1	54.7	26.0	15.4
2001	100.0	0.3	4.0	50.9	25.2	19.6
2002	100.0	0.3	5.3	50.1	24.7	19.6
Female						
1999	100.0	0.1	3.9	35.5	29.3	31.2
2000	100.0	0.4	1.9	36.3	26.1	35.3
2001	100.0	0.1	2.6	29.7	29.7	38.0
2002	100.0	0.5	3.4	31.6	25.5	39.0

Source: Quarterly Labor Force Statistics (QFLS Statistics)

CHAPTER 3. EARNINGS AND EMPLOYMENT EFFECTS OF YOUTH TRAINING PROGRAMS IN SRI LANKA

A paper to be submitted to the Review of Development Economics

Murali Kuchibhotla

I. Abstract

This paper evaluates the impact of post-school training programs for Sri Lankan youth over the period from 1999-2005. We evaluate the effects of training on earnings and employment. We find that while training has weak effects on the probability of finding paid work, it does deliver large wage gains to training participants. Trainees earn about 38% more, on average, relative to non-trainees with otherwise equivalent backgrounds. These program effects are robust to the use of both parametric and non-parametric estimation methods.

II. Introduction

The lack of marketable skills is generally thought to be a key driver of social problems, such as unemployment and poverty in developing countries. Traditionally, developing countries have presumed that the problems of youth transition to work could be solved by expanding education programs through such measures as increasing the number of schools and teachers, making primary school enrollment compulsory, or reducing the cost of attending school. However, recent studies have shown that even the more educated youth in developing countries can encounter difficulties finding work after leaving school, sometimes experiencing longer periods of unemployment than their less-educated counterparts.[27] A plausible source of the slow transition from school to work is that graduates leave school lacking key skills demanded by employers. Post-school training, whether offered informally by firms through on-the-job training or more formally through trade schools, internships or apprenticeships are a potential solution to the problem of lack of skills. However, while there are good reasons to advocate the use of training programs for youth, there is little reliable evidence on the impact of training programs on the labor market prospects of young people in developing countries. Indeed, prior evaluations of government training programs in the United States and other industrialized countries have established that training programs tend to have weak effects on the future employment and earnings of trainees, thus raising the question as to whether such interventions can be expected to deliver positive results within a middle- and/or low-income country context.

However, as Attanasio et al. (2010) argue, there is reason to expect that the returns to training could be higher in middle- and low-income countries, as the level of skills of the population are low to begin with. Standard human capital theory would predict that the incremental return to an additional year of training would be quite large in such countries. Indeed, training programs introduced recently in several Latin American countries such as Argentina, Brazil, Chile, Colombia, the Dominican Republic, Panama, Peru, and Uruguay have all shown positive returns in terms of employment and earnings.[28]

This brings us to the issue that motivates this paper, namely, the evaluation of training programs targeted towards the youth in Sri Lanka. Sri Lanka is a poor developing country

[27] Youth in many developed countries are also experiencing increasing difficulties making the transition from school to work. For recent reviews of studies focusing on youth transition to employment in OECD economies, see Ryan (2001). Also see Betcherman et. al (2007) for studies pertaining to both developed and developing countries.
[28] It needs to be mentioned here that the vast majority of these programs have largely been evaluated using nonexperimental techniques, casting some doubt on the validity of the estimates, which could be biased if there is selection into the program on the basis of unobservables.

located in the South Asian region that has long faced a serious youth employment problem. Young men between the ages of 19-29 experienced an unemployment rate of 17% in the year 2002, with the corresponding rate for females being 27%. In contrast, unemployment rates for the overall working population aged 19-60 are 6% for men and 11% for women.[29] The World Bank has argued that a major contributing factor to youth unemployment in Sri Lanka has been the failure of young people to acquire skills through the formal schooling system. [30]

To remedy this problem, the Government of Sri Lanka (GOSL) has established formal technical education and vocational training (TVET) institutes across the country. As a consequence of the increasing penetration of such training institutes, exposure to training programs among the working population in Sri Lanka has steadily risen over time. According to successive Sri Lankan Labor Force Surveys (LFS), the proportion of working age population receiving any form of training has risen from 11% in 1992 to 13% in 2002[31]. Moreover, a higher proportion of youth (aged 15-29 years) received training in each year over the period 1992-02 relative to the older working population (30-65 years) and this gap has shown a tendency to widen over time[32]. However, in spite of the growing prevalence of TVET institutes in Sri Lanka, little systematic work has been undertaken examining the effects of such programs on the employment and wage outcomes for youth. We aim to fill this gap through this study.

In the Sri Lankan context, one factor holding back the lack of evidence on training program effects is limited data availability Another important factor holding back reliable estimates of the effects of these programs is selection bias. There are two different types of

[29] These figures are drawn from the Sri Lankan Labor Force Survey (LFS) of 2002.
[30] See World Bank (2005).
[31] See World Bank (2005).
[32] World Bank (2005).

selection bias that cause problems for the evaluating the impact of training programs. First, there is likely to be *selection into training*, so that the people who participate in training are likely to be different from those who do not participate. In the absence of statistical corrections, simply comparing employment outcomes across trainees and non-trainees is likely to yield biased causal estimates of the effect of training on employment.

Second, even if training is randomly assigned, employed trainees and employed non-trainees may not be comparable because of *selection into employment*, which occurs after the decision to train has been made. Given this, simply comparing mean wages across training participants and non-participants does not provide an accurate estimate of the effects of training on wages.

Standard parametric or semi-parametric methods for correcting for selection bias require the invoking of exclusion restrictions that have little justification in this case. Instead, this paper applies an alternative method, a general procedure for bounding the treatment effects associated with training. The method amounts to first identifying the excess number of individuals who are induced to be selected (employed) because of the treatment and then "trimming" the upper and lower tails of the outcome (*e.g.*, wage) distribution by this number, yielding a worst-case scenario bound.

Specifically, this paper focuses on how participation in training programs affects the labor market outcomes of Sri Lankan youth. We focus on two different outcomes of interest, namely, (1) the probability of finding paid employment and (2) productivity (as measured by the hourly wage rate). Our main findings are as follows. Training has economically weak and statistically insignificant effects on the likelihood of obtaining paid work. However, training does have a strong positive impact on wages, with trainees experiencing a 38% gain in productivity, on average, relative to non-trainees.

It is clear from our results that while the impact of training programs on paid employment is small and insignificant, these programs do tend to have unambiguously large and positive effects on productivity. This is an important finding that stands in strong contrast to the results that have been obtained in many developed countries (Heckman, LaLonde, and Smith 1999). In these countries, training effects are often small and seldom positive. However, our results are consistent with training program evaluations for disadvantaged youth introduced in recent years in a number of Latin American countries including Argentina, Brazil, Chile, Colombia, the Dominican Republic, Panama, Peru, and Uruguay.[33]

The rest of this paper is organized as follows. Section 3 provides a review of the relevant literature on the returns to training programs from both developed and developing countries. Sections 4, 5 and 6 describe the analytical approach used in this paper to estimate the relevant treatment effects of interest. Section 7 provides a description of the data, while section 8 provides evidence of the presence of substantial selection into both training and employment. Sections 9 and 10 lay out the specification of the propensity score function and conduct balancing tests to check for sufficient overlap between propensity scores across the two treatment arms. Sections 11 and 12 describe the main results of the paper and section 13 concludes.

[33] See Betcherman et. al (2007).

III. Literature Review

Training programs are among the most popular tools for enhancing human capital the world over, among both employed and unemployed workers. This section reviews the literature on active labor market programs (ALMPs), drawing attention to the labor market impact(s) of training programs. In addition, the section explains how this paper contributes to the identification of what appears to work for youth in developing countries- in terms of improving employment and wage outcomes- through skills based training interventions.

Regional studies on the impact of ALMPs are mainly restricted to developed economies, given the extent of evidence that is available. Martin and Grubb (2001) summarize previous evaluations and provide a descriptive analysis of programs implemented in OECD countries. They highlight the importance of on-the-job training components in public training programs, as well as small scale, links with local employers, and skills certification schemes.

Regional analyses have also been undertaken by Heckman et al. (1999), Kluve and Schmidt (2002), and Kluve (2006). Based on a sample of ALMPs implemented in Europe and the U.S. before 1994, Heckman et al. (1999) assess the impact of job training, job search assistance, and wage subsidies programs on employability. They find small positive effects of employment and training programs on adult earnings that tend to fade away quickly with time. The only lasting impact of these programs is through their influence on raising the probability of finding employment post-training.

While the Heckman et al. (1999) survey focuses on the effects of training on different groups of workers, of particular interest to us are the impact evaluation results from *youth* employment and training programs. This they establish to be either null or negative, especially in the U.S. In their review the authors also point out that there is no optimal method of choice for conducting program evaluations; both experimental and non-experimental methods can be equally appropriate for measuring labor market impacts as long as the quality of the underlying data is quite good.

Kluve's (2006) paper is a meta-analytical survey of the positive effects on employment of ALMPs in Europe, with special attention given to programs implemented in the late 1990s and in the early 2000s. Kluve evaluates training programs, private sector incentive programs such as wage subsidies, direct employment programs in the public sector and services and sanctions (such as job search assistance). A key finding from his review is that the *type* of program is the only clear factor affecting employment. Training programs appear to increase the probability of success relative to direct employment programs in the public sector, but are less likely to do so when compared to services and sanctions programs.

While the literature on ALMPs in developed counties is substantial, a much smaller literature exists on ALMPs in low- and middle-income countries, reflecting the relatively small number of impact evaluations that have been conducted in these countries. Moreover, evaluation studies that have been carried out in developing countries have been limited to the Latin American region, where countries have been investing significantly in recent years in youth employment training and public works programs. The vast majority of the impact evaluations in this region use non-experimental data.[34]

Betcherman et al. (2004) and Dar and Tzannatos (1999) have previously reviewed the worldwide evaluation evidence on ALMPs, with a focus on developing and transition countries.

[34] Two key exceptions here are the recent studies by Atanassio et al. (2010) and Card et al. (2010), which base their analysis on experimental data.

Their results indicate positive effects of combining classroom instruction with on-the-job training for the unemployed and negative effects of youth training programs on labor market outcomes.

Betcherman et al. (2007) did a follow-up study that built on the previous studies and put together a large sample of ALMPs implemented around the world with a focus on young people. They built the Youth Employment Inventory (YEI), which is an inventory of the different youth ALMPs around the world that work. Of the 289 ALMPs that make up the YEI, skills training programs are the most popular interventions to support young workers, amounting to 38 percent (111 out of 289) of the inventory. A descriptive analysis of the YEI shows positive impacts from programs that offer multiple services, i.e., combinations of vocational training, job training, job search assistance, entrepreneurial services, and a range of other social and employment-related support services.

Finally, by far the most comprehensive survey of training programs in recent years is the study by Fares and Puerto (2009). Their study is a meta-analysis of 345 training related interventions from both developed and developing countries. However, the quality of the evaluation evidence they provide is non-systematic, varying from rigorous impact evaluations to the provision of mainly descriptive information on training program characteristics and gross outcomes. Moreover, the indicators of labor market outcomes that they use vary across the different interventions. For example, some of these interventions use earnings gains as a performance indicator (such as the U.S. Job Corps) while others use the transition rate from training to employment to assess success (such as for a publicly-sponsored further-training program in Germany).

Of the total 345 interventions in the available database, they find that an overwhelming 64% of interventions had a positive impact on labor market outcomes. However, the quality of evaluation evidence varies by region- incidence of net impact evaluations is greater in OECD countries (with 86 out of 155 studies using rigorous treatment and control groups), followed by East and Central Asia (16 out of 48), and Latin American Countries (23 out 75).

One of the key findings of Fares and Puerto (2009) - which is similar to that established by Betcherman et al. (2004) and Dar and Tzannatos (1999) - is that programs that combine training along with other services report higher probabilities of positive impacts on the labor market prospects of trainees as compared to in-classroom training alone. The marginal effect coefficients imply that programs that combine in-classroom with workplace training increase the likelihood of positive impacts by 21-37 percentage points, while programs that offer this combination plus additional services increase the probability in 44-55 percentage points, with respect to in-classroom training.

Another important finding is that programs that target primarily young people tend to display *lower* chances of creating positive impacts compared to programs that target adults. In fact, the marginal effect indicates that training for youth reduces the probability of positive impacts by nearly 30 percentage points, as compared to adults. This result is consistent with findings from Kluve (2006), Betcherman et al. (2004), and Heckman et al. (1999) when examining ALMPs focusing on youth.

While these results are instructive, it is not at all clear that the results from Latin America and East and Central Asia generalize to other developing country settings, given the great heterogeneity in institutional and cultural characteristics across countries.

Outside of Latin American region, there are two noteworthy studies on developing country training programs. Ariga and Brunello (2006) examine the relationship between education and employer-provided training in Thailand, using survey data from a sample of employees drawn from twenty large firms. Using data on both on-the-job training (OJT) as well as off-the-job training (OFFJT) provided by the firms, they estimate bivariate probit and tobit models for the incidence and intensity of these two types of training. Individuals with higher education devote less time to OJT but more time to OFFJT. They also find fairly large effects of both OJT and OFFJT on wages, with returns to an additional year of OFFJT being similar to the returns from an additional year of formal schooling. Moreover, the return to an additional year of OJT is nearly twice the return to an additional year of OFFJT.

Rosholm et al. (2007) examine the effects of on-the-job training in Kenyan and Zambian manufacturing firms on the wages of employees. Using matching estimators, they find that training leads to a statistically significant 2.3% increase in wages for trainees in Kenyan firms. In Zambia, the effects of training on wages are insignificant. However, in both countries, training raises wages by 25% when the analysis focuses only on larger firms.

While providing valuable evidence, firm-based samples are subject to a serious shortcoming in that they are not representative of the entire population. To the extent that some firms offer training and others firms do not, the population in these firms will reflect atypically workers interested in engaging in training. More seriously, when examining the role of training on developing country youth, they will only reflect the youth who successfully found employment. That would be a highly selected group in many countries. In addition, such studies will focus on training investments paid for by firms, but they will miss training that is paid for by individuals. Returns based on the population of youth rather than the population of firms will provide less selected information.

Sri Lanka affords us a unique opportunity to examine whether the findings from Latin American and East and Central Asian countries generalize to the South Asian region. As such, this study makes the following four contributions. First, the rich nature of the data available to us allows us to follow survey respondents for a period of up to three years after the completion of formal schooling, enabling us to trace out the medium term impact of training on employment and wage outcomes. Second, since we have data on training that is privately undertaken, we are able to avoid the sort of selection problems that arise under firm based training. Third, by focusing on wages, we are able to pin down the productivity enhancing effects of training programs. And fourth, the use of the non-parametric bounding procedure we adopt in this paper allows for a more robust characterization of training program effects on productivity.

IV. Matching Methods and Theoretical Framework

Our objective in this paper is to estimate the effect of training program participation on both the probability of obtaining paid employment as well as on the wage growth. To this end, this paper reports training effects based on non-parametric matching and bounding methods. These methods are described in detail in the next two sections.

Matching methods seek to determine the effect of a "treatment" on participant outcomes, when there is non-random selection into the treatment state. The matching approach aims to mimic the conditions of a social experiment in which the treatment (which is the receipt of training in our application) is randomly assigned, by matching the sample of participants (treated group members) and non-participants (comparison group members) with respect to the vector of characteristics X that influence both treatment and outcomes. While regression techniques have traditionally been used to adjust for background differences and estimate causal effects, these methods are heavily dependent on modeling assumptions and give misleading results in the presence of substantial covariate differences across treatment and comparison groups.

Matching methods are designed to complement traditional regression methods, instead of acting as substitutes for them. Once matching has created similarly balanced treatment and control groups, causal effects can be estimated using regression adjustments. This is often a stronger approach for estimating causal effects than simple regression on an unmatched sample. Moreover, after the covariate adjustment has been carried out through matching, the estimates of treatment effects should be much less sensitive to the manner in which outcomes are modeled. Moreover, matching methods employ diagnostics that are easy to understand and implement.

Matching can be performed through the use of any number of potential "algorithms". In this paper, we estimate treatment effects using nearest neighbor matching. Under nearest neighbor matching, each treated subject is matched to a single control subject, with the matching done by using the so-called nearest available algorithm. This algorithm moves down the list of treated observations, at each step matching the treated subject to the nearest control observation, which is then removed from the set of controls that are available to match at the next step.

Successful application of the matching method requires access to a sufficiently rich dataset such that the identifying assumption of the matching estimator is satisfied. Our dataset contains detailed information on individuals' socio-economic background and labor market histories, allowing us to justifiably assert that this identifying assumption holds in our application. However, it should be borne in mind that the matching method only corrects for selection on observable factors and does not address selection based on unobservables.

Potential Outcomes Model[35]

Consider a general sample selection model with potential outcomes:

$$(Y(1), Y(0), S(1), S(0), T) \text{ i.i.d across individuals}$$

$$S = S(1)T + S(0)(1 - T)$$

$$Y = S\{Y(1)T + Y(0)(1 - T)\}$$

$$(Y, S, T) \mathit{are\ observed}$$

where T, $S, S(1)$ *and* $S(0)$ are binary indicators.

Let T denote treatment status, which in our application measures enrollment in a training program within the first year after the completion of formal schooling. When T=1, the individual enrolls in a training program in the first year after leaving school; when T=0, the individual does not enroll in any training program over this period.

An individual's employment status is picked up by the S, S(1) and S(0) variables. S(1) and S(0) are potential sample selection indicators under the treatment and control states. For example, when S(1)= 1 and S(0)= 0, wages for the individual are observed under training, but are missing in the absence of training. However, while S(1) and S(0) are unobservable, what we actually see in the data is the variable S, which is equal to 1 if an individual is employed and 0 if not employed.

[35] We do not attempt here to provide a detailed review of either the potential outcomes approach or of the associated matching techniques used in the literature. For an overview of these concepts, see the survey by Imbens and Wooldridge (2009).

Finally, Y(1) and Y(0) represent the two latent potential outcomes (i.e., wages) for each individual, based on whether or not he/she trains. These potential wages are measured over a period of two years *after* the completion of formal schooling. Now, because these are latent outcomes, we don't actually observe them in the data. What we actually observe for each individual is the variable Y, which is the realized wage for each individual. Y is constructed by taking information on an individual's latent wages and combining it with information on his/her *realized* employment and training status. If an individual is unemployed, Y=0.

V. Estimating the Effect of Training on Future Employability

We first show how to identify the effects of training on the probability of obtaining future wage employment, followed by a discussion of the additional assumptions necessary to identify the impact of training on wages.

The average treatment effect (ATE) of training on employment is defined as the average effect on employment of moving all individuals in the population from a state of training to a state of non-training. Formally, we have:

$$ATE = E[S(1) - S(0)] = E[S(1)] - E[S(0)]$$

Note that the ATE cannot be identified from the data at hand, since we do not observe the employment outcomes for all survey respondents under the treated state only (or non-treatment only). Instead, what we observe are employment outcomes under a *mix* of both the treatment and non-treatment states.

The ATE can be identified under a set of two assumptions, called the unconfoundedness and overlap (or common support) assumptions. Unconfoundedness requires that, conditional on a set of observed covariates, there are no unobserved variables that are correlated with both treatment participation and potential outcomes. Formally, we have:

$$\text{Unconfoundedness: } (Y(1), Y(0), S(1), S(0)) \perp T|X$$

where X denotes a vector of covariates. The overlap (or common support) assumption requires that both treatment and control units must exist for all values of the covariate distribution.

$$\text{Overlap: } 0 < Pr(T = 1|X = x) < 1, \forall x$$

These two assumptions are then sufficient to identify the ATE. Note that:

$$ATE(x) = E[S(1)|X = x] - E[S(0)|X = x] = E[S(1)|T = 1, X = x] - E[S(0)|T = 0, X = x]$$
$$= E[S|T = 1, X = x] - E[S|T = 0, X = x] \quad (1)$$

where the second-to-last equality follows from the unconfoundedness assumption, which states that the treatment is uncorrelated with potential outcomes, once we condition on the set of covariates . We can identify $ATE(x)$ for all values of x, with the unconditional ATE obtained by taking expectations over the population distribution of the covariates.

When the vector X contains a number of covariates, exact matching on covariates is not possible. In such cases, Rosenbaum and Rubin (1983) suggest the use of the *propensity score*- defined as the conditional probability of selection into treatment[36]- as a basis for carrying out the match between the treatment and control units. If the distribution of the covariates X is the same for those treatment and control units sharing the same value of the propensity score, then matching on the basis of the propensity score can act as a substitute for matching on the basis of the set of covariates X. Thus, we have:

[36] Formally, this can be written as follows: $p(X = x) = Pr(T = 1|X = x)$

$$(Y(1), Y(0), S(1), S(0)) \perp T | p(X)$$

with $p(X)$ denoting the propensity score. The $ATE(x)$ can then be computed as in (1) above, but by conditioning on the propensity score instead of the values of the covariates. The unconditional ATE can then be obtained by taking expectations over the population distribution of the propensity scores. In practice, the propensity score needs to be estimated from the data. We use a logit model to estimate these propensity scores below.

VI. Bounding the Effects of Training Participation on Wages

While the assumptions underlying matching are sufficient to determine the effects of training on the probability of finding paid work, they are insufficient to identify the impact of training on wages. When trying to characterize the training effect on wages, we not only have to account for selection into training, but we also need to allow for the non-random nature of selection into employment. Estimating this causal effect is complicated by the fact that individuals' decisions to obtain employment occur *after* the entry into the training program and are thus endogenous in nature. To overcome this problem, we adopt an approach that combines the matching strategy described above with non-parametric bounding in such a way as to provide robust estimates of the training effect on wages.

Horowitz and Manski (2000) Bounds

We shall estimate the effect of training program participation on wages using two related sets of bounding techniques. The first set of bounds, called the Horowitz and Manski (H-M) bounds, allow us to tease out the wage effect without making *any* distributional assumptions on the process through which individuals self-select into wage employment. The H-M procedure can be used to construct bounds on treatment effects when the outcome variable in question has a *bounded* support.

To illustrate the H-M bounds, start by defining the ATE of training on wages as follows:

$$ATE_{HM} = E[Y(1) - Y(0)] = E[Y(1)] - E[Y(0)] \qquad (2)$$

Conditional on T and the observed employment indicator S, ATE_{HM} in (2) can be written as:

$$ATE_{HM} = E[Y|T=1, S=1] * \Pr(S=1|T=1) + E[Y(1)|T=1, S=0] * \Pr(S=0|T=1)$$
$$- E[Y|T=0, S=1] * \Pr(S=1|T=0) - E[Y(0)|T=0, S=0]$$
$$* \Pr(S=0|T=0) \qquad (3)$$

An examination of (3) reveals that- under the randomization like conditions that matching produces- we can identify from the data all the conditional probabilities (Pr (S=s, T=t), for (t, s)=(0, 1)) and also the expectations of the wage when conditioning on S=1 (i.e., E[Y|T=1, S=1] and E[Y|T=0, S=1]). Unfortunately, sample selection into non-employment makes it impossible to identify E[Y(1)|T=1, S=0] and E[Y(0)|T=0, S=0]. However, it is possible to construct H-M bounds for these unobservables, provided that the support for the wage distribution lies in a *bounded* interval $[Y^{LB}, Y^{UB}]$.

Formally, under the Manski-Horowitz approach, the upper bound for the effect of the training program on wages is computed as follows:

$$E[Y|S=1, T=1] * \Pr[S=1|T=1] + Y^{UB} * \Pr[S=0|T=1]$$
$$- E[Y|T=0, S=1] * \Pr[S=1|T=0] - Y^{LB} * \Pr[S=0|T=0]$$

And the lower bound is constructed as follows:

$$E[Y|S=1, T=1] * \Pr[S=1|T=1] + Y^{LB} * \Pr[S=0|T=1]$$
$$- E[Y|T=0, S=1] * \Pr[S=1|T=0] - Y^{UB} * \Pr[S=0|T=0]$$

Note that these bounds do not employ distributional assumptions and are non-parametric. One cost of dispensing with such distributional assumptions is that the H-M bounds are often uninformative in nature. Hence, we take this approach as a basic building block and then impose additional assumptions to back out bounds that provide more information on the treatment effect in question. These bounds are called Lee bounds, which derive their name from the seminal paper by David Lee (2009).

Lee (2009) Bounds

Lee's (2009) approach is to impose monotonicity assumptions that lead to a tightening of the H-M bounds. His approach builds off of the principal stratification (PS) approach of Frangakis and Rubin (2002), which provides a framework for estimating causal effects when controlling for a post-treatment variable that has been affected by the treatment in question. In our application, the post-treatment variable is an individual's employment status, which can be influenced by prior training. Under the PS approach, individuals are classified into *principal strata*, depending on the potential values of employment based upon their training status. Causal effects can then be estimated by comparing wages by training status within strata, since the strata than an individual belongs to is *not* affected by his/her training status.

In formal terms, we can partition the population into strata based on the values of the vector S(T)={S(1), S(0)}; such a vector is defined for each individual in the population. Given that both S and T are binary, the population of individuals can be partitioned into four principal strata:

NN: {S(0)=0, S(1)=0}

EE: {S(0)=1, S(1)=1}

EN: {S(0)=1, S(1)=0}

NE: {S(0)=0, S(1)=1}

In our context, NN is the stratum of individuals who would be unemployed regardless of whether or not they participate in training; EE is the stratum of individuals who would be employed regardless of whether or not they participate in training; ; EN is the stratum of individuals who would be employed in the absence of training, but unemployed if they were to participate in training and NE is the stratum of individuals who would be unemployed in the absence of training, but employed under training. The strata that an individual belongs to is unobserved information, since it requires us to know what an individual's employment status would be under different training assignments. However, it is possible to map the observed groups of (S, T) to the unobserved principal strata; such a mapping is provided in table 1 below.

Lee (2009) focuses on deriving the ATE of a treatment on that segment of the population that would be employed regardless of the training assignment; that is, he constructs these bounds for the EE strata. Since this strata is the only one for which wages are observed under both treatment arms, fewer assumptions are required to construct bounds for this sub-population of individuals. In this paper, we shall also focus on obtaining ATE estimates for the EE strata. Formally, the ATE parameter we are interested in estimating is:

$$ATE_{EE} = E[Y(1)|EE] - E[Y(0)|EE]$$

Lee's (2009) approach tightens the H-M bounds by imposing, in addition to the unconfoundedness and overlap assumptions, the following crucial assumption:

$$Positive\ Weak\ Monotonicity\ of\ T\ on\ S(T){:}\ S(1) \geq S(0) \quad (4)$$

This weak monotonicity assumption holds for all individuals. Under the monotonicity assumption, training program participation is allowed to affect future employment prospects in only one direction: the probability of employment for an individual engaged in training must be at least as large as the employment probability for that same individual if he/she were not to train. The monotonicity assumption is inherently *untestable* in nature and captures how the treatment affects the sample selection process. However, this assumption is plausible, as an important goal of youth training programs in Sri Lanka is to augment the human capital of participants, thereby increasing their earnings potential (i.e., wages).

An immediate implication of this assumption is to rule out the EN stratum in the population, which allows for the identification of individuals in the EE and NE strata. Also, monotonicity allows us to identify the proportion of each principal strata in the population. Let π_k denote the population proportions of each principal strata $k = NN, EE, EN, NE$ and let $p_{S|T} \equiv Pr(S = s|T = t)$ for t,s=0,1. Then, $\pi_{EE} = p_{1|0}, \pi_{NN} = p_{0|1}, \pi_{NE} = p_{1|1} - p_{1|0} = p_{0|0} - p_{0|1}, and\ \pi_{EN} = 0$. From table 1, we see that individuals in the observed group (T, S) = (0, 1) belong to the stratum EE. This allows us to point identify E[Y(0)|EE] with E[Y|T=0, S=1]. However, it is not possible to point identify E[Y(1)|EE] with E[Y|T=1, S=1], since the observed group (T, S) = (1,1) is a mix of individuals from the EE and NE strata. However, E[Y|T=1, S=1] can be bounded in such a way that E[Y(1)|EE] falls within a known range of values. To show this, note the connection between E[Y|T=1, S=1] and E[Y(1)|EE].

$$E[Y|T = 1, S = 1] = \frac{\pi_{EE}}{\pi_{EE}+\pi_{NE}} E[Y(1)|EE] + \frac{\pi_{NE}}{\pi_{EE}+\pi_{NE}} E[Y(1)|NE] \quad (5)$$

The proportion of EE individuals in the group (T,S)=(1,1) can be identified by $\frac{\pi_{EE}}{\pi_{EE}+\pi_{NE}} = \frac{p_{1|0}}{p_{1|1}}$. From equation 5, we see that E[Y(1)|EE] can be bounded from *above* by the expected value of Y for the $\frac{p_{1|0}}{p_{1|1}}$ fraction of individuals with the *largest* values of Y in the observed group (T,S)=(1,1). That is, this upper bound is constructed under the assumption that the largest $\frac{p_{1|0}}{p_{1|1}}$ of Y within the (T,S)=(1,1) group belongs to members of the EE strata. This upper bound can be computed by *trimming* the lower tail of the distribution of Y in the (T, S)=(1,1)group by a proportion $1-(\frac{p_{1|0}}{p_{1|1}})$. Similarly, E[Y(1)|EE] can be bounded from *below* by the expected value of Y for the $\frac{p_{1|0}}{p_{1|1}}$ fraction of individuals with the *smallest* values of Y in the observed group (T,S)=(1,1). This lower bound is constructed under the assumption that the smallest $\frac{p_{1|0}}{p_{1|1}}$ of Y within the (T,S)=(1,1) group belongs to members of the EE strata.

Under the unconfoundedness, overlap and monotonicity assumptions, *sharp* bounds for the average treatment effect of interest (ATE_{EE}) are computed as follows:

$$\Delta_0^{LB} \equiv E\left[Y \middle| T=0, S=1, Y \leq y^{11}_{(\frac{p_{1|0}}{p_{1|1}})}\right] - E[Y|T=0, S=1] \quad (6)$$

$$\Delta_0^{UB} \equiv E\left[Y \middle| T=0, S=1, Y \geq y^{11}_{1-(\frac{p_{1|0}}{p_{1|1}})}\right] - E[Y|T=0, S=1] \quad (7)$$

where $y^{11}_{1-(\frac{p_{1|0}}{p_{1|1}})}$ and $y^{11}_{(\frac{p_{1|0}}{p_{1|1}})}$ denote the $1-(\frac{p_{1|0}}{p_{1|1}})$and $(\frac{p_{1|0}}{p_{1|1}})$ quantiles of Y, conditional on T=1 and S=1. To estimate the bounds in 6 and 7, the sample quantities can be substituted for the population quantities.

VII. Data Construction

The data used in this study is obtained from a 2006 survey on school-to-work transition that was administered in Sri Lanka by the University of Colombo, with support from the World

Bank. Data was collected on respondents who left school and were between the ages of 15-26 years at the time of the survey. The survey was administered between April and May of 2006 to 1026 individuals from 450 different households who completed formal schooling between 1999 and 2006. Care was taken to ensure that the sample was representative of the nation, with the exception of the conflict ridden provinces. Information was also obtained on the socio-economic background of respondents as well as their personal characteristics.

The distinctive feature of the dataset is that it contains detailed retrospective questions on individual labor market behavior. Specifically, individual labor market histories can be constructed on the basis of information on monthly labor force status. These states include wage employment, self-employment, inactivity, unemployment and training and post-schooling education. Individual histories vary from a minimum of 5 months to a maximum of 78 months. A pictorial description of the data available for analysis is given in figure 1.

Our evaluation of Sri Lankan training programs is based on considering (a) pre-treatment labor force status information over a period of 12 months and (b) post-treatment employment and wage outcomes over a period of 24 months.

To be able to work with such rich data, we had to condense information on labor force status into the four labor market states, “wage employment”, “self-employment”, “training” and “non-employment”. We combine unemployment and inactivity into a single variable as it is difficult to conceptually distinguish between these two states. Similarly, since non-university post-schooling education captures job-oriented and vocational courses to a large extent, we combine this with traditional training.

Outcomes and Treatment Defined

The treatment variable of interest is enrollment in a training program. We define training participants to be those individuals who enroll in a training program provided by an agency of the GOSL, the private sector or by an NGO, within a period of 12 months after entering the labor market. The two post-treatment outcome variables that are the focus of this evaluation are employment status and earnings capacity. Employment status is a 0-1 measure, taking on a value of one if an individual is able to obtain wage work between the second and third years after entering the labor market. The second outcome variable is the natural logarithm of hourly wages, which is only defined for those individuals who obtain paid employment, that is, only for those whose employment status is coded as one in the data.

Covariates

All individual and household level variables used in the analysis below are laid out in Table 2.We first start with a description of individual level covariates. First, the measure of ability used in our analysis is constructed from the results of an ability test which was administered to survey respondents. The test had a reasoning ability module as well as an English language skills module. The ability score so constructed is scaled to lie between 0 and 1. Second, the educational achievement variables that we use are dummy variables for O-level and A-level certifications. The dummy variables are cumulative, and so anyone who completed the A-level also completed the O-level. Other individual characteristics include age, which is computed as the number of years since birth and a dummy variable capturing whether or not the individual is a male.

Household characteristics include parental education and household wealth. Parental education is captured by including dummy variables for parental O-level and A-level qualifications. These dummy variables take on a value of one if either parent achieves an O-level or A-level qualification, respectively.

Our survey does not collect information on household income or expenditures. Instead, there are a series of questions on asset ownership (bicycle, car, etc.) and housing characteristics (number of rooms, quality of materials used to construct the house, etc.). We construct a household wealth index from such detailed information on asset ownership and housing characteristics through the use of the method of principal component analysis (PCA). Essentially, we aggregate these asset ownership indicators into a single index, using weights that are chosen through the PCA method.

VIII. What Determines Training and Employability?

We first show that substantial selection into both training programs and employment occurs among youth in Sri Lanka.

Selection into training programs: As table 3 illustrates, training program participants are more educated, able and tend to have more educated parents relative to non-participants. Participants are also more likely to experience unemployment in their first year after the completion of formal schooling and tend to hail from disproportionately poorer household backgrounds.

Selection into wage employment: The employed tend to be more educated and are likely to have more educated parents relative to the non-employed. They are also more likely to be male, and are more likely to spend some time in unemployment in their first year after schooling.

There is thus evidence to indicate that selection into both training and employment is in practice non-random. We thus need to correct for both of these sources of selection bias in order to be able to recover the effect of training on our two labor market outcomes of interest.

To correct for non-random treatment assignment, we use the matching approach. Using a rich set of variables that influence training participation as well as employment and earnings, we construct a matched comparison group member for each treatment individual in our sample using nearest neighbor matching. This reduces our sample from 553 to 266 individuals, since the non-matched comparison units are not used any further in the analysis.

IX. Specifying the Propensity Score Function

Several important patterns related to training are documented in the 1992, 1997 and 2002 snapshots of the Labor Force Surveys (LFS)[37]. First, these surveys document complementarities between formal education and training, with the incidence of training undertaken by those with university degrees about 10 to 20 times higher than for those individuals with no formal education. This pattern holds across both sexes. Second, females are less likely to obtain training relative to males at all levels of education. Third, the penetration of training varies by region, with individuals living in districts in the Western Province having the highest incidence of training and those living in the North-Central Province registering the lowest incidence of training.

These trends in training program participation are also borne out by a study conducted by the World Bank[38]. This study was based on pooled cross-section time series data, and found a strong positive correlation between training and individual and parental educational achievement. District location was also found to play an important role in influencing this probability. A rising time trend associated with the training of males was also documented, while the training profile for females was found to be flat over time.

[37] As reported in "Treasures of the Education System in Sri Lanka: Restoring Performance, Expanding Opportunities and Enhancing Prospects".
[38] Reported in World Bank (2005).

Another World Bank study characterized the effects of training on the labor market outcomes of youth using pooled LFS data from 1996-2002[39]. Among the various findings of this study, more educated individuals are able to transition into employment faster after the completion of schooling (compared to youth with only a primary school education). Gender differences matter, with men finding employment faster than females. Location and time of entry into the labor force are also important- individuals in the Western Province find jobs faster than those in other provinces and the average job search time declines in those years when the overall unemployment rate dips. This study also documents the importance of these covariates for determining the effects of training on wages.

The discussion above thus paints a picture of the main forces driving selection into training, as well as the factors that tend to be important for the labor market outcomes of Sri Lankan youth. We have argued that past research has indicated that age, educational qualifications, gender, parental background, geographical location and local labor market conditions are all important drivers of both training participation and labor market outcomes such as employment and earnings. These are precisely the variables that need to enter our propensity score specification, since the explicit aim of the propensity score is to control for the influence of all such factors that jointly influence both treatment as well as potential outcomes.

A few additional comments are in order here. First, we account for district fixed effects in our study by including dummy variables for district location in our propensity score specification. The inclusion of these variables has the additional benefit of accounting for differences in local labor market conditions across districts. Second, since cohort effects are likely to be important determinants of both training participation and labor market outcomes (to the extent that youth may face different training opportunities and macroeconomic conditions

[39] Also reported in World Bank (2005).

and policies based on when they enter the labor market) we include age at the time the survey was undertaken as a covariate, which tracks the year of labor market entry of individuals quite closely.

Finally, three additional sets of variables are also used to model the propensity score. First, we include ability scores into our analysis. Past research has shown how the exclusion of measures of ability and motivation from the analysis of labor market outcomes constitutes an important source of omitted variable bias. Second, we include a measure of household wealth into our propensity score specification. Since private sector training represents a non-negligible source of training opportunities for young people in Sri Lanka, wealth is likely to constitute an important constraint on individual decisions to participate in training. Lastly, we also include information on unemployment patterns in computing our propensity score, given the documented importance[40] of past labor market performance in determining both employment status as well as the selection process into training.

X. Assessing Balance after Estimating the Propensity Score

The first step of the analysis is to estimate the propensity score, which is just the conditional probability of participation in training. The variables chosen to enter the propensity score model are based on the discussion in the section above. A complete description of all variables entering the propensity score model is given in table 2. Once the propensity scores have been estimated, treatment-comparison matches are selected based on the proximity of their scores. This is done to reduce the bias in the estimated treatment effect. The performance of matching is assessed through examining the extent of the covariate balance in the matched treatment and comparison units. Table 4 provides an assessment of the extent of covariate

[40] See, for example, Lechner and Wunsch (2009).

balance between the treatment and control groups both before and after matching. If the matching procedure has worked well, the covariates should be better balanced in the matched sample compared to the original dataset. This is borne out in the table. The standardized mean differences between all covariates used in the propensity score specification are significantly smaller in the matched sample compared to the full sample. Figure 3 shows that the matched distribution of propensity scores is comparable across the two treatment arms. Moreover, an examination of figure 2 reveals a substantial overlap in the propensity score distributions in the two groups, and provides fairly strong evidence in favor of the "common support" assumption. Nearest neighbor matching on the propensity scores thus seems to perform quite well terms of selecting potential matching partners in our application.

XI. How Does Training Affect the Probability of Obtaining Wage Employment?

To determine how training affects the probability of obtaining wage work, we compute the difference in the proportion of employed individuals between the two treatment arms. Table 5 presents the estimated effect of training on the probability of obtaining paid work for those individuals with at least 3 years of labor market experience. The treatment-control difference without covariates represent the raw mean difference in employment outcomes between the treatment and comparison groups, while the treatment-control difference with covariates computes this difference by first adjusting for the influence of various covariates through OLS regression. The estimated treatment effect (with covariates) is -1.28, which implies that trainees are 1.28% *less* likely to be employed compared to non-trainees. However, this estimate is statistically insignificant at conventional significance levels, with the two-sided P-value for no employment effect being 0.81.

Thus, our results indicate that, on average, trainees do *not* outperform non-trainees in terms of their prospects of obtaining wage employment after the completion of training. In fact, trainees may well be worse off in the short run, on account of so-called “lock-in effects”. This refers to the reduced time generally available to trainees to participate in job search activities while they are still enrolled in the training program.

These results are surprising, as modest employment gains from youth training programs in both Latin America and the transition countries have been documented in the literature. For example, Fretwell et. al (1999) estimate a 6%-10% increase in employment probabilities for youth who finish training in Poland and Hungary.

XII. How Does Training Affect Wages?

<u>OLS Estimates for Employed Sub-Sample</u>

We begin this section with a brief discussion of the estimates of the wage returns to training obtained through ordinary least squares (OLS) regression, which is displayed in table 7. Table 7 displays the results from the OLS earnings equation from that subset of individuals in the matched sample that possess a wage. The estimated training coefficient is 0.373, and is significant at the 1% significance level for both samples. OLS estimates thus imply that trainees earn about 37% more, on average, relative to non-trainees. Moreover, educational qualifications also have a positive impact on wages, with positive coefficient estimates for the O-level and A-level variables. However, they are both individually and jointly insignificant (i.e., we fail to reject the F-test for the joint insignificance of O and A-level variables). We also find that older youth tend to earn more and men earn higher wages then women. Finally, the greater the length of time spent in a job, the higher the associated wage. The training estimates are consistent with the predictions from the standard human capital model, according to which human capital accumulation through training yields a significant wage payoff.

Horowitz-Manski (2000) Bounds

To correct for the sample selection bias that arises from non-random sorting into employment, we utilize non-parametric bounding techniques, as discussed in section 5 above. We start with the traditional approach for computing treatment effect bounds, which relies on the work by Horowitz and Manski (2000). The idea behind the H-M bounds is to impute the missing outcome (i.e., wage) data with either the largest or the smallest observed wages in order to compute the largest or smallest treatment effects that are consistent with the data. The H-M bounds are presented in table 8. The upper bound on treatment effects is 2.31 while the lower bound is -2.03. These estimates imply that program effects can range anywhere between + 235% to - 203%. The H-M bounds are thus as consistent with extremely large *positive* effects as they are with extremely large *negative* effects. This occurs because wages are not observed for more than 60% of the individuals in our sample. Imputing wages for such a large share of the sample results in an extremely wide interval for the treatment effect of interest.

Lee (2009) Bounds

The associated Lee (2009) bounds for ATE_{EE} are reported in table 9. Given that the proportion of employed individuals in both the T=1 and T=0 groups are the same, the trimming parameter $\frac{p_{1|0}}{p_{1|1}}$ simplifies to 1. This implies that both the upper and lower bound for ATE_{EE} are the same, namely 0.38. Thus, program participants earn a wage that is 38% higher than that of non-participants. These estimates indicate that the productivity gains brought about through training yields a substantial payoff in terms of the subsequent wage growth of trainees.

The estimated wage returns to youth training in Sri Lanka are relatively large compared to reported estimates of the wage returns from training programs found elsewhere around the world, such as the 12% average earnings gain from participation in the Job Corps program in the US. Positive wage returns have also been documented for the "Jovenes" programs that provide training to youth in Chile, Argentina, Peru and Uruguay.[41] However, it should be kept in mind that these are estimated effects on *realized earnings* and as such mix employment and wage effects. Our estimates, on the other hand, represent the effects of training on earnings potential and as such provide a clearer picture of the productivity gains brought about through training.

XIII. Conclusions

Training programs targeted towards youth have traditionally been an important component of the set of strategies used by the Government of Sri Lanka to combat widespread youth unemployment problems in recent decades. In this paper, we have evaluated the effects of training programs that started within the first year after the completion of formal schooling on labor market outcomes-employment status and earnings- over the following two years. Treatment effects are estimated using a combination of propensity score matching and non-parametric bounding methods. We have argued that matching is appropriate in this context, given our access to a rich set of covariates influencing both program participation and employment and wage outcomes. In addition, since matching is not robust to the presence of unobserved covariates that influence both training participation as well as outcomes, we test for the sensitivity of the estimates to the presence of hidden bias.

[41] See Betcherman, Olivas and Dar (2004).

Our major findings are as follows: we find that participation in training programs has economically weak and statistically insignificant effects on the probability of obtaining wage employment. However, these programs do have strong positive effects on earnings capacity. The effect of training on wages is large, with the implied earnings growth of 38%.

Thus, contrary to the view that youth unemployment is effectively and efficiently addressed through preventive policies rather than through training programs[42], the key lesson from the Sri Lankan evaluation is that youth training can lead to favorable improvements in the earnings prospects for program participants. Results from Sri Lanka tend to reinforce the favorable assessments of training programs in Latin America, and raise the possibility that the disappointing track record of youth training in industrialized countries may not apply in developing countries.

XIV. References

Aakvik, A. (2001) Bounding a matching estimator: the case of a Norwegian training program. *Oxford Bulletin of Economics and Statistics*, 63(1)**,** 115-143.

Ariga, K. & G. Brunello (2006) Are education and training always complements? Evidence from Thailand. *Industrial & Labor Relations Review*, 59**,** 613-629.

Attanasio, O., A. Kugler, and C. Meghir. 2010. Subsidizing Vocational Training for Disadvantaged Youth in Colombia: Evidence from a Randomized Trial. *American Economic Journal: Applied Economics*, 3(3): 188–220.

Becker, S and Caliendo, M. (2007) "mhbounds**:** Sensitivity Analysis for Average Treatment Effects," IZA Discussion Papers 2542, Institute for the Study of Labor (IZA).

[42] This comment is often made in the context of program evaluations in developed countries.

Betcherman, G., K. Olivas, and A. Dar. 2004. "Impact of Active Labor Market Programs: New Evidence from Evaluations with Particular Attention to Developing and transition Countries." Washington, D.C.: World Bank, Social Protection Discussion Paper Series 0402.

Betcherman, G., M. Godfrey, O. Susana Puerto, F. Rother, and A. Stavreska. 2007. "Global Inventory of Interventions to Support Young Workers: Synthesis Report." Washington, D.C.: World Bank, Social Protection Discussion Paper Series 0715.

Dar, A. and P. Zafiris Tzannatos. 1999. "Active Labor Market Programs: A Review of the Evidence from Evaluations," Social Protection Discussion Paper no. 9901, January. Washington, D.C.: World Bank.

Fares, J. and O. S. Puerto. 2009. "Towards Comprehensive Training," Social Protection Discussion Paper no. 0924, November. Washington, D.C.: World Bank.

Filmer, D. & L. H. Pritchett (2001) Estimating wealth effects without expenditure data - Or tears: An application to educational enrollments in states of India. *Demography,* 38**,** 115-132.

Frangakis, C. & D. Rubin (2002) Principal Stratification in Causal Inference. *Biometrics,* 58**,** 21-29.

Friedlander, D., D. H. Greenberg & P. K. Robins (1997) Evaluating government training programs for the economically disadvantaged. *Journal of Economic Literature,* 35**,** 1809-1855.

Heckman, J.J., R.J. LaLonde and J.A. Smith. 1999. "The Economics and Econometrics of Active Labour Market Programs", in O. Ashenfelter and D. Card (eds.), *Handbook of Labor Economics 3*. Elsevier, Amsterdam.

Daniel Ho; Kosuke Imai; Gary King; and Elizabeth Stuart (2007), Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference. *Political Analysis* 15(3): 199-236, http://gking.harvard.edu/files/abs/matchp-abs.shtml.

Daniel Ho; Kosuke Imai; Gary King; and Elizabeth Stuart (2007b) "Matchit: Nonparametric Preprocessing for Parametric Causal Inference," *Journal of Statistical Software*, http://gking.harvard.edu/matchit/.

Horowitz, J. L. & C. F. Manski (2000) Nonparametric Analysis of Randomized Experiments with Mising Covariate and Outcome Data. *Journal of the American Statistical Association,* 95**,** 77-84.

Hujer, R. & S. Thomsen (2010) How do the employment effects of job creation schemes differ with respect to the foregoing unemployment duration? *Labour Economics***,** 38-51.

Imbens, G. W. & J. M. Wooldridge (2009) Recent Developments in the Econometrics of Program Evaluation. *Journal of Economic Literature,* 47**,** 5-86.

Kluve, J. & C. M. Schmidt (2002) Can training and employment subsidies combat European unemployment? *Economic Policy***,** 409-448.

Kluve, J. 2006. "The Effectiveness of European Active Labor Market Policy". Bonn: IZA Discussion Paper, No.

Lechner, M. & C. Wunsch (2009) Are Training Programs More Effective When Unemployment Is High? *Journal of Labor Economics***,** 27(4),653-692.

Kletzer, L and Koch, W. 2004. "International Experience with Job Training." In *Job Training Policy in the United States*, Christopher O'Leary, Robert A. Straits and Stephen A.Wandner, eds. W. E. Upjohn Institute for Employment Research.

Lee, D. (2009) Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects. *Review of Economic Studies*, 76(3), 1071-1102.

Rosenbaum, P. R.(2002) Observational Studies. Springer, New York.

Rosenbaum, P. R. & D. B. Rubin (1983) THE CENTRAL ROLE OF THE PROPENSITY SCORE IN OBSERVATIONAL STUDIES FOR CAUSAL EFFECTS. *Biometrika,* 70**,** 41-55.

Rosholm, M., H. S. Nielsen & A. Dabalen (2007) Evaluation of training in African enterprises. *Journal of Development Economics,* 84**,** 310-329.

The World Bank. *Treasures of the Education System in Sri Lanka: Restoring Performance, Expanding Opportunities and Enhancing Prospects*. Washington, D.C. 2005

Table 1: Breakdown of Principal Strata and Observed Groups Based on (T,S)

Groups by observed (T, S)	**Principal Strata (PS)**	**PS under Monotonicity**
(0,0)	NN and NE	NN and NE
(0,0)	EE and NE	EE and NE
(0,0)	NN and EN	NN
(0,0)	EE and EN	EE

Table 2: Description of Variables Entering the Propensity Score Model

Variable	**Description**
Outcomes	
Training Dummy	=1 if enrolled in training program; =0 otherwise
Covariates	
Age	Number of years since birth
Wealth	Asset index computed from household asset ownership
Ability Score	Ability index created from scores on a English language and reasoning ability test
O/L Dummy	=1 if passed O-level exams; =0 otherwise
A/L Dummy	=1 if passed A-level exams, =0 otherwise
Parent O/L Dummy	=1 if either parent passed O-level exams; =0 otherwise
Parent A/L Dummy	=1 if either parent passed A-level exams; =0 otherwise
Male Dummy	=1 if Male; =0 otherwise
Non-employment Dummy	=1 if unemployed in first year after school; =0 otherwise
District *i* Dummy	=1 if residing in district *i*; *i*=1,..,13

Table 3: Summary Statistics of Variables by Treatment and Employment Status

	Non-Participants			**Program Participants**		
Description	**Employed**	**Not Employed**	**Overall**	**Employed**	**Not Employed**	**Overall**
Sample Size	134	286	420	47	86	133
O/L Dummy	.80	0.64	0.70	1	0.93	0.95
A/L Dummy	0.28	0.19	0.22	0.60	0.56	0.57
Parent O/L Dummy	0.32	0.24	0.26	0.42	0.51	0.48
Parent A/L Dummy	0.14	0.11	0.12	0.42	0.23	0.26
Ability Score	0.28	0.28	0.28	0.43	0.47	0.46
Age	22.64	22.52	22.56	23.55	23.62	23.60
Male Dummy	0.50	0.47	0.48	0.64	0.34	0.44
Wealth	0.001	0.50	0.34	-1.257	-0.976	-1.07
Non-employment Dummy	0.83	0.73	0.76	0.89	0.84	0.86

Table 4: Checking for Covariate Balance after Matching

Variables	**Standardized Mean Differences**	
	Before Matching	**After Matching**
Propensity Score	0.978	0.129
O/L Dummy	1.246	-0.036
A/L Dummy	0.700	0.045
Ability Score	0.647	0.124
Parent O/L Dummy	0.433	-0.075
Parent A/L Dummy	0.326	0.102
Wealth	-0.694	0.034
Male	-0.075	0.060
Age	0.611	0.176
Non-employment Dummy	0.264	0.086
District 1 Dummy	0.093	-0.071
District 2 Dummy	0.206	0.058
District 3 Dummy	-0.037	-0.032
District 4 Dummy	-0.153	0.049
District 5 Dummy	-0.067	0.085
District 6 Dummy	0.145	0.022
District 7 Dummy	0.098	0.030
District 8 Dummy	-0.127	-0.158
District 9 Dummy	-0.183	-0.108
District 10 Dummy	-0.010	-0.101
District 11 Dummy	-0.228	0.062
District 12 Dummy	-0.270	0.087
District 13 Dummy	0.041	0.085
Sample Size	553	266

Note: Standardized mean differences are computed as follows:

$$\Delta_X = \frac{X_1^- - X_0^-}{\sqrt{S_1^2}}$$

where $S_1^2 = \sum_{i:D_i=1} \frac{(X_i - X_1^-)}{(N_1 - 1)}$ is the sample variance of the covariate X in the sub-sample with treatment $D_i = 1$ and X_1^- and X_0^- are mean values of X in the treatment and control sub-samples.

Table 5: Estimates of the Impact of Training on the Probability of Finding Paid Work

	Matched Sample (without covariates)	**Matched Sample (with covariates)**
Treatment-Control Difference	**-1.67**	**-1.28**
P-value	0.81	0.81
Observations	266	266

Note: The treatment-control difference represents the difference in the *probability* of obtaining wage employment between training participants and non-participants.

Table 6: Description of Variables Used in OLS Earnings Equation

Variable	**Description**
Outcome	
Log Wage-Hour	Natural logarithm of hourly wage
Covariates	
Training Dummy	=1 if enrolled in training program; =0 otherwise
Ability Score	Ability index created from scores on a English language and reasoning ability test
Tenure (months)	Number of months spent in wage employment
O/L Dummy	O-level passed? Yes=1, No=0
A/L Dummy	A-level passed? Yes=1, No=0
Age	Number of years since birth
Male Dummy	Male? Yes=1, No=0

Table 7: OLS Estimates of the Earnings Function for Employed Sub-Sample

Variables	**Matched Sample**
Training Dummy	.373***
	(.108)
Male	.243**
	(.110)
O/L	.290
	(.571)
A/L	.317
	(.160)
Ability	-.162
	(.236)
Age	-.061
	(.049)
Tenure	.013**
	(.004)
Observations	96
R-Squared	0.251

Notes: (1) Standard errors reported in parentheses.

(2) * Significant at the 10% level; ** Significant at the 5% level; *** Significant at the 1% level

Table 8: Manski–Horowitz Bounds on Treatment Effects for Log Wages

Control Group		Notation	
(i)	Observations		133
(ii)	Employment Rate	Pr (S=1|T=0)	0.36
(iii)	Mean Log(Wage)	E[Y|T=0, S=1]	3.20
(iv)	Upper Bound	ii*iii+(1-ii)*Y^{UB}	4.46
(v)	Lower Bound	ii*iii+(1-ii)*Y^{LB}	2.29
Treatment Group			
(vi)	Observations		133
(vii)	Employment Rate	Pr (S=1|T=1)	0.36
(viii)	Mean Log(Wage)	E[Y|T=1, S=1]	3.58
(ix)	Upper Bound	vii*viii+(1-vii)*Y^{UB}	4.60
(x)	Lower Bound	vii*viii+(1-vii)*Y^{LB}	2.43
Treatment Effect			
(xi)	Upper Bound	(ix)-(v)	**2.31**
(xii)	Lower Bound	(x)-(iv)	**-2.03**

Notes:(1)1.78 and 5.17 are the lower and upper bounds of the support of log(wage); (iv)=(ii)*(iii)+[1-(ii)]*5.17; (v)=(ii)*(iii)+[1-(ii)]*1.78; (ix) and (x) are defined accordingly.

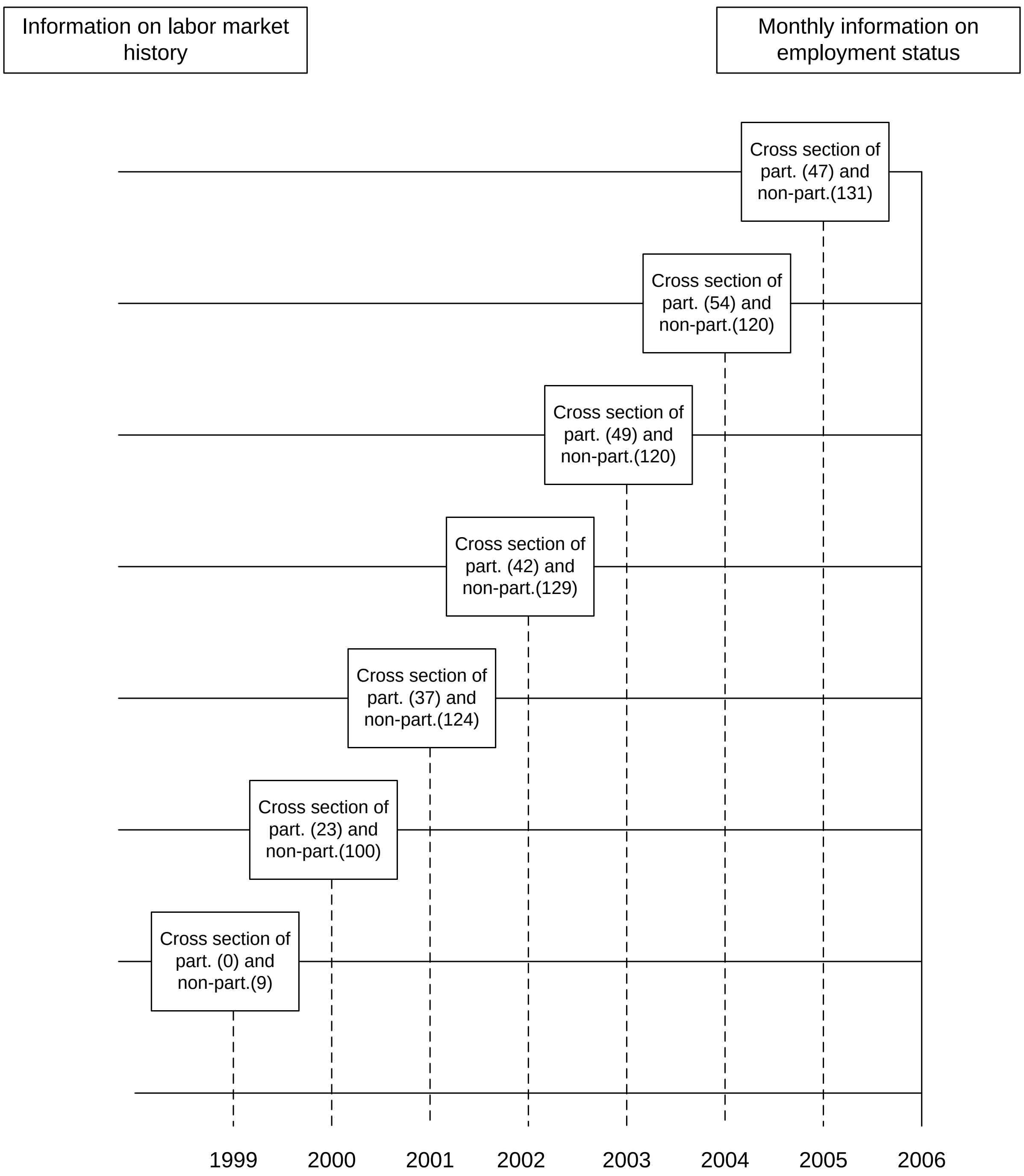
(2) The upper (lower) bound estimate of the treatment effect implies a 231% (-203%) increase (decrease) in wages associated with training.

Table 9: Lee Bounds on Treatment Effects for Log Wages

Control		
(i)	Observations	133
(ii)	Employment Rate	0.36
(iii)	Mean log(wage) for employed	3.20
Treatment		
(iv)	Observations	133
(v)	Employment Rate	0.36
(vi)	Mean log(wage) for employed	3.58
Trimming Threshold	p=[((v)-(ii)]/(v)	0.00
(vii)	Trimmed Mean : $E[Y\|Y > y_p]$	3.58
(viii)	Trimmed Mean : $E[Y\|Y < y_{1-p}]$	3.58
Treatment Effect		
(xi)	Upper bound estimate: (vii)-(iii)	**0.38**
(xii)	Lower bound estimate: (viii)-(iii)	**0.38**

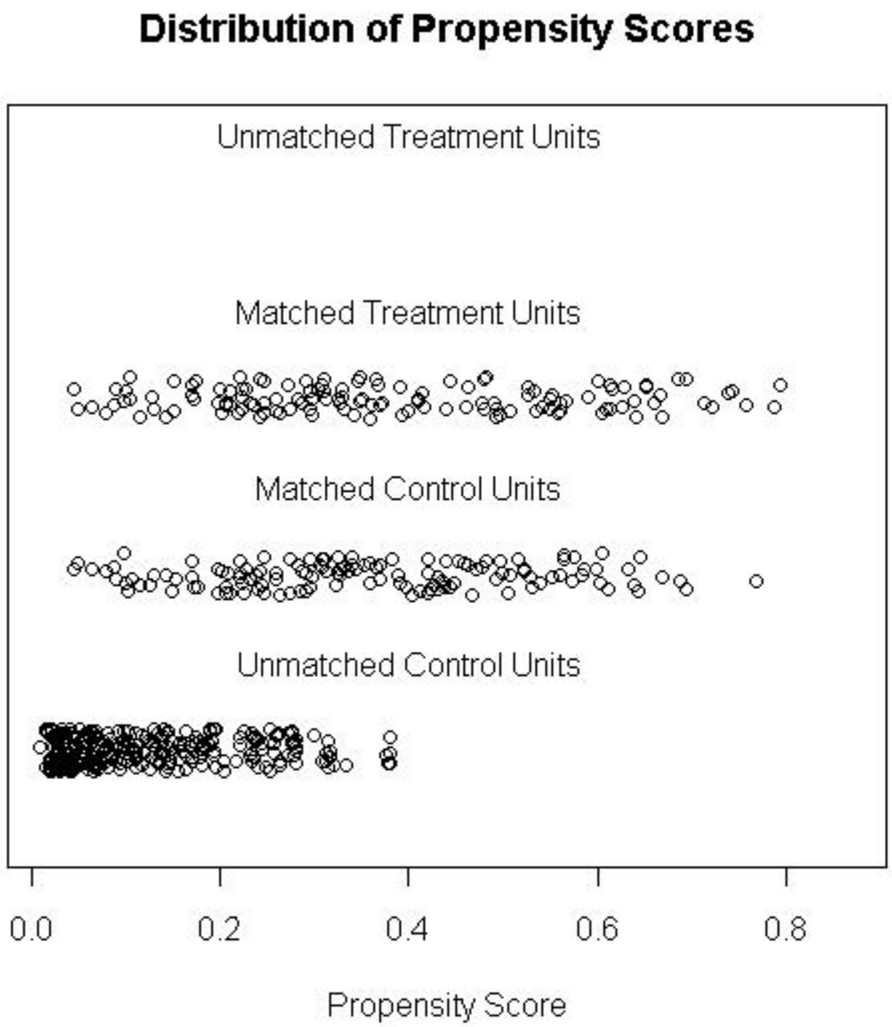
Notes: (1) Both the upper and lower bound estimates of the treatment effect imply a 38% *increase* in wages associated with training program participation.

Figure 1: Data available for Analysis



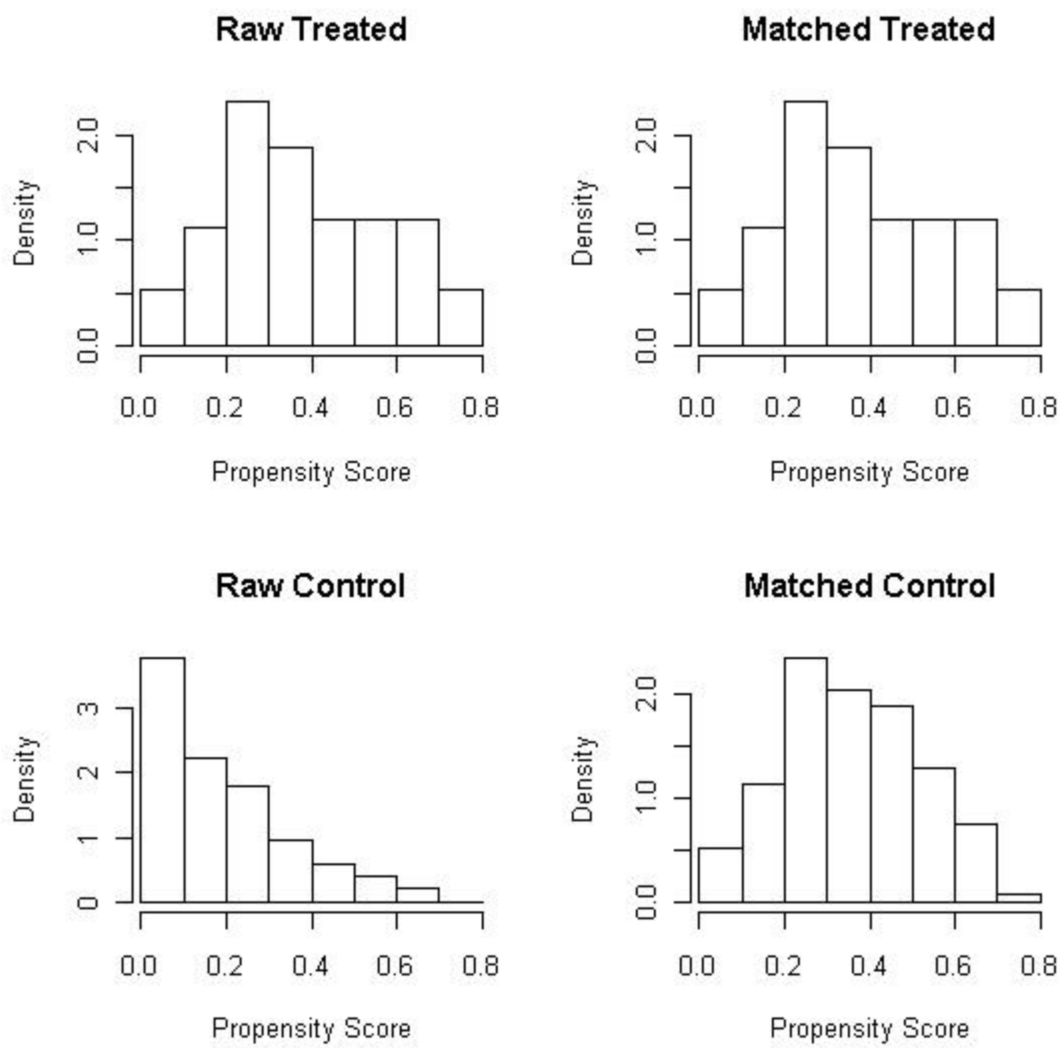
Note: The text in the box captures the number of training program participants and non-participants by graduating year, with graduation year indicated below on the horizontal axis.

Figure 2: Checking for Overlap in Propensity Scores



Note: Matches chosen using 1:1 nearest neighbor matching on propensity score.

Figure 3: Histograms Depicting the Distribution of Propensity Scores across Treatment arms



Note: Raw treated and raw control refer to the propensity score distributions for the full, unmatched sample.

Appendix- A

Institutional Background on Training Providers in Sri Lanka

High unemployment rates among educated young people and their low skill levels are issues of critical policy concern for the Government of Sri Lanka (GOSL). Youth unemployment, resulting mainly from prolonged job search, is of particular concern given the history of social unrest over youth joblessness. This is combined with another policy concern that school-leavers-grade 9, GCE O and A-levels, and university graduates are entering the labor market ill prepared for the world of work. An important response from the GOSL has been to develop technical education and vocational training to facilitate the school to work transition and to reduce skills gaps and skill mismatches in the labor market.

The Technical Education and Vocational Training (TEVT) sector in Sri Lanka is currently made up of an extensive system of public, private and NGO sector training providers. In 1994, the TEVT sector was elevated to a ministerial function and the key TEVT providers placed under the supervision of one ministry, the Ministry of Tertiary Education and Vocational Training (MTEVT). The Tertiary and Vocational Education Commission (TVEC) was established and now functions as the primary body for setting policy and regulating TEVT sector activities. In 2001, there were about 920 training institutes registered with the TEVC comprising of 556 institutions in the public sector, 252 in the private sector and 112 in the NGO sector. In addition, a sizable number of private sector providers operate in the market without registering with the TEVC.

The non-state TEVT sector, while quite small initially, has grown and expanded into many areas during the post-liberalization period. Private training institutes are now well established in occupational areas such as machining, welding, radio repair, motor repair,

Appendix- A (continued)

electrical wiring, refrigeration and air conditioning, television, computer and communications technology, tourism and hotel industry. Many private providers operate on a fee basis, especially in urban centers for bookkeeping and accountancy, computer technology and office management. The NGOs offer craft-level training, fee free or on a nominal fee basis, targeting unemployed youth, rural women, school leavers, and semi- or unskilled workers. The information base on private and NGO sector TEVT providers - their enrollments, regional distribution, course offerings, and operations – is limited and coverage relatively incomplete.

Student intake grew at an annual growth rate of 8.9 percent between 1990 and 2002 for the major public TEVT providers. The proportion of female enrollments in TEVT courses has remained relatively constant at about 40 percent, clustered in several traditionally female occupations (e.g. dressmaking, ISM operator, beauty culture, secretarial jobs). In general, female participation in TEVT is low relative to their participation rates in school and higher education.

Moreover, there is a regional concentration of TEVT activities in the Western, Southern, Sabaragamuwa, North-Western and Central Provinces. The Western Province alone accounts for about 30 percent of enrollments while the other four provinces together account for more than 45 percent of total enrollments (public sector providers only).

CHAPTER 4. THE SCARRING EFFECTS OF YOUTH JOBLESSNESS: MICRO-EVIDENCE FROM SRI LANKA

A paper to be submitted to the Review of Development Economics

Murali Kuchibhotla

I. Abstract

This paper investigates how early periods of joblessness-specifically, within the first year of school leaving- affect the future prospects for finding employment. We find that early periods of joblessness greatly increase the prospects of experiencing jobless spells in the future. Using both parametric and semi-parametric approaches for modeling duration data, we find that differences in early jobless exposure among individuals contribute to between 6 -33 months of additional joblessness in the future.

II. Introduction

Developing and developed country labor markets are characterized by relatively high rates of youth joblessness. While it is true that youth are more likely to be out-of-work at the start of their professional careers, early jobless episodes can lead to large future penalties, both in terms of lost wages and diminished work prospects. For example, Mroz and Savage (2006) report that unemployment experiences as long as ten years ago for U.S. youth continued to adversely affect their earnings in the present. Indeed, the existence of persistent wage penalties associated with unemployment has been confirmed in both the U.S.[43] and in Europe[44]. Less is known about the

[43] See Ellwood (1982) and Kletzer and Fairlie (2003).
[44] See Arulampalam (2001), Gregg (2001), Gregg and Tominey (2005) and Gartell (2009).

effect of early jobless experiences on being jobless in the future in developing countries. This study investigates the causal effects of early periods of joblessness for youth in Sri Lanka.

A negative influence of early jobless episodes on the future labor market prospects is referred to as “scarring”. Scarring is said to be present when an individual who has been jobless in the past is more likely to suffer from adverse labor market experiences in the future, when compared to an otherwise identical individual who has not been jobless. To highlight the presence of this scarring phenomenon in the Sri Lankan context, we begin by displaying the strong persistence in joblessness among Sri Lankan youth over time. Figures 1-4 display the patterns of employment and joblessness in our data.[45] These figures display time use patterns for young workers for a period of up to six years after the completion of schooling, for a total of 609 individuals. Figures 1 and 2 display the proportion of time spent in future joblessness and future employment, respectively, for individuals who spend all of their first year after leaving school (Year 1) in a jobless state. Figures 3 and 4 then replicate these figures for those individuals who are able to escape joblessness altogether during their first year after school. Figure 1 displays strong persistence in the pattern of joblessness over time. Almost 60% of those who are stuck in a jobless state in year 1 end up spending at least half of their next few years being jobless. On the other hand, Figure 3 tells us that 78% of those individuals without any early exposure to joblessness spend their next few years being employed. The data thus suggest that interruptions to employment may inflict long term ‘scars’ on individuals through the increased future incidence of joblessness.

Such strong persistence in the patterns of joblessness over time can be the result of either *state dependence* or negative *duration dependence* or both. Negative duration dependence is said to be present when the probability of exiting joblessness declines with the length of the

[45]Joblessness is defined to include time spent in unemployment as well as time spent out-of-the labor force.

experienced jobless spell, while state dependence concerns the adverse effect of current jobless spells on the prospects for future employment.

Two main explanations have been advanced for the scarring phenomenon. First, if human capital deteriorates due to idleness and there is wage stickiness, then individual re-employment probabilities decline over the length of a jobless spell. Under such circumstances, long-term joblessness is the likely outcome. Alternatively, even in the absence of a decline in human capital during a jobless period, prospective employers may use the duration of an individual's previous jobless spell(s) as a signal of expected productivity. If firms rank applicants for vacancies based the length of previous jobless spells, simple matching models can be used to show that there will be a decline in the exit rate from joblessness as the jobless spell progresses. Whatever the specific cause(s) of scarring, the implication is that the longer a particular jobless spell has lasted, the less likely it is that this joblessness will end in the future. Previous studies of youth unemployment have had difficulty distinguishing between these two explanations.

The above implies that unemployment or joblessness spells may not cause poor future job prospects, but may instead reflect the prior existence of unobservable attributes that lead to poor job prospects. Since all relevant individual characteristics can never be adequately captured in the data, unobserved heterogeneity will complicate any effort to distinguish between the scarring effects of past unemployment/jobless spells and unobserved fixed effects.

To address this selection issue, we adopt a sequence of estimated models. First, we follow the standard approach by measuring the scarring effect *without* accounting for the presence of unobservable fixed effects. We find large differences in the extent of future joblessness between youth who found work immediately after leaving school vs. youth who experience some joblessness in their first year after leaving school. The implied effect of initial joblessness on future joblessness ranges from 6 months to 33 months over the subsequent five year period.

This paper is organized as follows. Section 3 briefly reviews the literature on the evidence in favor of scarring in different countries. Section 4 describes the structure of our data. Sections 5- 9 detail the methods used for analysis in this paper. Sections 10-12 describe the main results. Section 13 concludes.

III. Literature Review

A large empirical literature has developed over the years which attempts to test for whether spell(s) of unemployment or inactivity represent more than just a temporary interruption in a person's working life. The concern is that experiencing a spell of joblessness may tarnish a worker in the future, either by diminishing the prospects for finding work, or lowering the wage obtained in the future relative to the current wage. Much of this literature on scarring is focused on rich countries. The evidence provided by this literature is, for the most part, mixed. On one hand, there are studies such as Lancaster (1979), Nickell (1979) and Van den Berg and Van Ours (1994), which all document evidence in support of negative duration dependence in the UK. On the other hand, Arulampalam, Booth and Taylor (2000) and Narendranathan and Elias (1993) find no evidence of long-term duration dependence in UK data. Such mixed evidence is also common for European countries other than the UK, as discussed by Ryan (2001). Mixed evidence has also been documented for the United States. Early studies by Heckman and Borjas (1980) and Ellwood (1982) concluded that the effect of early joblessness on future employment in the US was small. However, an influential recent study by Mroz and Savage (2006) found stronger evidence of adverse effects on future unemployment as well as negative effects on earnings that diminish slowly over time. To complicate things further, some studies have found a non-linear relationship between unemployment duration and exit probabilities from unemployment, with both positive and negative duration dependence shown to exist for different levels of unemployment duration[46]. This is true irrespective of whether the focus is on adult or youth unemployment[47].

In their survey of long-term unemployment in Europe, Machin and Manning (1999) emphasize how the evidence on duration and state dependence is often tied to the modeling technique employed. They show that apparent negative duration dependence in the raw data for 12 out of 13 European countries disappears when account is taken of both observed and unobserved heterogeneity.

Many recent studies have examined unemployment in the transition economies. Prominent among them are Earle and Pauna (1996) on Romania, Foley (1997) and Grogan and van den Berg (2001) on Russia, Kupets (2006) on Ukraine and Lubyova and van Ours (1997) on the Slovak Republic. In developing countries, evidence on unemployment duration has been advanced by Tunali and Assaad (1992) on Egypt; Serneels (2007) and Dendir (2006) on Ethiopia; Aranki and Daoud (2006) on Palestine; Tansel and Taşçı (2010) on Turkey; Byrne and Strobl (2004) on Trinidad and Tobago; Suryadarma, Suryahadi and Sumarto (2007) on Indonesia; Knight and Li (2006) on China; and Rama (1998) on Tunisia.

We use two of these studies to highlight the issues and analytical methods employed in most of these studies. Tansel and Tasci (2010) explore the issue of duration dependence in unemployment for the case of Turkey using reduced-form event-history models. They find a non-monotonic relationship for duration dependence, with the probability of finding a job initially falling (negative duration dependence) and then eventually rising (positive duration

---

[46]See Bheim and Taylor (2000) for a study focusing on the UK that documents the existence of such a non-monotonic relationship.

[47] Heckman and Borgas (1980) and Russell and O'Connell (2001) are two such papers focusing on duration dependence in youth unemployment, focused on the US and nine EU countries, respectively.

dependence) over the course of the unemployment spell. They argue that the initial period of negative duration dependence may be due to various factors such as the loss of human capital and motivation experienced by workers as well as the negative productivity signal that unemployment sends to prospective employers. They attribute the subsequent positive duration dependence to the exhaustion of family support, especially given the widespread prevalence of family support in Turkey.

Dendir (2006) also employs a reduced-form duration analysis to model unemployment duration in Ethiopia using a nationally representative urban household survey. Using a semi-parametric Cox-proportional hazard model, Dendir finds both positive and negative duration dependence in the data. Dendir's main finding is that the computed mean unemployment durations are in the 3-5 year range, which is about seven times the duration found in developed countries.

These two developing country studies rely on the use of reduced-form event history models to evaluate duration dependence. Unobserved heterogeneity is often confounded with true duration dependence. As Heckman (1991) demonstrates, studies that do not attempt to correct for unobserved heterogeneity are more likely to find spurious duration dependence in the data. However, "it does not really seem possible in practice to identify separately the effect of heterogeneity from that of duration dependence without making very strong assumptions about functional form which have no foundation in any economic theory," as noted by Lancaster (1990). Indeed, inferences regarding duration dependence have been quite sensitive to distributional assumptions about the unobserved heterogeneity, and so the distortions introduced by incorporating the wrong functional form for this heterogeneity may be no less serious than ignoring it altogether.[48]

A drawback of developing country studies that focus on the scarring effects of unemployment is that these studies, by construction, tend to ignore those who are inactive or out of the labor force. Focusing on unemployment exclusively while ignoring the broader issue of joblessness can bias results, given evidence that unemployment and inactivity may not constitute separate labor market states[49]for individuals prone to long unemployment spells.

Our study makes several improvements on the methodologies previously employed to study youth joblessness in developing countries. First, we conduct our analysis using both the traditional unemployment measure as well as a measure that incorporates both unemployment and inactivity (called joblessness). This allows us to check how sensitive the estimated scarring effects are to alternative measures of time spent way from employment. Second, we account for the presence of unobservable confounders that may bias estimates of scarring. We conduct sensitivity exercises to show that these unobserved confounders are unlikely to be sufficiently large enough to invalidate the findings of substantial scarring effects. Finally, we address the problems of measurement error in recall by grouping together months spent in early joblessness into broad categories. These categories then form the basis for further analysis. To the extent that such grouped data is less likely to be subject to measurement error, our estimated scarring estimates are less likely to be incorrect.

IV. Data

Detailed information on the time order of labor market spells is obtained from a survey of Sri Lankan households, administered by the University of Colombo with support from the World

[48] See Narendranathan and Stewart (1993) and Ridder (1987).
[49] See section 3 for more details.

Bank. Data were collected on respondents who had formally left school and were between the ages of 15-26 years at the time of the survey. The survey was administered between April and May of 2006 to 1026 individuals from 450 different households. Care was taken to ensure that the sample was representative of the nation, with the exception of the conflict ridden provinces. The survey consists of individuals who left the formal schooling system between 1999 and early 2006. We thus have a panel dataset that is unbalanced in nature, with labor market history varying between 1 month and 78 months for individuals, depending on the year and month in which they left school.

When the survey was conducted, each respondent was asked to provide retrospective work-history data since the time of leaving school. Respondents provided a detailed month-by-month time history, characterizing their labor market status as ‘unemployed’, ‘inactive, ‘wage employed’, ‘self-employed and ‘in training’. Since the data is based on a retrospective survey, it is subject to the usual measurement problems associated with recall data. However, the surveyors were specially trained to assist the respondent in making the information as accurate as possible. Monthly time allocations to the various activities were required to add up to the total time available and discrepancies were resolved. In addition, because respondents are early in their work careers, they would have had relatively few employment and/or unemployment spells, and recollections about how they spent their time in the six years or less since leaving school should be reasonably accurate.

We work with a subsample of these 1026 individuals for whom we have at least 2 years of complete work history data. In addition, we exclude individuals who spent no time in the labor force after leaving school, meaning that they were in the inactive state every month. We view such individuals, most of whom are women, as having no inclination of participating in the work force. We are left with a sub-sample consisting of 609 individuals on which to base our analysis.

To be able to work with these data, we had to condense information on labor force status into three labor market states, namely, ‘employment’, ‘training’ and ‘non-employment’. We aggregate wage employment and self- employment into a single category which we call ‘employment’. We combine unemployment and inactivity into a single category labeled ‘non-employment’ as there may be no true difference between the unemployed and those who are out of the labor force. A vast literature has developed over the years which attempts to distinguish between these two states in developed countries. Clark and Summers (1979), Clark and Summers (1982) and Gonul (1992) investigated this issue in the USA. Clark and Summers (1979) in general and Clark and Summers (1982) for teenagers found that unemployment and out of the labor force are not distinct labor market states. Gonul (1992) found that while for young women the two states are distinct, for young men they are not. There are also studies that have been undertaken for countries other than the US, which have also found it hard to distinguish between unemployment and inactivity.[50]

The survey collects a wealth of pre-labor market information normally unobserved in labor market databases. Information on parental/family characteristics includes household wealth and location and number of siblings. Each household provides information on asset ownership[51] and housing characteristics.[52] We construct a household wealth measure as the weighted sum of the individual assets using the first principal component of the wealth measures as the weights.

[50] Examples include Jones and Riddell (1999) and Jones and Riddell (2006) for Canada; Schweitzer (2003) for the UK; Garrido and Toharia (2004) for Spain and Brandolini, Cipollone and Viviano (2006) for Italy.

[51] All respondents answer a series of questions on whether their household owns a car, television, computer and refrigerator.

[52] Questions relate to the ownership of housing units, the floor area and number of rooms within the house and the provision of amenities such as toilet facilities, protected drinking water and source of energy for cooking and lighting.

We then aggregate these assets into a single index. Individual ability/educational measures include schooling and a composite of test scores obtained in math and reasoning. These tests were administered separately to each respondent. These measure ability not captured by educational qualifications. We also have information on the year of school leaving which we use to indicate the respondents' labor market entry cohort.

V. The Fractional Logit Model

We start with a standard regression approach to the issue of estimating the scarring effects associated with early joblessness. Our interest is in determining how the proportion of time spent jobless *early* on in the work career, U_o , affects the proportion of time spent jobless *later* on in life, U_{1+}. We require an empirical strategy that allows us to take account of the fractional nature of the dependent variable as $0< U_{1+} <1$.[53]

We use the fractional logit model proposed by Papke and Wooldridge (1996) defined by the log-likelihood specification:

$$l(\gamma, \beta) = U_{1+} \ln[G(\gamma U_0, X\beta)] + (1 - U_{1+}) \ln[1 - G(\gamma U_0, X\beta)]$$

where γ is a coefficient attached to unemployment experienced in the year after leaving school and X is a vector of individual human capital and family background variables listed in Table 1. We assume that $G(\gamma U_0, X\beta) = \frac{e^{\gamma U_0 + X\beta}}{1+e^{\gamma U_0 + X\beta}}$. This specification fixes $G(\gamma U_0, X\beta) \in (0,1)$ which insures that the predicted values of $U_{1+} \in (0,1)$.

The results of this estimation exercise are displayed in Table 2. It should be kept in mind that the effects of early joblessness on later work careers that we identify in this paper extend

[53] There are two main issues which need to be addressed when working with proportional data. First, proportional data are only observed over the [0, 1] interval, which implies that the conditional expectation of the variable must be a non-linear function of the covariates. Second, the conditional variance must be a function of the conditional mean because the conditional variance must change as the conditional mean approaches either boundary.

only to a time frame of at most five years after the completion of schooling. Since we do not observe the work careers of individuals beyond a period of six years after school completion, we are unable to draw any inferences about the influence of early joblessness beyond this time frame.

Joblessness in the first year after leaving school has a large impact on the proportion of time spent in a jobless state later on in the working career. The marginal effect associated with this variable is 0.536. This implies that, conditional on all other variables remaining constant, individuals who are jobless through the entire first year period end up spending an additional 53.6% of years 2-6 of their working life jobless, when the comparison is made to individuals who are able to avoid early joblessness altogether. On average, this translates into about 20 months of additional jobless exposure in years 2-6.

Other variables that turn out to have large and statistically significant effects are gender, training and the presence of young children in the household. The marginal effect associated with training is 0.127, which implies a 12.7% *increase* in joblessness after the first year, when the comparison is made to those who do not train. Men spend 16.5% less time in a future jobless state compared to women, while the presence of young children in the household contributes to a 7.8% increase in jobless time in years 2-6.

Higher educational qualifications and ability levels tend reduce the amount of time spent in future jobless spells. Compared to those in the bottom decile of the ability distribution, individuals in the top decile spend about 6% less time in future joblessness. The completion of high school contributes to a 1.8% decline in future joblessness. However, none of these effects is statistically significant. Moreover, we are unable to reject the hypothesis that these three variables jointly have no effect, with a p-value of 0.56 on the joint Wald test.

Finally, the year of school exit does not seem to have much of an effect on later joblessness. Not only are the effect sizes economically small but they are statistically insignificant, and we are unable to reject the composite test for no cohort effect (p-value on Wald test of 0.87). Moreover, the joint effect for the district of residence (results for which are not displayed in the table) is statistically indistinguishable from zero (p-value of 0.60).

To check how sensitive these scarring effects are to the way in which time away from work is measured, we also present an alternative set of results where we model time spent only in the state of unemployment. These results are presented in Table 3. In this specification of the fractional logit model, we use the proportion of time spent in unemployment after year one as the dependent variable, while early unemployment is used as a regressor to pick up any potential scarring effects.

The marginal effect associated with early unemployment is similar to that obtained under the jobless measure. These estimates imply that individuals who are unemployed through year one end up spending an additional 61% of years 2-6 of their working life in an unemployment state, when the comparison is made to individuals who are able to avoid early unemployment altogether. On average, this translates into about 23 months of additional unemployment exposure in years 2-6.

There is strong evidence consistent with the scarring hypothesis, irrespective of how time away from employment and training is measured. The estimated effects of early unemployment/joblessness on future time use patterns imply substantial employment gains for workers able to avoid early spells of unemployment/joblessness. The size of these estimates is similar to the findings from the US and UK. For example, Ellwood (1982) finds that for U.S. youth, the scarring effects of early unemployment last at most two years. Similarly, for the

United Kingdom, a study by Narendranathan and Elias (1993) finds that state dependence for male early school-leavers evaporates within two years.

Our estimated scarring effects are harder to compare with the effects reported in developing country studies. This is because these studies tend to focus on analyzing the exit rate from unemployment, and their results are presented in terms of how exit rate probabilities vary across the various determinants of unemployment. No clear evidence is presented in these studies on the extent to which long-term unemployment is prolonged as a result of early problems in the labor market.

VI. Problems with the Fractional Logit Model Approach

The fractional logit model is not the most appropriate approach to evaluating the scarring effects associated with early joblessness for two main reasons. First, measurement error in the timing or duration of joblessness early or late in the post-schooling period may bias our results. Because our measures are based on recall, this measurement error would be greater soon after leaving school than in the more recent periods. In the context of this study, early joblessness is used as a regressor, while the more recent employment spells constitute the dependent variables. Classical measurement error in the dependent variable should not bias our results, but measurement error will bias our estimates of the effect of early spells of joblessness on later labor market success. These errors are particularly likely if respondents recall having been jobless but make mistakes about the fraction of time spent jobless, the very data that underlies the fractional logit model. Second, our fractional logit regression model relies on the specification of the functional form of the relationship between the outcome and covariates.

When the relationship between covariates and outcomes is incorrectly specified, this leads to biased estimates of the effects of early joblessness on the outcome measure.[54]

We deal with each of these problems in what follows. First, we group together similar levels of early joblessness into broad categories. For example, individuals with between 1-3 months of early jobless exposure are grouped into a single jobless category. The analysis that follows uses these broad categories as covariates. By focusing on such broad groups of joblessness, we are able to mitigate the problems associated with measurement error in our early jobless measure.

Second, we move away from the fractional logit method and adopt two different non-parametric methods to model the relationship between early and late joblessness. Our first set of methods is based on matching techniques. Compared to the fractional logit model, matching methods impose fewer assumptions on the exact functional form linking covariates to outcomes. Moreover, it is easy to detect whether or not confounding covariates have similar distributions across the different jobless categories under matching. To the extent that these confounding covariates are distributed differently across jobless categories, the data will not support estimates of the scarring effects associated with early joblessness.

Our second method is similar to the randomized experiment in that it exploits local variation in the assignment of individuals across the various grouped jobless categories to back out the scarring estimates of interest. We provide a more detailed exposition of these methods in section 7.

VII. Dealing with Measurement Error in Early Joblessness

We measure exposure to joblessness among individuals during their first year in the labor market after leaving school. These exposure levels range from 0 (no time spent jobless in year 1) to 12 (all of year 1 spent jobless). Instead of working with 13 jobless categories, we collapse the

[54] For other advantages of matching methods over regression, see the discussion in Imbens and Wooldridge (2009).

data to form only 5 categories of joblessness. Jobless category 1 consists of individuals with 0 months of jobless exposure. Jobless category 2 consists of individuals with 1-4 months of exposure, category 3 consists of individuals with 5-8 months of exposure and category 4 consists of individuals with 9-11 months of jobless exposure. Finally, category 5 comprises of individuals with 12 months of jobless exposure. The rationale for collapsing the data on early joblessness into these five categories is twofold: first, to make meaningful comparisons in the extent of joblessness after year 1 among individuals facing different levels of early exposure to joblessness and second, to mitigate the presence of measurement error in the extent of the early jobless measure.

To be able to make meaningful comparisons in jobless outcomes after year 1 across individuals in different jobless categories, we need a sufficient number of individuals populating each jobless category. When the data are collapsed into five jobless categories, there are at least 70 individuals that populate each jobless category. This allows for more robust comparisons across jobless categories. Moreover, since the data are retrospective in nature, there might be recall bias in the months spent in early joblessness. Grouping monthly jobless experiences into groups of longer duration increases the likelihood that we place jobless spells into the correct time frame. Figure 5 displays the distribution of early joblessness in months, while Figure 6 displays the distribution of early joblessness once the data have been collapsed into the five jobless categories.

VIII. Methods

i. Sub-classification and Paired Matching

If individuals are randomly assigned into labor market states after leaving school, then jobless duration early in life can be viewed as (potentially) having a causal effect on later labor market outcomes. If instead, the jobless state is at least partially due to choice, then the non-random selection process can result in group differences in later labor market outcomes that are incorrectly attributed to the length of the jobless spell rather than to the factors underlying the choice to be jobless. Such self-selection can seriously bias the outcomes of observational studies.[55]

Propensity score methods[56] have been widely used to obtain causal estimates in observational studies that involve non-random selection, when the sorting process satisfies an important assumption known as 'selection on observables'. This assumption asserts that *conditional* on a set of observed covariates, the sorting of individuals across groups is essentially random in nature. Once we condition on the relevant covariates, we can pretend as if the assignment of individuals across groups is the result of a process of randomization. Selection on observables is a powerful assumption, as it does not allow for the presence of unobservables which influence both the sorting process and the outcome of interest.

Propensity score methods have traditionally been used to correct for differences in observed covariates in the presence of two groups, labeled as treatment and control. In our application, these groups are defined on the basis of the extent of joblessness in the first year after leaving school. The propensity score is defined as the conditional probability of being in the treatment group, where the conditioning is done on a set of pre-treatment covariates. Conditional on the propensity score, the distributions of observed covariates are independent of treatment assignment.[57] For a given value of the propensity score, outcome differences between individuals

[55] Evidence is presented in Rosenbaum (2010), who defines an observational study as "an empiric investigation of treatment effects when random assignment to treatment or control is not feasible."
[56] Propensity score methods originated with the seminal paper by Rosenbaum and Rubin (1983).
[57] Rosenbaum and Rubin (1983)

in the treatment and control groups yield an unbiased estimate of the treatment effect at *that* propensity score value.

In our study, school leavers face different levels of exposure to joblessness in the first year. Some are jobless for the whole year while others experience shorter spells. When there are more than two groups into which individuals can self-select, there is no longer a neat separation between treatment and control individuals.[58] As such, a standard application of the propensity score methods outlined above is no longer applicable in the presence of more than two treatment levels.

Joffee and Rosenbaum (1999) show that there are circumstances under which a scalar *balancing* score performs a role similar to that of the propensity score, such that the impact of early jobless exposure on labor market outcomes can be assessed by comparing outcomes between individuals with different exposures in early joblessness, but with the same or similar values of the balancing score. The key requirement is that the distribution of early jobless exposure conditional on the covariates has to be a scalar function of the covariates. One circumstance under which this condition is satisfied is when the jobless exposure distribution can be modeled using McCullagh's ordinal logit model[59].

Under McCullagh's ordinal logit model[60], we break the distribution of first year jobless spells U_0, into a series of segments. The number of jobless segments experienced in the first year after leaving school, u_{0i}, is assumed to be a function of observed covariates, X_i with specification:

$$\log\left\{\frac{\Pr(u_{0i} \geq d)}{\Pr(u_{0i} < d)}\right\} = \theta_d + \beta_T X_i, \quad \text{for } d = 2,3,4,5$$

[58] Indeed, every individual can be thought to be subject to a different level of the "treatment".
[59] This condition is also satisfied in a Gaussian multiple regression model with homoskedastic errors. See Lu, Zanutto, Hornik and Rosenbaum (2001) for more details.
[60] This is also known as the proportional odds logit model in the literature.

where *i* denotes the individual and β_T is the coefficient vector associated with the covariates.

Under this model, the distribution of first year jobless spells given covariates depends on the covariates only through a scalar function $b(X_i) = \beta_T X_i$. This implies that $b(X_i)$ fulfills a role similar to what the propensity score does in the two treatment case, since the distribution of first year jobless spells U_0 is independent of X_i, conditional on $b(X_i)$.

Using $b(X_i)$ as a balancing score has the advantage that we are able to simultaneously balance the distribution of a number of covariates across different jobless categories by matching or sub-classifying on this scalar quantity. Since the true value of the balancing score is not known, it has to be estimated from the data. The estimated balancing score used for sub-classification and matching is $\beta_T^{\wedge} X_i$, where $\beta_T^{\wedge}$ is an estimate of β_T.

We believe that the assumptions behind the ordinal logit model are appropriate for our application, since the treatment -the number of months spent in the jobless state in year one- has a natural ordering. Concern about the appropriate jobless distribution is mitigated by recognizing that the role of the balancing score is to approximately balance the distribution of covariates across different jobless categories. As such, this requirement is easy to check in the data and we conduct checks to ensure that this requirement is satisfied.

With multiple levels of the treatment, treatment effects compute the change in the months spent in joblessness in years 2-6 as individuals move across different jobless categories in year one. It is these treatment effects that capture the scarring effects associated with early joblessness. To compute these treatment effects, we rely on the use of both paired matching and matching based on sub-classification on the propensity score.[61]

[61] The balancing score plays an important role in both of these exercises, as we explain further in what follows.

Under balancing score sub-classification, individuals are allocated to one of a number of subclasses constructed from the balancing score. It is typical to stratify the data based on five sub-classes. The choice of five subclasses is based on the results presented in Rosenbaum and Rubin (1983), where it is shown that creating five propensity score subclasses removes 90% of the bias in the estimated treatment effect associated with the covariates that enter the propensity score.

The adequacy of the ordinal logit model for modeling the distribution of early jobless exposure can be assessed by checking whether balance is achieved on each covariate, both in terms of jobless category as well as within each subclass. Looking at table 4, within each subclass (represented as a row in Table 4), if early joblessness were randomly assigned, we would expect the distribution of observed covariates to be similar across the various jobless categories. We test for this requirement below using both graphical checks as well as through formal ANOVA analysis.

Under matching, we construct matched pairs based on the proximity of balancing scores within pairs. Our interest is in identifying pairs of individuals that have similar values of the balancing score, but which differ in terms of the extent of early joblessness. Matched pair differences in the exposure to joblessness in years 2-6 then provide us with an estimate of the extent of scarring associated with early joblessness. The details of this matching procedure, including a detailed description of the matching algorithms used to construct the matches, are given in Lu, Zanutto, Hornik and Rosenbaum (2001).

We rely on both matching and sub-classification to compute treatment effects in order to check how robust the estimated scarring effects are to differences in the matching methodology employed. This is an important goal of any balancing score exercise, given the ample evidence that now exists on the extent to which the efficiency of the various matching methods, and the degree of bias reduction achieved (relative to experimental design methods) varies with the exact matching method that is employed.

ii. An Analogue to the Randomized Experiment

The other method that we rely on to compute treatment effects is analogous to the randomized experiment. This follows from the observation that individuals are unlikely to have *precise* control over the particular jobless category that they get slotted into. While an individual is likely to be able to manipulate his jobless category to some extent, the lack of complete control over this assignment implies that the jobless category can be thought of as being *locally* randomized. For example, consider an individual who is choosing how long to be jobless in the first year after leaving school (i.e., he is making a choice over the various jobless categories). Typically, the individual is unlikely to have precise control over this choice since early joblessness is likely to depend on a host of factors outside of the individual's control, such as the state of the local labor market, access to facilities/institutions that permit the matching of employers and prospective employees, etc. Given this lack of control over all relevant factors affecting the jobless category, there is likely to be some randomness (from the individual's perspective) in terms of the jobless category that he gets slotted into. Note, however, that this randomness in allocation is local in nature. That is, while the individual's background characteristics may not precisely control which jobless category he gets placed into, they nonetheless exert a strong influence over it. So while a given individual may not be able to control whether he is placed into jobless category 2 or 3, his background characteristics may nonetheless rule out the possibility of his ever being placed in category 5. It is in this sense that the jobless category can be thought of as being locally randomized.

As a consequence of the local randomization result, treatment effects can be computed using simple linear regression methods. For individuals populating every adjacent set of jobless categories, a regression can be run using as covariates both the individual's personal and family characteristics and a dummy variable for the jobless category occupied by the individual. Once the conditioning is done on the baseline covariates that affect selection into the various jobless categories, the coefficient on the dummy variable captures the effect of moving from one jobless category to the next adjacent jobless category. This is the procedure we use below.

IX. Covariates and Balancing Score Checks in Matching Methods

i. Covariates

The covariates used in modeling the balancing score are, for the most part, the same as those used in the fractional logit regression model. The one exception is that we exclude the receipt of training in year one as a covariate from the balancing score model. A key requirement of any balancing score analysis is that the covariates used to model the selection process into early joblessness must not themselves be affected by the extent of early joblessness. That is, the covariates must capture information that was available pre-treatment (i.e., before individuals entered the labor market). However, since the decision to train in the first year is likely to be influenced by the amount of time spent jobless in the first year, this variable must be excluded from the balancing score model. The inclusion of such covariates in the balancing score model leads to treatment effect estimates that are biased.

Following Zanutto, Lu and Rosenbaum (2005), we assess the extent of imbalance in the distribution of covariates across the various jobless categories by fitting one-way analysis of variance models (ANOVA) for each continuous and discrete variable entering the balancing score model. This helps provide an indication of the extent of covariate imbalance before the implementation of matching. The F-statistic for the effect of jobless category was significant at the .1 level for the ability measure and at the .01 level for household wealth. The F-statistic for the main effect for gender was significant at the .001 level, while both educational measures were statistically significant at the .1 level. Only one out of the four cohort dummy variables was associated with the jobless category at the .1 level, while five out of the 12 district dummy variables were associated with jobless category at the .1 level. In all, 11 out of our 23 covariates were found to have an association with jobless category prior to matching at the .1 level. This indicates that there is a large degree of imbalance in the distribution of the covariates across jobless categories, as we would only expect roughly 2 out the 23 covariates to be out of balance if jobless category were randomly assigned across individuals.

ii. Balancing Score Specification and Covariate Balance Checks

While the choice of variables to be included in the balancing score model is informed by prior evidence on the determinants of unemployment in Sri Lanka, a decision needs to be made about whether these covariates should enter the ordinal logit model as linear terms, as quadratic terms or as interaction terms between covariates. We used the following procedure to decide on this. First, we fit an ordinal logit model with main effects for all 23 covariates. Second, we checked for extent of the overlap in the balancing scores across the five jobless categories. This is to verify the comparability of individuals across different jobless categories. This ensures that there are no observations in any category with excessively high or excessively low balancing scores, relative to the observations in other jobless categories. Observations with such high or low balancing scores are removed from further analysis, and the resulting region of overlap of balancing scores across jobless categories is referred to as the region of "common support". Third, individuals were sub-classified into 5 roughly equally sized strata[62] based on their respective balancing score values. Fourth, the extent of imbalance in each covariate, within each jobless category and across the various quintiles of the balancing score is assessed using two-way ANOVA models. A statistically significant effect associated with either the main effect of jobless category or the interaction effect for jobless category and balancing score strata indicates an imbalance in that covariate. Finally, if an imbalance in any covariate is detected, the process is repeated from the start, but with quadratic and interaction terms for the out-of- balance covariates now entering the ordinal logit model. This process is repeated till we are able to settle on a balancing score specification that is able to achieve balance over all covariates.

As is turns out, our initial model with only main effects included for the 23 covariates achieves fairly good balance across all covariates by both balancing score strata and jobless category. The results from the ordinal logit model for the propensity score are displayed in Table 5. Because the main use of the balancing score is to balance the distribution of covariates, we do not discard statistically insignificant predictors from the model.

Figures 7 and 8 show the distribution of balancing scores across the five jobless categories, both before and after the common support condition is imposed. Figure 8 in particular shows that the balancing scores are fairly well distributed across the different jobless categories. However, 18 observations had to be dropped from the analysis on account of the common

[62] Stratum 1 has 118 observations, Stratum 2 has 118 observations, Stratum 3 has 118 observations, Stratum 4 has 118 observations and Stratum 5 has 119 observations, for a total of 591 observations.

support condition. It should be kept in mind that this restricts the inferences that we are able to draw to that sub-population represented by the region of common support.

X. Matching Results

i. Sub-classification on the Balancing Score

The results from sub-classification on the balancing score are presented in Table 6. All the individuals in our sample are placed into one of the 25 cells in the table, depending on their jobless category and the value of their balancing score. The number in each cell of the table then captures the average number of months spent in joblessness in years 2-6. Within each quintile of the balancing score, the general trend is for the average exposure to joblessness to rise as we move from lower to higher jobless categories.[63] For example, within the lowest balancing score quintile, joblessness rises from an average of 5.76 months to around 26 months as we move from category 1 to category 5. Similarly, within the highest balancing score quintile, joblessness rises from an average of 6.6 months to 34.5 months as we move from category 1 to category 5. A similar pattern holds in each of the remaining three quintiles. Early joblessness is thus highly correlated with the amount of joblessness experienced in the future, with jobless exposure in years 2-6 rising between 19.5 months – 28 months (depending on the balancing score quintile) as individuals move from 0 to 12 months of joblessness in year 1. The effect of one year of early joblessness is thus very costly for young adults, contributing to at least an additional year and a half of additional joblessness in their future work careers.

As before, the amount of time spent in joblessness after year 1 rises with an increase in the period of early joblessness. The estimated difference in mean outcomes between those individuals who experience 0 months of early joblessness and those who experience between 1-4

[63] Where jobless category 1 is the lowest category and jobless category 5 is the highest category.

months of early joblessness is quite large, with jobless differences between these two groups averaging around 12 months. Similarly, a move from 9-11 months to 12 months of joblessness is associated with an increase in future joblessness of about 10 months. In all, the difference in outcomes between category 1 (no joblessness in year 1) and category 5 (full joblessness in year 1) is 23 months. Between jobless category 1 and category 4, however, average jobless durations increase by only about 1.6 months.

ii. **Paired Matching**

The 591 observations in our sample are grouped into 295 pairs, with one observation being discarded. The matching algorithm pairs individuals from different jobless categories. The joint distribution of treatment categories within these pairs is given in Table 7. The counts within this table relate to pairs of individuals. The data is arranged such that within a pair, the individual with the higher level of jobless exposure is represented along the rows, while columns capture individuals with lower levels of exposure. Individuals within a given pair differ by at least one level of jobless category. For example, in 18 pairs, the distribution of early joblessness is such that 18 individuals are in jobless category 2, while the other 18 are in jobless category 1. Also, in 91/295=30% of pairs, the differences in jobless category between individuals is four levels.

Results for matched pair differences in joblessness after year 1 are displayed in Table 8. We present estimates for differences in joblessness after year 1 across all matched pairs, as well as pairs stratified by different levels of early jobless exposure. Positive outcome differences in pairs indicate that individuals exposed to higher jobless categories spend more time in joblessness after year 1. For pairs in which the difference in jobless categories corresponds to four levels, the median and mean differences in the outcome are 22 months and 24 months, respectively. Across all matched pairs, mean differences between high and low jobless category individuals average about 16 months.

XI. Local Randomized Experiment Results

The results from local randomization are presented in Table 9. As explained in section 7, separate regressions are run on groups of individuals populating adjacent jobless categories. In all, four sets of regressions are run, one each for individuals from jobless categories 1 & 2, 2 & 3, 3& 4 and 4 & 5. In each regression, we include indicator variables for the jobless category occupied by each individual as well as any baseline individual and family level characteristics that may affect joblessness in years 2-6. As such, these background variables are the same as those used in the modeling of the balancing score above. The causal effect- on future joblessness- of moving from one jobless category to the next adjacent jobless category is picked up by the regression coefficient on the indicator variable. These are the numbers that are displayed in Table 9.

Moving from jobless category 1 to category 2[64] increases the exposure to future joblessness by around 11 months. A move from category 2 to 3 increases future joblessness by around 2 months, from 3 to 4 by 2 and a half months and from 4 to 5 by around five and a half months. To compare the effects of a move from jobless category 1 (0 months early joblessness) to jobless category 5 (12 months of early joblessness) on future joblessness, we extrapolate the results obtained above by multiplying by 3. This then gives us four sets of estimates of the effects of one year long spells of early joblessness. The results imply that one whole year spent in early joblessness increase future exposures to joblessness by between 6 months - 33 months. These

[64] On average, a move from one jobless category to the next jobless category implies an increase of around 4 months in early joblessness.

causal estimates of the effects of early joblessness are thus wider than those obtained under matching and sub-classification, though the upper bound is fairly similar under both sub-classification and local randomization.

Under jobless category can be thought of as being *locally* randomized, the results presented above are likely to be most accurate for comparisons across adjacent jobless categories. This is because individuals are likely to have more control over a move from jobless category 1 to jobless category 5 than they are over a move from jobless category 1 to jobless category 2.

The results presented for local randomization also compare favorably with the results obtained under sub-classification. The general pattern under both is that small periods of early joblessness—primarily a move from jobless category 1 to category 2—tend to have a large impact on jobless durations experienced in the future. This effect tends to attenuate with further increases in early joblessness. A move from category 2 to 3, or from category 3 to 4 tends to contribute much less to future jobless durations.

XII. Comparisons with Fractional Logit Results

The results from the fractional logit model as well as from matching, sub-classification and local randomization suggest that early joblessness contributes to the lengthening of the duration of future jobless spells. The fractional logit estimates imply that the scarring effects of early joblessness rise linearly with the duration of the jobless spell. These estimates imply that a full year spent jobless after leaving school contributes to an additional 20 months of joblessness in years 2-6 of an individual's working life.

By comparison, the matching and sub-classification estimates imply scarring effects that do not rise linearly with the duration of the jobless spell. These estimates indicate that it is very small and very large levels of early jobless exposure that have the largest effects on future joblessness. Compared to those who do not experience any early jobless spells, even small levels of early exposure to joblessness (between 1-4 months) contributes to about 12 months of additional joblessness in the future. A rise in jobless exposure beyond 1-4 months (but not beyond 11 months) contributes little to future levels of joblessness. Only when we move past 11 months of jobless exposure do we see a significant jump-about an increase of 10 months- in future jobless levels. Sharp increases in future joblessness for small changes in early jobless exposure are consistent with the hypothesis that prospective employers may use the occurrence of an individual's previous jobless spell as a negative signal of expected productivity, such that even small levels of early exposure to joblessness may carry significant costs.

This asymmetric pattern of effects associated with early joblessness is also borne out under local randomization. A move from jobless category 1 to 2 contributes to an additional 11 months of future jobless exposures, while a move from category 4 to 5 adds five and a half additional months to future joblessness. By comparison, a move from category 2 to 3, or from category 3 to 4, only adds an additional two months to future joblessness. The effects of increases in early joblessness thus seem to matter more at the extremes of the first year period.

XIII. Conclusions

This paper has analyzed the determinants of joblessness among a sample of young workers in Sri Lanka over the period from 1999-2005. It documents strong evidence in favor of the "scarring" hypothesis. Individuals who experience early joblessness are disproportionately more likely to experience further joblessness in the future. The evidence suggests that there are large negative effects associated with fairly small periods of early post-schooling joblessness.

Moreover, the gap in future levels of joblessness tends to rise with the extent of early jobless exposure.

These results are found to be robust to the use of different methods for modeling the duration of joblessness. In this paper we have relied on the use of three sets of methods, starting with the highly parametric fractional logit model. We then relaxed the assumptions of this model by introducing a semi-parametric method (sub-classification and matching) as well as a non-parametric method (local randomization). All of the adopted methods imply large gains to avoiding early spells of joblessness.

Scarring could be the result of any number of factors. However, the results are consistent with the hypothesis that a major dimension of scarring appears to be the result of employer sorting of workers on the basis prior joblessness, which is perceived as a strong signal of expected productivity. Since the costs associated with joblessness are much higher than the immediate loss of earnings, policies to reduce early joblessness could have large long-run effects on the standards of living of young workers.

The results also suggest that a number of observable characteristics increase the duration of time spent in joblessness. These include poor educational attainment, gender, the presence of young children in the household, depressed local labor markets and low levels of ability.

The magnitude of the scarring effect provides a strong justification for early interventions to combat youth joblessness. The results suggest that multiple interventions may be required to prevent long term joblessness. Policies may be required to not just prevent the buildup of substantial periods of early joblessness, but also to address low levels of educational attainment as well as any special problems that women may face in making the transition to employment.

XIV. References

Addison J. T. and Portugal P. (2003) 'Unemployment Duration: Competing and Defective Risks', *Journal of Human Resources*, 38(1): 156-191.

Aranki T. N. and Daoud Y. (2006) 'Unemployment Duration in the Palestinian Territories: An Analysis of Personal and Political Factors', in T. N. Aranki, Wages Unemployment and Regional Differences, PhD Thesis, Orebro, Sweden: Orebro University.

Arulampalam, W., Booth, A. and Taylor, M. (2000). 'Unemployment persistence', *Oxford Economic Papers*, 52: (24-50).

ArulampalamW. and Stewart M. B. (1995) 'The Determinants of Individual Unemployment Durations in an Era of High Unemployment', *The Economic Journal*, 105: 321-332.

Bheim R. and Taylor M. P. (2000) 'Unemployment Duration and Exit States in Britain', Institute for Social and Economic Research, Discussion Paper No. , Essex, Colchester: University of Essex.

Brandolini A., Cipollone P. and Viviano E. (2006) 'Does the ILO Definition Capture All Unemployment?', *Journal of the European Economic Association*, 4(1): 153-179.

Brookhart, M., Schneeweiss, S., Rothman, K., Glynn, R., Avorn, J. and Sturmer, T. (2006) 'Variable selection for propensity score models.' *American Journal of Epidemiology*163: 1149-1156.

Byrne, D. and Strobl E. (2004) 'Defining Unemployment in Developing Countries: Evidence from Trinidad and Tobago', *Journal of Development Economics*, 73: 465-476.

Clark K. B. and Summers L. H. (1979) 'Labor Market Dynamics and Unemployment: A Reconsideration', *Brookings Papers on Economic Activity*, 1979(1): 13-72.

Clark K. B. and Summers L. H. (1982) ‘The Dynamics of Youth Unemployment’ in *The Youth Labor Market Problems: Its Nature, Causes and Consequences*, R. Freeman and D. Wise (ed.), Chicago: University of Chicago Press for National Bureau of Economic Research.(pp. 199-234).

Dendir S. (2006) ‘Unemployment Duration in Poor Developing Economies: Evidence From Urban Ethiopia’, *The Journal of Developing Areas*, 40(1): 181-201.

Devine T. J. and Kiefer N. M. (1991) *Empirical Labor Economics*, Oxford: Oxford University Press.

DickensW.T. and Lang K. (1996) ‘An Analysis of the Nature of Unemployment in Sri Lanka’, *Journal of Development Studies*, 31: 620-36.

Earle J. S. and Pauma C. (1996) ‘Incidence and Duration of Unemployment in Romania’, *European Economic Review*, 40(3-5): 829-837.

Ellwood, David T. (1982) “Teenage Unemployment: Permanent Scars or Temporary Blemishes? In *The Youth Labour Market Problem: its Nature, Causes and Consequences*. Richard B.Freeman and David A.Wise, eds. Chicago: U. of Chicago Press/NBER, pp.349-85.

Filmer, D. and Pritchett L. (2001) Estimating wealth effects without expenditure data - Or tears: An application to educational enrollments in states of India. *Demography*, 381:115-132.

Flinn C. J. and Heckman J. J. (1983) ‘Are unemployment and out of the Labor Force Behaviorally Distinct Labor Force States?’ *Journal of Labor Economics*, 1: 28-42.

Foley M. C. (1997) ‘Labor Market Dynamics in Russia’, Economic Growth Center. Discussion Paper no. 780, New Haven: Yale University.

Garrido L. and Toharia L. (2004) What does it take to be (counted as) unemployed: The case of Spain, *Labour Economics*, 11(4): 507523.

Gonul F. (1992) New Evidence on Whether Unemployment and out of the Labor Force are Distinct States, *Journal of Human Resources*, 27(2): 329-361.

Gregg P. (2000) 'The Impact of Youth Unemployment on Adult Unemployment in the NCDS', *The Economic Journal*, 111 (November): P626-P653.

Grogan L. and van den Berg G. J. (2001) 'The Duration of Unemployment in Russia', *Journal of Population Economics*, 14(3): 549-568.

Heckman J. (1991). 'Identifying the Hand Of The Past: Distinguishing State Dependence from Heterogeneity', *American Economic Review*, 81(2): 75:79.

Heckman J. and Borgas G. (1980). 'Does Unemployment Cause Future Unemployment? Definitions, Questions and Answers from a Continuous Time Model of Heterogeneity and State Dependence', *Economica*, 47: 247:283.

Imbens G. (2000) 'The Role of the Propensity Score in Estimating Dose-Response Functions', *Biometrika*, 87: 706-710.

Imbens G. and Wooldrige J. (2009) 'Some Recent Developments in the Econometrics of Program Evaluation', *Journal of Economic Literature*, 47(1): 5-86.

Jones S. R. G. and Riddell W. C. (1999) 'The Measurement of Unemployment: An Empirical Approach', *Econometrica*, 67(1): 147-161.

Jones S. R. G. and Riddell W. C. (2006) 'Unemployment and Non-Employment: Heterogeneities in Labour Market States', *The Review of Economics and Statistics*, 88(2):314-323.

Joffe M. and Rosenbaum P. (1999) 'Propensity Scores', *American Journal of Epidemiology*, 150: 327- 333.

Kingdon G. and Knight J. (2000) ‘Are Searching and Non-Searching Unemployment Distinct States When Unemployment is High? The Case of South Africa’, The Center of African Economics Working Paper Series, No: 2000/2, Oxford: University of Oxford.

Knight J. and Li S. (2006) Unemployment Duration and Earnings of Re-employed Workers in Urban China, *China Economic Review*, 17: 103-119.

Kupets O. (2006) ‘Determinants of Unemployment Duration in Ukraine’, *Journal of Comparative Economics*, 34: 228-247.

Lancaster T. (1979). ‘Econometric Methods for Duration of Unemployment’, *Econometrica*, 47: 939- 56.

Lancaster T. (1990). The Econometric Analysis of Transition Data, Cambridge University Press, Cambridge.

Lee M.J. and Lee S. J. (2009) Sensitivity analysis of job-training effects on reemployment for Korean women, Empirical Economics, 41(3-5):925-934.

Lubyova M. and van Ours J. C. (1997) Unemployment Dynamics and the Restructuring of the Slovak Unemployment Benefit System, European Economic Review, 41(3-5): 925-934.

Lu B., Zanutto E., Hornik R.and Rosenbaum P. (2001) ‘Matching with Doses in an Observational Study of a Media Campaign Against Drug Abuse’, *Journal of the American Statistical Association*, 96(456): 1245-1253.

Luellen J., Shadish W. and Clark M. (2005) ‘PROPENSITY SCORES: An Introduction and Experimental Test’, *Evaluation Review*, 29(6): 530-558.

Machin S. and Manning C. (1999) ‘The Causes and Consequences of Long-term Unemployment in Europe’, in *Handbook of Labor Economics, 3c*, A. Orley and D. Card(eds.), Amsterdam: North Holland, (pp: 3085-3139).

Manski, C.F.(2003) *Partial identification of probability distributions*. Springer, New York.

McCullagh, P.(1980) 'Regression Models for Ordinal Data', *Journal of the Royal Statistical Society, Series B,* 42: 109-142.

Mroz T.A. and Savage T.H. (2006)"The Long-Term Effects of Youth Unemployment," *Journal of Human Resources*, University of Wisconsin Press, vol. 41(2)

Narendranathan, W. and Elias, P. (1993). 'Influences of past history on the incidence of youth unemployment: empirical findings for the UK', Oxford Bulletin of Economics and Statistics, 55(2): 161-185.

Narendranathan W. and Stewart M. B. (1993) 'Modelling the Probability of Leaving Unemployment: Competing Risks Models with Flexible Baseline Hazards', *Applied Statistics*, 40(1): 63-83.

Nickell S. (1979) 'Estimating the probability of leaving unemployment', *Econometrica*, 47: 1249- 1266.

Papke, L and Woolridge J. 1996. Econometric Methods for Fractional Response Variables with an Application to 401 (K) Plan Participation Rates. *Journal of Applied Econometrics*. 11: 619-632.

Portugal P. and Addison J. T. (2008) 'Six Ways to Leaving Unemployment', *Scottish Journal of Political Economy,* 55(4): 393-419.

Rama M. (1998) How Bad is Unemployment in Tunisia? Assessing Labor Market Efficiency in a Developing Country, World Bank Research Observer, 13(1): 59-78.

Rama, M. (2003) 'The Sri Lankan Unemployment Problem Revisited', *Review of Development Economics*, 7: 510-525.

Ridder, G. (1987). 'The sensitivity of duration models to misspecified unobserved individual heterogeneity and duration dependence'. Unpublished paper, University of Amsterdam.

Rosenbaum, P. (2010). '*Design of Observational Studies*'. Springer.

Rosenbaum, P. and Rubin D. (1983) 'The Central Role of the Propensity Score in Observational Studies for Causal Effects', *Biometrika,* 70: 41-55.

Rosenbaum, P. and Rubin, D. (1985) 'Constructing a control group using multivariate matched sampling methods that incorporate the propensity score.' *Amer. Statistician* 39: 3338.

Russell, H. and O'Connell P. (2001) 'Getting a Job in Europe: The Transition from Unemployment to Work among Young People in Nine European Countries', *Work, Employment and Society*, 15: 001- 024.

Ryan, P. (2001). 'The school-to-work transition: a cross-national perspective', *Journal of Economic Literature*, 39, 3492.

Schweitzer M. (2003) 'Ready, Willing, and Able? Measuring Labour Availability in the UK.' Cleveland: Federal Reserve Bank of Cleveland Working Paper No. 03-03.

Serneels, P. (2007) 'The Nature of Unemployment among Young Men in Urban Ethiopia', *Review of Development Economics,* 11(1): 170-186.

Steiner, V. (2001) 'Unemployment Persistence in the West German Labor Market: Negative Duration Dependence or Sorting', *Oxford Bulletin of Economics and Statistics,* 63: 91-114.

Stuart, E. (2010) 'Matching Methods for Causal Inference: A Review and a Look Forward', *Statistical Science*, 25(1): 1-21.

Suryadarma D., Suryahadi A. and Sumarto S. (2007) ‘Measuring Unemployment in Developing Countries: The Case of Indonesia’, *Labour: Review of Labor Economics and Industrial Relations*, 21(3): 541-562.

Tansel A. and Tasi H. M. (2010) ‘Hazard Analysis of Unemployment Duration by Gender in a Developing Country: The Case of Turkey’, Institute for Study of Labor (IZA), Discussion Paper No: 4844, Bonn:IZA.

Tunali and Assaad R. (1992) ‘Market Structure and Spells of Employment and Unemployment: Evidence from the Construction Sector in Egypt’, *Journal of Applied Econometrics*, 7: 339-367.

Van den Berg, G. and Van Ours, J.(1994) ‘Unemployment dynamics and duration dependence in

France, The Netherlands and the United Kingdom’ *The Economic Journal* 104: 432443.

Van den Berg G. J. and Van Ours J. C. (1999) Duration Dependence and Heterogeneity in French Youth Unemployment Durations, *Journal of Population Economics*, 12(2): 273-285.

Zanutto E., Lu B. and Rosenbaum P. (2005) ‘Using Propensity Score Subclassification for Multiple Treatment Doses to Evaluate a National Antidrug Media Campaign’, *Journal of Educational and Behavioral Statistics*, 96(456): 1245-1253.

The World Bank. *Treasures of the Education System in Sri Lanka: Restoring Performance, Expanding Opportunities and Enhancing Prospects*. Washington, D.C. 2005

Table 1: Description of Variables Entering the Fractional Logit Model

Variable	**Description**
Outcome	
Fraction of Time Spent Jobless *After* Year 1	Takes a value in [0, 1]
Covariates	
Fraction of Time Spent Jobless *In* Year 1	Takes a value in [0, 1]
Cohort	Year of formally leaving school
District i Dummy	=1 if residing in district i, with i=1,..,13; =0 otherwise
Training Dummy	=1 if enrolled in training program in year 1; =0 otherwise
Male Dummy	=1 if Male; =0 otherwise
O/L Dummy	Indicates whether individual passed the O-level exams
A/L Dummy	Indicates whether individual passed the A-level exams
Ability Score	Ability index created from scores on a English language and reasoning ability test
Household Wealth	Asset index computed from household asset ownership
Children Dummy	=1 if child under the age of 7 present in household; =0 otherwise
Elderly Dummy	=1 if individuals above the age of 60 are present in household; =0 otherwise

Table 2: Fractional Logit Model Results Using Joblessness as Outcome Measure

Covariates	Coefficient	Robust Standard Error	Marginal Effect
Proportion of Time Spent Jobless in Year 1	2.313***	0.186	0.536
Cohort 2000	0.118	0.203	0.027
Cohort 2002	-0.041	0.190	-0.009
Cohort 2003	0.086	0.194	0.020
Cohort 2004	0.201	0.257	0.046
Training Dummy	0.547***	0.186	0.127
Male Dummy	-0.712***	0.140	-0.165
O/L Dummy	-0.153	0.197	-0.035
A/L Dummy	-0.078	0.185	-0.018
Ability Score	-0.265	0.329	-0.061
Household Wealth	0.040	0.040	0.009
Children Dummy	0.336**	0.152	0.078
Elderly Dummy	-0.022	0.159	-0.005

Table 2 (Continued)

Note: (1) * denotes significance at the 10% level; ** denotes significance at the 5% level and *** denotes significance at the 1% level.

(2) Sample consists of 609 observations.

Table 3: Fractional Logit Model Results Using Unemployment as Outcome Measure

Covariates	Coefficient	Robust Standard Error	Marginal Effect
Proportion of Time Spent Unemployed in Year 1	2.631***	0.190	0.610
Cohort 2000	-0.099	0.233	-0.023
Cohort 2002	-0.091	0.212	-0.021
Cohort 2003	-0.113	0.216	-0.026
Cohort 2004	0.188	0.265	0.043
Training Dummy	-0.493***	0.192	-0.114
Male Dummy	-0.158	0.152	-0.037
O/L Dummy	0.066	0.200	0.015
A/L Dummy	-0.235	0.208	-0.054
Ability Score	-0.063	0.357	-0.014
Household Wealth	0.049	0.041	0.011
Children Dummy	-0.005	0.159	-0.001
Elderly Dummy	-0.045	0.181	-0.010

Table 3 (Continued)

Note: (1) * denotes significance at the 10% level; ** denotes significance at the 5% level and *** denotes significance at the 1% level.

(2) Sample consists of 609 observations.

Table 4: Illustrating the Method of Sub-classification on Balancing Score

Jobless Category						
		1	2	3	4	5
Quintiles of Balancing Score	1					
	2					
	3					
	4					
	5					

Table 5: Ordinal Logit Modeling of the Balancing Score

Covariates	**Coefficient**	**Standard Error**
Cohort 2001	0.631***	0.243
Cohort 2002	0.235	0.240
Cohort 2003	0.094	0.248
Cohort 2004	0.1482	0.284
Male Dummy	-0.484***	0.154
O/L Dummy	-0.245	0.224
A/L Dummy	-0.261	0.202
Ability Score	-0.035	0.357
Household Wealth	0.076*	0.043
Children Dummy	0.118	0.224
Elderly Dummy	-0.236	0.172

Note: * denotes significance at the 10% level; ** denotes significance at the 5% level and *** denotes significance at the 1% level.

Table 6: Sub-classification Results

Jobless Category						
		1	2	3	4	5
Quintiles of Balancing Score	1	5.76	21.55	11.91	15.35	26.0
	2	1.97	12.41	21.5	13.84	27.36
	3	4.86	27.23	13.7	16.5	28.61
	4	4.25	8.64	19.5	24.14	23.75
	5	6.66	14.92	9.25	23.2	34.47

Note: Cell numbers refer to the months spent in a jobless state after the first year of labor market exposure

.

Table 7: Joint Distribution of Jobless Categories in Matched Pairs

Jobless Category	**1**	**2**	**3**	**4**	**5**	**Total**
1	0	0	0	0	0	0
2	18	0	0	0	0	18
3	23	7	0	0	0	30
4	32	10	7	0	0	49
5	91	35	45	27	0	198
Total	164	52	52	27	0	295

Notes: (1) Each count within the table represents a pair of individuals.

(2) Rows represent individuals in high jobless categories, while columns represent individuals in low jobless categories.

Table 8: Outcome Differences across Matched Pairs

	Difference=1	**Difference=2**	**Difference=3**	**Difference=4**	**All**
Minimum	-33	-50	-33	-53	-53
Quartile 1	0	0	0	10	0
Median	12	15	11	22	16
Quartile 3	29.5	24	27.5	39.5	30.5
Maximum	68	65	60	66	68
Mean	**12.8**	**10.3**	**13.8**	**24.1**	**15.8**
Pairs	59	78	67	91	295

Notes: (1) Results represent the quantiles of 295 matched pair differences in outcomes for individuals in high and low jobless categories. A positive difference in a pair indicates that individuals exposed to higher levels of early joblessness spend more time in joblessness after year 1.

(2) Columns capture the extent of differences in early joblessness among matched pairs. For example, Difference =1 groups together pairs for which the difference between early jobless exposure is only 1 level.

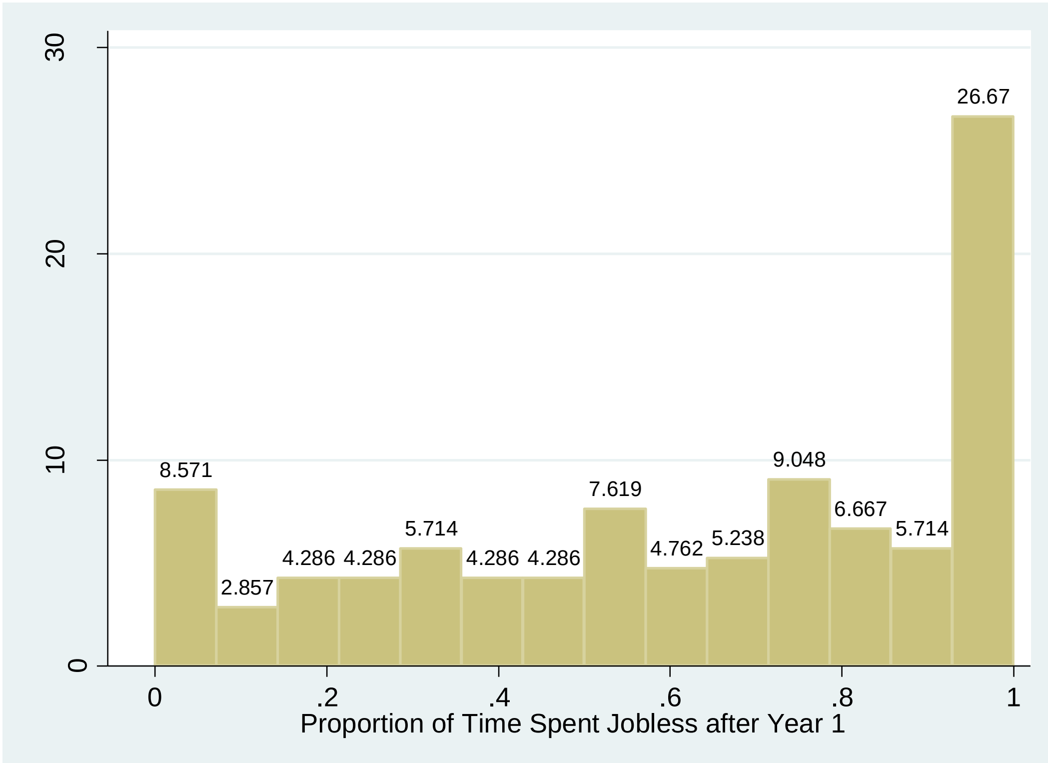
Table 9: Local Randomization Results

Jobless Categories Modeled	**Increase in Average Jobless Durations**
Jobless Category 2 - Jobless Category 1	11.25
Jobless Category 3 - Jobless Category 2	1.84
Jobless Category 4 - Jobless Category 3	2.50
Jobless Category 5 - Jobless Category 4	5.60

Note: (1) Average jobless duration refers to the average months spent in joblessness in years 2-6.

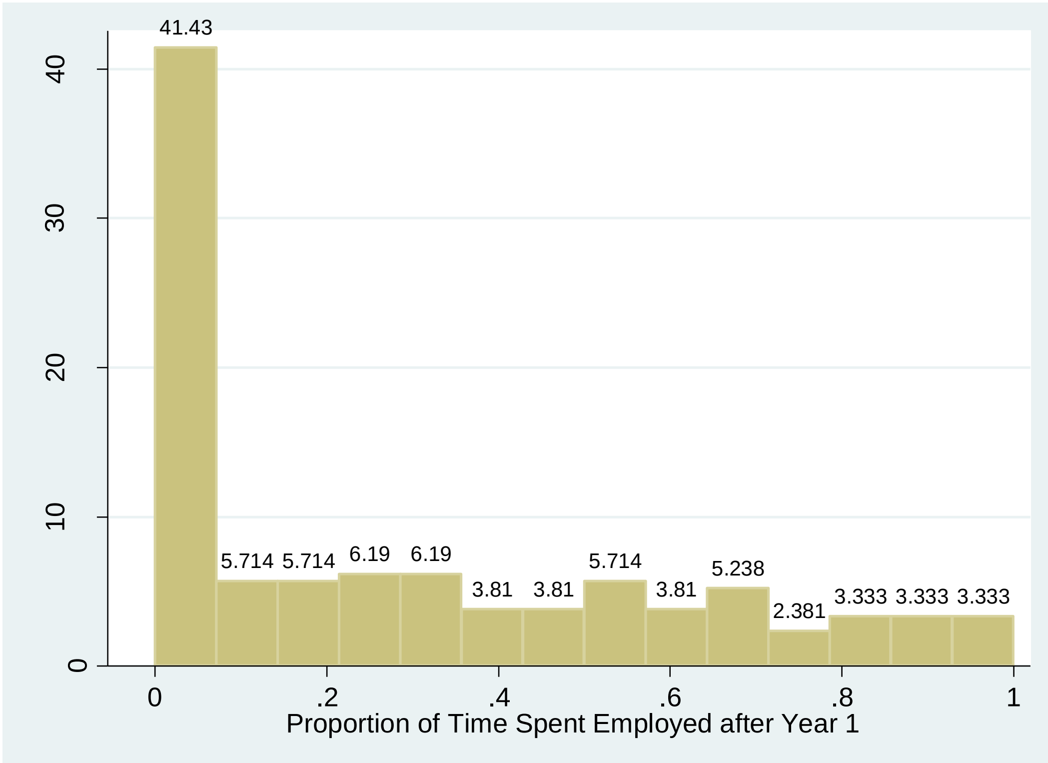
(2) Results above are from four sets of regressions, with all individuals from two adjacent jobless categories constituting the sample for each regression.

Figure 1: Subsample of individuals who spend all of year 1 in the jobless state



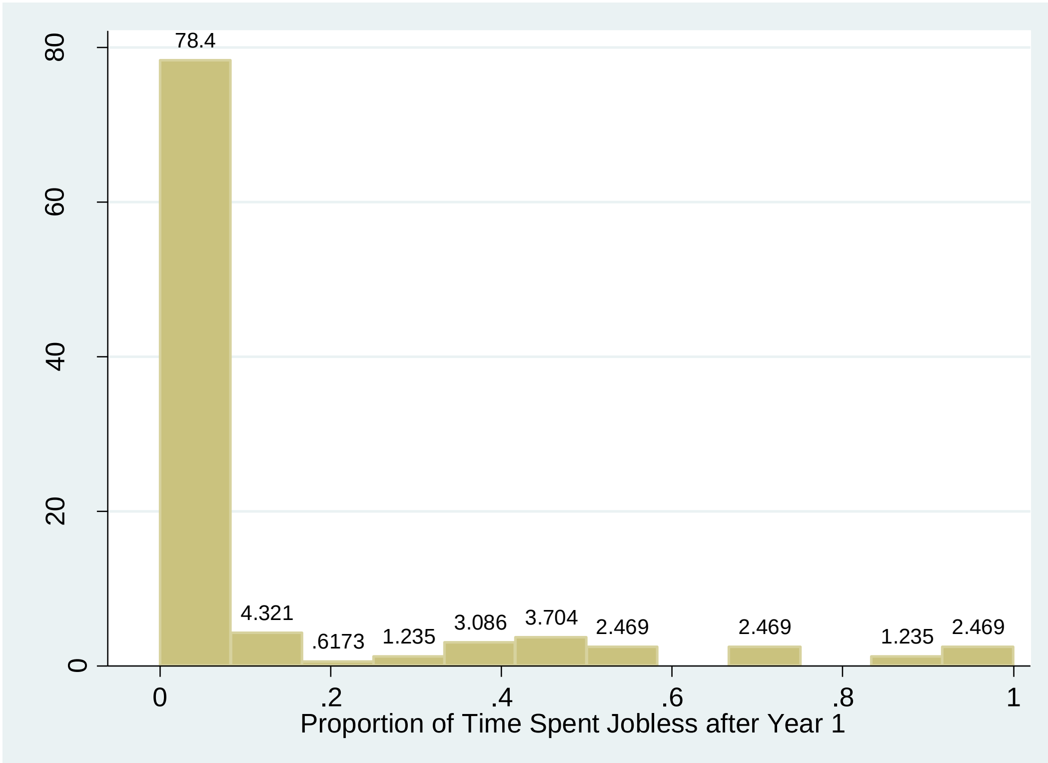
Note: The top of each bar is labeled with the fraction of the sub-sample represented by that bar.

Figure 2: Subsample of individuals who spend all of year 1 in the jobless state



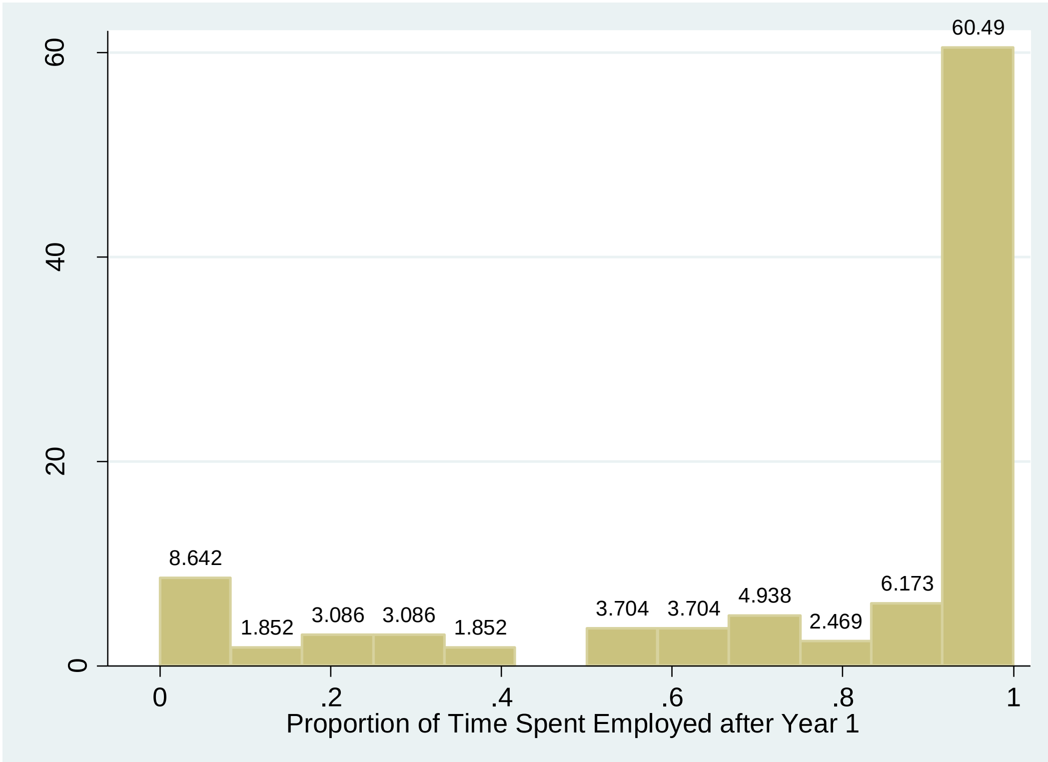
Note: The top of each bar is labeled with the fraction of the sub-sample represented by that bar.

Figure 3: Subsample of individuals experiencing no joblessness in year 1



Note: The top of each bar is labeled with the fraction of the sub-sample represented by that bar.

Figure 4: Subsample of individuals experiencing no joblessness in year 1



Note: The top of each bar is labeled with the fraction of the sub-sample represented by that bar.

Figure 5: Histogram Depicting the Months Spent in Joblessness in Year 1

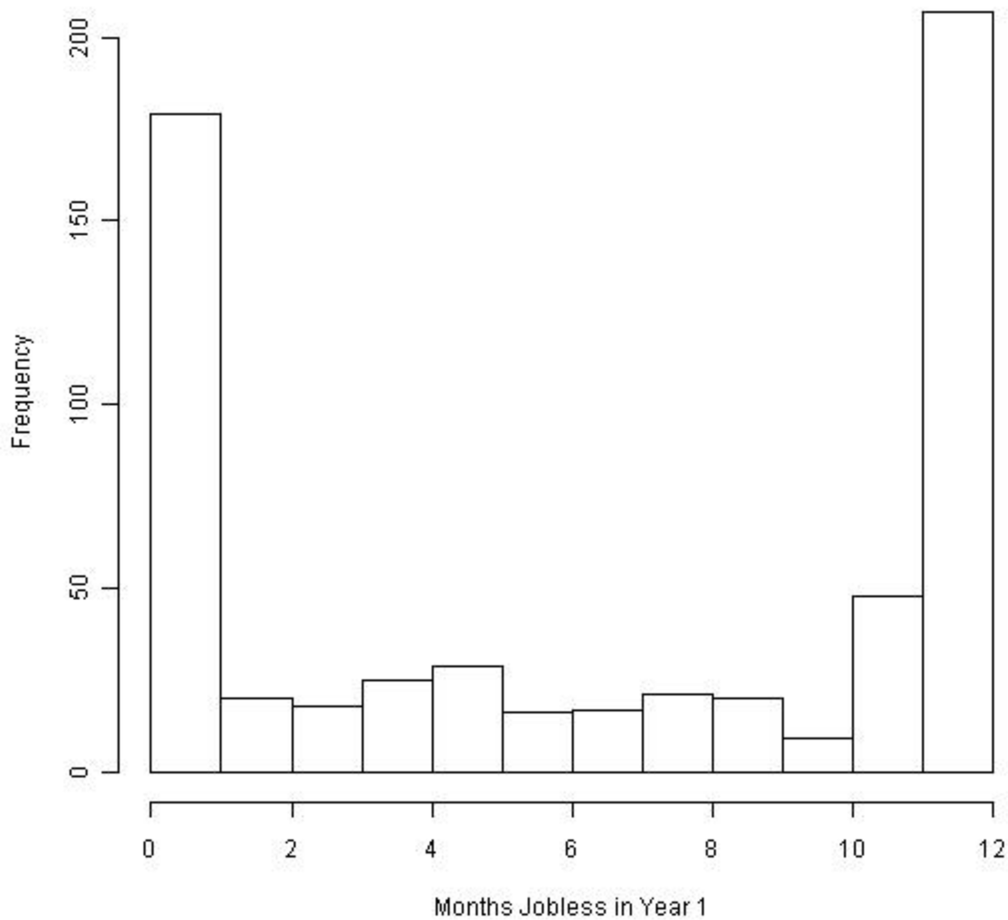


Figure 6: Histogram Depicting Categories of Joblessness in Year 1

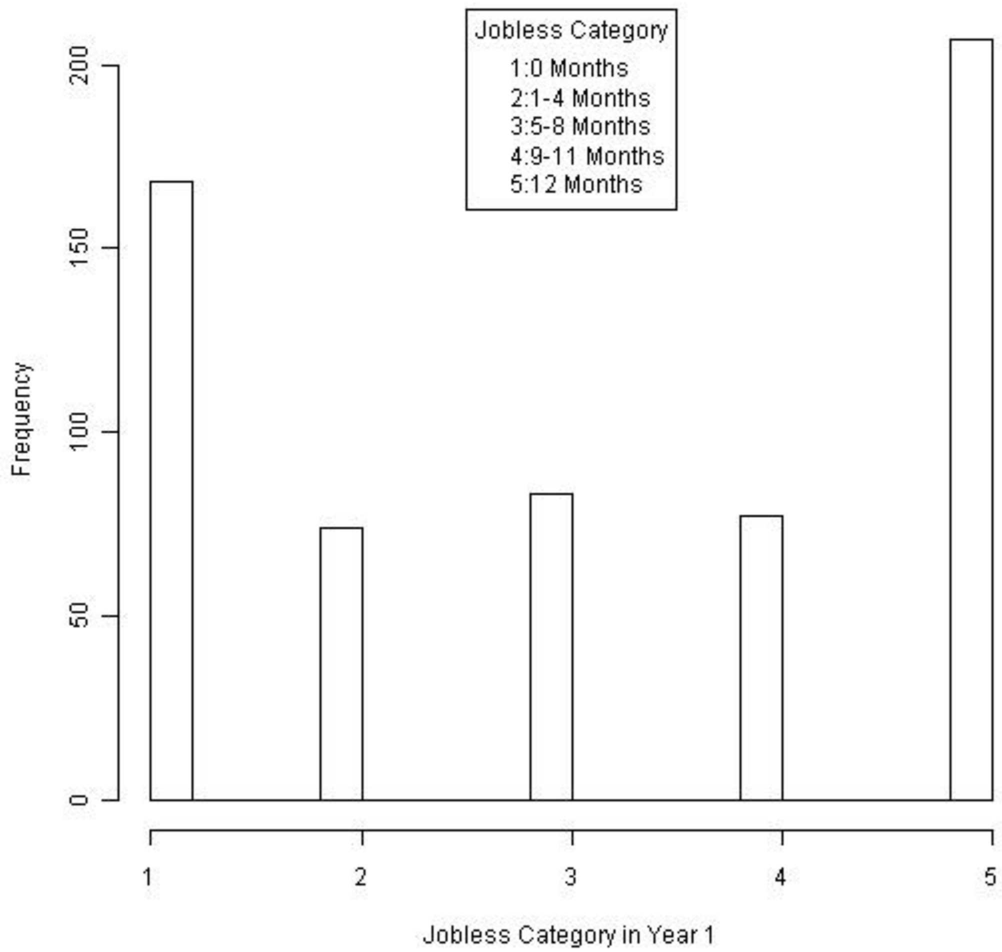
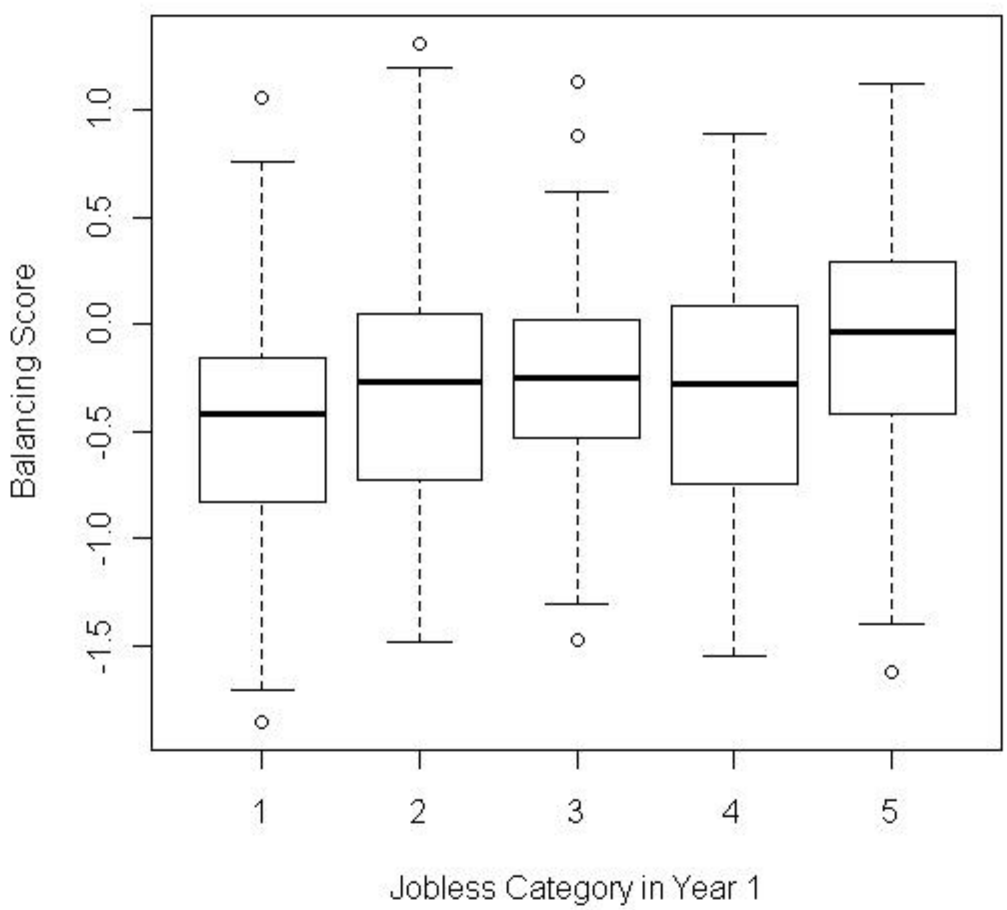
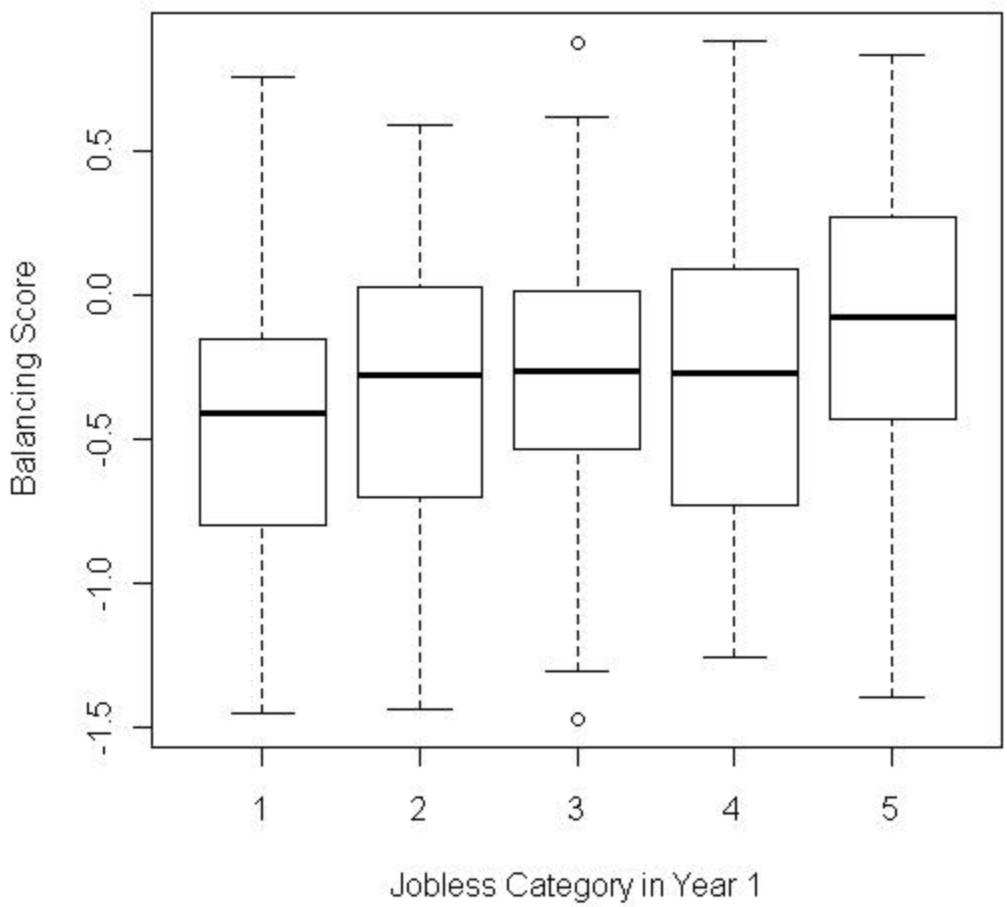


Figure 7: Distribution of Estimated Balancing Scores across Jobless Categories



Note: Sample consists of 609 observations. Balancing scores lie between -1.855 and 1.309.

Figure 8: Distribution of Estimated Balancing Scores with Common Support



Note: Imposing common support leads to the dropping of 18 observations. Balancing scores now lie between -1.47 and 0.886.

CHAPTER 5. CONCLUSION

The paucity of data on Sri Lanka has generally limited the ability to assess the choke points in the school-to-work transition process. Also lacking is the presence of an evidence based approach to youth employment policy. Knowledge about youth employment outcomes in Sri Lanka is rare; this study is one of only a handful of additional studies that have been able to obtain labor market data for young people in Sri Lanka. More needs to be done to support the collection of such data, so that rigorous monitoring and evaluation of youth based projects can be undertaken.

A key focus area of this dissertation has been on assessing whether one component of active labor market programs -vocational and on-the-job training programs- have worked in terms of raising employment prospects and/or wages for participants. However, such a narrow focus on training tends to obscure the broader interactions among the various elements of ALMPs that that are essential to improving youth employment outcomes.

In practice, the success or failure of such programs often depends less on the type of intervention than on the details of the implementation. For example, Betcherman et. al (2007)-in their cross-country meta-analysis of what ALMPs work best in their objective to promote youth employment -have shown that program success is seldom driven by the type of intervention involved. Instead, the key driver of the success is whether or not the intervention in question is appropriate to the problem being tackled and how it is designed.

Beyond this, complementary services (such as job search and placement assistance) can often enhance the value of isolated training interventions. A meta-evaluation of 345 training programs by Fares and Puerto (2010) finds that comprehensive training programs-which incorporate - are more effective than simple classroom instruction only programs.

Our impact assessment of training is thus incomplete because our study lacks information on participants' access to such complementary services. Future work on Sri Lanka should strive towards examining how the entire bundle of employment enhancing services affects the labor market outcomes of youth.

Also absent from this study is any consideration of the labor market regulations within which training programs are implemented in Sri Lanka. Labor legislation in South Asia is strict. Strict legislation has mostly entailed protecting the small cadre of formal sector workers who are already employed from competition from young workers clamoring to enter this sector. Of course, on the surface such strict regulations should reduce the impact of any youth employment enhancing initiatives, such as training. In practice though, it is unclear whether these regulations have influenced youth employment much, since the vast majority of job creation in the South Asian region in the past decade has been in the informal sector, where such rules tend to be less rigorously enforced. How regulation affects job creation is essentially an empirical question that needs to be settled with data. Our survey did not allow for the collection of such information. Future work should strive to fill this gap through more detailed data collection.