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The Scarring Effects of Youth Joblessness in Sri Lanka

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Abstract
Retrospective data on labor market spells for successive cohorts of school leavers in Sri Lanka are used to examine whether early spells of joblessness lead to subsequent difficulty in finding or keeping a job. A matching method based on the Joffee and Rosenbaum (1999) balancing score approach is used to generate pairs of school leavers that have similar expected levels of joblessness but that differ in realized levels of joblessness. Assuming that youth are not able to perfectly control whether they are employed or not employed, we argue that marginal differences in joblessness between otherwise observationally equivalent youth can be viewed similarly to a regression discontinuity in experienced joblessness. We find evidence of scarring in that spending the first year after leaving school without a job or training increases subsequent time spent jobless by between 11 to 16%.

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JEL: J64; C41; O17

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I. Introduction

Youth unemployment rates are over 3 times higher than overall unemployment rates in Sri Lanka (World Bank, 2013). Rama (2003) has argued that the youth unemployment is largely voluntary due to considerable rents earned in public sector jobs. Consequently, youth queue up for these choice jobs rather than taking less attractive jobs. However, it is not obvious that such a strategy is uniformly optimal for income maximization. In Europe and North America, persistent wage and employment penalties have been linked to spells of unemployment at the time of school leaving. If the same holds for Sri Lankan youth, remaining jobless while waiting for a job opening in the public sector may come at a considerable cost in future earnings and employment. This study investigates whether such a link between early periods of joblessness and later labor market success exists for youth in Sri Lanka.

A first look at the data on cohorts of school leavers in Sri Lanka suggests that there may be such a link between early career and subsequent unemployment. 'Scarring' is the term used to describe when an individual who has been jobless in the past is more likely to suffer from adverse labor market experiences in the future. Figures 1-4 illustrate the data we will use to test for the persistence in joblessness among Sri Lankan youth.[1] The figures display time use patterns for 609 young workers for a period of up to six years after the completion of schooling. Figures 1 and 2 show the fraction of time spent jobless or employed for individuals who spend all of their first year jobless after leaving school Figures 3 and 4 replicate these figures for individuals who were employed all of their first year after leaving school. Figure 1 shows that the median jobless youth in year 1 spends 60% of their subsequent years jobless and 27% spend the entire time jobless. Figure 2 shows that the median time spent employed for this group is less than 15%. Figures 3-4 shows that 78% of youth who spent their first year employed

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

experienced less than 10% of their subsequent years jobless and 60.5% are always employed during those years.

The persistence of unemployment in subsequent years seems inconsistent with individuals who are using unemployment strategically to enter more stable and better paying employment. Instead, it appears that initial joblessness leads to less successful attachment to the labor market. This pattern holds even after we eliminate individuals who are never employed in the years after leaving school and are presumably not making career employment choices.

There are two main explanations for this pattern of persistence in employment or joblessness. The first is negative duration dependence, the statistical measure of the scarring effect. If human capital depreciates due to idleness, then the individual's productivity will fall as the length of the jobless spell increases. Consequently, the individual's attractiveness as a potential employee declines as the spell lengthens. The second explanation is state dependence. If the initial jobless spell may reflect the individuals unobserved ability or ambition. Prospective employers may use the duration of an individual's previous jobless spell as a signal of this unobserved productivity. Longer jobless spells signal greater problems with unobserved productivity.

This study distinguishes between the effects of duration dependence and state dependence using Joffee and Rosenbaum's (1999) balancing score approach. Paired school leavers with similar expected levels of joblessness but different realized levels of joblessness are used to measure the marginal effect of added nonemployment on subsequent employment. Assuming that youth are not able to perfectly control whether they are employed or not employed, we argue that marginal differences in joblessness between otherwise observationally equivalent youth can be viewed similarly to a regression discontinuity in experienced joblessness. We conclude that spending the first year after leaving school without a job or training increases subsequent time spent jobless by between 11 to 16%.

The next section presents a summary of past research on scarring in developed and developing countries. Sections 3-5 present the empirical strategy and describe the data. Sections 6-7 present the results of the analysis.

II. Literature Review

Most of the research on youth unemployment has focused on developed countries. This is due in part to the greater access to labor market data in developed countries. It also reflects the greater difficulty in measuring unemployment in developing country labor markets characterized by large informal sectors and heavy reliance on household production.

It is even more difficult to study the effects of jobless spells on subsequent employment in developing countries. Most longitudinal data on youth have been collected in Europe and the United States. The earliest studies of scarring generated mixed evidence on whether early spells of unemployment led to greater labor market difficulty later in life (Ryan (2001). Evidence on duration and state dependence was often fragile so that apparent negative duration dependence in the raw data for 12 out of 13 European countries disappeared when accounting for both observed and unobserved heterogeneity among workers (Machin and Manning, 1999). However, more recent studies in the U.S. (Mroz and Savage, 2006; Kahn, 2012; Kroft, Lange and Notowidigdo, 2013), Canada (Oreopoulos, Von Wachter, and Heisz, 2012), and the U.K. (Arulampalam, Gregg, and Gregory, 2001; Bell and Blanchflower, 2011; Cockx and Picchio, 2013) found that unemployment spells led to lower employment and labor market earnings later in life.

Evidence that unemployment spells lead to persistent unemployment later in life has also been found in the transition economies. Similar qualitative findings consistent with scarring has been found in Romania (Earle and Pauna, 1996); Russia (Grogan and van den Berg, 2001);

Ukraine (Kupets, 2006), the Slovak Republic (Lubyova and van Ours, 1999) and China (Knight and Li (2006). In these countries, unemployment persistence appears to be most severe for young and less educated men.

Less consistent results have been found for developing countries. Evidence of scarring has been found in Egypt (Tunali and Assaad, 1992), South Africa (Ismail and Kollamparambil, 2015), and Palestine (Aranki and Daoud, 2011). However, in Ethiopia (Dendir, 2006) and Turkey (Tansel and Taşçı, 2010), duration dependence had uneven effects on later employment.

A common concern with studies of reduced-form event history models such as the ones commonly used to evaluate duration dependence in unemployment is that unobserved heterogeneity is often confounded with true duration dependence. As Heckman (1991) demonstrated, studies that do not attempt to correct for unobserved heterogeneity are more likely to find spurious duration dependence in the data.

A further drawback of developing country studies that focus on the scarring effects of unemployment is the difficulty of distinguishing between unemployment, inactivity, household production and self-employment. Measurement error has proven confusing in assessing the extent of developing county unemployment.[2] For example, 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.

Our study makes several improvements on the methodologies previously employed to study youth joblessness in developing countries. First, we conduct our analysis using joblessness rather than unemployment to avoid artificially truncating jobless spells by movements in and out of the labor force. Second, we account for the presence of unobservable confounders that may bias

[2] See for examples, Rama (1998) for Tunisia, Serneels (2007) for Ethiopia, and Suryadarma, Suryahadi, and Sumarto (2007) for Indonesia.

estimates of scarring. We do this by matching individuals with the same expected unemployment risk and then examine how small differences in observed joblessness affects later labor market outcomes, a form of regression discontinuity which minimizes the bias from unobserved heterogeneity. 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.

III. Methodology

If individuals are randomly assigned into labor market states after leaving school, then realized jobless duration early in life can be used as an exogenous variable having a causal effect on later labor market outcomes. The estimated scarring effect would be the coefficient from a regression of subsequent jobless spells on initial labor market status upon leaving school. 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 scarring. Such self-selection can seriously bias the outcomes of observational studies.[3]

Propensity score methods[4] are used to obtain causal estimates in studies that involve non-random selection, assuming the sorting process is based on a known vector of observable factors. Conditioning on the vector of observed factors affecting initial joblessness, the remaining variation in joblessness is random. Consequently, if we match two individuals with the same predicted probability of being jobless at the time of school leaving based on their observed

[3] 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.”
[4] Propensity score methods originated with the seminal paper by Rosenbaum and Rubin (1983).

attributes, differences between them in the observed jobless or employed state must be based on random assignment.[5]

In our study, school leavers face different levels of exposure to joblessness. Some are jobless for the whole year while others experience shorter spells. For that reason, our matching must be based not on the extensive margin (predicted exposure to joblessness), but on the intensive margin (predicted length of the initial jobless spell). Joffee and Rosenbaum (1999) showed that a scalar balancing score performs a role similar to that of a propensity score in the case of varying intensity of a treatment.

Suppose that the length of time the i^{th} individual is jobless after leaving school is U_i^0. The length of that initial jobless spell ranges from 0 to 12 months. Let the length of the spell be defined by a vector of observables according to

$$(1) \qquad U_i^0 = \sum_{d=1}^{D} \theta_i^d + A_i'\beta_A + S_i'\beta_S + H_i'\beta_H + \varepsilon_i$$

where θ_i^d is a district-specific fixed effect that measures common factors influencing exposure to joblessness in the area such as the strength of the local labor market for youth and local quality of schooling; A_i includes measures of cognitive ability, S_i includes completed years of schooling, and H_i is a vector of household attributes that may affect the individual's nonmarket value of time.

We could use the individual recollection of the length of time spent jobless, but recall may be subject to errors. Individuals will be better able to approximate the length of time they spent jobless than to specify the duration exactly. To accommodate that measurement problem,

[5] Rosenbaum and Rubin (1983)

we apply McCullagh's ordinal logit model.[6] We divide our joblessness spell U_i^0 into 5 jobless intensity groups: 1: 0 months of joblessness; 2: 1-4 months of joblessness; 3: 5-8 months of joblessness; 4: 9-11 months of joblessness; and 5: all 12 months spent jobless. Our presumption is that it is straightforward to remember that one spent either no time or the full year jobless, and that the other jobless ranges can be reasonably accurately assessed. The jobless categories were sufficiently broad to ensure at least 70 individuals in each jobless category. 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.

Assuming the error terms in (1), ε_i, are logistically distributed, the parameters may be estimated using the specification

$$u_i^0 = 1 \;\; if \; \sum_{d=1}^{D} \theta_i^d + A_i'\beta_A + S_i'\beta_S + H_i'\beta_H \leq (\mu_1 - \varepsilon_i)$$

$$u_i^0 = j \;\; if \; (\mu_1 - \varepsilon_i) < \sum_{d=1}^{D} \theta_i^d + A_i'\beta_A + S_i'\beta_S + H_i'\beta_H < \left(\mu_j - \varepsilon_i\right); j = 2, 3, 4$$

$$u_i^0 = 5 \;\; if \; \sum_{d=1}^{D} \theta_i^d + A_i'\beta_A + S_i'\beta_S + H_i'\beta_H > (\mu_4 - \varepsilon_i)$$

where u_i^0 is the individual's joblessness groups *j*=1, 2, 3, 4, 5, and μ_j are threshold measures estimated simultaneously with the parameters. The model parameters generate an ordinal index of predicted jobless intensity

$$\widehat{U_i^0} = \sum_{d=1}^{D} \widehat{\theta_i^d} + A_i'\widehat{\beta_A} + S_i'\widehat{\beta_S} + H_i'\widehat{\beta_H} \;. \tag{2}$$

The values $\widehat{U_i^0}$ are the balancing scores. We construct matched pairs of individuals with similar values of the balancing score, but who differ in the proportion of their first year spent jobless. Scarring is measured by the proportion of time spent jobless in years 2-5 between the matched pairs. Details of this matching procedure, including a detailed description of the

[6] This is also known as the proportional odds logit model in the literature. The method satisfies our requirements that the balancing score be a scalar function of the observed covariates (Lu, Zanutto, Hornik and Rosenbaum, 2001).

matching algorithms used to construct the matches, are given in Lu, Zanutto, Hornik and Rosenbaum (2001).

An alternative to the pairwise matching is to match within subgroups. Individuals are allocated to one of a number of subclasses constructed from the balancing score. As shown by Rosenbaum and Rubin (1983), grouping individuals into subclasses removes some of the bias related to the sampling distribution of the parameters used in generating the balancing scores, a philosophy similar to our using grouped jobless durations. Our choice of five subclasses is based on Rosenbaum and Rubin's finding that five subclasses removes 90% of the bias in the estimated treatment effect associated with the paired matches.

IV. Data

Information on labor market spells is obtained from a survey of Sri Lankan households, administered by the University of Colombo with support from the World Bank. Data were collected on respondents who had formally left school between 2000 and 2006 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 with 1026 individuals from 450 different households. With the exception of the conflict-ridden provinces, it was ensured that the sample was nationally representative. The labor market histories varying between 24 - 78 months depending on the year and month of school leaving.

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 which means that we have to drop the last 2 school leaving cohorts in 2005 and 2006. 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 desire to participate in the work force. We are left with a sub-sample consisting of the 609 individuals on which to base our analysis.

We collapse the labor force status into three labor market states: 'employment' composed of wage employment and self-employment; 'nonemployment'composed of unemployment and inactive; and 'training. Many studies in the U.S. and elsewhere have found it hard to distinguish between unemployment and inactivity, especially for youth.[7]

Because the survey was conducted at the household level, it also includes information on parental/family characteristics includes household wealth, location and number household residents. Locational dummy variables will be used to control for local school quality and strength of the youth labor market. Household composition is indexed by dummy variables

[7] Many studies have tested whether unemployment and inactivity are distinct states. In the United States, Clark and Summers (1979) and Clark and Summers (1982) found that the two states were not different for teenagers. Gonul (1992) found evidence of differences for young women but not young men. Studies that found little evidence of differences between unemployment and inactivity in other countries 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.

indicating presence of children under 7 and adults over 60 in the household. The household wealth measure is a weighted average of household ownership of various assets and housing amenities including a car, television, computer, refrigerator, home ownership, floor area, number of rooms, toilet facilities, protected drinking water, and access to electricity. The weights were derived using the first principal component of the assets

Our measures of individual ability include completed schooling and ability measures. The schooling levels include passage of the exams for the O-Level (ordinary level roughly equivalent to completion of 11 grades) and A-Level (advanced level roughly equivalent to 13 grades). Ability is measured by a composite test scores on math and reasoning that were administered separately to each respondent. We also have information on the year of school leaving which we use to indicate the respondents' labor market entry cohort.

About 86% of the respondents in our sample have experienced at least one spell of non-employment in their first year out of school. Only 43% had an employed spell in the first year. The probability of experiencing an employment spell rises after the first year so that 70% of the oldest cohorts had been employed at some point since leaving school. As noted, we drop from the analysis those who are never employed or unemployed.

V. Balancing Score Specification and Covariate Balance Checks

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 (1). The F-statistics for the effects of Ability Score, Household Wealth, the educational levels, and the Male dummy variable were all significant at least at the 0.1 significance level. One out of the four cohort dummy variables and five of the district dummy variables were also significant. These variables were used as the covariates reported in table 1. There are significant differences in the intensity of joblessness across the 13 districts included in the sample. Men are less likely to experience extended periods of joblessness. More educated and more cognitively skilled youth are less likely to experience joblessness, although the individual coefficients are not statistically significant. Youth in households with young children, that do not have elderly members, or whose families are wealthier have longer periods of joblessness, but again, the individual parameters are not precisely estimated. The weak link between observables and employment or jobless states makes it easier to find matches between individuals with similar balancing scores but different initial labor market success.

Our balancing scores $\widehat{U_i^0}$ are based on predicted unemployment category using the parameters in table 1. We checked for extent of the overlap in the balancing scores across the five jobless categories. This is to verify the comparability of balancing scores for individuals with different jobless categories. The ideal is to have similar balancing scores across all 5 jobless categories so that observables are not driving the employment outcomes. Observations with outlier balancing scores were removed as unbalanced because they violated the "common support" assumption. The remaining individuals were subclassified into 5 roughly equally sized strata[8] based on their balancing score values. 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 support condition. It should be kept in mind that this restricts the

[8] 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.

inferences that we are able to draw to that sub-population represented by the region of common support.

VI. Matching Results

i. Paired Matching

The 591 remaining observations in our sample were 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 2A. 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. The difference in jobless category was 1 for 20% of the pairs, 2 categories for 26.4% of the pairs, 3 categories for 22.7% of the pairs, and 4 categories for 30% of the pairs.

Results for matched pair differences in joblessness after year 1 are displayed in Table 2B. 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 averaged about 16 months. The finding supports the existence of scarring from early joblessness.

ii. Subclassification on the Balancing Score

The results from subclassification on the balancing score are presented in Table 3. All the individuals in our sample are placed into one of the 25 cells in the table, depending on their observed jobless category in their first post-schooling year and the quintile 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, average joblessness tends to increase as we move from lower to higher levels of initial joblessness. For the lowest balancing score quintile, joblessness rises from an average of 5.8 months to around 26 months as we move from jobless category 1 to jobless category 5. For 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. Early joblessness is highly correlated with the amount of joblessness experienced in the future, consistent with the existence of scarring.

These results will be biased upward if the initial jobless state is based unobserved heterogeneity. For example, it seems plausible that the individuals who experience no joblessness in their first year after leaving school are different than the individuals who spend at least some time jobless. The unobserved factors that lead to experiencing no jobless spell are likely to affect later joblessness and bias upward our estimated effect of early joblessness. As an example, the estimated difference in mean joblessness between the individuals who experience 0 months of early joblessness and those who experience just 1-4 months of early joblessness is very large, with subsequent joblessness 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, suggesting that there may be a difference in unobservable factors affecting joblessness between those who spent at least some months of their first year employed compared to those who spent their entire first year idle. For that reason, we propose an alternative estimate that is less likely to confuse duration dependence and state dependence.

VII. Comparison of Pairs with Small Differences in Initial Joblessness

Individuals are unlikely to have precise control over the time spent employed in their first year. While they can control their intensity of job search and their reservation wage, they still have to match with an employer whose decisions are not full predictable. As a result, 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, the hiring plans of local employers, variation in the distribution of wage offers, or local training availability and timing of offerings. Lack of perfect control over the hiring process means that there will be some randomness in the number of months spent jobless at the margin. But, while the youth may not be able to control placement into jobless category 2 versus 3, he would likely be able to control being always employed (category 1) and being always jobless (category 5). That suggests that the best evidence for scarring would come in examining differences in subsequent joblessness for comparable individuals in categories 2, 3 or 4. In this way, changes in realized joblessness for individuals with similar predicted balancing scores can be viewed as a form of regression discontinuity.[9]

[9] Lee and Lemieux (2010) review regression discontinuity designs.

For individuals populating every adjacent set of jobless categories, we ran regressions including all of the variables used to estimate the balancing score in equation (1) plus a dummy variable indicating jobless category. Conditioning on the baseline covariates is the same as estimating the effect of a small change in jobless category, holding fixed the individual's balancing score.

The results from this local randomization strategy are presented in Table 4. Four sets of regressions are run, one each for individuals from jobless categories 1 and 2; 2 and 3; 3 and 4; and 4 and 5. Moving from jobless category 1 to category 2 increases the exposure to future joblessness by around 11 months. A move from category 2 to 3 increases future joblessness by 1.8 months, from category 3 to 4 by 2.5 months, and from category 4 to 5 by 5.6 months. We presume that the first and last estimate are the most clouded by state dependence related to unobserved productivity or tastes. The middle two estimates are closest to the regression discontinuity approach which would yield the scarring effect. A one jobless classification increase implies 4 months of added joblessness on average. The implied effect of a 4-month spell of joblessness in the first year after leaving school is having 3.8% to 5.2% more time spent jobless over the next 4 years, while 12 months of joblessness increases subsequent idleness by 11-16%.

VIII. Conclusions

This study analyzed the determinants of joblessness among a sample of Sri Lankan youth who left school between 2000 and 2004. We show that there is a strong link between joblessness in the first year after leaving school and subsequent joblessness over the next 4 years. While some of the linkage relates to state dependence from unobserved heterogeneity in individual productivity or attitudes toward work, we also find significant evidence in favor of the “scarring” hypothesis. The implied scarring effect is between 11-16% greater joblessness in years 2-5 from an initial year of joblessness upon leaving school. Individuals who experience early joblessness are disproportionately more likely to experience further joblessness in the future. The findings suggest that if youth strategically choose joblessness to queue for public sector jobs, they pay a price in the form of more discontinuous employment later in their young work career. Moreover, we find evidence that young workers cannot perfectly control the extent of their employment, and so some of the joblessness in the first year out of school is outside the control of the individual. Hence, the scarring effect represents an inefficiency in the labor market that would justify policies aimed at smoothing the school to work transition.

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Table 1: 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. Regressions also included controls for district fixed effects.

Table 2A: Joint Distribution of Jobless Categories in Matched Pairs

	Jobless category of the less jobless pair member					
Jobless Category of the more jobless pair member	**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: Each count within the table represents a pair of individuals.
Jobless Categories 1: 0 months of joblessness; 2: 1-4 months of joblessness; 3: 5-8 months of joblessness; 4: 9-11 months of joblessness; and 5: all 12 months spent jobless.

Table 2B: Differences in Months Spent Jobless across Matched Pairs, by Differences in Jobless Categories between the Pair

	Difference in Jobless Category				
	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 3: Average of Months Spent Jobless by Jobless Balancing Score Quintile and Observed Classification of Initial Joblessness Subclass

		Observed Initial 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
Jobless Categories 1: 0 months of joblessness; 2: 1-4 months of joblessness; 3: 5-8 months of joblessness; 4: 9-11 months of joblessness; and 5: all 12 months spent jobless.

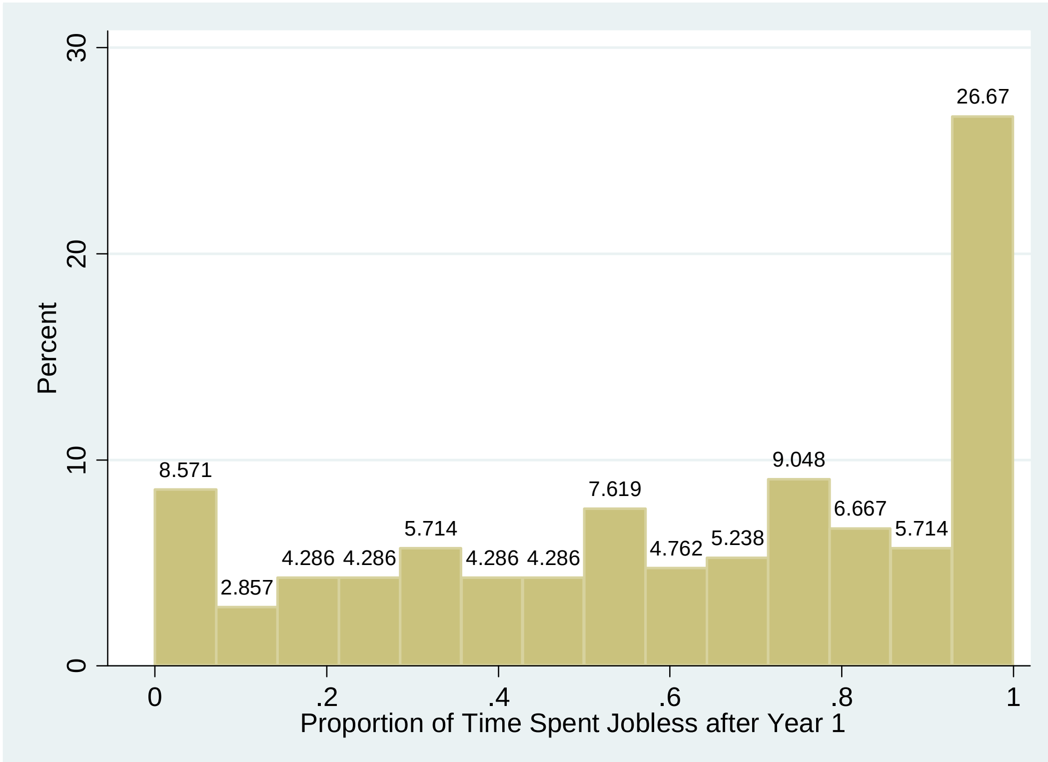
.

Table 4: Changes in Months of Joblessness Associated with Small Changes in Jobless Category for Individuals with Comparable 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

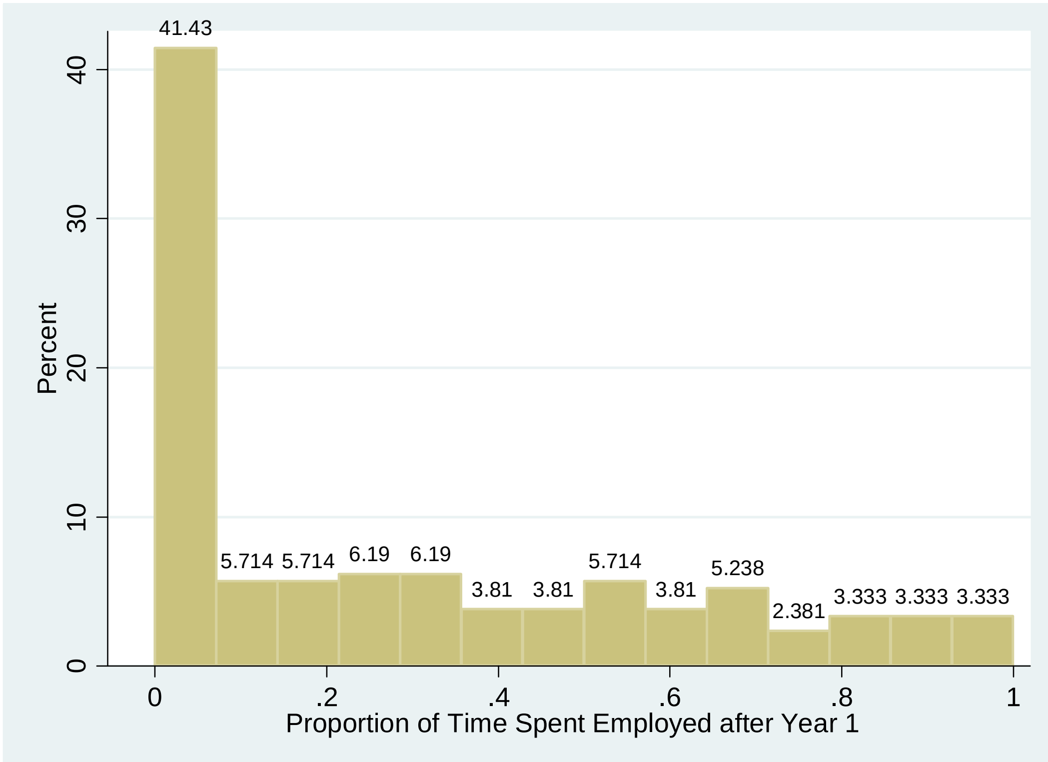
Note: (1) Average jobless duration refers to the average months spent in joblessness in years 2-6.
(2) Results above are from four sets of regressions, with all individuals from two adjacent jobless categories constituting the sample for each regression.

Figure 1: Fraction of time spent jobless for youth whose first year was spent jobless



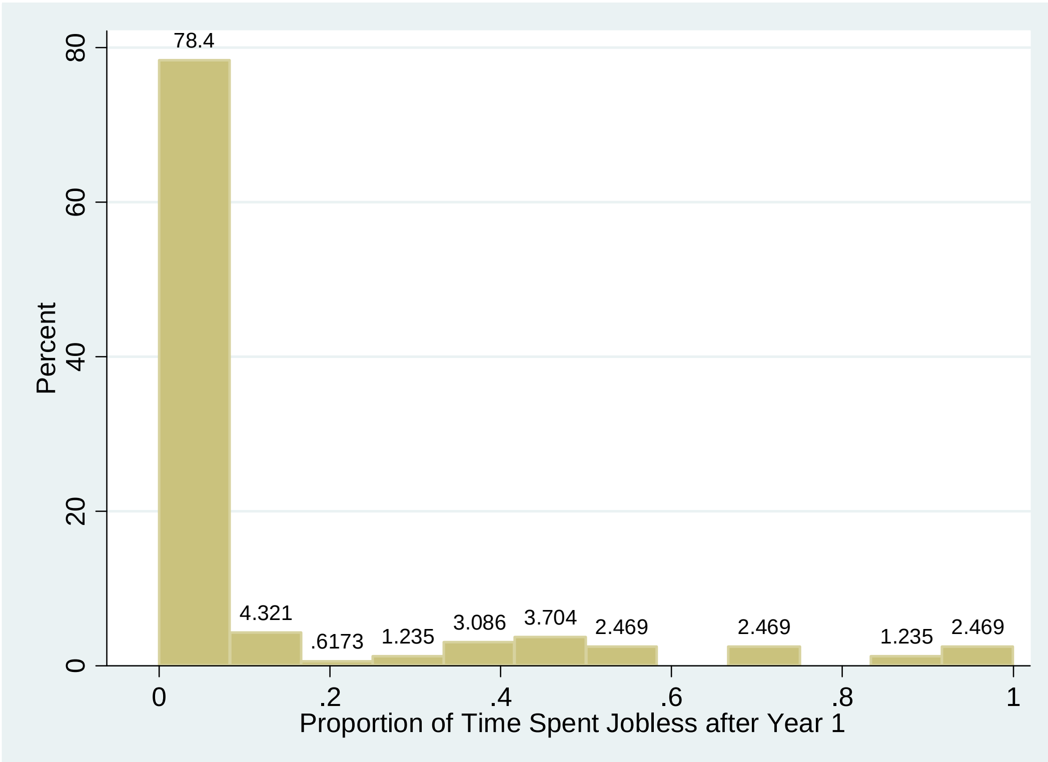
Note: The top of each bar is labeled with the fraction of the sub-sample represented by that bar.

Figure 2: Fraction of time spent employed for youth whose first year was spent jobless



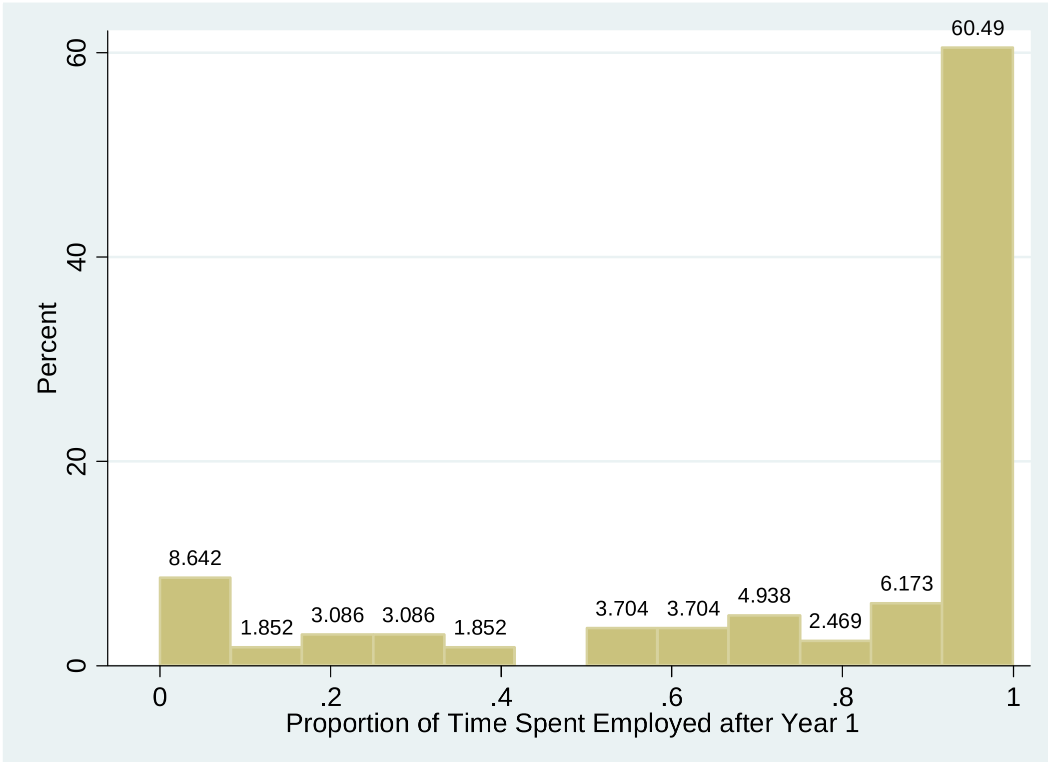
Note: The top of each bar is labeled with the fraction of the sub-sample represented by that bar.

Figure 3: Fraction of time spent jobless for youth whose first year was spent employed



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

Figure 4: Fraction of time spent jobless for youth whose first year was spent employed



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

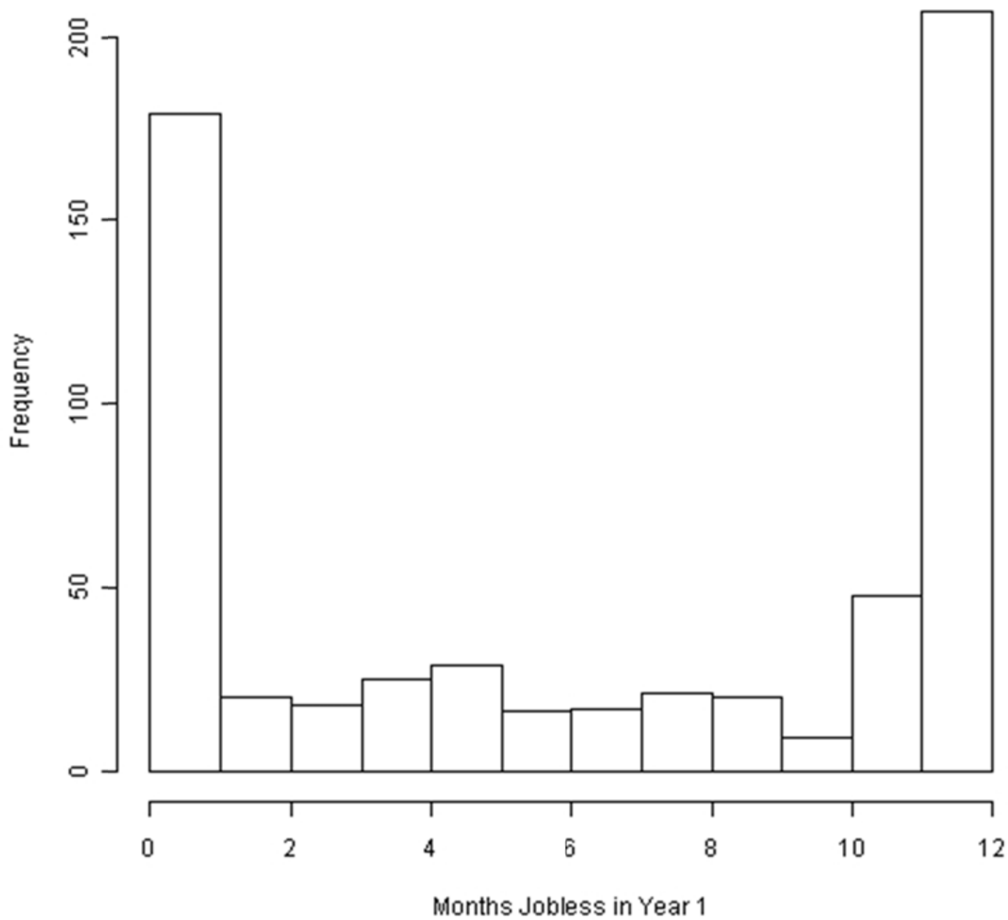


Figure 6: Histogram Depicting Categories of Joblessness in Year 1

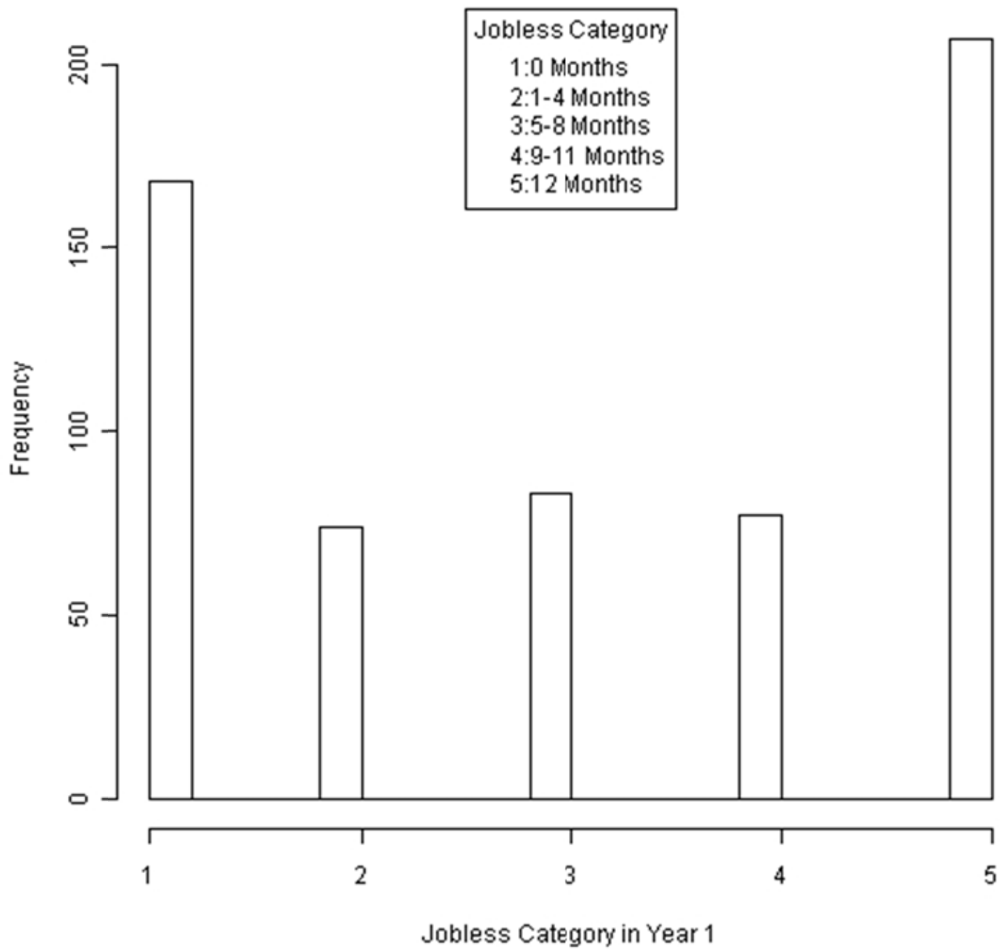
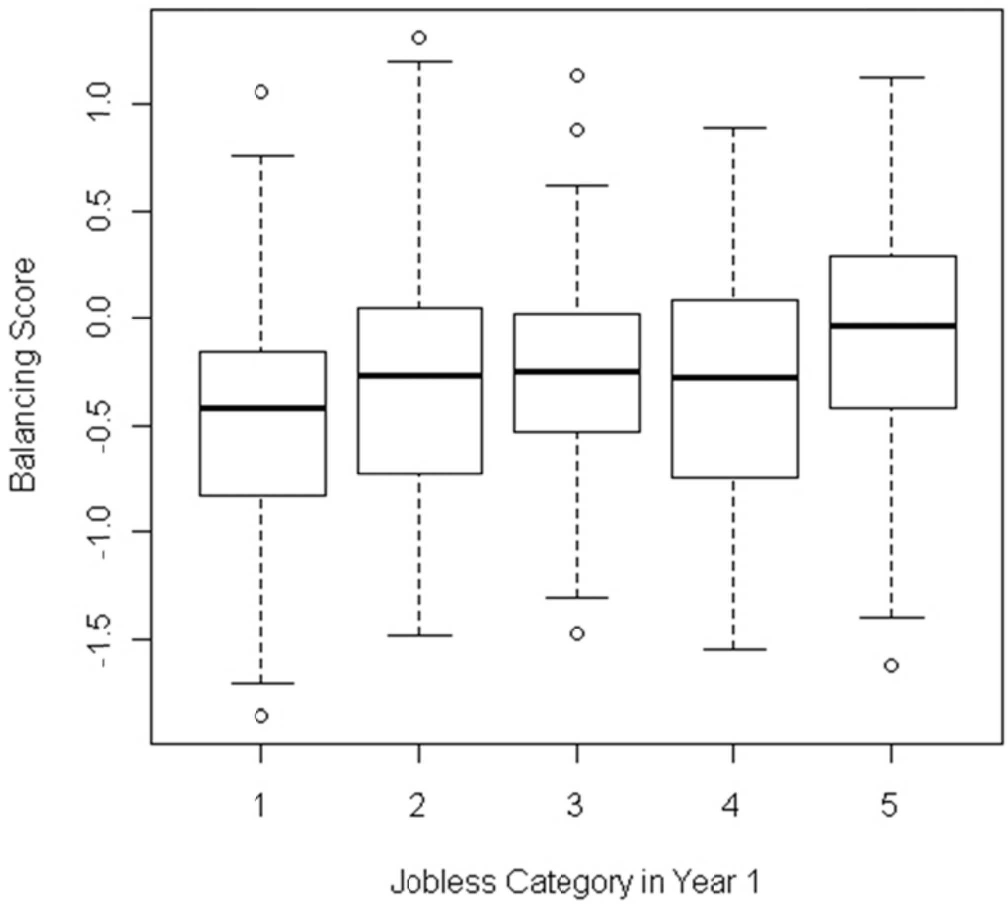
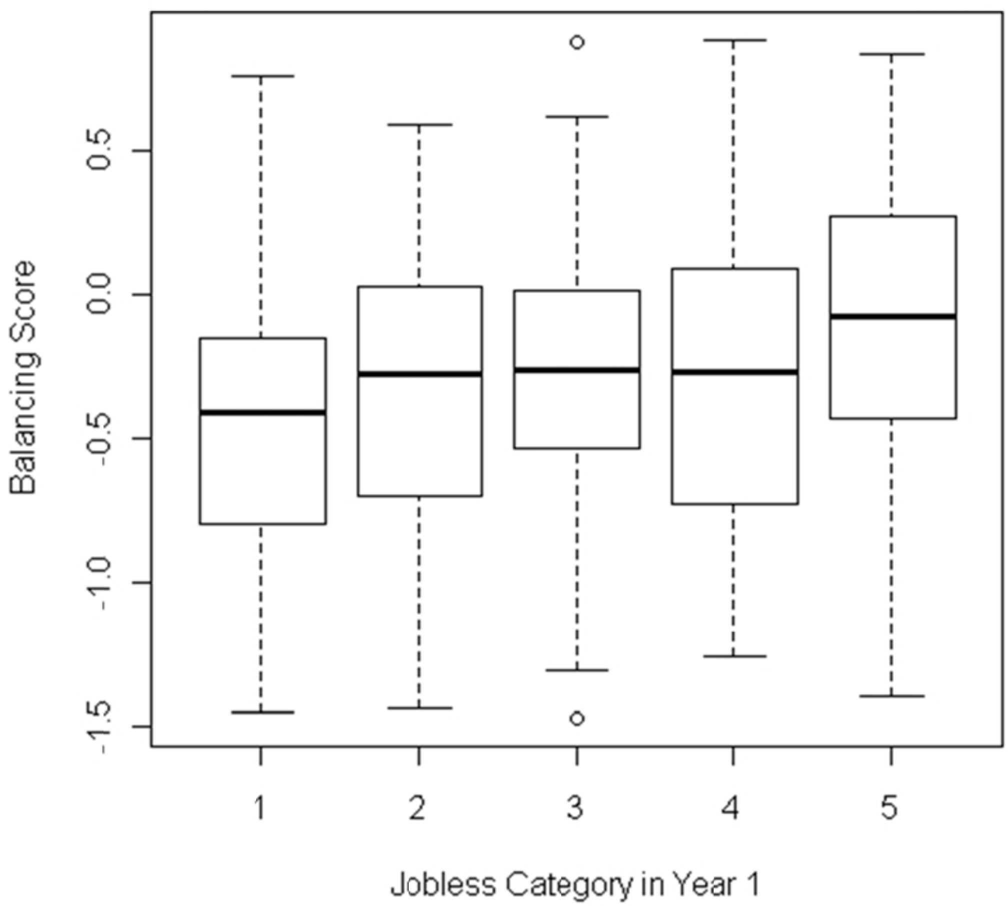


Figure 7: Distribution of Estimated Balancing Scores across Jobless Categories



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.

Appendix 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
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