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THE EFFECT OF YOUTH LABOR MARKET EXPERIENCE ON ADULT EARNINGS

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This paper investigates the effect of multiple youth jobs on adult earnings using the 1997 National Longitudinal Survey of Youth along with multiple regression specifications to identify treatment effects and a set of relatively weak nonparametric assumptions that provide tight bounds on treatment effects. Various specifications under an exogenous selection assumption indicate that an additional youth job increases adult yearly income by about $600 with the effect on men being larger than the effect on women. These specifications control for the number of adult jobs as well as the number of weeks worked as a youth. The partial identification strategy bounds the effect for men to be greater than zero, yet substantially smaller than the regression results. However, the confidence intervals on these estimates do not exclude a zero effect. Though a spurious explanation cannot be completely ruled out by the analysis, the results in this paper seem to imply that working multiple jobs as a youth has positive effects on adult earnings beyond pure labor market experience in contrast to the negative effect of multiple jobs as an adult.

Keywords: Youth Labor, Income, Partial Identification
JEL classification: C14, J31

1. INTRODUCTION

Explaining wages and their distributions has been, and continues to be, a central theme in the labor economics literature. Recently, the effects of youth and early adult labor market experiences has increasingly become a concern as the current recession has had severe negative impacts on labor markets with the concentration being among younger individuals. Moreover, the effects of the recession seem to have exacerbated a prior trend: the employment to population ratio in the U.S. for youths aged 16-19 declined from 45.2% in 2000 to 34.8% in 2007 then further declined to 25.8% in 2011 (Fernandes-Alcantara, 2012). Early studies of the effects of working while young

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(particularly while in school) found substantial gains in adult earnings (see Hotz *et al*,. 2002, for a review). However, a reassessment by Hotz *et al*. (2002) found these earlier results were spurious, and after controlling for selection, the effects were either dramatically reduced or disappeared altogether. This paper takes a second look at these effects with a newer cohort and extends the literature by focusing on the number of unique youth jobs rather than pure experience. The findings indicate that even after controlling for time spent in the labor market as a youth, having multiple jobs leads to increases in adult earnings, though a possible spurious explanation cannot be completely ruled out.

There are several reasons to suspect working as a youth should have a positive causal effect on adult outcomes. It likely develops responsibility, some appreciation for work, and some idea of what will be expected of them in future working environments. Furthermore, having multiple jobs might increase this return as individuals not only learn about their skills and interests, but also gain experience in actual job search thus increasing efficiency of search in the future. Alternatively, working as a youth might direct inputs away from educational attainment that would have had larger benefits. In addition, a stable work experience with fewer jobs might lead to greater specific skill development implying multiple jobs as a youth might have negative impacts on adult earnings. This last point is supported by a study by Neumark (2002) who finds that job stability in early career leads to increased adult earnings later in life.

This paper estimates the effect of the number of youth jobs on adult earnings using data from the 1997 National Longitudinal Survey of the Youth (NLSY). Initially I investigate this relationship with multiple regression specifications controlling for important covariates. However, a central issue in addressing the question of the causal effect of youth labor experience is self-selection. Though a clear correlation shows up in the data under multiple specifications that control for youth experience and number of adult jobs, it may simply be that individuals who undertook many youth jobs have other unobserved characteristics which themselves improve individuals' incomes in adult life. As noted by Schoenhals, Tienda and Schneider (1998) a complex array of background characteristics affect youths' decisions to work and it is possible these have lasting effects on adult earnings. Such endogeneity concerns are common. To address this concern this paper also employs a partial identification method stemming from work by Manski (1989, 1990, 1997) and Manski and Pepper (2000) and is organized as follows. Section 2 introduces the data. Section 3 reports and discusses regression results. Section 4 discusses the partial identification strategy. Section 5 discusses estimation and bounding results and section 6 concludes.

2. DATA

The data used in this study comes from individual respondents from the 1997 NLSY. The NLSY is a nationally representative sample of nearly 9,000 men and women in the U.S. born between 1980 and 1984 with minorities over-represented. This is newer data and a younger cohort than used in earlier studies on related topics (both Hotz *et al.*, 2002; and Neumark, 2002; use the 1979 NLSY cohort). Yearly income is reported income for the 2006 calender year. If respondents did not answer the income question but answered the subsequent 'range' income question the mean of that range was used for their yearly income. Youth jobs is defined as the number of jobs the respondent held between the ages of 14 and 19. Two related variables are the number of weeks worked as a youth and the number of adult jobs held. Weeks worked as a youth is defined as the number of weeks the respondent worked between the ages of 14 and 19. Adult jobs are defined as the number of jobs an individual worked since turning 20. I restrict the population to those reporting at least $2,500 in yearly income and reporting no more than 10 jobs as a youth.

Table 1. Means of Select Variables of Interest for Data Used

	Data Used for Regression			Data Used for Bounds		
Variable	All	Female	Male	All	Female	Male
Sex	0.51			0.52		
	(0.50)			(0.50)		
Youth Jobs	3.98	4.01	3.96	4.06	4.07	4.05
	(2.16)	(2.20)	(2.13)	(2.15)	(2.19)	(2.13)
Adult Jobs	4.90	5.04	4.77	4.91	5.11	4.72
	(2.86)	(2.81)	(2.90)	(2.86)	(2.88)	(2.83)
Youth Weeks	123.6	123.2	123.9	125.19	123.99	126.35
	(71.9)	(70.4)	(73.3)	(71.87)	(70.79)	(72.90)
Income 2006 ($)	23,892	20,816	26,834	24,359	21,319	27,217
	(17,729)	(14,844)	(19,664)	(18,018)	(15,241)	(19,868)
White	0.55	0.53	0.57	0.56	0.54	0.58
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.49)
Highest Degree	2.41	2.58	2.24	2.45	2.64	2.28
	(1.26)	(1.27)	(1.23)	(1.26)	(1.26)	(1.23)
Age	25.98	25.97	26.00	25.96	25.97	25.96
	(1.39)	(1.39)	(1.38)	(1.39)	(1.38)	(1.38)
Sample Size	5,002	2,446	2,556	4,857	2,354	2,503

Notes: Standard Deviations are in parenthesis. Figures for bounds sample are for those with available data. Youth Jobs is measured as number of jobs between ages 14-19; Adult Jobs is measured as number of jobs after turning 20; Youth Weeks is the number of weeks worked between ages 14-19; Ed. is measured as 0-5 for for no degree, GED, High School diploma, 2-yr degree, 4-yr degree, and degrees post bachelors or professional.

The race variable is a dummy variable and is coded as a 1 if an individual is non-hispanic white and zero otherwise. The sex dummy variable is coded as a 1 for males. Highest degree is coded as 0-5 for no degree, GED, High School diploma, 2-yr degree, 4-yr degree, and degrees post bachelors or professional. For the regression results the population is restricted to those without missing or unreported responses for included covariates. This produces a sample of 5,002. For the bounding analysis, the population was restricted according to number of jobs and income as above and to those who have recorded test scores as this will be used as an instrument in the analysis. The test score variable comes from the Armed Services Vocational Aptitude Battery (ASVAB) administered between the summer of 1997 and spring of 1998. These restrictions lead to a sample size of 4,857 used for the bounding analysis. Summary statistics for the populations used in this analysis are given in Table 1.

3. REGRESSION RESULTS

A visual depiction of the relation between number of youth jobs and earnings is given in Figure 1 and seems to indicate a clear trend relating number of youth jobs and average adult earnings, particularly in the range of youth jobs 0-4, which shows a monotonic increase in average wages ranging from about $16,000 to $25,000.

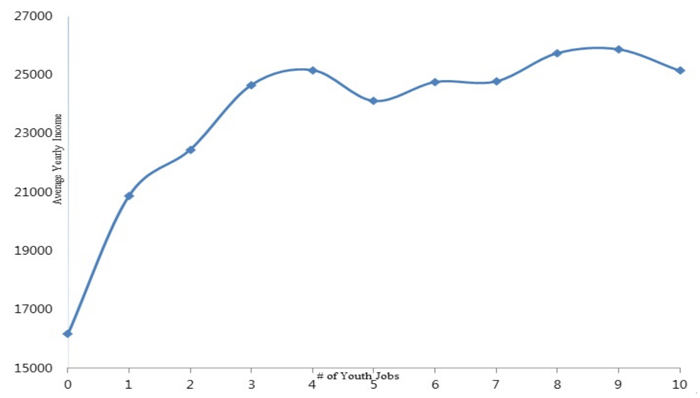

Figure 1. Average Yearly Earnings as Function of Youth Jobs

Two versions of base specifications are given in columns one through six in Table 2 with the first three using number of youth jobs as the regressor of interest and the second three using the natural log of youth jobs (with a constant of one added) to capture the nonlinear nature of the relationship seen in Figure 1. Across all specifications the effect of the number of youth jobs appears stable around $500-$600 per extra job in the linear specifications and a gain of about $2,000 for the first youth job, $660 for the fourth youth job, and steadily decreasing thereafter in the linear-log specifications. A possible important related variable is the number of adult jobs the respondent has had. As mentioned in the introduction, job stability can have positive effects, especially in early adult career, as one likely builds valuable human capital. Related, while on one hand changing jobs might lead to wage increases, too many moves might signal instability leading to lower wages. Of possible importance then is the high degree of correlation between adult and youth jobs in the data (26%). It seems plausible an important predictor of adult earnings is also linked to the number of jobs as a youth and adult and in this sense the number of adult jobs could act as an important control variable for relevant unobservable characteristics. Due to these concerns I include the number of adult jobs in specifications seven through ten. The inclusion of adult jobs has a significant impact on the coefficients on youth jobs leading to much larger implied effects. Additionally the coefficients on adult jobs are large, negative, and significant across all four specifications (though I would caution against interpreting these as causal effects).

Table 2. Results: Dependent Variable is 2006 Yearly Income ($)

Ind.	Specification									
Vars.	I	II	III	IV	V	VI	VII	VIII	IX	X
Youth	599	513	524				900	957		
Jobs	(5.18)	(4.64)	(4.74)				(7.60)	(8.55)		
ln*(Y.*				3,250	2,683	2,749			4,641	4,710
Jobs)				(6.32)	(5.46)	(5.59)			(8.81)	(9.46)
Adult							-883	-1,272	-911	-1,292
Jobs							(9.85)	(14.88)	(1018)	(15.14)
Male		6,578	6,561		6,564	6,548		6,280		6,243
		(13.80)	(13.82)		(13.79)	(13.80)		(13.50)		(13.45)
Age		2,786	2,785		2,783	2,782		3,340		3,350
		(16.41)	(16.45)		(16.40)	(16.45)		(19.69)		(19.76)
White		2,904	2,974		2,833	2,906		2,979		2,909
		(5.88)	(5.84)		(5.74)	(5.71)		(5.98)		(5.85)
Ed		2,201	2,190		2,187	2,176		2,368		2,339
		(11.37)	(11.31)		(11.32)	(11.27)		(12.47)		(12.36)
North			-3,088			-3,115		-3,055		-3,087
			(3.99)			(4.03)		(4.03)		(4.08)
South			-2,405			-2,391		-2,207		-2,197
			(3.43)			(3.41)		(3.21)		(3.20)
West			459			478		495		518
			(0.60)			(0.63)		(0.66)		(0.69)
Urban			-468			-484		-172		-186
			(0.77)			(0.80)		(0.29)		(0.31)

Note: t-statistics are in parenthesis. Youth Jobs is measured as number of jobs between ages 14-19; Adult Jobs is measured as number of jobs after turning 20; Ed. is measured as 0-5 for for no degree, GED, High School diploma, 2-yr degree, 4-yr degree, and degrees post bachelors or professional; North, South, West, and Urban are geographical dummies with North East and Rural omitted.

Table 3. Results: Dependent Variable is 2006 Yearly Income ($)

Ind. Vars.	Specification							
	I	II	III	IV	V	VI	VII	VIII
Youth Jobs	86	126			400	598		
	(0.70)	(1.07)			(3.13)	(4.99)		
ln*(Y. Jobs)*			846	861			2,344	3,066
			(1.51)	(1.62)			(4.04)	(5.65)
Adult Jobs					-786	-1,195	-811	-1,213
					(8.81)	(13.97)	(9.08)	(14.18)
Youth Weeks	40	34	39	33	37	29	35	27
	(10.80)	(9.46)	(10.26)	(9.06)	(9.86)	(8.01)	(9.16)	(7.40)
Male		6,513		6,513		6,256		6,234
		(13.84)		(13.84)		(13.54)		(13.51)
Age		2,827		2,826		3,345		3,349
		(16.85)		(16.83)		(19.82)		(19.86)
White		2,162		2,144		2,300		2,294
		(4.23)		(4.49)		(4.58)		(4.57)
Highest Degree		2,058		2,060		2,246		2,236
		(10.70)		(10.73)		(11.87)		(11.85)
North		-3,431		-3,436		-3,344		-3,348
		(4.47)		(4.48)		(4.44)		(4.45)
South		-2,232		-2,226		-2,075		-2,076
		(3.21)		(3.20)		(3.04)		(3.04)
West		427		438		466		483
		(0.56)		(0.58)		(0.63)		(0.65)
Urban		-289		-303		-41		-58
		(0.48)		(0.50)		(0.07)		(0.10)

Note: t-statistics are in parenthesis. Youth Jobs is measured as number of jobs between ages 14-19; Adult Jobs is measured as number of jobs after turning 20; Youth Weeks is the number of weeks worked between ages 14-19; Ed. is measured as 0-5 for for no degree, GED, High School diploma, 2-yr degree, 4-yr degree, and degrees post bachelors or professional; North, South, West, and Urban are geographical dummies with North East and Rural omitted.

Another concern in trying to identify the effect of multiple youth jobs is the high degree of correlation between it and the amount of weeks spent working as a youth (38%) and whether the results in the specifications in Table 2 are merely picking up the effect of youth experience rather than an additional effect of having multiple jobs. To address this issue I include a variable for total weeks worked as a youth in multiple specifications in Table 3. When looking at specification one through four it seems that the inclusion of weeks worked, while itself highly significant, removes any significance from an additional effect from having multiple jobs. However, drawing from the findings in Table 2, I then include adult jobs along with youth jobs and weeks worked as a youth in specifications five through eight. This inclusion again leads to the effect of additional youth jobs being economically and statistically significant above the effect of weeks worked. This appears strong evidence that having multiple jobs as a youth has a positive impact on adult earnings even when controlling for overall youth experience.

One more possible concern is the possibility that these effects differ for men and women.[1] As such I rerun key specifications for men and women separately and report the results in Table 4. The results are quite dramatic as not only are the effects of youth jobs more than 50% larger for men than women, but the coefficients on adult jobs and weeks worked as a youth are also at least 50% larger for men then for women. It appears the mechanism linking experience as a youth and adult with earnings are quite different for the sexes.

Table 4. Results: Dependent Variable is 2006 Yearly Income ($)

Ind. Vars.	Specification							
	Women				Men			
	I	II	III	IV	V	VI	VII	VIII
Youth Jobs	730	463			1,124	712		
	(5.47)	(3.20)			(6.32)	(3.77)		
ln*(Y. Jobs)*			3,676	2,466			5,449	3,560
			(6.17)	(3.73)			(6.92)	(4.21)
Adult Jobs	-849	-795	-872	-816	-1,653	-1,556	-1,669	-1,571
	(7.95)	(7.44)	(8.16)	(7.61)	(12.69)	(11.94)	(12.84)	(12.07)
Youth Weeks		21		19		34		32
		(4.65)		(4.17)		(6.25)		(5.88)
Age	2,491	2,486	2,500	2,493	4,160	4,169	4,164	4,171
	(12.01)	(12.04)	(12.08)	(12.08)	(15.76)	(15.92)	(15.80)	(15.94)
White	1,835	1,347	1,779	1,349	4,290	3,465	4,211	3,451
	(3.06)	(2.22)	(2.97)	(2.23)	(5.47)	(4.39)	(5.38)	(4.38)
Highest Degree	2,939	2,828	2,926	2,829	1,684	1,577	1,639	1,553
	(12.94)	(12.44)	(12.91)	(12.47)	(5.59)	(5.26)	(5.46)	(5.21)
North	-3,3769	-3,560	-3,418	-3,573	-2,674	-3,053	-2,965	-3,049
	(3.62)	(3.86)	(3.70)	(3.87)	(2.28)	(2.62)	(2.30)	(2.62)
South	-3,033	-2,880	-3,037	-2,894	-1,365	-1,293	-1,335	-1,276
	(3.66)	(3.49)	(3.67)	(3.51)	(1.27)	(1.21)	(1.24)	(1.19)
West	-838	-748	-781	-716	1,966	1,752	1,937	1,744
	(0.92)	(0.83)	(0.86)	(0.79)	(1.68)	(1.51)	(1.66)	(1.51)
Urban	1,752	1,753	1,720	1,731	-1,988	-1,692	-1,971	-1,695
	(2.39)	(2.40)	(2.35)	(2.37)	(2.17)	(1.86)	(2.15)	(1.86)

Note: t-statistics are in parenthesis. Youth Jobs is measured as number of jobs between ages 14-19; Adult Jobs is measured as number of jobs after turning 20; Youth Weeks is the number of weeks worked between ages 14-19; Ed. is measured as 0-5 for for no degree, GED, High School diploma, 2-yr degree, 4-yr degree, and degrees post bachelors or professional; North, South, West, and Urban are geographical dummies with North East and Rural omitted.

[1] This suggestion is due to an anonymous referee and I kindly thank the referee for that.

4. PARTIAL IDENTIFICATION

4.1. The Selection Problem

Though under various specifications presented above the parameter on the youth jobs regressor remains stable and significant, this does not imply this is a causal effect. One could argue there are important omitted variables driving the results. Consider the following potential outcome framework. Define t as 'potential' treatment and d as 'realized' treatment. The distributional characteristic of interest is the average treatment effect (ATE):

$$ATE = E[y(t) - y(t')] = E[y(t)] - E[y(t')] . \tag{1}$$

The ATE is defined as the expected treatment effect if treatment were randomly assigned to the population. If interest is in the ATE, what is problematic is that neither $E[y(t)]$ nor $E[y(t')]$ is observed, but rather $E[y(t)|d = t]$ and $E[y(t')|d = t']$. This is simply the endogeneity problem stated in terms of potential outcomes.

To see where further assumptions are necessary to identify the treatment effect, we can rewrite $E[y(t)]$ using the law of iterated expectations:

$$E[y(t)] = E[y(t)|d = t]P(d = t) + E[y(t)|d \neq t]P(d \neq t) . \tag{2}$$

The data identify sample analogues of all of the right hand side quantities except the counterfactual $E[y(t)|d \neq t]$. This might represent expected income under a treatment of 1 youth job for those who actually had a different number of youth jobs. The data bring us part of the way towards identifying the ATE, but the remaining distance must be covered by credible assumptions. The following three sections introduce three assumptions that will be used to help identify the treatment effects of interest. The first two introduced directly bound the unobserved counterfactual. The third then tightens the resulting identification region with a weakened version of the traditional instrumental variable (IV) assumption.

4.2. Monotone Treatment Selection

An exogenous selection assumption may be viewed suspiciously in the present setting. However, a weaker Monotone Treatment Selection assumption (Manski and Pepper, 2000) seems more credible:

MTS Assumption: *Let T be ordered. For each* $t \in T$ *and all* $(u_0, u_1) \in T \times T$ *such that* $u_1 \geq u_0$,

$$E[y(t) | d = u_1] \geq E[y(t) | d = u_0] . \tag{3}$$

MTS assumes a characteristic concerning the relationship between the selection process and the outcome process. Specifically, MTS presumes, for example, that those with a 'lower' realized treatment (a smaller number of youth jobs) exhibit characteristics that would lead them to have no greater expected incomes under either potential treatment than those with a 'higher' realized treatment (a larger number of youth jobs) under that same potential treatment.[2] This is precisely why standard regression methods might be considered suspect in the current setting; there are likely reasons to believe the respondents with a higher number of youth jobs are the same respondents who have other characteristics that are correlated with higher earnings such as confidence for example. If one were to assume the reversed inequality then the OLS results could simply be viewed as a lower bound on the actual treatment effect.

4.3. Monotone Treatment Response

The Monotone Treatment Response (Manski, 1997) assumption specifies a relationship between $y(t)$ and $y(t')$. It maintains that if treatments have some natural ordering then outcomes vary monotonically with them.

MTR Assumption: *Let T be ordered. For each* $j \in J$,

$$t \geq t' \Rightarrow y_j(t) \geq y_j(t') . \tag{4}$$

In the present study, this assumption implies that yearly income for each individual will be no greater with a smaller number of youth jobs. MTR also implies a weaker variant:

Mean MTR (MMTR):

$$E[y(t)] \geq E[y(t')] . \tag{5}$$

This follows from MTR by definition of the expectation function. In the current application, only the weaker assumption of MMTR will be implemented[3]. Bounds stemming from the joint imposition of MMTR and MTS provide a simple first estimate of the range of the causal effect of interest and take the following form:

[2] See Manski and Pepper (2000) for an in depth derivation of general bounds under MTS.

[3] See Manski (1997) for derivation of bounds under MTR.

$$\begin{aligned} &E[y(t)|d=t]P(d \geq t) + E[y(d<t)|d<t]P(d<t) \\ &\qquad \geq E[y(t)] \geq \\ &E[y(t)|d=t]P(d \leq t) + E[y(d>t)|d>t]P(d>t). \end{aligned} \tag{6}$$

The imposition of the joint MMTR and MTS assumptions can have significant identification power and directly relate to the response and selection process. In what follows, a monotone instrumental variables (MIV) assumption brings to bear a different type of assumption that, when invoked along with MMTR and MTS, can further tighten the identification region.

4.4. Monotone Instrumental Variables

The method of instrumental variables is widely used in the evaluation of treatment effects. Though standard IV assumptions can aid greatly in identification, the credibility of the instrument is often a matter of disagreement. This provides motivation for considering weaker, and thus more credible, assumptions to aid identification. First, consider a *mean independence* form of the standard IV condition:

IV Assumption: *Covariate z is an instrumental variable if, for each* $t \in T$ *and all* $(z, z') \in (Z \times Z)$,

$$E[y(t)|z'] = E[y(t)|z] .$$

A Monotone Instrumental Variable (Manski and Pepper, 2000) assumption weakens this IV condition by replacing the equality with an inequality:

MIV Assumption: *Let Z be an ordered set. Covariate z is a monotone instrumental variable if, for each* $t \in T$ *and all* $(z, z') \in (Z \times Z)$ *such that* $z_2 \geq z_1$,

$$E[y(t)|z_2] \geq E[y(t)|z_1] .$$

In what follows, the instrument is discrete. The implementation of MIV is straightforward. First, the researcher separates the data according to instrument realizations. Then upper and lower bounds are found on $E[y(t)|v=u]$ for each realization of the instrument by imposing the MTR and MTS assumption. With slight abuse of notation, let us denote them $UB_t|u$ and $LB_t|u$. Maintaining an MIV assumption would imply that when $u' \geq u$ the lower bound given u cannot be lower than the lower bound for u' . A similar argument holds for the upper bound. Following this procedure, the bounds on $E[y(t)]$ when v is an MIV become:

$$\sum_{u \in V} \Pr(u)[\max_{u' \le u} LB_t | u'] \le E[y(t)] \le \sum_{u \in V} \Pr(u)[\max_{u' \ge u} UB_t | u'] \,. \tag{7}$$

The instrument used in this analysis is the respondents' test scores from the Armed Services Vocational Aptitude Battery (ASVAB) administered between the summer of 1997 and spring of 1998. In treating this variable as an MIV, it is assumed that under either treatment, those with lower instrument levels (low test score) have expected incomes no better than those with higher instrument levels.

5. ESTIMATION AND RESULTS

5.1. Estimation

In this analysis I group the treatments as ‘zero or one youth job’ (t_1), ‘two youth jobs’ (t_2), ‘three or four youth jobs’ (t_3), and ‘greater than five youth jobs’ (t_4). Estimates of bounds are functions of expected incomes, probabilities of having specified number of youth jobs, and probabilities of realized instrument values, all of which can easily be computed nonparametrically. For bounds under MMTR/MTS, these values are calculated by sample analogs. For bounds under the test score MIV, expectations and probabilities are estimated via kernel estimation:

$$\hat{\mu}(z) = \frac{\sum_{i=1}^{n} y_i K\left(\frac{z - Z_i}{h}\right)}{\sum_{i=1}^{n} K\left(\frac{z - Z_i}{h}\right)} \,, \tag{8}$$

where $K(\cdot)$ is the Gaussian kernel weighting function and *h*, the bandwidth, is chosen using Silverman’s (1986) rule-of-thumb: $h = 1.06\sigma_z n^{-1/5}$.

Although nonparametric estimators allow researchers to estimate free of functional form, they are limited by the number of conditioning variables. The estimates in this paper condition on gender and the relevant instrument where an MIV is utilized. But this limited number of conditioning variables should not affect the consistency of the results as long as the assumptions defined above hold. In a standard regression, the consistency of the results relies on an orthogonality condition surrounding the disturbance term and the regressors. In such a setting, missing regressors might cause a failure in this condition leading to inconsistent results. In the present setting however, there is no equivalent condition necessary above the MMTR, MTS, MIV assumptions. Due to data limitations such a refinement is not feasible here.

An important concern when estimating bounds with MIVs is that analog estimates of such bounds exhibit finite-sample bias which lead the bounds to be narrower (more optimistic) than the true bounds. To counter this bias, I implement a correction proposed by Kreider and Pepper (2007). The approach is to estimate the bias by using the bootstrap distribution and then adjust the analogue estimate in accordance with the estimated bias. While heuristic and not derived from theory, this correction seems reasonable and performs well in Monte Carlo simulations (Manski and Pepper, 2009).

5.2. Inference

Statistical inference for partially identified parameters is somewhat more challenging than estimation itself and is the focus of a currently active literature. A consensus on the 'correct' type of confidence interval that should be reported is still evolving. The results of partial identification analysis are regions of identification defined by upper and lower bounds which contain the parameter of interest. When considering confidence intervals in these settings, the question arises of whether to construct intervals over the region of identification or over the actual parameter of interest. Intervals presented here cover the parameter of interest with fixed probability and were derived by Imbens and Manski (2004).

5.3. Results

Table 5 gives the bounds of the treatment effects between various treatments under both the MTR/MTS assumptions as well as combined with the MIV assumption for women and Table 6 gives analogous results for men. The bounds for various treatment effects under the joint MTR/MMTS assumption, while do not rule out a zero treatment effect, are nonetheless quite informative. For example, by simply imposing these two assumptions regarding the selection and response function one can bound the effect of going from 'three or four jobs' to 'five or more jobs' to lie between increasing ones' yearly income by $0 and $1,200 for men and $0 and $2,100 for women. Given the average yearly income for the sample, this implies the gains from more youth jobs after three or four is at most about a 5% increase in yearly income for men while at most 10% for women. But once the MIV assumption is combined with the MTS/MMTR assumptions the bounds become even more informative and in some cases can bound the treatment effect for men away from zero.

Table 5. Bounds on the Effect of Additional Youth Jobs on 2006 Yearly Income ($) for Women

	MMTR+MTS		95% conf. int.		MMTR+MTS+ MIV		95% conf. int.	
	LB	UB	LB	UB	LB	UB	LB	UB
$E[y(t_1)]$	17,372	21,319	15,916	21,825	18,705	21,554	17,205	22,199
$E[y(t_2)]$	19,075	21,520	18,048	22,064	19,496	21,653	18,390	22,314
$E[y(t_3)]$	20,850	22,165	20,193	22,783	20,824	22,174	20,258	22,875
$E[y(t_4)]$	21,319	22,950	20,812	23,509	21,201	22,471	30,669	23,414
$E[y(t_4)]-E[y(t_3)]$	0	2,100	0	3,215	0	1,647	0	2,803
$E[y(t_3)]-E[y(t_2)]$	0	3,081	0	4,219	0	2,678	0	3,935
$E[y(t_2)]-E[y(t_1)]$	0	4,148	0	5,600	0	2,948	0	4,588
$E[y(t_3)]-E[y(t_1)]$	0	4,784	0	6,272	0	3,469	0	5,122
$E[y(t_4)]-E[y(t_2)]$	0	3,875	0	5,196	0	2,975	0	4,422
$E[y(t_4)]-E[y(t_1)]$	0	5,578	0	7,191	0	3,766	0	5,566

Table 6. Bounds on the Effect of Additional Youth Jobs on 2006 Yearly Income ($) for Men

	MMTR+MTS		95% conf. int.		MMTR+MTS+ MIV		95% conf. int.	
	LB	UB	LB	UB	LB	UB	LB	UB
$E[y(t_1)]$	23,320	27,217	21,483	27,881	26,635	27,107	24,153	28,029
$E[y(t_2)]$	26,276	27,566	24,701	28,300	26,898	27,238	25,091	28,236
$E[y(t_3)]$	26,986	27,810	26,123	28,597	26,968	27,168	25,935	28,308
$E[y(t_4)]$	27,127	28,186	26,552	29,291	27,220	28,030	26,358	29,381
$E[y(t_4)]-E[y(t_3)]$	0	1,200	0	2,600	52	1,062	0	2,462
$E[y(t_3)]-E[y(t_2)]$	0	1,534	0	3,281	0	270	0	2,385
$E[y(t_2)]-E[y(t_1)]$	0	4,247	0	6,232	0	603	0	3,267
$E[y(t_3)]-E[y(t_1)]$	0	4,490	0	6,509	0	533	0	3,298
$E[y(t_4)]-E[y(t_2)]$	0	1,910	0	3,814	0	1,132	0	3,384
$E[y(t_4)]-E[y(t_1)]$	0	4,866	0	6,999	103	1,395	0	4,231

Looking at the treatment effect $y(t_4)-y(t_1)$, which is the effect from going from zero or one youth job to more than five youth jobs, the effect is bound between $103 and $1,395 for men. This implies at a minimum the experienced gained from these additional youth jobs increases expected yearly income by about $103. The effect of going from three or four youth jobs to more than five is also bounded between $52 and $1,062.

However neither of these lower bounds are significantly different from zero at the 95% confidence level. Additionally the upper bounds on the treatment effects $y(t_3) - y(t_2)$ and $y(t_2) - y(t_1)$ for men are are substantially lower than the estimates coming from regression results at $270 and $603 - implying at most a 1% and 3% increase in wages, though the upper bounds on the 95% confidence intervals are quite large. The bounds on the treatment effects for women tend to be larger than those for men and neither can rule out a zero effect nor the results coming from the regressions of the previous sections. However these larger bounds are not in contrast to the smaller effects found for women in the regression results. It rather simply implies the MIV used does not have as much identifying power for women. The range of the bounds should be thought of as an area of ignorance, thus higher upper bounds for women simply means the bounding strategy yields less information, not that the effect is larger.

6. CONCLUSION

This paper investigates the causal effect of youth labor market experience on adult earnings. Various regression specifications find a stable and significant positive effect of the number of youth jobs on adult earnings after controlling for possible confounding factors of number of adult jobs and weeks worked as a youth. These results highlight the unique role the number of youth jobs plays not only in contrast to the number of adult jobs, but also above simple measures of youth job market experience. Due to concerns over endogeneity of the treatment, a partial identification analysis is conducted. Under three monotonic assumptions regarding the treatment selection, response function, and an instrument, informative identification regions emerge for the average treatment effect. In particular, for men, bounds on two treatment effects can rule out a zero mean effect, though these are not significant at the 95% level.

The results found here add to the literature on the effects of youth employment by attempting to identify the effect of the number of youth jobs on subsequent adult earnings. Though the causal interpretation of the results is not definitive, there is some strong evidence that multiple youth jobs lead to increases in adult earnings. Also, the results here seem to reconfirm earlier findings by Neumark (2002) and highlight the difference in youth and adult job market experience. The negative effects of multiple jobs as an adult (which can be viewed as job instability) found here are large and significant, while youth jobs have a positive and significant effect. It is very likely that multiple jobs as a youth have positive matching effects, while later job instability leads to lower skill development and a negative signal to employers.

The youth labor market plays a unique role in individuals' experiences. The knowledge acquired by entering the labor market multiple times as a youth appears to have strong impacts on individuals later in their working lives. Actively seeking and finding employment multiple times when young seems to be a rewarding experience that should be encouraged.

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