

The Effect of Degree Attainment on Female Fertility

Matthew Dempsey

Spring 2018

Abstract

This paper looks to estimate the causal relationship between degree (GED, high school, or vocational) attainment and young (16-24) economically disadvantaged women's fertility (has baby or is pregnant). To estimate this causal relationship, bounds constructed from an invalid instrumental variable estimation were used. This paper finds that there is no effect of degree attainment on the fertility for any of the various different samples.

1. Introduction

Early motherhood represents a significant challenge for lawmakers. When teens become pregnant, they are significantly more likely to end up on public assistance relative to similar young women in the same socio-economic group. This makes it important for lawmakers to pursue policies which enable young women to take control of their fertility decisions (Taniguchi 1999). One avenue whose effect has been studied is the impact of education on young women's fertility choice. Prior research suggests that young women involved in child birth incur large private and public cost. Private costs show themselves as lower labor force participation and lower wages. Social costs exist not only as an increase in reliance on public assistance, but also as an increase in the usage of school services. Children born to young mothers are disadvantaged in a multitude of ways. First is an increased risk of having children early themselves thus leading to an intergenerational poverty trap. A second disadvantage is the increased chances of repeating a grade in high school or dropping out (Azevedo et al, 2012).

Education has been shown in past literature as a prominent factor in determining a woman's fertility choice. There appears to be two primary avenues by which education impacts fertility choices. The first avenue is through the accumulation of human capital at school which then increases expected future earnings and, thereby, increases the opportunity cost of having a child early in life (Becker 1981). The second avenue is that schooling may have an "incarceration" effect. This effect of school states that because a student is in school they are not participating in risky behaviors such as sex (Black et al, 2008).

There is extensive empirical literature showing a negative relation between female education and early fertility. However, very little of this research focuses on the existence on a causal relationship between female education and early fertility. A primary issue with studying a causal relationship is the endogeneity with the decision to accrue more human capital. This issue primarily exists as reverse causality from joint decision making, where unobservables cause selection bias.

To deal with these problems, previous literature has relied on instruments of varying degrees of strength or using experimental/quasi-experimental data. A prominent instrumental variable (IV) used in the previous literature are changes in compulsory schooling laws (CSL). In their paper DeCicca and Krashinsky (2015) find that an extra year of schooling reduces the likelihood of fertility at ages 17 and 18 by two and three percentage points, respectively. Similarly, Alzua and Velaquez (2017) found that an increase in one additional year of schooling decreased teenage fertility rate by 2.7 to 3.6 percentage points in Argentina.

This paper contributes to the previous literature by estimating the impact of obtaining a high school, General Educational Development (GED), or vocational degree on female fertility rates using the exogenous variation from random assignment in the U.S. training program for disadvantaged youth, Job Corps (JC). The paper uses the JC experiment's random assignment as an invalid instrumental variable to estimate the causal effect of educational attainment on female fertility rates. This compliments previous literature in a multitude of ways. First, previous literature primarily focuses on the impact of one additional year of schooling while this paper focuses on the impact of obtaining a variety of educational degrees. Second, previous literature using CLS primarily focuses on teenage individuals. This paper looks at the impact on a larger population, women aged 16-24. Third, CLSs are blanket legislation which lacks random assignment. This lack of random assignment implies that there could be an endogenous source of variation which the

random assignment of the JC experiment prevents. Fourth, unlike other studies which use CLS as an instrument, this paper allows for the instrument to violate the exclusion restriction by allowing random assignment to JC affect fertility through avenues other than education. This paper replaces the exclusion restriction assumption with the weak monotonicity assumption on average potential outcomes. Replacing the exclusion restriction, however, means that point estimation cannot be calculated; and instead, nonparametric bounds are used to estimate the causal impact of degree attainment on fertility rates.

2. Job Corps and the National Job Corps Study Data

2.1 Job Corps¹

This paper analyzes the impact of earning a degree on fertility rates using data from the National Job Corps Study (NJCS). Job Corps is an educational and vocational training program for disadvantaged youths, aged 16-24, in the U.S. The purpose of the training program is to educate, train, and prepare youths for employment in careers. To fulfill its purpose, JC provides academic, high school, GED, and vocational training. JC also offers additional services, such as counseling, health services, job placement, and others. JC exists throughout the U.S. and Puerto Rico. There are 125 centers which service roughly 60,000 new applicants every year.

To qualify for JC the applicant must satisfy the following requirements:

- (1) Be a legal U.S. resident;
- (2) Be between 16-24 in age;
- (3) If male, have registered with the Selective Service System;
- (4) Have parental consent if under the age of 18;
- (5) Be economically disadvantaged;²
- (6) Live in environment with limited opportunities to participate in other programs;
- (7) In need of additional education, training, or job skills;
- (8) Be free of serious behavioral issues;³
- (9) Have a clean bill of health;
- (10) Have sufficient child care arrangements while actively participating in the program;
- (11) Possess the capability and aspirations to benefit from JC.

Those selected to be enrolled in JC are given the opportunity to participate in many kinds of courses and services which vary slightly depending on the opportunities of the local area. The courses offered by JC fall into two broad categories, academic and vocational. The academic courses emphasize what is normally taught in comprehensive high schools (reading, writing, and

¹ Information about Job Corps comes from U.S. Department of Labor 2015 Job Corps Fact Sheet unless otherwise stated

² Economically disadvantaged is defined by JC as the applicant's family receives some form of public assistance and/or has an income which falls below the poverty line.

³ Behavioral issues are broadly defined. Allows those with a criminal record to be applicable for JC.

math). The goal of the academic classes is to enable the participant to obtain a GED or high school degree with the emphasis on obtaining a GED over a high school degree. The vocational skills training courses focus on giving participants the skills in demand for the local area which include business skills, health services, hospitality, and construction (Flores et al, 2016).

Along with offering a large assortment of academic and vocational classes, JC also offers several services for the participants currently enrolled and for those who have completed the program. These services include providing housing, health services, food aid, and providing aid for those with disabilities (Job Corp Support Services 2018)

2.2 National Job Corps Study

The National Job Corps Study was a randomized social experiment which began in 1993. It was tasked to examine the effectiveness of JC over many different categories. The NJCS was conducted at all JC centers in the contiguous 48 states and the District of Columbia. Applicants between November, 1994, to February, 1996, were screened; and roughly 15,000 were put into a control (5911) and treatment (9407) groups with the control group banned from enrolling in JC for three consecutive years. However, individuals in the control group were allowed to enter into other training programs. The treatment group was eligible to enroll in JC and subject to follow-up interviews at the 12, 30, and 48 months marks after random assignment.

Noncompliance in the treatment group was about 27%, meaning that percentage of the treatment group did not enroll in JC. Noncompliance in the control group was only 1.4%, meaning that percentage of the control groups was able to participate in JC. The high rate of noncompliance amongst the treatment group makes the interpretation of being treated as the impact of availability of the JC program. This has a minimal impact on the interpretation of the estimates for this paper. Along with the high noncompliance rate of the treatment group, the NJCS survey had an attrition rate of roughly 26%. However, the results of this paper are not affected by this attrition because the NJCS design probability weights used account for this attrition.

2.3 Data and Descriptive Statistics

This paper's sample comes from the NJCS data set and is representative of the population of eligible applicants to JC during 1994-1996. It contains 9090 total applicants and 4029 women of which 2599 are in the treated group and 1430 are in the control group. Of the 4029 women, 2189 are black; 835 are white; 747 are Hispanic; and the remainder are Indian or Other. In the data used there were two race indicators which were often the same; but at other times, one was missing or had a different value than the other. Without knowing how race was recorded, both indicators were taken to be true; and so, some observations were counted as in the estimates for multiple races. This will bias the results of the tests, but it is unclear as to the direction of the bias.

Table 1 shows the averages of selected characteristics at the baseline at the time of random assignment for females⁴, as well as the difference in the average characteristics for the women in the treated and control groups. This table shows that the average female participant is black, 18.6 years old, is not married, and does not have a child at random assignment. For education the average participant has completed 10.28 years of schooling; and by the 48th month interview, 61.5% will have obtained a degree. Also, at baseline 14.8% have been arrested; 18.6% have employment; and if employed in the previous year, they made about 2500 dollars. In the last

⁴ In future tables Female is replaced with Full.

column of Table 1, there are only two significant differences between the women in the treated and control groups. First is the age at baseline; but at the 95% level, we would expect to see significance 5% of the time. With all the other baseline differences being insignificant, it is safe to believe we are seeing a type one error. The other significant difference between the treated and control groups is obtaining a degree by the 48th month interview. This is expected and supports the assumption that the instrumental variable influences the mechanism.

Table 1: Summary statistics of selected variables for females by treatment.

Variable	Female	Treated	Control	Difference Treated - Control
<i>Selected demographic baseline variables</i>				
Age	18.620 (0.034)	18.683 (0.043)	18.506 (0.057)	0.176** (0.072)
White	0.207 (0.006)	0.210 (0.008)	0.201 (0.011)	0.009 (0.013)
Black	0.543 (0.008)	0.539 (0.010)	0.552 (0.013)	-0.013 (0.016)
Hispanic	0.185 (0.006)	0.189 (0.008)	0.180 (0.010)	0.009 (0.128)
Married	0.075 (0.005)	0.072 (0.006)	0.076 (0.008)	-0.004 (0.010)
Has child/children	0.332 (0.007)	0.334 (0.009)	0.330 (0.012)	0.003 (0.016)
<i>Selected education and crime variables at baseline</i>				
Highest grade completed	10.277 (0.0252)	10.291 (0.0311)	10.251 (0.0430)	0.039 (0.0527)
Any degree by 48 th month interview	0.615 (0.008)	0.684 (0.010)	0.472 (0.015)	0.212*** (0.017)
Arrested	0.148 (0.006)	0.143 (0.007)	0.156 (0.010)	-0.013 (0.012)
<i>Selected labor market variables at baseline</i>				
Employed	0.186 (0.006)	0.184 (0.008)	0.189 (0.010)	-0.006 (0.013)
Earnings in past year	2537.557 (108.660)	2588.819 (156.599)	2443.183 (110.400)	145.636 (227.536)
Observations	4029	2599	1430	

Standard errors in parentheses. *, **, and *** indicate significance at 90%, 95%, and 99% confidence levels

Table 2 shows the averages of selected characteristics of females at the baseline for black, white, and Hispanic females, as well as the differences between the different racial groups. While both whites and Hispanics are significantly more likely to be married, blacks are significantly more likely to have children than either whites or Hispanic females. Oddly, while white women are more

likely to be arrested, they are more likely to have a job and be paid significantly more over the last year than their black counterparts despite having no difference in schooling.

Table 2: Summary statistics of females by selected variables by race.

Variable	Black	White	Hispanic	Sample Differences		
				W-B	H-B	W-H
<i>Selected demographic baseline variables</i>						
Age	18.608 (0.047)	18.631 (0.075)	18.614 (0.017)	0.024 (0.089)	0.007 (0.093)	0.017 (0.109)
Married	0.045 (0.005)	0.103 (0.125)	0.123 (0.013)	.058*** (0.011)	.079*** (0.012)	-.020 (0.018)
Living Together	0.029 (0.004)	0.081 (0.009)	0.012 (0.009)	.052*** (0.008)	.043*** (0.008)	0.009 (0.013)
Separated	0.020 (0.003)	0.073 (0.009)	0.040 (0.007)	.053*** (0.007)	.021*** (0.007)	0.033** (0.012)
Has child/children	0.403 (0.011)	0.206 (0.014)	0.106 (0.017)	-.198*** (0.019)	-.097*** (0.021)	-.100*** (0.022)
<i>Selected education and crime variables at baseline</i>						
Highest grade completed	10.310 (0.033)	10.314 (0.054)	10.156 (0.064)	0.004 (0.063)	-.154* (0.067)	0.158 (0.083)
Arrested	0.139 (0.741)	0.199 (0.011)	0.092 (0.011)	.060*** (0.015)	-.047*** (0.014)	0.106*** (0.018)
<i>Selected labor market variables at baseline</i>						
Employed	0.1689944 (0.008)	0.258 (0.015)	0.158 (0.013)	.089*** (0.161)	-0.011 (0.016)	.100*** (0.021)
Earnings in past year	2239.176 (79.284)	3359.007 (177.030)	2646.707 (485.603)	1119.83*** (168.769)	407.531 (315.829)	712.299 (500.591)
Observations	2189	835	747			

Standard errors in parentheses. *, **, and *** indicate significance at 90%, 95%, and 99% confidence levels

Table 3 shows the averages of selected characteristics and the difference between the different samples by degree attainment by the 48th month interview. In the full sample, those young women who are most likely to earn a degree are white, completed more years of schooling, and made more money over the last year than their non-degree earning counterparts. Being married and being employed have significant impact on decreasing degree attainment for the full sample of women; however, the magnitude of the effect is small. Black women who are more likely to earn a degree are older, completed more years of schooling, were employed at baseline, and made more money over the last year than their non-degree earning counterparts. Unlike the full sample, being married does not appear to have any impact on a young black woman's decision to obtain a degree. White women who are more likely to earn a degree are older, completed more years of schooling, were not arrested, were employed at baseline, and made less money over the last year than their non-degree earning counterparts. Unlike all other samples, earning more money over the past year decreases the odds that a young white woman obtains a degree by the 48th month interview. Hispanic women who are more likely to earn a degree are older, have completed more years of schooling, and were less likely to be married than their non-degree earning counterparts.

Table 3: Summary statistics female by selected variables by degree attainment

Variable	Full			Black			White			Hispanic		
	D=0	D=1	Diff	D=0	D=1	Diff	D=0	D=1	Diff	D=0	D=1	Diff
<i>Selected demographic baseline variables</i>												
Age	18.440 (0.057)	18.889 (0.047)	0.449 (0.075)	18.473 (0.071)	19.011 (0.059)	0.538*** (0.093)	18.437 (0.108)	18.672 (0.075)	0.235* (0.132)	18.576 (0.113)	18.995 (0.099)	0.419*** (0.152)
Married	0.086 (0.008)	0.066 (0.005)	-0.019** (0.009)	0.046 (0.007)	0.042 (0.005)	-0.004 (0.009)	0.098 (0.016)	0.071 (0.010)	-0.0267 (0.018)	0.166 (0.021)	0.095 (0.013)	-0.071*** (0.023)
Black	0.564 (0.014)	0.541 (0.011)	-0.023 (0.018)	-	-	-	-	-	-	-	-	-
White	0.167 (0.010)	0.208 (0.009)	0.040*** (0.014)	-	-	-	-	-	-	-	-	-
Hispanic	0.203 (0.011)	0.189 (0.009)	-0.013 (0.014)	-	-	-	-	-	-	-	-	-
Has child/ children	0.346 (0.035)	0.282 (0.021)	-0.064 (0.040)	0.256 (0.028)	0.256 (0.020)	0.009 (0.035)	0.125 (0.026)	0.071 (0.012)	-0.054** (0.026)	0.244 (0.047)	0.222 (0.031)	0.023 (0.055)
<i>Selected education and crime variables at baseline</i>												
Highest grade completed	9.724 (0.091)	10.583 (0.070)	0.859*** (0.126)	9.731 (0.072)	10.639 (0.057)	0.908*** (0.096)	9.500 (0.108)	10.277 (0.070)	0.777*** (0.130)	9.837 (1.430)	10.233 (0.134)	0.396* (0.216)
Arrested	0.184 (0.029)	0.151 (0.016)	-0.032 (0.032)	0.345 (0.031)	0.227 (0.019)	-0.118*** (0.035)	0.405 (0.038)	0.317 (0.026)	-0.088** (0.043)	0.326 (0.051)	0.251 (0.033)	-0.074 (0.059)
<i>Selected labor market variables at baseline</i>												
Employed	0.243 (0.032)	0.342 (0.022)	-0.099*** (0.018)	0.223 (0.027)	0.300 (0.021)	0.077** (0.035)	0.280 (0.035)	0.359 (0.023)	0.080* (0.043)	0.337 (0.051)	0.330 (0.036)	-0.008 (0.062)
Earnings in	3742.940 (378.370)	4587.480 (190.420)	844.55** (386.490)	3595.599 (257.348)	4907.670 (219.678)	1312*** (362.657)	6394.068 (569.619)	5281.678 (233.663)	-1112.39** (515.893)	4768.967 (456.003)	5568.250 (362.875)	799.283 (609.269)
Observations	1308	2093		738	1132		338	720		326	494	

Standard errors in parentheses. *, **, and *** indicate significance at 90%, 95%, and 99% confidence levels

3. Econometrics

3.1 Methodology

This paper analyzes the impact of earning a degree on the probability that a female gives birth or gets pregnant during the course of the NJCS, using the random assignment of access to JC as the instrumental variable (IV) for degree attainment. As stated in section 1, because random assignment of access JC is exogenous, it most likely does not satisfy the exclusion restriction. For this reason, the nonparametric bounds method described in Flores et al, (2013) and used in Flores et al, (2016). The nonparametric methodology described in these two papers shows that by using a randomized variable from an unrelated existing experiment can be used as an IV to estimate the effects of a nonrandomized treatment. Both papers also use the NJCS data to implement the nonparametric bounds methodology.

The random assignment into the ability to participate in JC is indicated by S , with $S = 0$ indicating assignment into the control group and $S = 1$ indicating assignment into the treatment group. S will be used as the IV. The variable D indicates if an individual obtained a degree by the 48th month checkup interview, with $D = 0$ indicating the failure to obtain a degree by the 48th month interview and $D = 1$ indicating that a degree was obtained. The variables s and d indicate the value of S and D for each individual observed. As D is impacted by instrument S , $D(s)$ can indicate degree values control or treatment groups. Such that $D(0)$ and $D(1)$ indicate degree attainment given assignment to the control or treatment groups, it is possible to partition the population into four categories based on the values of the strata vector (Angrist et al, 1996).

Strata Chart

	Strata Vector	Abbreviation	Definition
Compliers	$\{D(0) = 0, D(1) = 1\}$	c	Earn Degree only if assigned to treatment group
Always Takers	$\{D(0) = 1, D(1) = 1\}$	at	Earn Degree independent of assignment to treatment group
Never Takers	$\{D(0) = 0, D(1) = 0\}$	nt	Never earn degree independent of assignment to treatment group
Defiers	$\{D(0) = 1, D(1) = 0\}$	d	Earn degree only if not assigned to treatment group

The outcome of whether the individual became pregnant or had a child by the 48th month interview is indicated by Y . Y can be denoted as $Y(s, D(s))$, and have four potential outcomes:

- (1) $Y(1, D(1)) \equiv Y(1)$
- (2) $Y(0, D(0)) \equiv Y(0)$
- (3) $Y(0, D(1))$
- (4) $Y(1, D(0))$

The first two outcomes can be observed in the data but not from the same individual observation. The first outcome relates to the outcome given that the individual was assigned to the treatment group. The second outcome relates to the outcome given that the individual was assigned to the control group. The other two outcomes relate to the counterfactual outcomes, or outcomes which are never seen in the data. The third outcome relates to the outcome in which the individual has no interaction with the IV, but the outcome has the value of an individual who was treated. The fourth outcome relates to the outcome where the individual has been treated with the IV, but the effect of the instrument on the individual has been blocked.

3.2 Assumptions

To find the treatment effect of an IV, previous literature has computed a point estimate by finding the Local Average Treatment Effect (LATE) for compliers, defined as:

$$LATE_c \equiv E[Y(s, 1) - Y(s, 0) | D(1) - D(0) = 1]$$

The $LATE_c$ is computed under the following assumptions:

- (1) Random assignment of instrument S
- (2) Non-zero average effect of the instrument on the treatment $E[D(1) - D(0)] = 0$
- (3) Individual level monotonicity of the instrument on the treatment $D(1) \geq D(0)$ for all individuals
- (4) Exclusion restriction: the instrument effects the outcome exclusively through the treatment

$$Y(0, d) = Y(1, d) \text{ for all individuals}$$

As previously stated, the exclusion restriction assumption (4) is most likely not satisfied and, therefore, is discarded. Flores and Flores-Lagunes (2010) shows that replacing the exclusion restriction with weak monotonicity on average potential outcomes is possible to decompose the Average Treatment Effect into two separate effects, the Mechanism Average Treatment Effect (MATE) and the Net Average Treatment Effect (NATE). These can be represented mathematically as:

$$MATE \equiv E[Y(1) - Y(1, D(0))] \quad NATE \equiv E[Y(1, D(0)) - Y(0)]$$

The MATE represents the impact the treatment, S (random assignment to JC), has working through the mechanism, D (obtainment of a degree), on the outcome, Y (female fertility). NATE represents the impact the treatment, S , has on the outcome, Y , outside of the impact it has through the mechanism, D . If the exclusion restriction holds, then $NATE = 0$. From Flores et al, (2013) it is possible to show the relationship between the $LATE_c$ and MATE as:

$$LATE \equiv E[Y(1) - Y(1, 0) | D(1) - D(0) = 1]$$

$$LATE = \frac{MATE}{E[D(1) - D(0)]}$$

From this relationship between $LATE_c$ and MATE, it is possible to construct bounds on the MATE. Following the process found in Flores and Flores-Lagunes (2010), the first step is to make bounds on the Local $MATE_i$ and Local $NATE_i$, where $i \in \{c, at, nt\}$. Because the counterfactual $Y(1, D(0))$ outcome is never observed for compliers, two additional assumptions are needed to create nonparametric bounds (Flores et al, 2010; 2013; 2016).

- (4) Weak monotonicity of mean potential outcomes within strata, (replaces Exclusion Restriction)
 - a) $E[Y(1) | c] \leq E[Y(1, D(0)) | c]$
 - b) $E[Y(1, D(0)) | i] \leq E[Y(0) | i]$, $i \in \{c, at, nt\}$

(5) Weak monotonicity of mean potential outcomes across strata

- a) $E[Y(1, D(0)) | c] \leq E[Y(1) | nt]$
- b) $E[Y(1) | at] \leq E[Y(1, D(0)) | c]$
- c) $E[Y(0) | c] \leq E[Y(0) | nt]$
- d) $E[Y(0) | at] \leq E[Y(0) | c]$
- e) $E[Y(1) | c] \leq E[Y(1) | nt]$
- f) $E[Y(1) | at] \leq E[Y(1) | c]$

Table 4: Testable implications of all assumptions

	Full	Black	White	Hispanic
$E[Y S=0, D=1] - E[Y S=0, D=0] \leq 0$	-0.007 (0.019)	0.080 (0.098)	0.418*** (0.131)	-0.056 (0.179)
$E[Y S=1, D=1] - E[Y S=1, D=0] \leq 0$	-0.034** (0.013)	0.192** (0.079)	0.082 (0.109)	0.091 (0.172)
$E[Y S=1, D=1] - E[Y S=0, D=0] \leq 0$	-0.013 (0.016)	0.125 (0.083)	0.165 (0.103)	0.174 (0.155)

Standard errors in parentheses. *, **, and *** indicate significance at 90%, 95%, and 99% confidence levels

It is possible to test if assumptions 1-5 all hold at the same time (Shown in Table 4). While having high significance for these tests implies that the assumptions hold, it does not prove that the assumptions hold. In Table 4, no group fully satisfies any of the testable implications. This implies that using these assumptions together is not suitable for performing invalid IV analysis with this data for fertility as an outcome.

Besides the lack of evidence for the assumption 1-5 holding another issue with using this methodology is with the creation of confidence intervals for the nonparametric bounds. To create these confidence intervals, some of the bounds are computed using minimum and maximum operators over several conditional expectations. This can bias the estimation of the bounds by making the bounds smaller in magnitude (Flores et al, 2016). A second issue is that a locally asymptotically unbiased estimator may not exist using the minimum and maximum operators. To obtain valid estimates the methodology of Flores et al, (2016; 2013) use a modified version of the methodology detailed in Cherozhkov, Lee, Rosen (2013). This paper applies the same modified method which is detailed in Flores et al, (2013)

4. Results

Table 5 shows the NATE, MATE, LATE of random assignment into Job Corps on the probability of becoming pregnant considering degree attainment as the mechanism of interest. The NATE bounds indicate a violation of the Exclusion restriction while the MATE bounds provide an initial intuition of the LATE bounds. The first row of Table 5 shows the point estimate of the effect of random assignment into JC access on births. This is later decomposed into the indirect(NATE) and direct(MATE) effects in the last panel of Table 5. The effect of random assignment into access of JC on births is near zero at .0006 with a standard error of .012. The insignificance of this result is shared across black and white women. The impact on Hispanic women is slightly significant at the 90% level; but given the insignificance of all of the other

values, this significance is most likely due to a type two error. An interesting observation of the results of the ATE of assignment on births for white women is its negative value unlike the unexpected positive values seen in the other groups. The interaction between this negative effect and the two positive effects of black and Hispanic women may cancel each other out, resulting in the near zero effect seen in the full sample.

Table 5: Point estimates and estimated bounds of NATE and MATE

	Full		Black		White		Hispanic	
<i>Treatment Effects</i>								
ATE of instrument on fertility	0.0006	(0.012)	0.037	(0.059)	-0.069	(0.086)	0.191*	(0.110)
ATE of instrument on degree	0.220***	(0.018)	0.220***	(0.055)	0.298***	(0.076)	0.294***	(0.099)
LATE degree on fertility	0.003	(0.054)	0.169	(0.347)	-0.231	(0.41)	0.651	(0.672)
<i>Strata Proportions</i>								
Precent _{at}	0.467***	(0.016)	0.550***	(0.048)	0.526***	(0.070)	0.556***	(0.088)
Precent _{nt}	0.313***	(0.010)	0.229***	(0.025)	0.176***	(0.033)	0.150***	(0.044)
Precent _c	0.220***	(0.018)	0.220***	(0.055)	0.298***	(0.076)	0.294***	(0.099)
<i>Conditional Means</i>								
E[Y S=0]	0.895***	(0.010)	0.467***	(0.048)	0.503***	(0.072)	0.366***	(0.090)
E[Y S=1]	0.896***	(0.007)	0.505***	(0.033)	0.434***	(0.042)	0.557***	(0.059)
E[Y D=0]	0.907***	(0.009)	0.401***	(0.053)	0.310***	(0.069)	0.420***	(0.107)
E[Y D=1]	0.888***	(0.008)	0.530***	(0.035)	0.532***	(0.045)	0.487***	(0.059)
<i>NATE</i>	LB	UB	LB	UB	LB	UB	LB	UB
Bounds	0.008	0.021	-0.005	0.135	-0.093	0.043	0.165	0.329
Confidence Interval under assumptions 1-4	[-0.015,	0.043]	[-.122,	.279]	[-.270,	.282]	[-.075,	.329]
<i>MATE</i>	LB	UB	LB	UB	LB	UB	LB	UB
Bounds	0	-0.005	0	0.05	0	-0.0346	0	0.08
Confidence Interval under assumptions 1-5	[0,	0.001]	[0,	.078]	[0,	.087]	[0,	.172]

Standard errors in parentheses. *, **, and *** indicate significance at 90%, 95%, and 99% confidence levels

The second row in Table 5 shows the point estimate of the effect of random assignment into JC access on degree attainment by the 48th month interview. Across all samples this effect is significant at the 99% level and positive. This high level of significance for all samples shows that assumption 2 is valid. Row three of Table 5 shows the point estimate of the LATE_c of degree attainment on female fertility. Similar to row one's lack of effect, there is a lack of effect seen in the impact of degree obtainment of fertility choices. The full sample has a near zero effect of .003 with a standard error of .054. Like row one, the white sample is the only one with the expected negative effect.

The next section of Table 5 shows the point estimate of the strata proportions across all samples. For all samples the proportion estimates are significant at the 99% level, but the proportions of the different race samples seem to be more similar to each other than the proportion of the full sample. For the full sample; the proportion of always takers is estimated to be 47%,

never takers 31%, and compliers 22%. The following section shows the point estimations of the outcome (having a child or getting pregnant between the baseline period and the 48th month after assignment) for the four different groups which come about through random assignment into access of JC and degree attainment status. Comparing the conditional means by degree attainment, an estimate for an effect of degree attainment on female fertility can be calculated. What is strange with these results is that they are all significant at the 99% level but the $LATE_c$ effect is nonexistent. This could be because the effect obtained by comparing the conditional means does not control for selection bias and, therefore, is most likely larger than the true effect of degree attainment on female fertility.

The next section of Table 5 shows evidence for the likelihood of the exclusion restriction being violated. This is shown through the estimated bounds for NATE. These bounds were calculated without assumption 4 which imposes a negative sign on the NATE bounds. The female and Hispanic bounds are both positive. While the black and white bounds both have positive and negative values in their bounds. The 95% confidence intervals for all samples have both negative and positive values so a $NATE=0$ is possible. These results can be interpreted as weak evidence that assumption 4 does not hold.

The last section of Table 5 shows the estimated bounds for MATE under assumptions 1-5. For all samples estimated the lower bound is zero and the upper bound is either a positive or even negative number that is close to zero. These irrational estimates of the mechanism bounds are most likely a direct result of trying to decompose the near zero effect shown in the first row of Table 1 into two separate effects (NATE and MATE). These estimates further show a lack of effect that the random assignment of access to JC has on fertility.

Table 6 attempts to deal with the lack of effect found by looking at the full sample of young women by introducing two new samples based on age, teenage (16-19) and twenties (20-24). While the effect of random assignment of access to JC has on fertility is still small, it is statistically significant at the 99% level. What is interesting about the first row of Table 6 is that the ATE of teenage appears to be equal and opposite in magnitude. This could be a possible explanation for the lack of effect the instrument had on the outcome.

For the Table 6 samples, the point estimate effect of random assignment on degree acquisition is significant for only the teenage sample and is relatively small in magnitude for the twenties sample. This lack of significance shows that assumption 2 may not be as valid for the full female sample as the previous Table, Table 5, shows. The point estimate of the $LATE_c$ of degree attainment on female fertility in Table 6 is still statistically insignificant similar to the estimates in Table 5.

The point estimate of the strata proportions across the new samples of Table 6 are nearly identical in significance and magnitude to the estimates from Table 5 with two exceptions. The first exception is the percent of always takers in the twenties sample being significant at the 99% level and having a magnitude of 72.1%. The second exception is the estimated percent of compliers in the twenties sample being not significant and only having a magnitude of 9.4%. This drastic change in proportions may indicate a change in behavior as young women age. The point estimations of the outcome for the four different groups in Table 6 is nearly identical to the estimates of Table 5 and so the interpretation is the same as above.

Table 6: Point estimates and estimated bounds of NATE and MATE by age

	Full		Teenage(16-19)		Twenties(20-24)	
<i>Treatment Effects</i>						
ATE of instrument on fertility	0.0006	(0.012)	-0.134***	(0.050)	0.139***	(0.087)
ATE of instrument on degree	0.220***	(0.018)	0.288***	(0.048)	0.094	(0.064)
LATE degree on fertility	0.003	(0.054)	-0.047	(0.185)	1.478	(1.723)
<i>Strata Proportions</i>						
Precent _{at}	0.467***	(0.016)	0.503***	(0.042)	0.721***	(0.061)
Precent _{nt}	0.313***	(0.010)	0.209***	(0.023)	0.185***	(0.031)
Precent _c	0.220***	(0.018)	0.288***	(0.048)	0.094	(0.064)
<i>Conditional Means</i>						
E[Y S=0]	0.895***	(0.010)	0.446***	(0.040)	0.473***	(0.079)
E[Y S=1]	0.896***	(0.007)	0.432***	(0.029)	0.612***	(0.040)
E[Y D=0]	0.907***	(0.009)	0.367***	(0.043)	0.413***	(0.087)
E[Y D=1]	0.888***	(0.008)	0.478***	(0.030)	0.604***	(0.042)
<i>NATE</i>	LB	UB	LB	UB	LB	UB
Bounds	0.008	0.021	-0.049	0.105	0.126	0.171
Confidence Interval	[-0.015,	0.043]	[-.148,	.244]	[-.047,	.359]
<i>MATE</i>	LB	UB	LB	UB	LB	UB
Bounds	0	-0.005	0.000	0.037	0.000	0.032
Confidence Interval	[0,	0.001]	[0,	.073]	[0,	.058]

Standard errors in parentheses. *, **, and *** indicate significance at 90%, 95%, and 99% confidence levels

The NATE bounds for the samples of Table 6 have both strictly positive (twenties) estimates and bounds which contains zero (teenage). Like Table 5, the 95% confidence interval for these bound both contain zero so a NATE=0 is possible. The last section of Table 6 shows the estimated bounds for MATE under assumptions 1-5. While the estimated lower bounds for the samples are still 0, for the new samples, the estimates of the upper bounds are nonnegative. This is most likely due to the teenage and twenties samples having an effect significantly different from zero effect of access for JC on fertility.

5. Conclusion

This paper attempted to analyze the causal effect of attaining a GED, high school, or vocational degree on the fertility choice of young disadvantaged women from the United States using the exogenous variation from the random assignment to access to the Job Corps program found in the data collected by the National Job Corps Study. Unlike other studies which use a traditional instrumental variable, this paper uses an invalid instrumental variable by replacing the exclusion restriction assumption with weak monotonicity of mean potential outcomes within and across strata. This replacement prevented point estimates. Instead, bounds were created to estimate the causal effect.

The primary finding of this paper is that for young disadvantaged women from the United States, degree attainment status does not appear to have any effect on this group's fertility choice. The evidence for a lack of any effect is supported when looking at both black and white samples. However, there does appear to be some small, but significant degree effects for the teenager and twenties samples. The teenager and twenties sample effects appear to be equal and opposite in magnitude which would indicate a change in behavior as teenagers age and become twenty-year olds. Despite the significance of these results, the magnitudes are too close to zero to estimate the bounds for the mechanism. Further research into the effects of education on fertility could examine the impact of college degree obtainment on fertility choices or a more detailed analysis of variables which differently impact teenage fertility and fertility of older age cohorts.

Finally, this paper shows some of the short comings of using an invalid instrumental variable without exploring if all the assumptions hold. This is exemplified by the irrational bounds calculated for the MATE in the full sample. This does not mean that usage of an invalid instrument should not be used, but does mean that methodology selection should be given more thought before analyzing data.

References

- Alzúa, María, and Laura Velázquez. "The Effect of Education on Teenage Fertility: Causal Evidence for Argentina." *IZA Journal of Development and Migration* 7, no. 1 (2017): 1-23.
- Becker, Gary S. *A Treatise on the Family*. Enl. ed. Cambridge, MA: Harvard University Press, 1993.
- Alzúa, María, and Laura Velázquez. "The Effect of Education on Teenage Fertility: Causal Evidence for Argentina." *IZA Journal of Development and Migration* 7, no. 1 (2017): 1-23.
- Amin, Flores, Flores-Lagunes, and Parisian. "The Effect of Degree Attainment on Arrests: Evidence from a Randomized Social Experiment." *Economics of Education Review* 54, no. C (2016): 259-73.
- Angrist, Joshua D., Guido W. Imbens, and Donald B. Rubin. "Identification of Causal Effects Using Instrumental Variables." *Journal of the American Statistical Association* 91, no. 434 (1996): 444-55.
- Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes. "Staying in the Classroom and out of the Maternity Ward? The Effect of Compulsory Schooling Laws on Teenage Births*." *Economic Journal* 118, no. 530 (2008): 1025-054.
- Blanco, German., Carlos A. Flores, and Alfonso. Flores-Lagunes. "Bounds on Average and Quantile Treatment Effects of Job Corps Training on Wages." *Journal of Human Resources* 48, no. 3 (2013): 659-701.
- Chen, Xi-Kuan, Shi Wu Wen, Nathalie Fleming, Kitaw Demissie, George G Rhoads, and Mark Walker. "Teenage Pregnancy and Adverse Birth Outcomes: A Large Population Based Retrospective Cohort Study." 36, no. 2 (2007): 368-73.
- Chernozhukov, Victor, Sokbae Lee, and Adam M. Rosen. "Intersection Bounds: Estimation and Inference." *Econometrica* 81, no. 2 (2013): 667-737.
- DeCicca, Philip, and Harry Krashinsky. "Does Education Reduce Teen Fertility? Evidence from Compulsory Schooling Laws." *NBER Working Paper Series*, 2015, N/a.
- Lang, Kevin, and Kropp, David. "Human Capital versus Sorting: The Effects of Compulsory Attendance Laws." *Quarterly Journal of Economics* 101 (1986): 609.
- "Support Services." Possible Accommodations: Intellectual Disabilities. Accessed June 7, 2018. <https://supportservices.jobcorps.gov/Pages/default.aspx>.
- Taniguchi, Hiromi. "The Timing of Childbearing and Women's Wages." *Journal of Marriage and the Family* 61, no. 4 (1999): 1008-1019.
- U.S. Department of Labor. "Job Corps fact sheet", (2015)