Economics 152, Lecture 3: Labor Supply Evidence

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Empirical Evidence on Labor Supply

- We used the neoclassical model of labor supply to generate predictions
  - Effects of changes in wages and non-labor income
  - Effects of welfare programs
- Next: Do the data confirm these predictions?

Econometrics Review

- Start with a review of econometric techniques (Angrist and Krueger 1999)
- Two types of empirical research in labor economics:
  - **Descriptive analysis:** Establish facts about the labor market
  - **Causal inference:** Determine the effects of changes in policies or other variables
- Example:
  - Descriptive: Do people with more schooling earn more?
  - Causal: Will an extra year of schooling increase a person's earnings?

OLS Regression

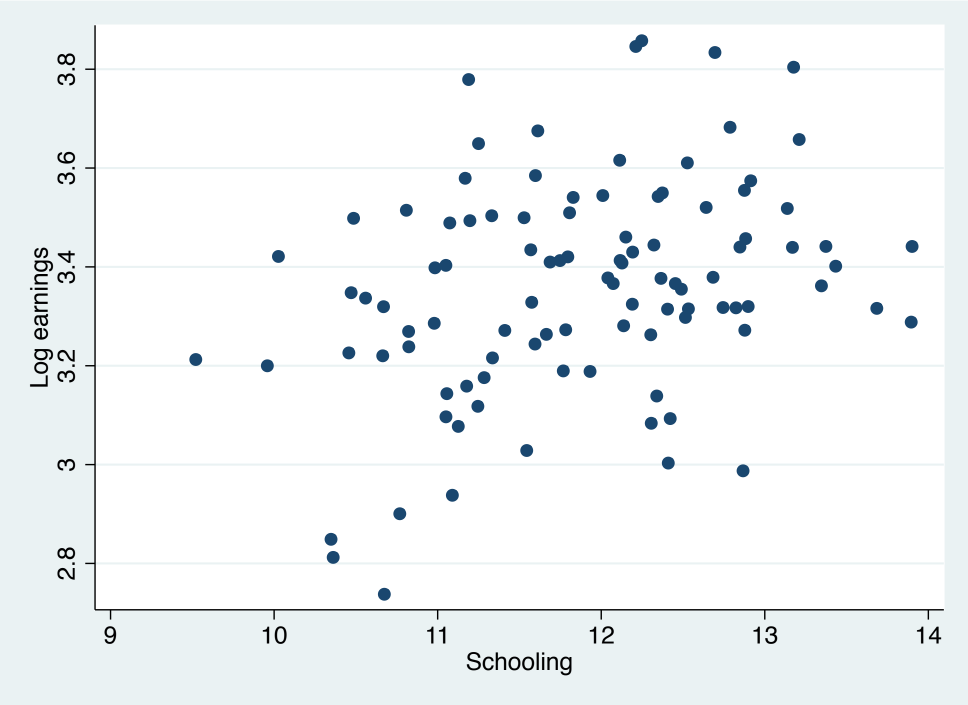
- Key empirical tool in labor economics: **Ordinary least squares (OLS) regression**
- Regression summarizes statistical relationships between variables
- A regression equation relating y_i to x_i is:

$$y_i = \alpha + \beta x_i + \epsilon_i$$

OLS Regression

$$y_i = \alpha + \beta x_i + \epsilon_i$$

- The **dependent variable** y_i is on the left – it's the variable to be explained
- The **independent variable** x_i is on the right – it's the variable doing the explaining
- The **regression coefficient** β measures the average increase in y_i associated with a one-unit increase in x_i
- Regression produces the line that best fits the relationship between y_i and x_i



Log earnings
3.8
3.6
3.4
3.2
3
2.8
9
10
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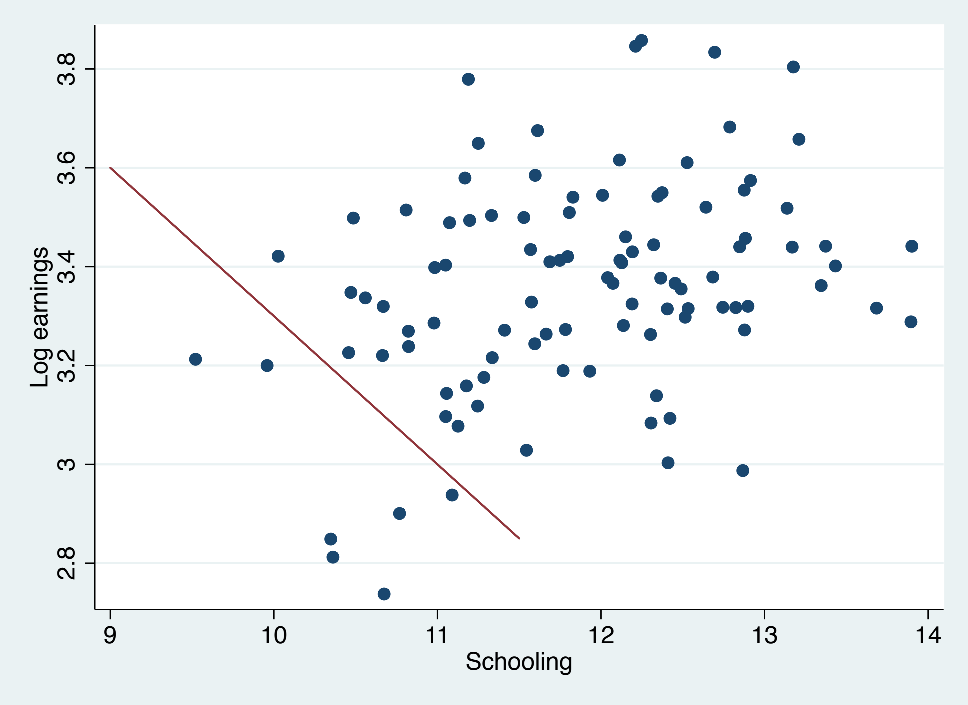
13
14
Schooling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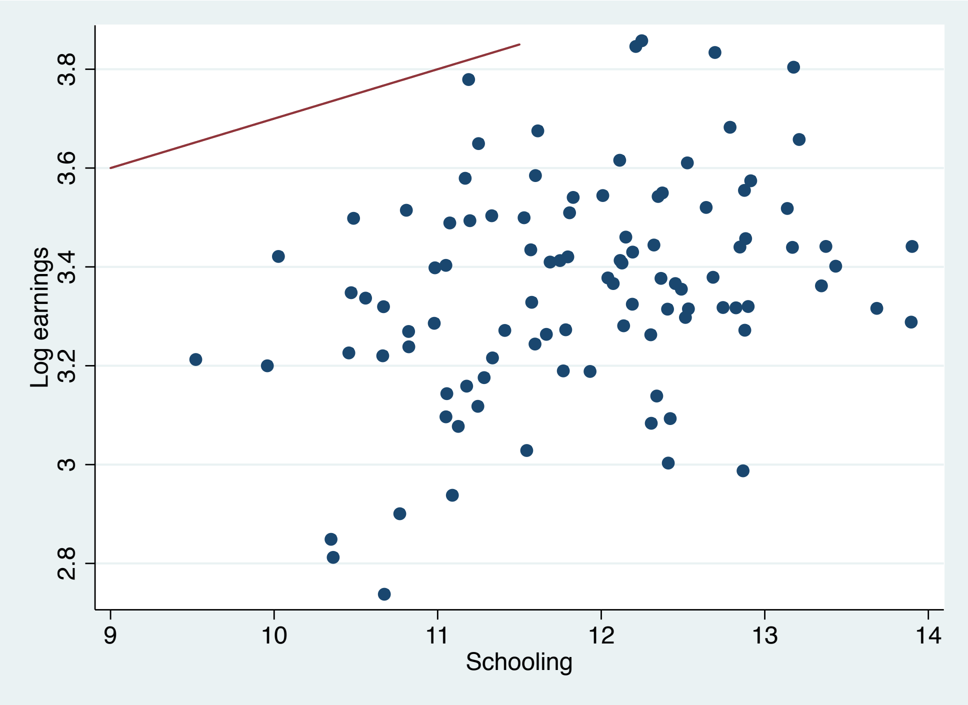

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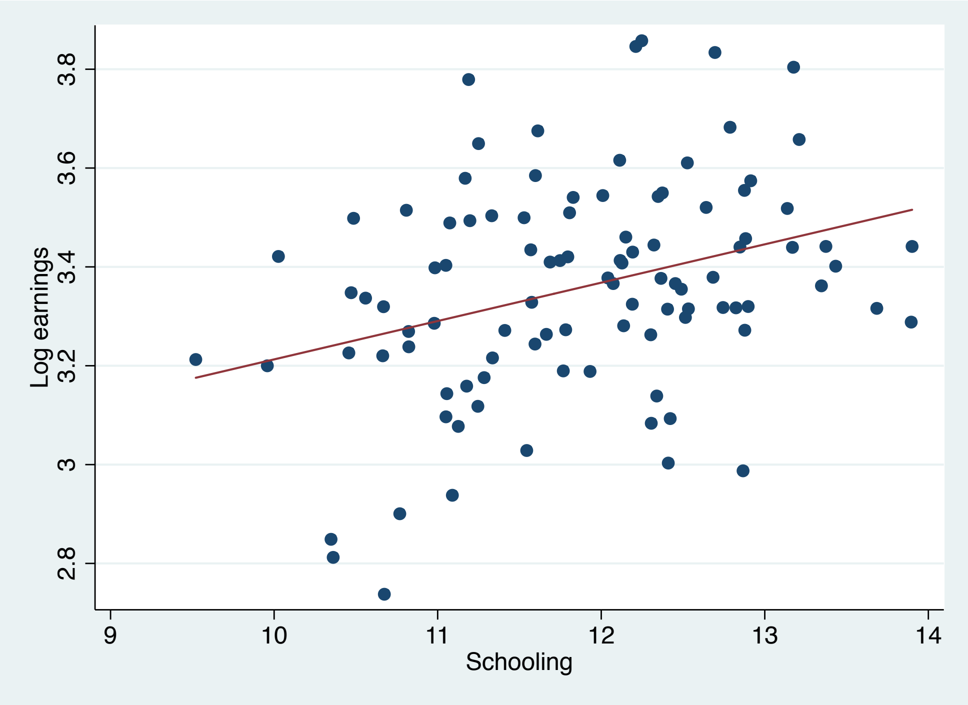
Schooling



Log earnings
3.8
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10
11
12
13
14
Schooling

OLS Regression

$$y_i = \alpha + \beta x_i + \epsilon_i$$

- The coefficients α and β minimize the average squared difference between the dependent variable and the model's prediction
- They solve

$$\min_{a,b} E\left[(y_i - a - bx_i)^2\right]$$

- Here E means expectation, which is the average
- The slope β is

$$\beta = \frac{Cov\left(y_i, x_i\right)}{Var\left(x_i\right)}$$

Sampling Error

- Regressions coefficients are estimated using samples of data
- The estimated slope coefficient is denoted $\hat{\beta}$
- Because we don't have data for everyone, $\hat{\beta}$ is subject to **sampling error**
- Example: we might sample people with atypically high earnings and schooling by chance

Standard Errors

- The **standard error** of an estimated regression coefficient reflects its sampling error
- The standard error is constructed so that there is a 95 percent chance the interval $(\hat{\beta} - 2SE, \hat{\beta} + 2SE)$ includes the true β
- Rule of thumb: if the **t-statistic,** $t = \hat{\beta}/SE$, is greater than 2, we call $\hat{\beta}$ **statistically significant**

Regression and Causality

- Regressions are always useful for descriptive purposes
- They may or may not capture causal relationships
- Example: Is a regression relating earnings to schooling likely to capture a causal effect?
- Key problem: Possible presence of **confounding variables** that cause y_i, and are correlated with x_i
- This is known as **omitted variables bias**
- Example: Natural ability may increase both earnings and schooling

Multiple Regression

- We can expand our regression model to include additional variables:

$$y_i = \alpha + \beta x_i + \gamma c_i + \epsilon_i$$

- β now measures the relationship between y_i and x_i, holding c_i constant
- Example: In the earnings/schooling regression, c_i may be a test score capturing natural ability
- If we control for all confounding variables, may interpret the coefficient β as causal
- Problem: how do we know if we've controlled for all confounders?

Estimates of the Labor Supply Elasticity

- Typical regression used to estimate the elasticity of labor supply σ:

$$h_i = \alpha + \beta w_i + \gamma V_i + \delta c_i + \epsilon_i$$

- h is hours, w is the wage, V is non-labor income, and c is other controls
- Multiplying β by ratio of mean wages to hours converts it to an estimate of σ
- Consensus estimate for men is $\sigma = -0.1$
- This implies income effects dominate – may explain decreasing hours over the 20th century
- Estimates for women are usually around 0.2

Problems with Estimated Elasticities

$$h_i = \alpha + \beta w_i + \gamma V_i + \delta c_i + \epsilon_i$$

- Many problems with this regression:
  - Measurement error in h, w and V
  - Sample selection (w_i not observed for non-workers)
  - Remaining omitted variables bias

Randomized Experiments

- Ideal way to eliminate omitted variables bias: run an experiment and randomly assign the variable of interest
- If x_i is randomly assigned, it cannot be correlated with anything else that causes y_i
- We can therefore be sure that the relationship is causal
- Often randomized experiments aren't feasible – but sometimes they are!
- Ashenfelter and Plant (1990) use a randomized experiment to study the labor supply effects of negative income taxes

Ashenfelter and Plant (1990)

- Ashenfelter and Plant (1990) study the Seattle/Denver Income Maintenance Experiments (SIME/DIME)
- The SIME/DIME was a randomized experiment conducted in the 1970s
- Households were randomly assigned to a "treatment" group eligible for an NIT, or an ineligible "control" group
- Within the treatment group, 11 different combinations of the guarantee G and implicit tax t

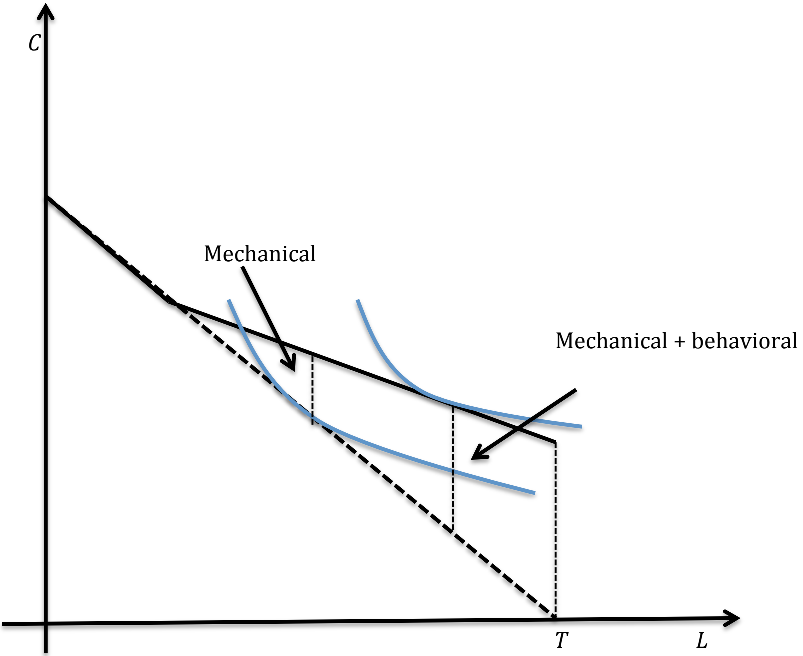
Table 1
Parameters of the 11 Negative Income Tax Programs

Program Number	G ($)	τ	Declining Tax Rate	Break-even Income ($)
1	3,800	.5	No	7,600
2	3,800	.7	No	5,429
3	3,800	.7	Yes	7,367
4	3,800	.8	Yes	5,802
5	4,800	.5	No	9,600
6	4,800	.7	No	6,857
7	4,800	.7	Yes	12,000
8	4,800	.8	Yes	8,000
9	5,600	.5	No	11,200
10	5,600	.7	No	8,000
11	5,600	.8	Yes	10,360

NOTE.—Terms are explained in text.

Ashenfelter and Plant (1990)

- NIT payments owed to the treatment group can be decomposed into two pieces:
  - Mechanical: Payments that would have been owed if behavior didn't change
  - Behavioral: Extra payments owed because behavior changes
- The behavioral piece comes from households that "opt in" to the program, and eligible households that reduce labor supply
- Ashenfelter and Plant compute the NIT payments owed to the treatment group
- Then subtract predicted payments that would have been owed to the control group – this captures the mechanical piece
- The remaining payments capture the behavioral response to the program



C
Mechanical
Mechanical + behavioral
T
L

Table 3
Experimental Payment minus Predicted Control Payment for 3-Year Dual-headed Experimental Families, Attrition Families Excluded (Standard Errors in Parentheses)

G ($)	τ	Declining Tax Rate	Preexperimental Payment ($)	Payments for Year of Experiment ($) 1	2	3	Postexperimental Payment ($)
3,800	.5	No	193.78	248.46	368.95*	389.24*	138.56
			(143.45)	(149.58)	(170.75)	(182.99)	(188.20)
3,800	.7	No	124.96	185.18	317.28	218.37	−47.85
			(223.77)	(237.91)	(252.99)	(325.57)	(314.66)
3,800	.7	Yes	−33.37	68.94	158.44	324.84	29.28
			(178.05)	(176.07)	(213.59)	(230.50)	(222.42)
3,800	.8	Yes	75.40	336.06	221.54	160.83	91.52
			(229.44)	(237.18)	(245.92)	(264.53)	(261.84)
4,800	.5	No	52.02	85.17	294.55	337.23	70.22
			(192.31)	(184.85)	(201.73)	(221.73)	(219.58)
4,800	.7	No	220.76	288.33	496.85*	543.25*	178.32
			(160.04)	(169.04)	(197.88)	(204.50)	(194.03)
4,800	.7	Yes	136.99	281.98*	423.30*	348.03*	23.96
			(127.36)	(137.19)	(157.51)	(162.38)	(140.58)
4,800	.8	Yes	−16.87	305.09	417.90	317.39	121.47
			(175.54)	(209.24)	(234.32)	(274.11)	(239.59)
5,600	.5	No	−163.12	200.75	664.41*	717.15*	124.93
			(252.05)	(258.13)	(283.28)	(280.65)	(287.04)
5,600	.7	No	−59.97	23.34	386.12	744.94*	267.69
			(164.95)	(156.41)	(200.59)	(263.80)	(259.45)
5,600	.8	Yes	−27.64	−51.03	117.85	273.44	121.53
			(121.47)	(126.67)	(138.52)	(157.96)	(169.26)

NOTE.—Terms are explained in text.
* Denotes mean is more than twice its standard error.

Ashenfelter and Plant (1990)

- The introduction of an NIT reduced labor supply, consistent with our theory
- As expected, effects are larger for more generous NITs
- But there are some problems with the SIME/DIME experiment
- Income was measured by a survey of experimental participants
  - The treatment group had an incentive to under-report earnings
  - Many households didn't answer the survey (attrition)
  - Response rates were higher for the treatment group
- These problems cast some doubt on the experimental results

Eissa and Liebman (1996): The EITC

- Eissa and Liebman (1996) study the Earned Income Tax Credit (EITC)
- The EITC is a refundable tax credit administered through the income tax system
- This credit transfers money to low-income workers
- Much more generous for households with children
- The EITC significantly expanded in a series of reforms starting in 1986
- Now a more important part of the social safety net than traditional NITs (costs $56 billion annually vs. $17 billion for TANF)
- No randomized trial for the EITC – we need another strategy to identify its effect

The Structure of the EITC

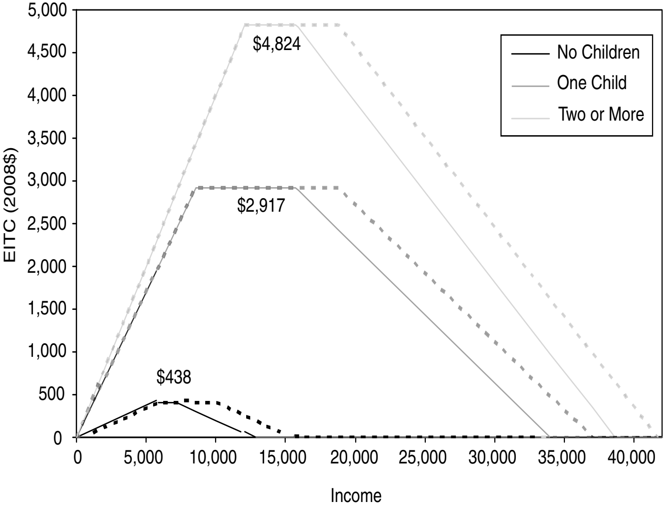
- The EITC benefit is zero for individuals who don't work
- It then provides an earnings subsidy at low levels of earnings, up to a maximum credit
- The credit is constant at the maximum over a range of earnings
- Finally, it phases out like a traditional NIT

Table 2 EITC Parameters, Tax Year 2008

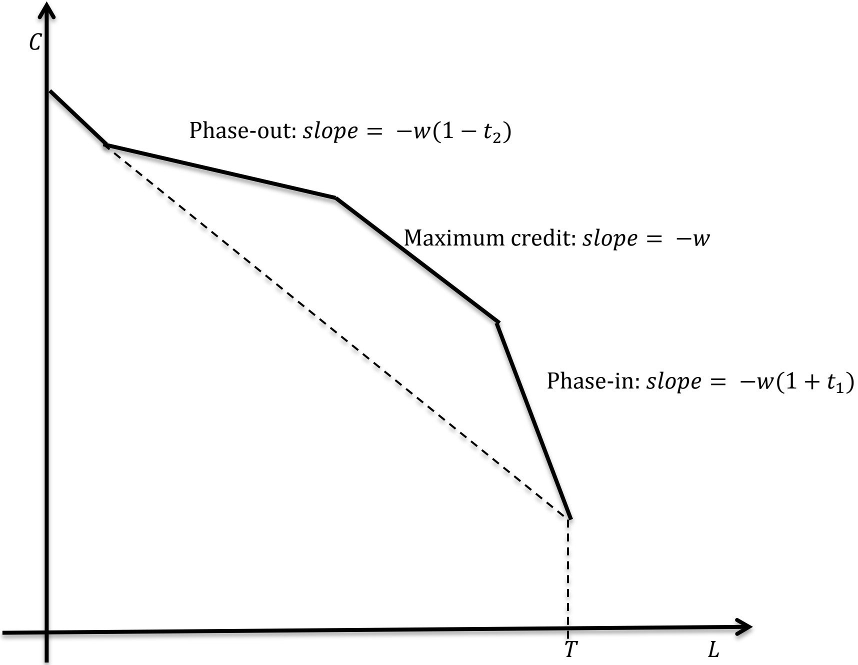
	Single, No Qualifying Children	Single, One Qualifying Child	Single, Two+ Qualifying Children	Married, No Qualifying Children	Married, One Qualifying Child	Married, Two+ Qualifying Children
Phase-In Rate	7.65%	34.00%	40.00%	7.65%	34.00%	40.00%
Phase-In Ends	$5,720	$8,580	$12,060	$5,720	$8,580	$12,060
Maximum Credit	$438	$2,917	$4,824	$438	$2,917	$4,824
Phase-Out Begins	$7,160	$15,740	$15,740	$10,160	$18,740	$18,740
Phase-Out Rate	7.65%	15.98%	21.06%	7.65%	15.98%	21.06%
Eligibility Ceiling	$12,880	$33,995	$38,646	$15,880	$36,995	$41,646

Source: Minnesota House Research Department.

Figure 1 EITC Structure by Income, Tax Year 2008

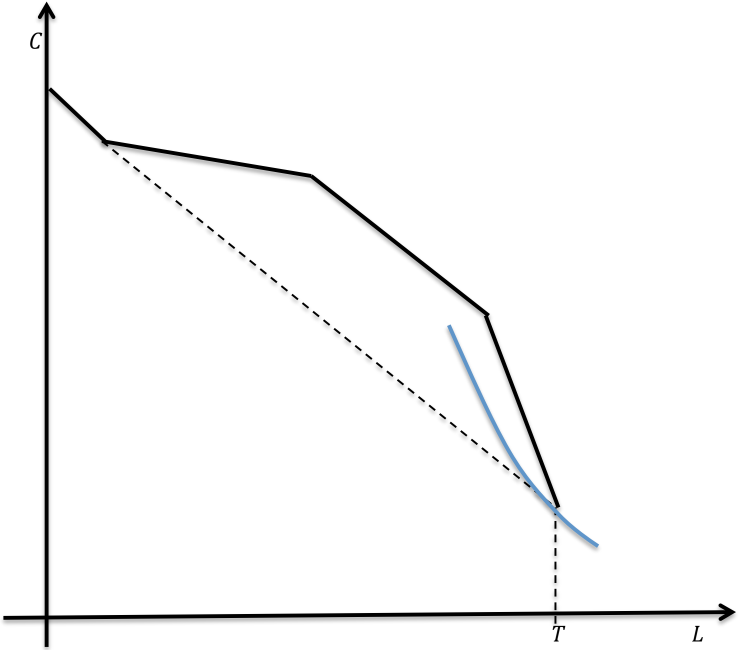


Note: Solid line represents single/head of household filers; dashed line represents married filers.


C
Phase-out: $slope = -w(1 - t_2)$
Maximum credit: $slope = -w$
Phase-in: $slope = -w(1 + t_1)$
T
L

Responses to the EITC

- The EITC creates complicated labor supply incentives
- Think about responses of:
  - People who aren't working
  - People in the phase-in range
  - People in the maximum credit range
  - People in the phase-out range
  - People who earn too much to receive the credit



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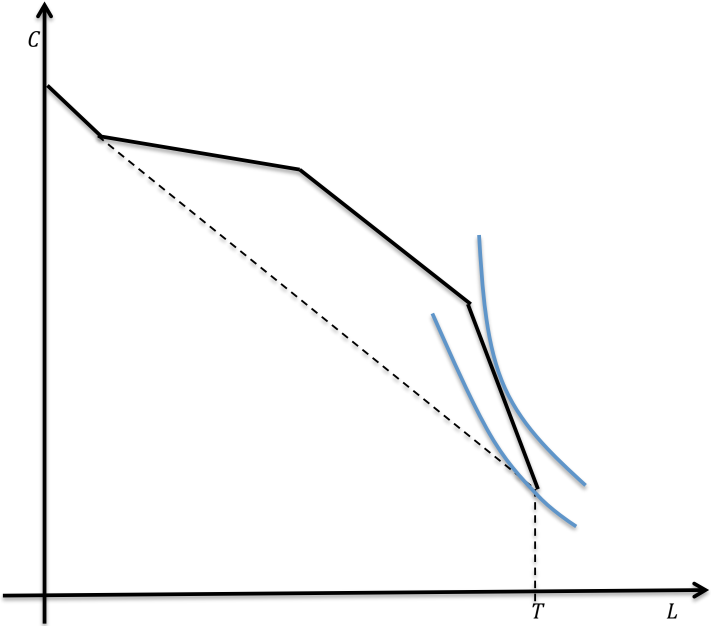
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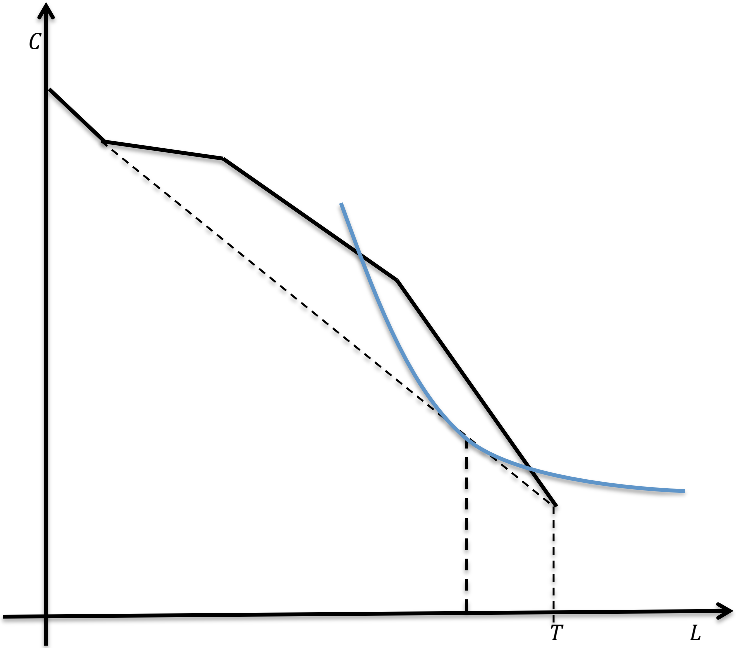
Responses to the EITC

- People who aren't working receive no benefit from the EITC
- Some people may enter the labor market to receive the credit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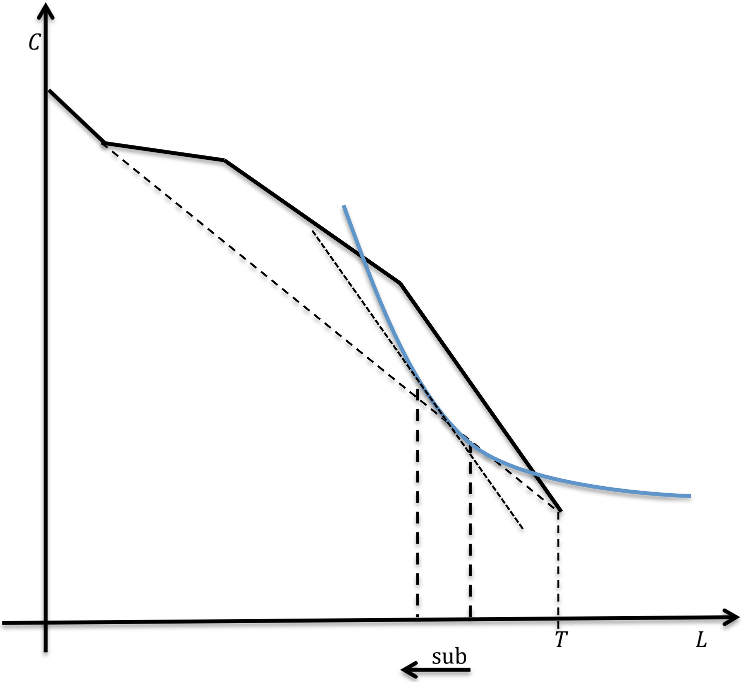
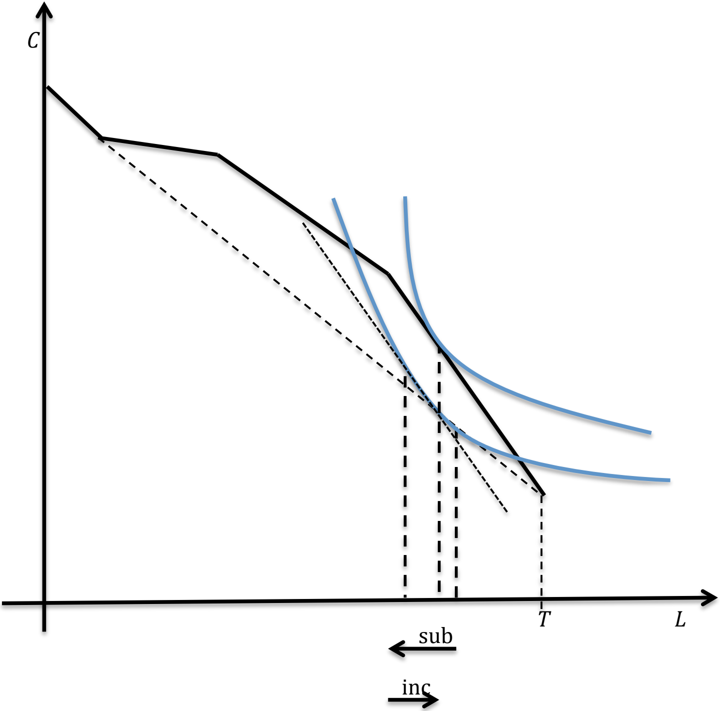
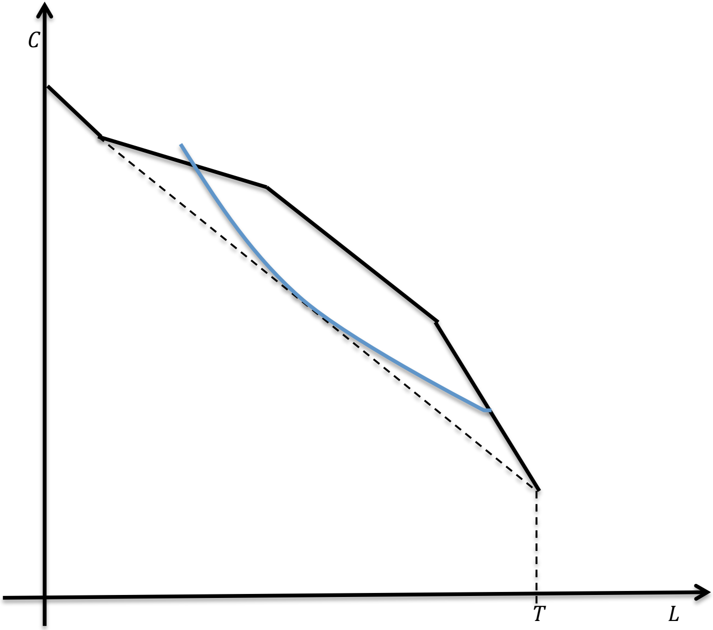


C
T
L
sub


C
T
L
sub
inc

Responses to the EITC

- People in the phase-in range get a wage increase and an increase in real income
- The substitution and income effects therefore work in opposite directions, and their response is ambiguous
- Note, however, that no one will exit the labor market



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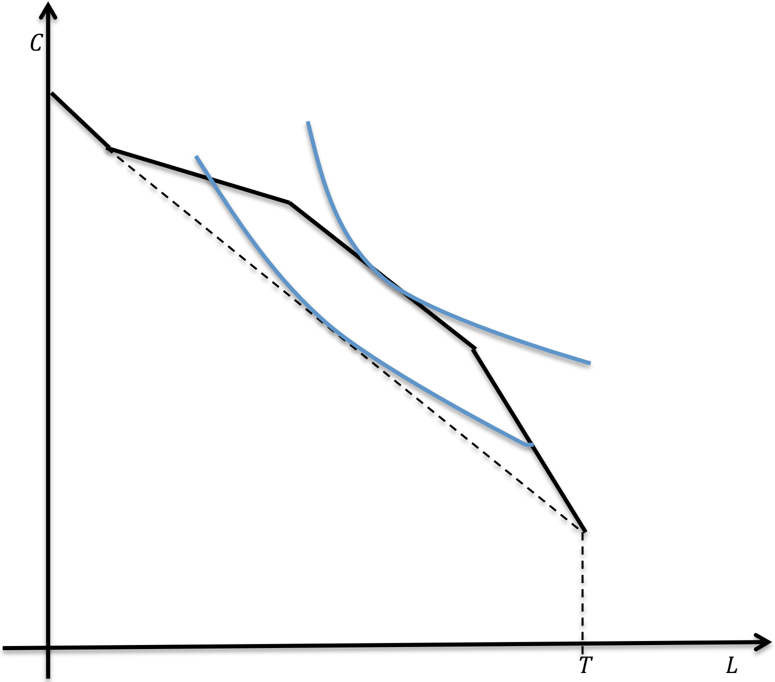
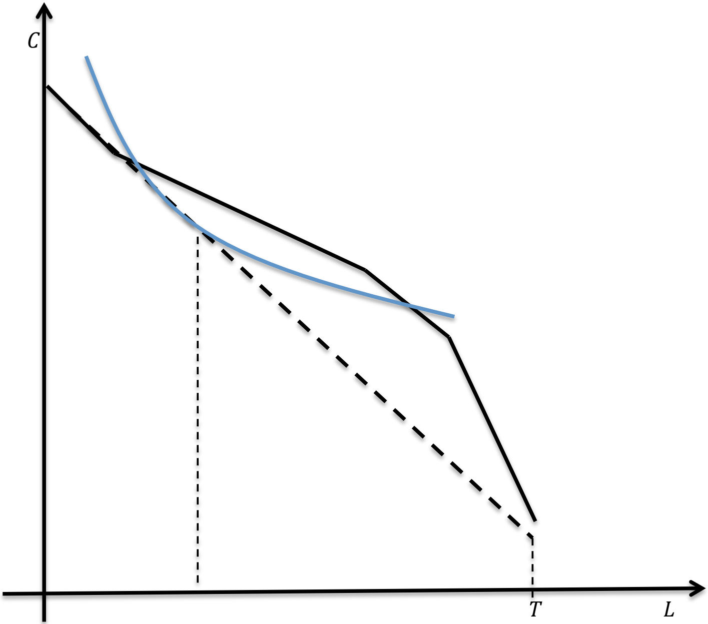
T
L



C
T
L

Responses to the EITC

- People in the maximum credit range receive an increase in real income
- The income effect pushes down labor supply, and there is no substitution effect
- Their labor supply will therefore decrease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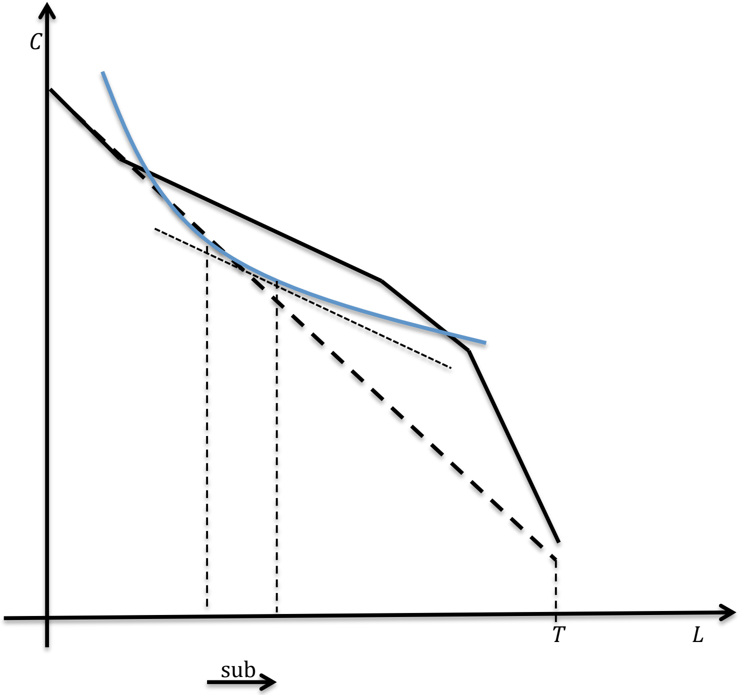
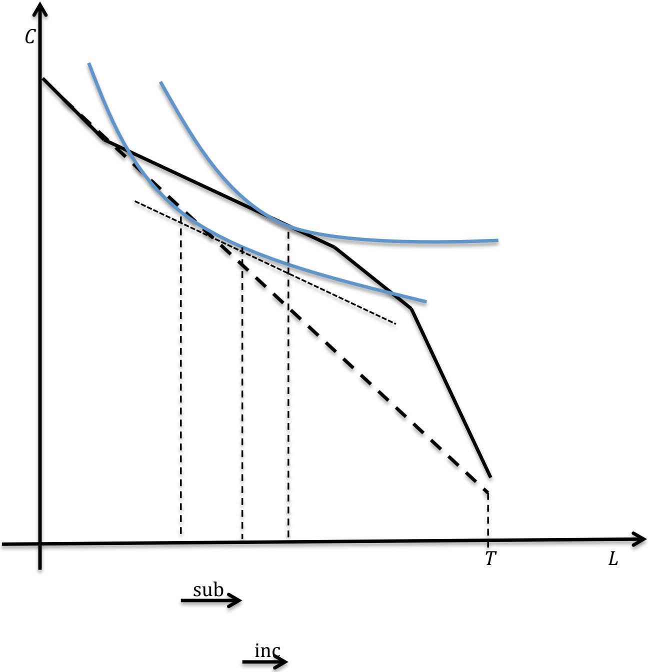

C
T
L
sub


C
T
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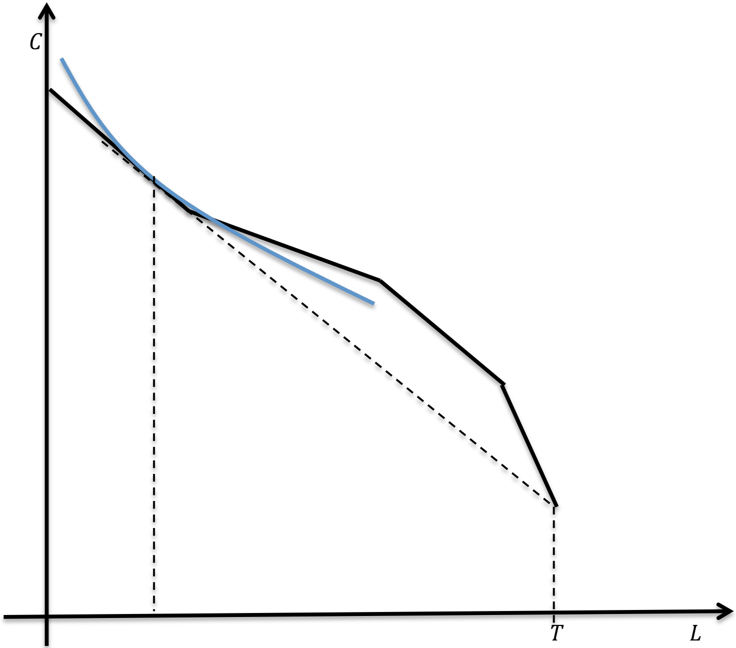
sub
inc

Responses to the EITC

- People in the phase-out range get lower wages and an increase in real income
- The substitution and income effects both push them to reduce labor supply
- Their labor supply will therefore decrease



C
T
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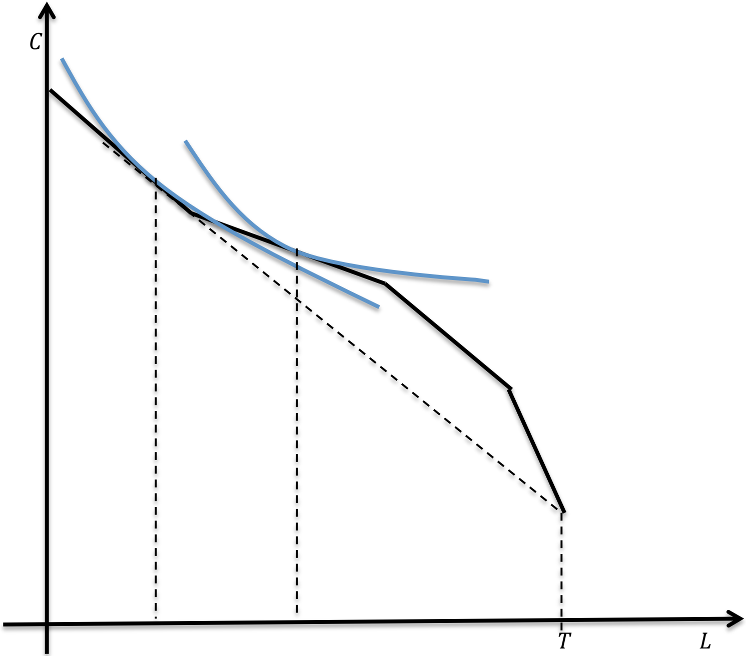


C
T
L

Responses to the EITC

- People who earn too much to be eligible get no benefit unless they reduce labor supply
- Some people may work less to “opt in” to the program and receive a credit

Eissa and Liebman (1996)

- Eissa and Liebman seek to understand the labor supply effects of a 1986 expansion of the EITC
- They focus on single women
- Problem: EITC eligibility is not randomly assigned
- Eissa and Liebman exploit the fact that people without children were ineligible for the EITC
- Single women without kids are therefore a natural "control group" for single women with kids

Eissa and Liebman (1996)

- Think of EITC expansion as "treatment," and let Y_{treat}^{pre} and Y_{treat}^{post} denote labor supply for women with kids before and after
- $Y_{control}^{pre}$ and $Y_{control}^{post}$ denolate labor supply for women without kids before and after
- Two candidate estimates of the effect of the EITC expansion:
  1. $Y_{treat}^{post} - Y_{treat}^{pre}$
  2. $Y_{treat}^{post} - Y_{control}^{post}$
- What could be wrong with these two estimates?

Difference in Differences

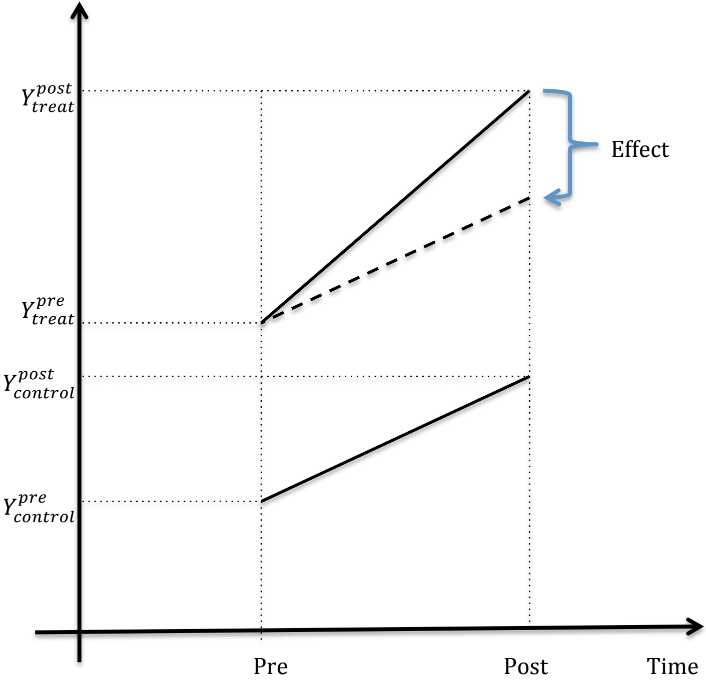
- A pre/post comparison for women with kids may capture changes in the labor market other than EITC expansion
- A comparison of women with and without kids in the post period may capture general differences in the behavior of these groups
- Consider a third estimator:

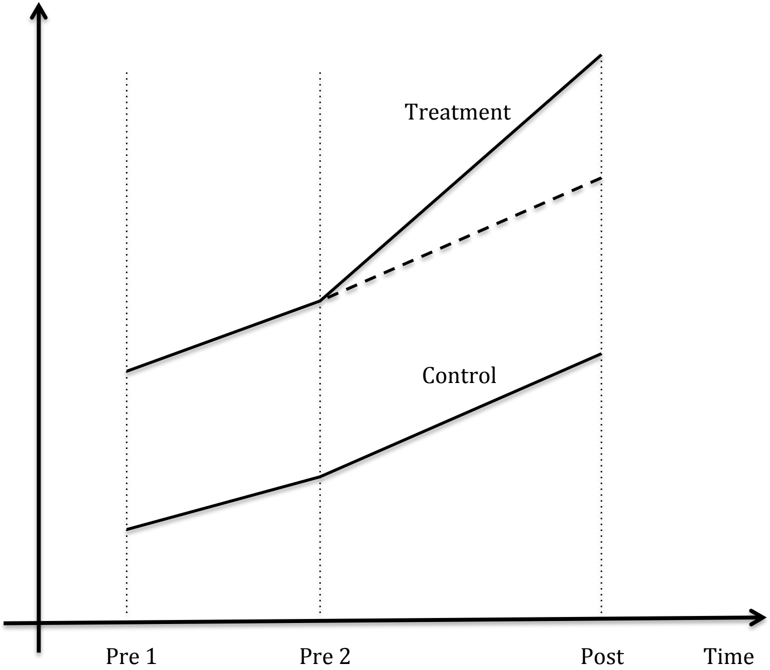
$$\left(Y_{treat}^{post} - Y_{treat}^{pre}\right) - \left(Y_{control}^{post} - Y_{control}^{pre}\right)$$

- This estimator uses the pre/post change for the treatment group, and removes the corresponding change for the control group
- This is known as a **difference in differences**, or "DD", approach

Difference in Differences

- Key DD assumption: The change for the control group captures what would've happened to the treatment group in the absence of treatment
- This is known as the "parallel trends" assumption: If the treatment hadn't happened, outcomes for the two groups would've moved in parallel
- This assumption is more plausible if outcomes appear to move in parallel in other periods when the treatment didn't change


Y_{treat}^{post}
Effect
Y_{treat}^{pre}
$Y_{control}^{post}$
$Y_{control}^{pre}$
Pre
Post
Time


Treatment
Control
Pre 1
Pre 2
Post
Time

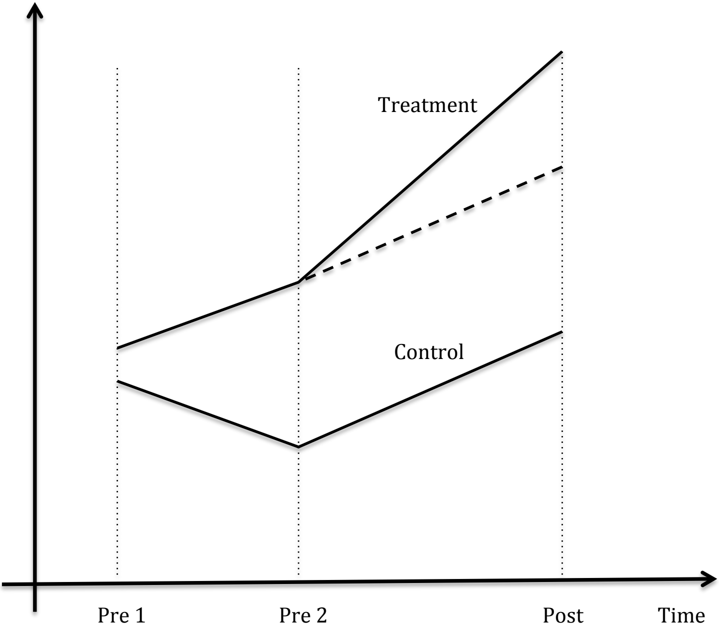
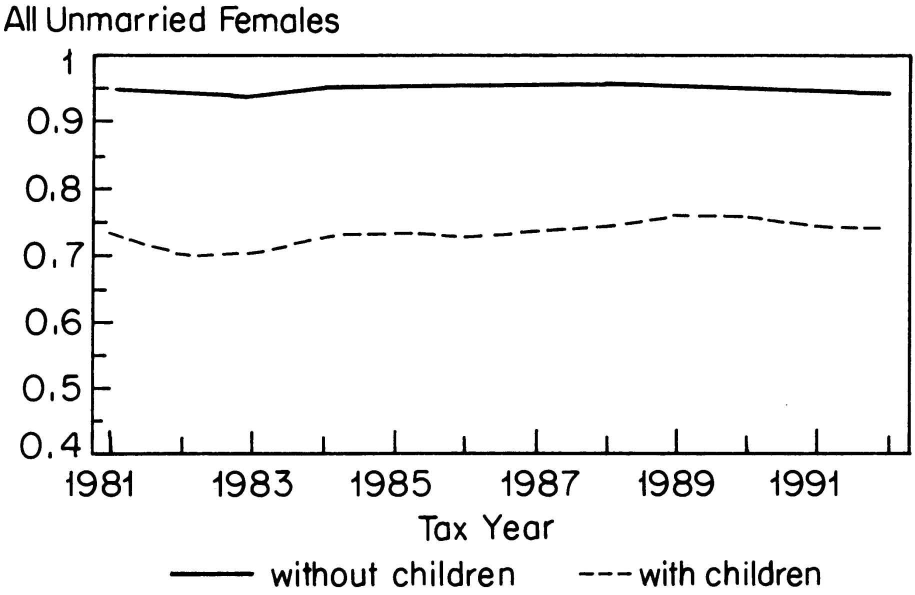

Treatment
Control
Pre 1
Pre 2
Post
Time

TABLE II

LABOR FORCE PARTICIPATION RATES OF UNMARRIED WOMEN

	Pre-TRA86 (1)	Post-TRA86 (2)	Difference (3)	Difference-in-differences (4)
A. *Treatment group:*				
With children [20,810]	0.729 (0.004)	0.753 (0.004)	0.024 (0.006)	
Control group:				
Without children [46,287]	0.952 (0.001)	0.952 (0.001)	0.000 (0.002)	*0.024 (0.006)*
B. *Treatment group:*				
Less than high school, with children [5396]	0.479 (0.010)	0.497 (0.010)	0.018 (0.014)	
Control group 1:				
Less than high school, without children [3958]	0.784 (0.010)	0.761 (0.009)	−0.023 (0.013)	*0.041 (0.019)*
Control group 2:				
Beyond high school, with children [5712]	0.911 (0.005)	0.920 (0.005)	0.009 (0.007)	*0.009 (0.015)*
C. *Treatment group:*				
High school, with children [9702]	0.764 (0.006)	0.787 (0.006)	0.023 (0.008)	
Control group 1:				
High school, without children [16,527]	0.945 (0.002)	0.943 (0.003)	−0.002 (0.004)	*0.025 (0.009)*
Control group 2:				
Beyond high school, with children [5712]	0.911 (0.005)	0.920 (0.005)	0.009 (0.007)	*0.014 (0.011)*

Data are from the March CPS, 1985–1987 and 1989–1991. Pre-TRA86 years are 1984–1986. Post-TRA86 years are 1988–1990. Labor force participation equals one if annual hours are positive, zero otherwise. Standard errors are in parentheses. Sample sizes are in square brackets. Means are weighted with CPS March supplement weights.


All Unmarried Females
1
0.9
0.8
0.7
0.6
0.5
0.4
1981
1983
1985
1987
1989
1991
Tax Year
without children
with children

Eissa and Liebman (1996)

- Eissa and Liebman show that between 1984 and 1990, labor force participation for unmarried women with kids trended up relative to unmarried women without kids
- This suggests the EITC increased LFP, consistent with our theoretical prediction
- Parallel trends assumption looks OK in other periods