Labor Supply Responses to Taxes and Transfers

131 Undergraduate Public Economics
Emmanuel Saez
UC Berkeley
MOTIVATION

1) Labor supply responses to taxation are of fundamental importance for income tax policy [efficiency costs and optimal tax formulas]

2) Labor supply responses along many dimensions:

(a) Intensive: hours of work on the job, intensity of work, occupational choice [including education]

(b) Extensive: whether to work or not [e.g., retirement and migration decisions]

3) Reported earnings for tax purposes can also vary due to (a) tax avoidance [legal tax minimization], (b) tax evasion [illegal under-reporting]

4) Different responses in short-run and long-run: long-run response most important for policy but hardest to estimate
**STATIC MODEL: SETUP**

Baseline model (same as previous lecture):

Let $c$ denote consumption and $l$ hours worked, utility $u(c, l)$ increases with $c$, and decreases with $l$

Individual earns wage $w$ per hour (net of taxes) and has $R$ in non-labor income

Individual solves

$$\max_{c,l} u(c, l) \text{ subject to } c = wl + R$$
LABOR SUPPLY BEHAVIOR

FOC: \( w_\partial u/\partial c + \partial u/\partial l = 0 \) defines uncompensated (Marshallian) labor supply function \( l^u(w, R) \)

Uncompensated elasticity of labor supply: \( \varepsilon^u = (w/l)\partial l^u/\partial w \) [% change in hours when net wage \( w \) increases by 1%]

Income effect parameter: \( \eta = w\partial l/\partial R \leq 0 \): $ increase in earnings if person receives $1 extra in non-labor income

Compensated (Hicksian) labor supply function \( l^c(w, u) \) which minimizes cost \( wl - c \) st to constraint \( u(c, l) \geq u \).

Compensated elasticity of labor supply: \( \varepsilon^c = (w/l)\partial l^c/\partial w > 0 \)

Slutsky equation: \( \partial l/\partial w = \partial l^c/\partial w + l\partial l/\partial R \Rightarrow \varepsilon^u = \varepsilon^c + \eta \)
BASIC CROSS SECTION ESTIMATION

Data on hours or work, wage rates, non-labor income started becoming available in the 1960s when first micro surveys and computers appeared:

Simple OLS (Ordinary Least Square) regression:

\[ l_i = \alpha + \beta w_i + \gamma R_i + X_i \delta + \epsilon_i \]

\( w_i \) is the net-of-tax wage rate

\( R_i \) measures non-labor income [including spousal earnings for couples]

\( X_i \) are demographic controls [age, experience, education, etc.]

\( \beta \) measures uncompensated wage effects, and \( \gamma \) measures income effects [can be converted to \( \varepsilon^u, \eta \)]

a) Small effects $\varepsilon^u = 0$, $\eta = -0.1$, $\varepsilon^c = 0.1$ with some variation across estimates

2. Female workers [secondary earners when married] (Killingsworth and Heckman, 1986):

Much larger elasticities on average, with larger variations across studies. Elasticities go from zero to over one. Average around 0.5. Significant income effects as well

Female labor supply elasticities have declined overtime as women become more attached to labor market (Blau-Kahn JOLE’07)
ISSUE WITH OLS REGRESSION:

\[ w_i \] correlated with tastes for work \( \epsilon_i \)

\[ l_i = \alpha + \beta w_i + \epsilon_i \]

Identification is based on cross-sectional variation in \( w_i \): comparing hours of work of highly skilled individuals (high \( w_i \)) to hours of work of low skilled individuals (low \( w_i \))

If highly skilled workers have more taste for work (independent of the wage effect), then \( \epsilon_i \) is positively correlated with \( w_i \) leading to an upward bias in OLS regression

Plausible scenario: hard workers acquire better education and hence have higher wages

Controlling for \( X_i \) can help but can never be sure that we have controlled for all the factors correlated with \( w_i \) and tastes for work: **Omitted variable bias** \( \Rightarrow \) Tax changes provide more compelling identification
Negative Income Tax (NIT) Experiments

1) Best identification method: exogenously increase the tax rate / non-labor income with a randomized experiment

2) NIT experiment conducted in 1960s/70s in Denver, Seattle, and other cities

3) First major social experiment in U.S. designed to test proposed transfer policy reform

4) Lump-sum transfers $G$ combined with a steep phaseout rate $\tau$ (50%-80%) [based on family earnings] for 3 or 5 years.


6) Several groups, with randomization within each; approx. $N = 75$ households in each group
<table>
<thead>
<tr>
<th>Program Number</th>
<th>$G$ ($)</th>
<th>$\tau$</th>
<th>Declining Tax Rate</th>
<th>Break-even Income ($)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>3,800</td>
<td>.5</td>
<td>No</td>
<td>7,600</td>
</tr>
<tr>
<td>2</td>
<td>3,800</td>
<td>.7</td>
<td>No</td>
<td>5,429</td>
</tr>
<tr>
<td>3</td>
<td>3,800</td>
<td>.7</td>
<td>Yes</td>
<td>7,367</td>
</tr>
<tr>
<td>4</td>
<td>3,800</td>
<td>.8</td>
<td>Yes</td>
<td>5,802</td>
</tr>
<tr>
<td>5</td>
<td>4,800</td>
<td>.5</td>
<td>No</td>
<td>9,600</td>
</tr>
<tr>
<td>6</td>
<td>4,800</td>
<td>.7</td>
<td>No</td>
<td>6,857</td>
</tr>
<tr>
<td>7</td>
<td>4,800</td>
<td>.7</td>
<td>Yes</td>
<td>12,000</td>
</tr>
<tr>
<td>8</td>
<td>4,800</td>
<td>.8</td>
<td>Yes</td>
<td>8,000</td>
</tr>
<tr>
<td>9</td>
<td>5,600</td>
<td>.5</td>
<td>No</td>
<td>11,200</td>
</tr>
<tr>
<td>10</td>
<td>5,600</td>
<td>.7</td>
<td>No</td>
<td>8,000</td>
</tr>
<tr>
<td>11</td>
<td>5,600</td>
<td>.8</td>
<td>Yes</td>
<td>10,360</td>
</tr>
</tbody>
</table>

Source: Ashenfelter and Plant (1990), p. 403
Negative Income Tax Experiment

\[ c = z - T(z) \]

NIT Treatment: Transfer \( G \) phased-out with earnings \( z \) at tax rate \( \tau \)

slope\(=1-\tau\)

Control group: slope\(=1\)
Negative Income Tax Experiment

\[ c = z - T(z) \]

NIT Treatment
Negative income and substitution effects on \( z \)

slope = 1 - \( \tau \)

Control group: slope = 1
NIT Experiments: Findings

1) Significant labor supply response but small overall

2) Implied earnings elasticity for males around 0.1

3) Implied earnings elasticity for married women around 0.5

4) Response of married women is concentrated along the extensive margin (dropping out of work)
From true experiment to “natural experiments”:
Estimating income effects with lottery winnings

True experiments are costly to implement and hence rare

However, real economic world (nature) provides variation that can be exploited to estimate behavioral responses ⇒ “Natural Experiments”

Natural experiments sometimes come very close to true experiments: Imbens, Rubin, Sacerdote AER ’01 did a survey of lottery winners and non-winners matched to Social Security administrative data to estimate income effects

Lottery generates random assignment conditional on playing

Find significant but small income effects: \( \eta = \frac{w \partial l}{\partial R} \) between -0.05 and -0.10. $1 in lottery reduces earnings by 5-10¢.

Identification threat: differential response-rate among groups
Type accounts, including IRA's, 401(k) plans, and other retirement-related savings. The second consists of stocks, bonds, and mutual funds and general savings. We construct an additional variable "total financial wealth," adding up the two savings categories. Wealth in the various savings accounts is somewhat higher than net wealth in housing, $133,000 versus $122,000. The distributions of these financial wealth variables are very skewed with, for example, wealth in mutual funds for the 414 respondents ranging from zero to $1.75 million, with a mean of $53,000, a median of $10,000, and 35 percent zeros.

The critical assumption underlying our analysis is that the magnitude of the lottery prize is random. Given this assumption the background characteristics and pre-lottery earnings should not differ significantly between nonwinners and winners. However, the t-statistics in Table 1 show that nonwinners are significantly more educated than winners, and they are also older. This likely reflects the differences between season ticket holders and single ticket buyers as the differences between all winners and the big winners tend to be smaller. To investigate further whether the assumption of random assignment of lottery prizes is more plausible within the more narrowly defined subsamples, we regressed the lottery prize on a set of 21 pre-lottery variables (years of education, age, number of tickets bought, year of winning, earnings in six years prior to winning, dummies for sex, college, age over 55, age over 65, for working at the time of winning, and dummies for positive earnings in six years prior to winning). Testing for the joint significance of all 21 covariates in the full sample of 496 observations led to a chi-squared statistic of 99.9 (dof 21), highly significant (p < 0.001). In the sample of 237 winners, the chi-squared statistic was 64.5, again highly significant (p < 0.001). In the sample of 193 small winners, the chi-squared statistic was 28.6, not significant at the 10-percent level. This provides some support for assumption of random assignment of the lottery prizes, at least within the subsample of small winners.

Note: Solid line = nonwinners; dashed line = winners; dotted line = big winners.

On average the individuals in our basic sample won yearly prizes of $26,000 (averaged over the $55,000 for winners and zero for nonwinners). Typically they won 10 years prior to completing our survey in 1996, implying they are on average halfway through their 20 years of lottery payments when they responded in 1996. We asked all individuals how many tickets they bought in a typical week in the year they won the lottery.11 As expected, the number of tickets bought is considerably higher for winners than for nonwinners. On average, the individuals in our basic sample are 50 years old at the time of winning, which, for the average person was in 1986; 35 percent of the sample was over 55 and 15 percent was over 65 years old at the time of winning; 63 percent of the sample was male. The average number of years of schooling, calculated as years of high school plus years of college plus 8, is equal to 13.7; 64 percent claimed at least one year of college.

We observe, for each individual in the basic sample, Social Security earnings for six years preceding the time of winning the lottery, for the year they won (year zero), and for six years following winning. Average earnings, in terms of 1986 dollars, rise over the pre-winning period from $13,930 to $16,330, and then decline back to $13,290 over the post-winning period. For those with positive Social Security earnings, average earnings rise over the entire 13-year period from $20,180 to $24,300. Participation rates, as measured by positive Social Security earnings, gradually decline over the 13 years, starting at around 70 percent before going down to 56 percent. Figures 1 and 2 present graphs for average earnings and the proportion of individuals with positive earnings for the three groups, nonwinners, winners, and big winners. One can see a modest decline in earnings and proportion of individuals with positive earnings for the full winner sample compared to the nonwinners after winning the lottery, and a sharp and much larger decline for big winners at the time of winning. A simple difference-in-differences type estimate of the marginal propensity to earn out of unearned income (mpe) can be based on the ratio of the difference in the average change in earnings before and after winning the lottery for two groups and the difference in the average prize for the same two groups. For the winners, the difference in average earnings over the six post-lottery years and the six pre-lottery years is -$1,877 and for the nonwinners the average change is $448. Given a difference in average prize of $55,000 for the winner/nonwinners comparison, the estimated mpe is (-1,877 - 448)/(55,000 - 0) = -0.042 (SE 0.016). For the big-winners/small-winners comparison, this estimate is -0.059 (SE 0.018). In Section IV we report estimates for this quantity using more sophisticated analyses.

On average the value of all cars was $18,200. For housing the average value was $166,300, with an average mortgage of $44,200.12 We aggregated the responses to financial wealth into two categories. The first concerns retirement

11 Because there were some extremely large numbers (up to 200 tickets per week), we transformed this variable somewhat arbitrarily by taking the minimum of the number reported and ten. The results were not sensitive to this transformation.

12 Note that this is averaged over the entire sample, with zeros included for the 7 percent of respondents who reported not owning their homes.
Labor Supply Substitution Effects: Tax Free Second Jobs in Germany

In 2003, Germany made secondary jobs (paying less than 400 Euros/month) tax free: amounts to a 20-60% subsidy on second job earnings: substitution labor supply effect

Tazhitdinova '22 uses social security admin monthly earnings data

Fraction of population holding second jobs increased sharply (from 2.5% to 6-7%) with bigger response overtime

Finds no offsetting effect on primary earnings ⇒ People did work more

Likely happened because employers willing to create lots of mini-jobs to accommodate supply
Figure 4: Secondary Job Holding Rates by Secondary Earnings Level
Source: Tazhitdinova (2019)

(a) same axis

Notes: This figure shows the share of individuals with secondary jobs paying less than €400 per month, paying between €400 and €1000, or more than €1000 per month. The vertical red line identifies the 2003 tax reform.

Source: Sample of Integrated Labour Market Biographies (SIAB) 1975 - 2010, Nuremberg 2013.

Electronic copy available at: https://ssrn.com/abstract=3047332

Source: Tazhitdinova (2019)
Responses to Low-Income Transfer Programs

1) Particular interest in treatment of low incomes in a progressive tax/transfer system: are they responsive to incentives?

2) Complicated set of transfer programs in US

   a) In-kind: food stamps (SNAP), Medicaid, public housing, job training, education subsidies

   b) Cash: TANF, EITC, SSI

US government (fed+state and local) spent $1000bn in 2016 on income-tested programs

   a) About 50% is health care (Medicaid)

   b) Only $250 billion in cash
1996 US Welfare Reform

1) Largest change in welfare policy

2) Reform modified AFDC cash welfare program to provide more incentives to work (renamed TANF)
   a) Requiring recipients to go to job training or work
   b) Limiting the duration of benefits (5 year max lifetime)
   c) Reducing phase-out rate of benefits

3) States got welfare waivers from Federal government to experiment during 1992-1996 before Federal welfare reform

4) EITC also expanded during this period: general shift from welfare to “workfare”

Did welfare reform and EITC increase labor supply?
Figure 1: Long-Run Evolution of EITC and Cash Welfare

Source: Internal Revenue Service (EITC) and Department of Health and Human Services (AFDC/TANF).

Notes: The red series show the annual number of federal EITC recipients between 1966-2016. The blue series show the average monthly number of Aid to Families with Dependent Children (AFDC) recipients between 1966-1996, and the average monthly number of Temporary Assistance for Needy Families (TANF) recipients between 1997-2016.
SNAP helps households with limited resources to purchase adequate food. Some 15 million households, with 40 million people, were food insecure in 2017. Studies show that SNAP benefits have reduced food insecurity for those households.

Protecting the overall economy
SNAP benefits are one of the fastest, most effective forms of economic stimulus because they get money into the economy quickly during a recession. Low-income individuals generally spend all of their income meeting daily needs such as shelter, food, and transportation, so every dollar in SNAP that a low-income family receives enables the family to spend an additional dollar on food or other items. Some 80 percent of SNAP benefits are redeemed within two weeks of receipt and 97 percent are spent within a month.

Moody's Analytics estimated that every $1 increase in SNAP benefits during 2009, when the economy was in a recession, generated about $1.70 in economic activity. Similarly, CBO has found that SNAP has one of the largest “bangs-for-the-buck” (i.e., increase in economic activity and employment per budgetary dollar spent) among a broad range of policies for stimulating economic growth and creating jobs in a weak economy.

SNAP Tracks Changes in Share of Population That Is Poor or Near-Poor

Note: Poverty estimates are annual estimates. SNAP shares of resident population are calendar year averages.
Sources: U.S. Census Bureau, U.S. Department of Agriculture
Randomized welfare experiment: 
SSP Welfare Demonstration in Canada

Canadian Self Sufficiency Project (SSP): randomized experiment that gave welfare recipients an earnings subsidy for 36 months in 1990s (but need to start working by month 12 to get it)

3 year temporary participation tax rate cut from average rate of 74.3% to 16.7% [get to keep 83 cents for each $ earned instead of 26 cents]

Card and Hyslop (EMA 2005) provide classic analysis. Two results:

1) Strong effect on employment rate during experiment (peaks at 14 points)

2) Effect quickly vanishes when the subsidy stops after 36 months (entirely gone by month 52)
and control groups. Unfortunately, these data have some critical limitations relative to the administratively based Income Assistance data. Most importantly, they are only available for 52 months after random assignment. Since some program group members were still receiving subsidy payments as late as month 52, this time window is too short to assess the long-run effects of the program. Indeed, looking at Figure 1a, there is still an impact on IA participation in month 52 that does not fully dissipate until month 69. Second, because of nonresponses and refusals, labor market information is only available for 85% of the experimental sample (4,757 people). Nevertheless, the labor market outcomes provide a valuable complement to the administratively based welfare participation data.

Figure 3 shows the average monthly employment rates of the program and control groups, along with the associated experimental impacts. After random assignment the employment rate of the control group shows a steady rise, while the program group shows a sharp increase in the first few months after random assignment, followed by a more gradual increase. The difference between the two groups is significant and persists over time.

18The distribution of response patterns to the 18-, 36-, and 54-month surveys is fairly similar for the program and control groups (chi-squared statistic = 11.4 with 7 degrees of freedom, p-value = 0.12). However, a slightly larger fraction of the program group have complete labor market data for 52 months—85.4% versus 84.0% for the controls. Moreover, the difference in mean IA participation between the treatment and control groups in month 52 is a little different in the overall sample (2.5%) than in the subset with complete labor market histories (3.3%).

19Each of the three post-random-assignment surveys asked people about their labor market outcomes in the 18 months since the previous survey. Many people report constant earnings over the recall period, leading to a pattern of measured pay increases that are concentrated at the seams, rather than occurring more smoothly over the recall period.

Source: Card and Hyslop, 2005, p. 1734

![Figure 3. Monthly employment rates.](source)

**FIGURE 3.**—Monthly employment rates.

Source: Card and Hyslop, 2005, p. 1734
Earned Income Tax Credit (EITC) program

1) EITC started small in the 1970s but was expanded in 1986-88, 1994-96, 2008-09: today, largest means-tested cash transfer program [$75bn in 2019, 30m families recipients]

2) Eligibility: families with kids and low earnings.

3) Refundable Tax credit: administered through income tax as annual tax refund received in Feb-April, year $t+1$ (for earnings in year $t$)

4) EITC has flat pyramid structure with phase-in (negative MTR), plateau, (0 MTR), and phase-out (positive MTR)

5) Theoretically, EITC should encourage labor force participation (extensive labor supply margin)

Kleven (2019) who looks at participation of single women (aged 20-50) with kids (treatment) vs without kids (control)
EITC Schedule in 2017

- 0 children
- 1 child
- 2 children
- 3+ children
EITC Maximum Credit Over Time

Maximum Annual Credit (2017 USD)

Year

Source: Kleven (2018)
Labor Force Participation of Single Women
With and Without Children

Annual Employment
Low Education

Source: Kleven (2018)
Labor Force Participation of Single Women
With and Without Children

50 years of relative stability, apart from these 5 years

Unemployment Rate

Year

Source: Kleven (2018)
Labor Force Participation of Single Women
With and Without Children

50 years of relative stability, apart from these 5 years

Source: Kleven (2018)
Labor Force Participation of Single Women
With and Without Children

Source: Kleven (2018)
Labor Force Participation of Single Women
With and Without Children

Source: Kleven (2018)
Labor Force Participation of Single Women
With and Without Children

Source: Kleven (2018)
Labor Force Participation of Single Women
With and Without Children

Source: Kleven (2018)
Welfare Reform and EITC Expansion: Labor supply

Kleven (2019) who looks at participation of single women (aged 20-50) with kids (treatment) vs without kids (control)

Large increase in labor force participation of single mothers during the 1990s during welfare reform and EITC expansion

Unlikely that the EITC can explain it fully because other EITC changes haven’t generated such large effects

Sociological evidence shows that welfare reform “scared” single mothers into working

Single moms in the US were suddenly expected to work

Maybe a unique combination of EITC reform, welfare reform, economic upturn, and changing social norms lead to this shift
Theoretical Behavioral Responses to the EITC

**Extensive margin:** EITC makes work more attractive (vs. non-work) ⇒ positive effect on Labor Force Participation

**Intensive margin:** earnings conditional on working;

1) Phase in: (a) Substitution effect: work more due to 40% increase in net wage, (b) Income effect: work less ⇒ Net effect: ambiguous; probably work more

2) Plateau: Pure income effect (no change in net wage) ⇒ Net effect: work less

3) Phase out: (a) Substitution effect: work less, (b) Income effect: also work less ⇒ Net effect: work less
EITC and intensive labor supply

\[ c = z - T(z) \]

Budget with EITC
EITC and intensive labor supply

\[ c = z - T(z) \]

Negative income and substitution effects on \( z \)

Negative income effect positive substitution effects on \( z \)

Negative income effects on \( z \)

pre-tax income \( z \)
EITC and Intensive Labor Supply Response: Bunching at Kinks

1) Basic labor supply theory predicts that we should observe bunching of individuals at the EITC kink points:

Some individuals find it worthwhile to work more when subsidy rate is 40% (2 kids) but not when subsidy rate falls to 0% ⇒ Utility maximizing labor supply is to be exactly at the kink

2) Amount of bunching is proportional to compensated elasticity: if labor supply is inelastic, then kinks in the budget set are irrelevant and do not create bunching

Saez AEJ’10 finds bunching around 1st kink point of EITC but only for the self-employed ⇒ likely due to cheating to maximize tax refund (and not labor supply)
Elasticity would no longer be a pure compensated elasticity, but a mix of the compensated elasticity and the uncompensated elasticity. Four points should be noted. First, the larger the behavioral elasticity, the more bunching we should expect. Unsurprisingly, if there are no behavioral responses to marginal tax rates, there

Panel A . Indifference curves and bunching

Before tax income \( z \)

Individual \( L \) indifference curve

Individual \( H \) indifference curves

Panel B. Density distributions and bunching

Before reform density

After reform density

Pre-reform incomes between \( z^* \) and \( z^* + dz^* \) bunch at \( z^* \) after reform

Figure 1. Bunching Theory

Notes:

Panel A displays the effect on earnings choices of introducing a (small) kink in the budget set by increasing the tax rate \( t \) by \( dt \) above income level \( z^* \). Individual \( L \) who chooses \( z^* \) before the reform stays at \( z^* \) after the reform. Individual \( H \) chooses \( z^* \) after the reform and was choosing \( z^* + dz^* \) before the reform. Panel B depicts the effects of introducing the kink on the earnings density distribution. The pre-reform density is smooth around \( z^* \). After the reform, all individuals with income between \( z^* \) and \( z^* + dz^* \) before the reform, bunch at \( z^* \), creating a spike in the density distribution. The density above \( z^* + dz^* \) shifts to \( z^* \) (so that the resulting density and is no longer smooth at \( z^* \)).

Source: Saez (2010), p. 184
elasticity would no longer be a pure compensated elasticity, but a mix of the compensated elasticity and the uncompensated elasticity. Four points should be noted.

First, the larger the behavioral elasticity, the more bunching we should expect.

Unsurprisingly, if there are no behavioral responses to marginal tax rates, there

Panel A. Indifference curves and bunching

Before tax income $z$

Slope $1 - t$

$z^* - t - dt$

Individual $L$ chooses $z^*$ before and after reform

Individual $H$ chooses $z^* + dz^*$ before and after reform

$dz^*/z^* = e dt/(1 - t)$ with compensated elasticity

Panel B. Density distributions and bunching

Density distribution

Pre-reform incomes between $z^*$ and $z^* + dz^*$ bunch at $z^*$ after reform

After reform density

Before reform density

Before tax income $z$

Source: Saez (2010), p. 184
indexes earnings to 2008 using the IRS inflation parameters, so that the EITC kinks are perfectly aligned for all years.

Two elements are worth noting in Figure 3. First, there is a clear clustering of tax filers around the first kink point of the EITC. In both panels, the density is maximum exactly at the first kink point. The fact that the location of the first kink point differs between EITC recipients with one child, versus those with two or more children, constitutes strong evidence that the clustering is driven by behavioral responses to the EITC as predicted by the standard model. Second, however, we cannot discern any

Panel A. One child

Panel B. Two children or more

Figure 3. Earnings Density Distributions and the EITC

Notes:
The figure displays the histogram of earnings (by $500 bins) for tax filers with one dependent child (panel A) and tax filers with two or more dependent children (panel B). The histogram includes all years 1995–2004 and inflates earnings to 2008 dollars using the IRS inflation parameters (so that the EITC kinks are aligned for all years).

Earnings are defined as wages and salaries plus self-employment income (net of one-half of the self-employed payroll tax). The EITC schedule is depicted in dashed line and the three kinks are depicted with vertical lines. Panel A is based on 57,692 observations (representing 116 million tax returns), and panel B on 67,038 observations (representing 115 million returns).

Source: Saez (2010), p. 191
indexes earnings to 2008 using the IRS inflation parameters, so that the EITC kinks are perfectly aligned for all years.

Two elements are worth noting in Figure 3. First, there is a clear clustering of tax filers around the first kink point of the EITC. In both panels, the density is maximum exactly at the first kink point. The fact that the location of the first kink point differs between EITC recipients with one child, versus those with two or more children, constitutes strong evidence that the clustering is driven by behavioral responses to the EITC as predicted by the standard model. Second, however, we cannot discern any

Figure 3. Earnings Density Distributions and the EITC

Notes: The figure displays the histogram of earnings (by $500 bins) for tax filers with one dependent child (panel A) and tax filers with two or more dependent children (panel B). The histogram includes all years 1995–2004 and inflates earnings to 2008 dollars using the IRS inflation parameters (so that the EITC kinks are aligned for all years).

Earnings are defined as wages and salaries plus self-employment income (net of one-half of the self-employed payroll tax). The EITC schedule is depicted in dashed line and the three kinks are depicted with vertical lines. Panel A is based on 57,692 observations (representing 116 million tax returns), and panel B on 67,038 observations (representing 115 million returns).

Source: Saez (2010), p. 191
systematic clustering around the second kink point of the EITC. Similarly, we cannot discern any gap in the distribution of earnings around the concave kink point where the EITC is completely phased-out. This differential response to the first kink point, versus the other kink points, is surprising in light of the standard model predicting that any convex (concave) kink should produce bunching (gap) in the distribution of earnings.

In Figure 4, we break down the sample of earners into those with nonzero self-employment income versus those zero self-employment income (and hence whose...)

Panel A. One child

Panel B. Two or more children

Figure 4. Earnings Density and the EITC: Wage Earners versus Self-Employed

Notes: The figure displays the kernel density of earnings for wage earners (those with no self-employment earnings) and for the self-employed (those with nonzero self employment earnings). Panel A reports the density for tax filers with one dependent child and panel B for tax filers with two or more dependent children. The charts include all years 1995–2004. The bandwidth is $400 in all kernel density estimations. The fraction self-employed in 16.1 percent and 20.5 percent in the population depicted on panels A and B (in the data sample, the unweighted fraction self-employed is 32 percent and 40 percent). We display in dotted vertical lines around the first kink point the three bands used for the elasticity estimation with $\delta = 1,500.$

Source: Saez (2010), p. 192
Similarly, we cannot discern any gap in the distribution of earnings around the concave kink point where the EITC is completely phased-out. This differential response to the first kink point, versus the other kink points, is surprising in light of the standard model predicting that any convex (concave) kink should produce bunching (gap) in the distribution of earnings.

In Figure 4, we break down the sample of earners into those with nonzero self-employment income versus those zero self-employment income (and hence whose

Figure 4. Earnings Density and the EITC: Wage Earners versus Self-Employed

Notes: The figure displays the kernel density of earnings for wage earners (those with no self-employment earnings) and for the self-employed (those with nonzero self employment earnings). Panel A reports the density for tax filers with one dependent child and panel B for tax filers with two or more dependent children. The charts include all years 1995–2004. The bandwidth is $400 in all kernel density estimations. The fraction self-employed in 16.1 percent and 20.5 percent in the population depicted on panels A and B (in the data sample, the unweighted fraction self-employed is 32 percent and 40 percent). We display in dotted vertical lines around the first kink point the three bands used for the elasticity estimation with $\delta = \$1,500.$

Source: Saez (2010), p. 192
EITC Empirical Studies

Some evidence of response along extensive margin but little evidence of response along intensive margin (except for self-employed)

⇒ Possibly due to lack of understanding of the program

Qualitative surveys show that:

Low income families know about EITC and understand that they get a tax refund if they work

However very few families know whether tax refund increases or decreases with earnings

Such confusion might be good for the government as the EITC induces work along participation margin without discouraging work along intensive margin
Chetty, Friedman, Saez AER’13 EITC information

Use US population wide tax return data since 1996

1) Substantial heterogeneity fraction of EITC recipients bunching (using self-employment) across geographical areas

⇒ Information about EITC varies across areas

2) Places with high self-employment EITC bunching display **wage earnings** distribution more concentrated around plateau

⇒ Evidence of wage earnings response to EITC along intensive margin

3) Omitted variable test: use birth of first child to test causal effect of EITC on wage earnings
Earnings Distributions in Lowest and Highest Bunching Deciles

Source: Chetty, Friedman, and Saez NBER'12
Fraction of Tax Filers Who Report SE Income that Maximizes EITC Refund in 1996

Source: Chetty, Friedman, and Saez NBER'12
Fraction of Tax Filers Who Report SE Income that Maximizes EITC Refund in 1999

Source: Chetty, Friedman, and Saez NBER'12
Fraction of Tax Filers Who Report SE Income that Maximizes EITC Refund in 2002

Source: Chetty, Friedman, and Saez NBER'12
Fraction of Tax Filers Who Report SE Income that Maximizes EITC Refund in 2005

Source: Chetty, Friedman, and Saez NBER’12
Fraction of Tax Filers Who Report SE Income that Maximizes EITC Refund in 2008

Source: Chetty, Friedman, and Saez NBER'12
Income Distribution For Single Wage Earners with One Child

Is the EITC having an effect on this distribution?

Source: Chetty, Friedman, and Saez NBER’12
Income Distribution For Single Wage Earners with One Child
High vs. Low Bunching Areas

Percent of Wage Earners

W-2 Wage Earnings

EITC Amount ($)

$0 $10K $20K $30K $25K $35K

$0K $1K $2K $3K $4K

Source: Chetty, Friedman, and Saez NBER'12
Earnings Distribution in the Year Before First Child Birth for Wage Earners

Source: Chetty, Friedman, and Saez NBER'12
Earnings Distribution in the Year of First Child Birth for Wage Earners

Percent of Individuals

- 2%
- 4%
- 6%

Wage Earnings

- $0
- $10K
- $20K
- $30K
- $40K

Lowest Sharp Bunching Decile
Middle Sharp Bunching Decile
Highest Sharp Bunching Decile

Source: Chetty, Friedman, and Saez NBER'12
Long-term effects of Redistribution: Evidence from the Israeli Kibbutz

Abramitzky (2018) book based on series of academic papers

Kibbutz are egalitarian and socialist voluntary communities in Israel, thrived for almost a century within a capitalist society

1) Social sanctions on shirkers effective in small communities with limited privacy

2) Deal with brain drain exit using communal property as a bond

3) Deal with adverse selection in entry with screening and trial period

4) Perfect sharing in Kibbutz has negative effects on high school students performance but effect is small in magnitude
Long-term effects of Redistribution: Evidence from the Israeli Kibbutz

Abramitzky-Lavy ECMA’14 show that high school students study harder once their kibbutz shifts away from equal sharing. They use a DD strategy: pre-post reform and comparing reform Kibbutz to non-reform Kibbutz. They find that

1) Students are 3 percentage points more likely to graduate

2) Students are 6 points more likely to achieve a matriculation certificate that meets university entrance requirements

Effect is driven by students whose parents have low schooling; larger for males; stronger in kibbutz that reformed to greater degree.
Culture of Welfare across Generations

Conservative concern that welfare promotes a culture of dependency: kids growing up in welfare supported families are more likely to use welfare

Correlation in welfare use across generations is obviously not necessarily causal

Dahl, Kostol, Mogstad QJE’14 analyze causal effect of parental use of Disability Insurance (DI) on children use (as adults) of DI in Norway

Identification uses random assignment of judges to denied DI applicants who appeal [some judges severe, others lenient]

Find evidence of causality: parents on DI increases odds of kids on DI over next 5 years by 6 percentage points

Mechanism seems to be learning about DI availability rather than reduced stigma from using DI [because no effect on other welfare programs use]
judge allowance rate in the other cases a judge has handled. We note the judge leniency measure is calculated from all cases the judge has ever handled, not just the cases in our estimation sample. On average, each judge has handled a total of 380 cases. The mean of the leniency variable is .15 with a standard deviation of .06. The histogram reveals a wide spread in judge leniency, with approximately 22% of cases allowed by a judge at the 90th percentile compared to approximately 9% at the 10th percentile.

Figure 3: Effect of Judge Leniency on Parents (First Stage) and Children (Reduced Form).

Notes: Baseline sample, consisting of parents who appeal an initially denied DI claim during the period 1989-2005 (see Section 3 for further details). There are 14,893 individual observations and 79 different judges. Panel (A): Solid line is a local linear regression of parental DI allowance on judge leniency. Panel (B): Solid line is a local linear regression of child DI receipt on their parent’s judge leniency measure. All regressions include fully interacted year and department dummies. The histogram of judge leniency is shown in the background of both figures (top and bottom 0.5% excluded from the graph).

Source: Dahl, Kostol, Mogstad (2013)
Social Determinants of Labor Supply (Saez ’21)

Concern that taxes funding social state could discourage work

**Standard econ view:** labor supply $l(w, R)$ coming out of
$$\max_{c, l} u(c, l) \text{ st } c = wl + R$$
is highly incomplete

**Social determinants of labor supply:**

a) Youth labor is regulated by labor laws/education

b) Old age labor regulated by retirement programs

c) Female market labor driven by norms + child care policy

d) Hours of work regulated by overtime + vacation mandates

Social labor supply with disutility for youth, old, overtime labor
Employment Rates of Men by Age, 2019

Source: OECD database online. Employment to population ratios.

Source: Saez AEA-PP'21
Employment Rates of Women by Age, 2019

Source: OECD database online. Employment to population ratios.

Source: Saez AEA-PP’21
Employment Rates of Men and Women, aged 25-54

Source: OECD database online.

Source: Saez AEA-PP'21
Employment Rates of Men and Women, aged 25-54

Source: OECD database online.

Source: Saez AEA-PP'21
US female labor force participation, age 16-64

Source: Saez AEA-PP'21

25% increase in 1943-1945 during WW2 planned economy

Child Penalties: Results

Our main outcome variable is gross labor earnings, excluding taxes or transfers, specified in levels rather than in logs to be consistent with underlying this approach, compare its results to alternative approaches in the literature, and pro-
lays out the identification assumptions coming
forth-

dummies because, conditional on age and year, we are able to identify the effects of all three sets of
dummies

The first term on the right-hand side includes age dummies

Earnings relative to event time

The time series have plateaued.

Despite these similarities, the graphs also reveal some striking differences. First, the

Figure 1. Child Penalties in Earnings in Scandinavian Countries

Notes: See the notes to Figure 1.

Source: Kleven et al. AEA-PP 2019
Child Penalties Across Countries: Evidence and Explanations

The first term on the right-hand side includes age dummies because, conditional on age and year, men are able to keep the zeros in the data after the birth of their first child, while men are large, immediate and persistent drop in earnings for life cycle and time trends—but diverge similarly before parenthood—after adjust-

country, the earnings of men and women evolve penalties is a pervasive phenomenon. In each results confirm that the existence of large child on earnings across the different countries. The

Tables have plateaued. Ten years after childbirth, women have not recovered and at this point the

Notes: See the notes to Figure 1.

Source: Kleven et al. AEA-PP 2019

Figure 2. Child Penalties in Earnings in English-Speaking Countries

Figure 2. Child Penalties in Earnings in English-Speaking Countries
Figure 3. Child Penalties in Earnings in German-Speaking Countries
Average Annual Hours of Work of Employees

Source: OECD database online. Includes all ages, genders, and part-time, full-time, overtime.

US has 40 hour/week and no mandatory paid vacation.

1968: 4th week of paid vacation

1982: 5th week + 39 hours/week

2000-2: 35 hours/week

Source: Saez AEA-PP’21
REFERENCES


Abramitzky, Ran and Victor Lavy, 2014 “How Responsive is Investment in Schooling to Changes in Redistributive Policies and in Returns?”, Econometrica, 82(4), 1241-1272 (web)


Munnell, Alicia H. ”Lessons from the income maintenance experiments.” Proceedings of a conference sponsored by the Federal Reserve Bank of Boston and the Brookings Institution, Melvin Village, NH. 1986. (web)

