### Table 1
Parameters of the 11 Negative Income Tax Programs

<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

Source: Ashenfelter and Plant (1990)
FIGURE 2. PROPORTION WITH POSITIVE EARNINGS FOR NONWINNERS, WINNERS, AND BIG 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. 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...
Figure II

Source: Federal Govt
Table IIa
Marginal Tax Rate

<table>
<thead>
<tr>
<th>Group</th>
<th>Before TRA86</th>
<th>After TRA86</th>
<th>Change</th>
<th>Relative Change</th>
</tr>
</thead>
<tbody>
<tr>
<td>High</td>
<td>.521</td>
<td>.382</td>
<td>-.139</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(.002)</td>
<td>(.001)</td>
<td>(.002)</td>
<td></td>
</tr>
<tr>
<td>75th Percentile</td>
<td>.365</td>
<td>.324</td>
<td>-.041</td>
<td>-.098</td>
</tr>
<tr>
<td></td>
<td>(.001)</td>
<td>(.001)</td>
<td>(.001)</td>
<td>(.002)</td>
</tr>
<tr>
<td>90th Percentile</td>
<td>.430</td>
<td>.360</td>
<td>-.07</td>
<td>-.069</td>
</tr>
<tr>
<td></td>
<td>(.001)</td>
<td>(.001)</td>
<td>(.001)</td>
<td>(.002)</td>
</tr>
</tbody>
</table>

The marginal tax rate is calculated using family wage and salary, self-employment, interest, dividend, farm and social-security income. I assume all couples file jointly, and that all itemize their deductions. Itemized deductions and capital gains are imputed using Statistics of Income data. These figures include the secondary earner deduction, as well as social security taxes. Standard errors are in parentheses. Before TRA86 is tax years 1983-1985; After TRA86 is tax years 1989-1991.

Source: Eissa 1995
Table III
Differences-in-Differences Estimates
CPS Married Women Before and After TRA86

A: Labor Force Participation

<table>
<thead>
<tr>
<th>Group</th>
<th>Before TRA86</th>
<th>After TRA86</th>
<th>Change</th>
<th>Difference-in-Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td>High</td>
<td>0.464 (.018)</td>
<td>0.554 (.018)</td>
<td>0.090 (.025)</td>
<td>0.037 (.028)</td>
</tr>
<tr>
<td></td>
<td>[756]</td>
<td>[718]</td>
<td>{19.5%}</td>
<td>{12.3%}</td>
</tr>
<tr>
<td>75th Percentile</td>
<td>0.687 (.010)</td>
<td>0.740 (.010)</td>
<td>0.053 (.010)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[3799]</td>
<td>[3613]</td>
<td>{7.2%}</td>
<td>{12.3%}</td>
</tr>
<tr>
<td>90th Percentile</td>
<td>0.611 (.010)</td>
<td>0.656 (.010)</td>
<td>0.045 (.010)</td>
<td>0.045 (.028)</td>
</tr>
<tr>
<td></td>
<td>[3765]</td>
<td>[3584]</td>
<td>{6.5%}</td>
<td>{13%}</td>
</tr>
</tbody>
</table>

Source: Eissa 1995
B: Hours Conditional on Employment

<table>
<thead>
<tr>
<th>Group</th>
<th>Before TRA86</th>
<th>After TRA86</th>
<th>Change</th>
<th>Difference-in-Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td>High</td>
<td>1283.0 (46.3)</td>
<td>1446.3 (41.1)</td>
<td>163.3 (61.5)</td>
<td><code>{12.7%}</code></td>
</tr>
<tr>
<td></td>
<td>[351]</td>
<td>[398]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>75th Percentile</td>
<td>1504.1 (14.3)</td>
<td>1558.9 (13.9)</td>
<td>54.8 (20.0)</td>
<td>108.6 (65.1)</td>
</tr>
<tr>
<td></td>
<td>[2610]</td>
<td>[2676]</td>
<td></td>
<td><code>{9.4%}</code></td>
</tr>
<tr>
<td>90th Percentile</td>
<td>1434.1 (16.4)</td>
<td>1530.1 (15.9)</td>
<td>96.0 (22.8)</td>
<td>67.3 (64.8)</td>
</tr>
<tr>
<td></td>
<td>[2303]</td>
<td>[2348]</td>
<td></td>
<td><code>{6.2%}</code></td>
</tr>
</tbody>
</table>

Each cell contains the mean for that group, along with standard errors in (), number of observations in [], and % increase in {}. Means are unweighted.

Source: Eissa 1995
Figure 10
Fraction of Married Women with Positive Annual Earnings by Income Group in March CPS

Notes: Groups are based on other household income (husband’s earnings plus asset income) as described in Eissa (1995). Group 1 <= 75th percentile. Group 75 is > 75th percentile and <= 80th percentile. Group 80 is > 80th and <= 90th. Group 90 is > 90th and <= 95th. Group 95 is > 95th and <= 99th. Group 99 is > 99th.

Source: Liebman and Saez (2006)
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). Third, there appear to be relatively large recall errors and seam biases in the earnings and wage data. 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

![Graph showing monthly employment rates with a legend indicating Control Group, Program Group, and Difference.]

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

Source: Card and Hyslop, 2005, p. 1734

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.
How Does the Current Assistance Caseload Level Compare with Historical Levels?

Figure 2 provides a long-term historical perspective on the number of families receiving assistance from TANF or its predecessor program, from July 1959 to September 2017. The shaded areas of the figure represent months when the national economy was in recession. Though the health of the national economy has affected the trend in the cash assistance caseload, the long-term trend in receipt of cash assistance does not follow a classic countercyclical pattern. Such a pattern would have the caseload rise during economic slumps, and then fall again during periods of economic growth. Factors other than the health of the economy (demographic trends, policy changes) also have influenced the caseload trend.

The figure shows two periods of sustained caseload increases: the period from the mid-1960s to the mid-1970s and a second period from 1988 to 1994. The number of families receiving assistance peaked in March 1994 at 5.1 million families. The assistance caseload fell rapidly in the late 1990s (after the 1996 welfare reform law) before leveling off in 2001. In 2004, the caseload began another decline, albeit at a slower pace than in the late 1990s. During the recent 2007-2009 recession and its aftermath, the caseload began to rise from 1.7 million families in August 2008, peaking in December 2010 at close to 2.0 million families. By September 2018, the assistance caseload had declined to 1.2 million families.

Figure 2. Number of Families Receiving Cash Assistance, July 1959-September 2018

Source: Congressional Research Service (CRS) with data from the U.S. Department of Health and Human Services (HHS).

Notes: Shaded areas denote months when the national economy was in recession. Information represents families receiving cash assistance from Aid to Dependent Children (ADC), Aid to Families with Dependent Children (AFDC), and TANF. For October 1999 through September 2018, includes families receiving assistance from Separate State Programs (SSPs) with expenditures countable toward the TANF maintenance of effort requirement. See Table A-1 for average annual data on families, recipients, adult recipients, and child recipients of ADC, AFDC, and TANF cash assistance for 1961 to 2017.

Table B-5 shows recent trends in the number of cash assistance families by state.
The landscape providing assistance to poor families with children has changed substantially.

Annual Employment Rates for Women
By Marital Status and Presence of Children, 1980-2009

EITC Amount as a Function of Earnings

- **Subsidy:** 40%
- **Subsidy:** 34%
- **Phase-out tax:** 21%
- **Phase-out tax:** 16%

**Graph Details:**
- **Married, 2+ kids**
- **Single, 2+ kids**
- **Married, 1 kid**
- **Single, 1 kid**
- **No kids**

*Source: Federal Govt*
Figure IV
1986 and 1988 Earned Income Tax Credit

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


Unmarried Males With Less Than High School Education

![Graph showing labor force participation rates for unmarried females ages 16-44 from 1981 to 1992.](image)

**Figure II**


elasticity $e$ 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

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 $z^*$).
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

Panel B. Density distributions and bunching

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

Figure 1. Bunching Theory
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

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).
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
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

\[ \text{Figure 4. Earnings Density and the EITC: Wage Earners versus Self-Employed} \]

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 fil-
ers 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 per-
cent 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
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
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
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

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%
- 0%
- 6%

Wage Earnings

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

Source: Chetty, Friedman, and Saez NBER'12
Total Consumption: Single Mothers 1984-2000

Fig. 2. Total consumption: single mothers, 1984–2000.


Source: Meyer and Sullivan 2004
Relative Consumption: single women with/without children

Fig. 3. Relative total consumption: single mothers vs. single women without children, 1984–2000.

Source: Meyer and Sullivan 2004
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)

Panel A shows the effect of judge leniency on a parent’s allowance rate. The graph is a flexible analog to the first stage equation (4), where we plot a local linear regression of actual parental allowance against judge leniency. The parental allowance rate is monotonically increasing in our leniency measure, and is close to linear. A one percentage point increase in the judge’s allowance rate in other cases is associated with an almost one percentage point increase in the probability the parent’s case is allowed. Panel B plots the reduced form effect of a parent’s judge leniency measure against their child’s DI participation, again using a local linear regression. The child’s DI rate is monotonically increasing in the leniency measure as well. Approximately two and a half percent of children whose parents had a relatively strict judge (leniency measure = .09, the 10th percentile) are predicted to participate in DI five years later. This can be contrasted with roughly three percent of children whose parents had a relatively lenient judge (leniency measure = .22, the 90th percentile).
EITC Schedule in 2017

- 0 children
- 1 child
- 2 children
- 3+ children

<table>
<thead>
<tr>
<th>Earnings (USD)</th>
<th>Annual Credit (USD)</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>2000</td>
<td>10000</td>
</tr>
<tr>
<td>4000</td>
<td>20000</td>
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<td>8000</td>
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<tr>
<td>10000</td>
<td>50000</td>
</tr>
<tr>
<td>12000</td>
<td>60000</td>
</tr>
</tbody>
</table>
Labor Force Participation of Single Women
With and Without Children

Unemployment Rate

Year

With Children 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

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

14.5pp
14pp
50 years of relative stability,
apart from these 5 years

Unemployment Rate
50 60 70 80 90 ...
(%)
68 70 72 74 76 78 80 82 84 86 88 90 92 94 96 98 00 02 04 06 08 10 12 14 16 18
Year

With Children Without Children

Annual Employment Low Education

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

Tax Reduction
Act of 1975
TRA86
OBRA90
OBRA93
PRWORA
ARRA

Unemployment Rate

Labor Force Participation (%)

Year

With Children
Without Children

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

![Graph showing labor force participation rates with and without children, including key events such as the Tax Reduction Act of 1975, OBRA 86, OBRA 90, OBRA 93, PRWORA, and ARRA. The graph highlights the impact of state welfare waivers on labor force participation. Source: Kleven (2018).]
Labor Force Participation of Single Women
With and Without Children

Source: Kleven (2018)
Labor Force Participation of Single Women
By Number of Children

Source: Kleven (2018)
Labor Force Participation of Single Women
By Number of Children

Tax Reduction Act of 1975 TRA86 OBRA90 OBRA93 ARRA
Much larger increase by those with 3+ kids

Source: Kleven (2018)
Labor Force Participation of Single Women
By Number of Children

Source: Kleven (2018)

But no increase here by those with 3+ kids
The first term on the right-hand side includes event-time dummies, the second term includes age dummies (to control for life cycle trends), and the third term includes year dummies (to control for time trends). We omit the event-time dummy at $t = -1$, implying that the event-time coefficients measure the impact of children relative to the year just before the first childbirth. We are able to identify the effects of all three sets of dummies because, conditional on age and year, there is variation in event time driven by variation in the age at which individuals have their first child. Kleven, Landais, and Søgaard (forthcoming) lay out the identification assumptions underlying this approach, compare its results to alternative approaches in the literature, and provides evidence of its ability to identify the causal effect of parenthood.

Our main outcome variable is gross labor earnings, excluding taxes or transfers, specified in levels. We convert the estimated level effects into percentages by calculating $\hat{P}^g_t \equiv \hat{\alpha}^g_t / E[Y^g_{ist} | t]$ where $Y^g_{ist}$ is the predicted outcome when omitting the contribution of the event dummies. Having estimated the impacts of children on women and men separately, we define the child penalty at event time $t$ as $P_t \equiv (\hat{\alpha}^m_t - \hat{\alpha}^w_t) / E[Y^g_{ist} | t]$. This measures the percentage by which women are falling behind men due to children.

II. Child Penalties: Results

Figures 1-3 show the effects of parenthood on earnings across the different countries. The results confirm that the existence of large child penalties is a pervasive phenomenon. In each country, the earnings of men and women evolve similarly before parenthood—but diverge sharply after parenthood. Women experience a large, immediate and persistent drop in earnings after the birth of their first child, while men are essentially unaffected. Ten years after childbirth, women have not recovered and at this point the series have plateaued.

Despite these similarities, the graphs also reveal some striking differences. First, the size of the long-run child penalty (defined as the average penalty from event time five to

3 We specify equation (1) in levels rather than in logs to be able to keep the zeros in the data (due to nonparticipation). In the online Appendix, we present separate results on the extensive margin impacts of children.

4 To be precise, we define $Y^g_{ist} \equiv \sum_k \hat{\gamma}^g_k \cdot 1[k = age_{it}] + \sum_{k} \hat{\gamma}_k \cdot 1[y = t]$. Hence, $P_t$ captures the year-$t$ effect of children as a percentage of the counterfactual outcome absent children.
The first term on the right-hand side includes event-time dummies, the second term includes age dummies (to control for life cycle trends), and the third term includes year dummies (to control for time trends). We omit the event-time dummy at \( t = -1 \), implying that the event-time coefficients measure the impact of children relative to the year just before the first childbirth. We are able to identify the effects of all three sets of dummies because, conditional on age and year, there is variation in event time driven by variation in the age at which individuals have their first child. Kleven, Landais, and Søgaard (forthcoming) lays out the identification assumptions underlying this approach, compare its results to alternative approaches in the literature, and provides evidence of its ability to identify the causal effect of parenthood.

Our main outcome variable is gross labor earnings, excluding taxes or transfers, specified in levels. We convert the estimated level effects into percentages by calculating

\[
P_t^g ≡ \hat{\alpha}^g_t / E \left[ \tilde{Y}^g_{ist} | t \right]
\]

where \( \tilde{Y}^g_{ist} \) is the predicted outcome when omitting the contribution of the event dummies. Having estimated the impacts of children on women and men separately, we define the child penalty at event time \( t \) as

\[
P_t \equiv (\hat{\alpha}_{it}^m - \hat{\alpha}_{it}^w) / E \left[ \tilde{Y}^m_{ist} | t \right]
\]

This measures the percentage by which women are falling behind men due to children.

II. Child Penalties: Results

Figures 1–3 show the effects of parenthood on earnings across the different countries. The results confirm that the existence of large child penalties is a pervasive phenomenon. In each country, the earnings of men and women evolve similarly before parenthood—after adjusting for life cycle and time trends—but diverge sharply after parenthood. Women experience a large, immediate and persistent drop in earnings after the birth of their first child, while men are essentially unaffected. Ten years after childbirth, women have not recovered and at this point the series have plateaued.

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The United States and the United Kingdom, are no long-run consequences. When considering a small short-run effect on men, although there is only one country where childbirth is associated with penalties as high as 51–61 percent. Second, the short-run dynamics of child penalties show some interesting differences. For example, while the Scandinavian countries are roughly similar in the long run, the short-run child penalty is about twice as large in Sweden as it is in Denmark. Swedish mothers catch up with Danish mothers over time such that their child penalty is only slightly larger after 10 years.5 Sweden is also the only country where childbirth is associated with a small short-run effect on men, although there are no long-run consequences. When considering the United States and the United Kingdom, we see that these countries feature less dramatic short-run effects, but that the effects are growing over time.

In general, the earnings penalties can come from three margins: the extensive margin of labor supply (employment), the intensive margin of labor supply (hours worked), and the wage rate. In the online Appendix, we provide evidence on child penalties along the extensive margin. While parenthood reduces female employment everywhere, the importance of this margin varies across countries. In the Scandinavian and Germanic countries, the extensive margin effects are significantly smaller than the earnings effects, implying that a substantial fraction of the earnings penalty is driven by the intensive margin and wage-rate effects. In the United States and the United Kingdom, the employment penalty is much closer in magnitude to the earnings penalty, suggesting that the extensive margin is a key driver of penalties in those countries.6

III. Child Penalties: Explanations

One set of explanations for the differences in child penalties focuses on government policies. These include taxes, transfers, and family policies such as parental leave and childcare provision that directly affect mothers’ incentive to work. There is a voluminous literature on the impact of such policies on female labor supply and gender gaps (see Olivetti and Petrongolo 2017 for a review). Of particular relevance, Kleven et al. (2019) considers the impacts of parental leave and public childcare on the dynamics of child penalties. Their setting is Austria, a country where the combination of rich administrative data and a series of parental leave reforms and childcare expansions allow for compelling quasi-experimental analyses of these questions.

5 Angelov, Johansson, and Lindahl (2016) estimate child penalties for Sweden using a different event-study specification. An advantage of implementing the same specification across countries is that it allows for direct comparisons. The fact that Denmark and Sweden are so different is a priori surprising. We note that our earnings measure in general includes any (non-mandated) parental leave benefits paid by the employer, implying that cross-country comparisons partly reflect variation in such benefits. While employer-provided parental leave benefits do tend to be higher in Denmark than in Sweden, this is likely to have a modest impact on the relative child penalties for two reasons. One is that such employer-provided benefits were relatively small during the period we study (in Denmark we are considering first child births between 1985–2003), and the other is that those benefits are provided only during event times 0 and 1.

6 Since we do not condition our samples on having only one child, the long-run child penalties will include the effects of subsequent children and therefore depend on total fertility. However, differential fertility is unlikely to drive the variation in child penalties across countries. For example, the German-speaking countries exhibit the largest penalties despite being characterized by the lowest realized fertility at event time ten. See Table A.I in the online Appendix for descriptive statistics in each country.
Figure 4: Secondary Job Holding Rates by Secondary Earnings Level

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.
Employment Rates of Men by Age, 2019

Source: OECD database online. Employment to population ratios.
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: Saez AEA-PP’21

Source: OECD database online.
Employment Rates of Men and Women, aged 25-54

Source: Saez AEA-PP’21

Source: OECD database online.
US female labor force participation, age 16-64

Source: Saez AEA-PP’21

25% increase in 1943-1945 during WW2 planned economy

Average Annual Hours of Work of Employees
Source: Saez AEA-PP’21

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

45°
EITC and intensive labor supply

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

Budget with EITC
EITC and intensive labor supply

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

- Negative income effect on \( z \)
- Positive substitution effects on \( z \)
- Negative income and substitution effects on \( z \)
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.
**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.