The Local Labor Market Effects of State Earned Income Tax Credit Supplements

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Preliminary Work

Abstract

Twenty six states spend $4 billion to supplement the federal Earned Income Tax Credit, with many explicitly justifying the tax expenditure as a pro-work incentive. Yet there has been no systematic evaluation of these supplements. I assess whether state expansions affect federal EITC take up, other federal transfers, migration, commuting, and employment at the county level. I use three methods for these evaluations: initial EITC demographic exposure, state border pair fixed effects, and state border regression discontinuity designs. I find mixed evidence that state programs increase federal EITC take up and State Unemployment expenditures. I find no evidence that other SNAP expenditures, migration, commuting. I find some evidence that employment increases in industries that correlate with women's employment choices. I find some evidence that earnings decrease implying firms capture some portion for of the expansion. My results imply that state supplements increase benefits to low income workers but do not necessarily increase local employment to offset state expenditures.

JEL: H23, H24, H31, H72, H73, I32

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1 Introduction

Twenty six states spend $4 billion annually to supplement the federal Earned Income Tax Credit.[1] In tax expenditure reports, a majority of states explicitly justify the supplements as a pro-work incentive and cite prior EITC research. However, there is much we do not know about these state level programs. Do they increase federal EITC take up? Do they decrease welfare usage? Do they cause workers to migrate across borders? Do they spur labor supply and employment? Do they pay for themselves?

I evaluate these questions at the county level using three empirical designs. First, I use a simple policy exposure design that uses a pre-expansion measure of potential EITC exposure interacted with state policy terms, similar to Duflo (2001); Finkelstein (2007). Second, I use a state border pair fixed effect design that uses differences in policy variation across state borders to identify effects, similar to Holmes (1998); Huang (2008); Dube, Lester and Reich (2010). Third, I use a state border distance regression discontinuity design that allows me to control for aggregation bias and spillover effects onto untreated counties, similar to Dieterle, Bartalotti and Brummet (2020).

While previous analyses used state policy variation for identification, there has been no systematic evaluation of these supplements on local labor markets absent the federal portion of the program.[2] Kleven (2019) uses stacked event study designs to investigate individual level effects of the programs and finds a precise zero.[3] Neumark and Williams (2016) find using state level tax return data that state expansions do increase federal EITC take-up. Additionally, Neumark and Shirley (2017) consider long run effects of anti-poverty policies for urban census tracts and find mixed evidence of long-run employment responses. I complement these efforts by focusing on local aggregate outcomes using different data, methods, and variation.[4]

For my outcome variables, I use data from the IRS Statistics of Income (EITC take up and migration), BEA Federal Transfer Receipts (SNAP and State UI expenditures), BLS Quarterly Census of Wages and Employment (employment and earnings by industry), the Census Longitudinal Origin and Destination Statistics (commuting and employment), the Census Quarterly Workforce Indicators and Quarterly Census of Employment Wages (employment, and earnings), and the 1990 and 2000 Census Summary Files (demographic exposure measures). Like previous studies, I use state expansion indicators and state maximum credit measures to measure state policy variation.

I find that state programs increase federal EITC take up and State Unemployment expenditures. I find no evidence that other SNAP, migration, or commuting are affected. I find some evidence that employment increases for industries and demographic groups most exposed to local EITC shocks, specifically industries where the probability of choosing that industry conditional on being a women is high. I find that earnings de-

[1] This is based on state tax return and tax expenditure reports from tax years 2017 to 2019, and as far as I am aware, this fact has not been documented given the decentralized nature of state tax expenditures.

[2] For example, consider Leigh (2010); Neumark and Williams (2016); Kasy (2017); Bastian (forthcoming) use the maximum state EITC credit as a continuous difference in difference style design.

[3] Specifically, he uses two different methods for this. In the first he creates a synthetic control state for each expansion state for unmarried women with children (and a check using a triple difference including unmarried women without children) for an aggregate state level regression, and in the second it is a more conventional event study design using individual level data.

[4] Buhlmann et al. (2018) use an event study and border pair design to look at tax bunching at EITC kink points, but do not look at other outcomes.

crease even when I do not find significant employment effects. The earnings decreases without employment effects (negative or positive) could either imply that firms are capturing benefits by paying workers less or be due to workers switching to industries with lower gross earnings but higher net earnings.[5]

My results imply that state supplements increase benefits to low income workers but do not necessarily increase local employment to offset state expenditures. This implies that state EITC function as a conditional cash transfer where the condition is having low gross earnings (and qualifying children) rather than as an economic development tool, which is the explicit rational for several of the state programs. Thus, while state EITC programs may be a worthwhile anti-poverty program, it is not obvious that the programs pay for themselves in term of labor market effects. However, it may be possible that state programs generate demand effects, which could indirectly increase tax income tax revenue. This remains to be explored.

2 Local Variation in the EITC

Table 1 reports state and federal tax returns with EITC claims and the expenditure amounts. The table shows that most state programs have claims that nearly match the federal claims; the average of column (f) is roughly 90%.[6] However, the amount that each state spends is typically much lower given that state supplement rates a bounded between 0% and 40% across states. The average of column (g) is 15.7% which is exactly the average state supplement rate.[7]

[Table 1 about here.]

Figure 1 shows the variation in State EITC supplement rates over time. The states in pink do not supplement the Federal EITC, while darker shades of blue correspond to larger state supplement rates.[8] Interestingly, there seems to be some spatial correlation in State EITC spread, where most states with a program border another state with a program.

[Figure 1 about here.]

Figure 2 shows for 2017 the state variation in State EITC program supplement rates and maximum credits and county level distribution of Federal EITC returns and average EITC amount deciles. There appears to be a negative correlation between State EITC programs and Federal EITC usage. Federal EITC usage appears to be concentrated in the South and Sunbelt while State EITC programs are mostly in the Plains and and Midwest.

[5]The incidence of the EITC is further explored in Leigh (2010); Rothstein (2010); Kasy (2017); Watson (2020); however, I know of no work on the sectoral choice implications of EITC expansion.

[6]Note: the New York number of claims uses both NY state and NY city EITC programs and likely double counts the number claims. I am exploring how to correct this.

[7]In Table 2, I report the average state supplement rate conditional on ever having a program, which includes the value zero for states before the program goes into effect.

[8]States in white do supplement the Fed EITC but do so using a non-linear supplement schedule. In the regression specifications, I include these states by finding the maximum state credit associated with the non-linear schedule.

The figure also shows the 2000 distribution of unmarried mothers and of all mothers (married and unmarried) in the labor force at the county level.[9] There appears to be some negative correlation between state EITC programs and the average Federal EITC amounts but positive correlation to the labor force participation of mothers.

[Figure 2 about here.]

3 Evaluating State EITC Supplements

To think about the motivations for a state EITC supplement, consider the fiscal externality of a tax reform in an environment where workers make residence and workplace decisions. This simplified treatment parsimoniously microfounds a sufficient statistic approach to evaluate state EITC supplements.[10]

Let $\mathcal{S}$ be the set possible locations – 'counties' – in the economy, and the counties in $\mathcal{S}$ can be partitioned into M 'states,' $\mathcal{S} = \{S_1, S_2, \ldots, S_M\}$. Let there be a unit mass of individuals indexed by $i \in N$ making a residence and work location choice with the option of unemployment. Suppose that individuals have preferences such that the probability that an individual chooses a work and residence pair (o, d) as:

$$\underbrace{\Pr((\mathsf{o}^i, \mathsf{d}^i) = (o, d))}_{:=\pi_{o,d}} = \underbrace{\Pr(\mathsf{d}^i = d \mid \mathsf{o}^i = o) \cdot \Pr(\mathsf{o}^i = o)}_{:=\pi_{d|o} \,\cdot\, \pi_o}. \tag{1}$$

That is, individuals have a two-stage decision process such that they first choose a residence location, $o \in \mathcal{S}$, and then a work choice $d \in \{\mathcal{S} \cup \{\text{Unemployment}\}\}$ based on some (potentially endogenous) indirect utility value; e.g., a residence-location specific amenity plus post-tax earnings. I denote agent i's choice bundle as $(\mathsf{o}^i, \mathsf{d}^i)$.[11]

The fiscal externality of a marginal tax reform is equivalent to the behavioral effect on tax revenues (Hendren, 2016; Finkelstein and Hendren, 2020; Kleven, 2020). If state government s uses residence based income taxation[12] with origin-destination specific tax rates, $R^s = \sum_{o\in s} R^o = \sum_{o\in s} \left(\sum_{d\in\mathcal{S}} t^o_d w^o_d \pi_{d|o}\pi_o\right)$, then the first-order[13] fiscal externality as a proportion of initial revenue is (where $\hat{x} = \mathrm{d}x/x$):

$$\frac{\mathsf{FE}^s}{R^s} = \sum_{o\in s} \frac{R^o}{R^s} \left(\underbrace{\hat{\pi}_o}_{\text{Migration}} + \underbrace{\frac{R^o_o}{R^o}(\hat{w}^o_o + \hat{\pi}_{o|o})}_{\text{Own Employment}} + \underbrace{\sum_{d\in\mathcal{S}\backslash o} \frac{R^o_d}{R^o} \cdot (\hat{w}^o_d + \hat{\pi}_{d|o})}_{\text{Commuting}} \right). \tag{2}$$

[9] The 2000 county level distribution of unmarried mothers in the labor force is not available.

[10] The model is similar to Monte et al. (2018), who document variation in local labor supply elasticities, and conceptually similar to Agrawal and Hoyt (2018) who document the effect of tax differentials on commuting patterns.

[11] Such preferences can be microfounded based a stochastic taste shifter drawn from a Generalized Extreme Value distribution, one example of which leads to the Nested Logit model.

[12] If residents spend a fixed portion of net income across goods (via homothetic preferences), then the income tax is isomorphic to a composite tax on labor income and purchases.

[13] That is, assuming multiplicative terms are negligible: $\hat{x} \cdot \hat{y} \approx 0$.

It can be shown that the fiscal externality is a sufficient measure of the change in aggregate welfare divided by the marginal cost of public funds (μ) when evaluated at utilitarian social welfare weights ($g^i = 1$): $\frac{\mathsf{d}W/\mathsf{d}\theta}{\mu}|_{g^i=1} = \mathsf{FE}$ (Hendren, 2016; Kleven, 2020). Kleven (2020) notes if it is possible to directly estimate the behavioral effect on tax liabilities, then this quantity can theoretically be estimated without estimating specific response elasticities. However, given the possibility of migration and commuting, it is not obvious what the appropriate control group would be for such an empirical exercise.

States, and state politicians, may have other goals that welfare maximization with state EITC supplements. For example, there could be ideological preferences that more lower-income people should work *or* a desire to increase local aggregate output by expanding the workforce. While these goals could be modeled using heterogeneous social welfare weights, $\{g^i\}$, these goals are directly estimable using observable data.

The empirical estimates below do not capture the local granularity of behavioral responses, but do give a hint towards their magnitude in order to assess the fiscal externality.

4 Empirical Designs

I use three empirical designs on the set of counties that are at state borders with a policy difference. The first is the two-way fixed effect (TWFE) design that serves as an empirical baseline. The second is a state-border fixed effect (SBFE) design that removes common time-varying shocks between each border county pair. The third is a state-border regression discontinuity (SBRD) design that parametrically controls for distance to the policy border.

For all the designs below, let y be the log of some outcome variable, let X be controls, let T be a treatment indicator that is equal to one if a state EITC program is in effect, and let C be the maximum EITC credit in the state (which varied due to state EITC programs).[14] For all the regressions, I control for log population (or log tax returns), log real state GDP, county fixed effects, and year fixed effects. In addition, I weight all regressions by county population in 2000.

I use four measures for potential EITC intensity that vary at the county level, designated P^j_c: the percent of families that are single mothers in 2000, the labor force participation of unmarried mothers in 1990, the probability that woman chooses a given industry in 2000, and the percent women in a given industry in 2000.[15] These measures a reduce form way of parameterizing heterogeneous effects of state EITC expansions.[16] If state EITC expansions are always more effective where more (single) mothers are present, then they should all agree with each other. If the measures disagree with each other, then this implies heterogeneous effects that need to be considered when assessing new state EITC programs.

[14] In Appendix B.1, I plot the coefficients from Event Study versions of these specifications that measure the dynamic treatment effects.

[15] The LFP of unmarried mothers at the county level is not released in the public 2000 Census files, and the percent of families that are single women in 2000 is highly correlated with the 1990 measure.

[16] An alternative method is used in Monte et al. (2018) who use a structural choice model to estimate distribution of local labor supply elasticities.

4.1 Two Way Fixed Effect

The TWFE design serves as a baseline. Under the assumption of common year effects and uncorrelatedness with the error term, differences correlated to state EITC policies are the interpreted causally. This estimator is the subject of an active line of research.[17] Roughly, De Chaisemartin and d'Haultfoeuille (2020) (and others) show that the estimator is a weighted average of ATT where the weights can be negative due to the composition of "control" groups.[18] This issue can be assessed empirically by estimating the weights. In Appendix B, I present event study estimates using the DID_M estimator from De Chaisemartin and d'Haultfoeuille (2020).

The regression equations are:

$$\begin{aligned} &[\text{Continuous}] \quad y_{cst} = \alpha + X_{cst}\beta + \gamma^C \ln\left[C_{st}\right] + \lambda_c + \lambda_t + u_{cst} && (3)\\ &[\text{Discrete}] \quad y_{cst} = \alpha + X_{cst}\beta + \gamma^T T_{st} + \lambda_c + \lambda_t + u_{cst} && (4)\\ &[\text{Interaction}] \quad y_{cst} = \alpha + X_{cst}\beta + \gamma^T T_{st} + \gamma^I (T_{st} \cdot P_c^j) + \lambda_c + \lambda_t + u_{cst}, && (5) \end{aligned}$$

where c indexes counties, s for states, and t for years. For inference, I cluster standard errors at the state level.

4.2 State Border Fixed Effect

The SBFE design uses every county pair with a policy difference to generalize the case study approach (Dube et al., 2010). The design residualizes by a pair-year fixed effect that is assumed to capture common unobservable trends. Under that assumption and uncorrelatedness with the error term, differences correlated to state EITC policies are the interpreted causally.

The regression equations are:

$$\begin{aligned} &[\text{Continuous}] \quad y_{cpst} = \alpha + X_{cpst}\beta + \gamma^C \ln\left[C_{st}\right] + \lambda_c + \lambda_{pt} + u_{cpst} && (6)\\ &[\text{Discrete}] \quad y_{cpst} = \alpha + X_{cpst}\beta + \gamma^T T_{st} + \lambda_c + \lambda_{pt} + u_{cpst} && (7)\\ &[\text{Interaction}] \quad y_{cpst} = \alpha + X_{cpst}\beta + \gamma^T T_{st} + \gamma^I (T_{st} \cdot P_c^j) + \lambda_c + \lambda_{pt} + u_{cpst}, && (8) \end{aligned}$$

where c indexes counties, p for county pairs, s for states, and t for years. For inference, I cluster standard errors at the state-border level.

4.3 State Border Regression Discontinuity

The SBRD design takes seriously the idea of a spatial discontinuity in policy at the state border. The difference in expected outcome is modeled as a function of distance to the border. However, as Dieterle et al. (2020) note, counties are not ideal for this analysis since counties are a political jurisdiction rather than an economic market area. This is

[17] See Borusyak and Jaravel (2017); Abraham and Sun (2018); Athey and Imbens (2018); Goodman-Bacon (2018); Callaway and Sant'Anna (2020); De Chaisemartin and d'Haultfoeuille (2020).

[18] Goodman-Bacon (2018) states clearly that the estimand is a "a weighted average of all possible two-group/ two-period DD estimators in the data;" thus, the estimator uses a treated group as 'control' which causes bias when treatment effects are heterogeneous.

dealt with by using a census block population weighted distance to the border for each county using a pooled global polynomial RD design. The global polynomial method is used because the implied measurement error from using counties forces the use of a parametric method rather than a non-parametric local method.

The regression equations are:

$$\begin{aligned}
\text{[Continuous]} \quad & y_{csbt} = \alpha + X_{csbt}\beta + \gamma^C \ln\left[C_{st}\right] + \lambda_c \\
& + D_{bt} \cdot \left[1 + \sum_k \theta_k^0 (1 - T_{st}) \cdot m_{csb}^k + \sum_k \theta_k^1 T_{st} \cdot m_{csb}^k\right] + u_{cst} \qquad (9)
\end{aligned}$$

$$\begin{aligned}
\text{[Discrete]} \quad & y_{csbt} = \alpha + X_{csbt}\beta + \gamma^T T_{st} + \lambda_c \\
& + D_{bt} \cdot \left[1 + \sum_k \theta_k^0 (1 - T_{st}) \cdot m_{csb}^k + \sum_k \theta_k^1 T_{st} \cdot m_{csb}^k\right] + u_{cst} \qquad (10)
\end{aligned}$$

$$\begin{aligned}
\text{[Interaction]} \quad & y_{csbt} = \alpha + X_{csbt}\beta + \gamma^T T_{st} + \gamma^I (T_{st} \cdot P_c^j) + \lambda_c \\
& + D_{bt} \cdot \left[1 + \sum_k \theta_k^0 (1 - T_{st}) \cdot m_{csb}^k + \sum_k \theta_k^1 T_{st} \cdot m_{csb}^k\right] + u_{cst}, \qquad (11)
\end{aligned}$$

where c indexes counties, s for states, b for state borders, t for years, and k for the order of the global polynomial. I consider only linear ($k = 1$) and quadratic ($k = 2$) terms but allow the distance regressions to vary depending on being on the treated or untreated side.[19] For inference, I cluster standard errors at the state-border level.

5 Data

The data used in the analysis is based on the contiguous border counties in the United States. There are 3,135 counties in the US of which 1,139 share a border segment with another county, but only 862 have a policy discontinuity due to a state EITC program. The median border county has two contiguous neighbors, but there are 30 counties with 5 or more neighbors. I observe these county-pairs from 2000 to 2018.[20] Figure 3 shows the specific counties used in the paper.

[Figure 3 about here.]

The following are sources for outcome variables. The tax return data is from the IRS Statistics of Income.[21] The transfers data, specifically SNAP and State Unemployment amounts, are from the BEA's Personal Income and Transfer Receipts. The migration data is also based on the IRS Statistics of Income County to County Flows.[22] The commuting

[19] This is similar to Dieterle et al. (2020) except they implement a more data-driven approach by allowing the number of polynomials to vary for each state border.

[20] I focus on the period after 2000 to avoid using variation from the 1994 OBRA expansion and welfare reform in the late 1990s.

[21] The returns data from 2000 to 2010 with EITC values was accessed from the Brookings Institute by contacting Cecile Murray.

[22] The years 1990 to 2000 are adapted from pre-formated files from Hauer (2019).

data is from the Census LEHD Origin-Destination Employment Statistics Data, where I aggregate to the county level. Finally, the employment and earnings data are from the Census Quarterly Workforce Indicators.

The following are sources for treatment and other right-hand side variables. I collect state EITC parameters from the supplementary information for NBER's Taxsim (Feenberg and Coutts, 1993).[23] County population is from the Census Population and Housing Unit Estimates, which estimates county level population between census years. State GDP data is from the BEA's Gross Domestic Product by State series. Finally, I get county level data on the number of families and labor force participation for the 1990 and 2000 Census (Steven Manson and Ruggles, 2020).

In Table 2 I present summary statistics for the data used. Column (a) includes all counties in the continental US while columns (b-d) only use the 862 contiguous border counties that I use in the estimation. Roughly, counties that are never treated appear to differ relatively more from all counties in column (a) than the ever treated counties.

[Table 2 about here.]

6 Results

Overall, there is some evidence that State EITC programs increase take up of the Federal EITC; however, I find little to no evidence that this causes a measurable effect on other labor market outcomes. I do find evidence of heterogeneous effects in both federal EITC take up and employment effects based on measures of potential EITC intensity. In Table 3, I show both the effect of State EITC programs on Federal EITC returns, using the log of the max state credit, an indicator function of a state EITC program, and interactions with intensity measures. In Tables 4-6, I show the effect of a 1% increase in the state maximum EITC amount on various log outcomes, so these can be interpreted as an elasticity. In Appendix B I present additional empirical findings on other outcome variables and other specifications.

Table 3 panel (A) appears to show that state program generosity induced additional take-up. However, panels (B, C) show that splitting the effect by the percent of families that are single mothers versus the labor force participation of single mothers causes a divergence. A 1% increase in state generosity on take-up is decreasing in the percent of single mothers but increasing in their labor force participation. One possibility is that the effect is stronger when there are many women working through word-of-mouth, akin to the finding in Chetty et al. (2013). However, the negative result is surprising and not easily explained.

[Table 3 about here.]

Table 4 shows much more mixed results on the EITC have an effect on other labor market outcomes. For many of the results, the estimates are appear noisy and difficult to distinguish from zero. In panel (A), SNAP elasticities vary in magnitude and sign across specifications and are not statistically significant. However in panel (B), the State UI

[23] I have also manually checked the values by going to the various state websites.

elasticities all appear to be large and statistically significantly different from zero.[24] One rationalization is that news coverage of the state expansion may increase knowledge of other government programs.

Panels (C,D) show the effect on cross county migration. While all the results but (D,c) are insignificant, the elasticities imply slightly positive net in migration; however, the estimates are so close that it similarly implies no change in net migration. Similarly, panels (E,F) show the net effect on cross county commuting and all are but (F,c) are insignificant. The same state commuting elasticities in panel (E) are all smaller than the cross state elasticities in panel (F), which makes sense since the incentives are roughly the constant within a state but may change the incentive to cross state lines.

Finally, panels (G,H) show the direct labor market effects for workers with less than a high school degree. The employment elasticities are all insignificant and the direction changes depending on the empirical method. However, the earnings elasticities in columns (b-d) are negative and statistically significant. With no labor supply effect but decreasing wages would imply that employers are able to capture some portion of the EITC expansion.

[Table 4 about here.]

Table 5 uses the QWI data to test for heterogeneous effects using the same interaction terms as Table 3. I find that the percent of unmarried mothers increases the employment elasticity while the LFP of unmarried women decrease the elasticity. This is the reverse of the expected result based on Table 3 Panels B and C. For example in Table 3 Panel C the LFP increases the returns elasticity, which should imply more workers entering the labor force, but Table 5 Panel B negative result implies lower employment. For earnings, I find somewhat mixed results based on specification in Panel C but Panel D is consistently negative.

[Table 5 about here.]

Unlike the other estimates, Table 6 has relatively consistent findings across specifications. This table creates a county-year-industry panel and uses two different treatment intensity measures. For the first measure I match industries with the probability a worker is a women conditional on industry, and for the second I match them to the probability that a worker works in that industry conditional on being a women; each of which is based on county level employment in 2000. I find that the employment elasticities are decreasing in the probability of being of women in that industry but increasing probability of choosing that industry conditional on being a woman. However, these results do not tell us if there is labor force entry or sector switching, which could arise if workers switch to jobs with lower gross earnings yet equal or greater net earnings.

[Table 6 about here.]

[24] This goes against a finding by Mikelson & Lerman (2004) who find that state EITC programs either reduce SNAP usage or is unrelated.

7 Conclusion

The Earned Income Tax Credit is one of the largest anti-poverty programs in the United States and is increasingly supplemented by the states. Many states explicitly justify their programs as an economic development tax expenditure. I documented variation in state EITC claims and expenditure amounts and test this claim using three empirical designs an a wide array of labor market margins. Specifically, I test for effects in federal EITC take up, SNAP and State Unemployment Insurance payments, county migration, county commuting, and aggregate, demographic, and industry level employment and earnings.

I find evidence that federal EITC take up and State UI payments increase, but I find little or mixed evidence on all other variables. However, using measures of potential EITC exposure, I do find evidence of heterogeneous effects on federal EITC take up and employment effects. I find that areas or industries with more unmarried women present, state EITC expansions are negatively correlated with take up and employment, but in areas with higher labor force participation or industries that attract more women expansions are positively correlated with outcomes.

My results imply that state EITC expansions do not function as economic development tools. Thus, state EITC function as an anti-poverty program but with little (or no) labor market distortions. My evaluation centered on the labor market effects, so it is possible that expansion increase local demand. This channel remains to be explored.

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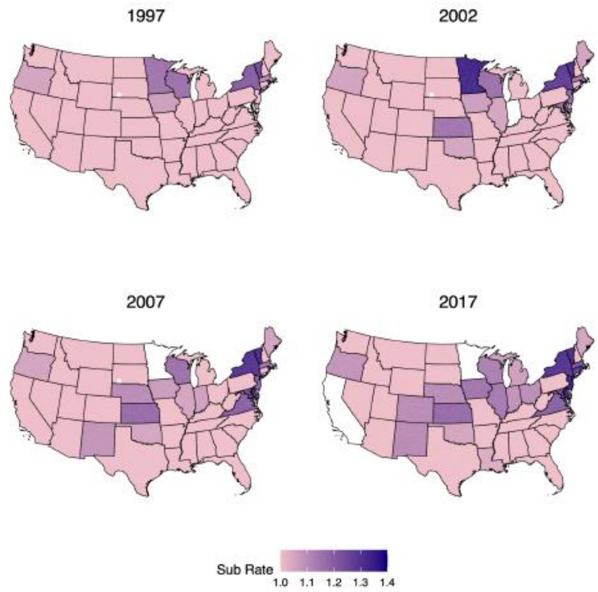
List of Figures

Figure 1 – State EITC Supplement Rates over Time



One plus state EITC supplement rate by state over time; CA, IN, MN programs are not based on uniform supplement rate.

Figure 2 – EITC Varitaion and Unmarried Mothers' Labor Supply

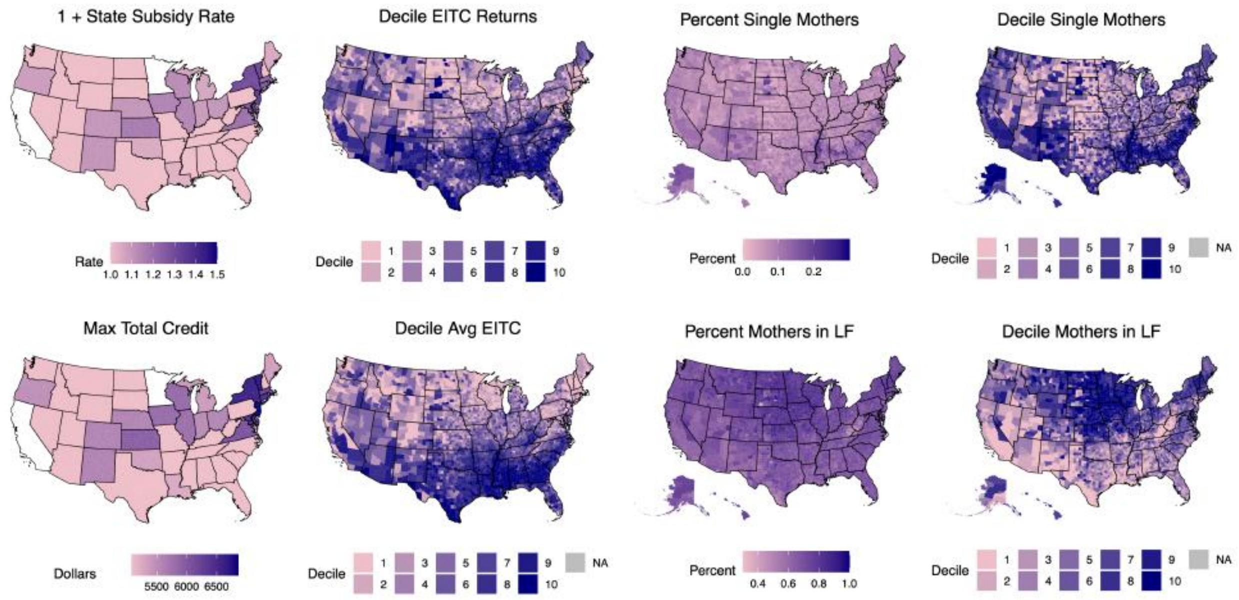
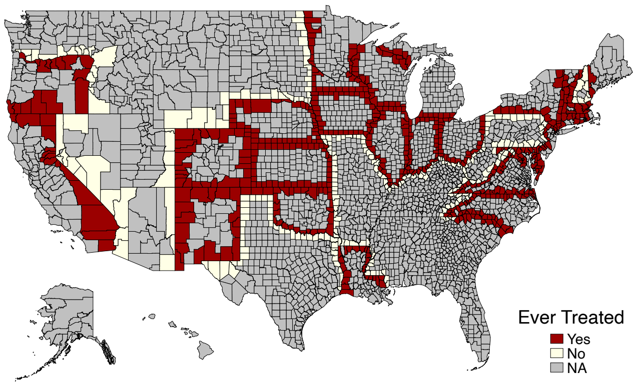


Figure 3 – Border Counties by Treatment Status



List of Tables

Table 1 – State EITC Returns and Amounts
Tax Years: 2017-2020 Most Recent Value

State (a)	Fed Returns (b)	Fed Amount (c)	State Returns (d)	State Returns (e)	% of Fed.Ret (f)	% of Fed.Amt (g)
CA	2,820,000	6,600,000,000	2,045,899	388,000,000	72.5%	5.9%
CO	332,000	729,000,000	342,908	74,004,128	103.3%	10.2%
CT	216,000	485,000,000	193,281	95,433,979	89.5%	19.7%
DE	72,000	174,000,000	–	13,600,000	–	7.8%
DC	49,000	115,000,000	62,513	78,728,000	127.6%	68.5%
HI	94,000	205,000,000	55,656	15,282,150	59.2%	7.5%
IL	922,000	2,300,000,000	913,663	315,509,710	99.1%	13.7%
IN	510,000	1,200,000,000	–	104,057,000	–	8.7%
IA	194,000	451,000,000	208,493	68,651,800	107.5%	15.2%
KS	195,000	469,000,000	196,790	79,324,844	100.9%	16.9%
LA	488,000	1,400,000,000	–	49,192,120	–	3.5%
ME	95,000	200,000,000	100,000	10,100,000	105.3%	5.1%
MD	394,000	922,000,000	–	165,949,106	–	18.0%
MA	382,000	811,000,000	–	204,700,000	–	25.2%
MI	743,000	1,900,000,000	–	117,940,000	–	6.2%
MN	316,000	699,000,000	315,274	243,663,067	99.8%	34.9%
MT	74,000	161,000,000	–	–	–	–
NE	126,000	301,000,000	119,640	28,610,000	95.0%	9.5%
NJ	583,000	1,400,000,000	–	440,400,000	–	31.5%
NM	199,000	493,000,000	197,794	49,549,000	99.4%	10.1%
NY	1,620,000	3,800,000,000	2,331,808	1,082,153,000	143.9%	28.5%
OH	887,000	2,200,000,000	783,641	179,394,697	88.3%	8.2%
OK	322,000	830,000,000	300,274	15,978,000	93.3%	1.9%
OR	257,000	542,000,000	246,729	48,958,843	96.0%	9.0%
RI	80,000	183,000,000	92,827	28,418,780	116.0%	15.5%
SC	468,000	1,200,000,000	59,529	21,426,714	12.7%	1.8%
VT	41,000	80,000,000	39,625	27,390,344	96.6%	34.2%
VA	583,000	1,400,000,000	346,817	135,826,748	59.5%	9.7%
WI	351,000	795,000,000	238,673	93,466,726	68.0%	11.8%

State EITC returns and amounts data accessed from individual state websites; typically state tax expenditure reports. Most recent value is reported. Federal EITC returns and amounts from tax year 2018 (IRS SOI). The New York number of returns uses both NY state and NY city EITC programs and likely double counts the number claims; I am exploring how to correct this. Sources for table in Appendix A.

Table 2 – Summary Statistics

Counties:	All Counties (a)	Full Sample (b)	Never Treated (c)	Ever Treated (d)
State EITC Program	40.4%	55.7%	0.0%	74.5%
	(0.70)	(0.74)	(0.00)	(0.73)
ln [Max State EITC]	8.38	8.40	8.32	8.43
	(0.00)	(0.03)	(0.00)	(0.00)
State EITC Supplement Rate	6.3%	9.0%	0.00	12.1%
	(0.13)	(0.14)	(0.00)	(0.18)
ln [County Population]	12.8	12.8	12.5	12.9
	(0.03)	(0.01)	(0.02)	(0.02)
ln [Real State GDP]	13.1	12.8	12.5	12.9
	(0.02)	(0.01)	(0.02)	(0.02)
ln [Fed Tax Returns w/ EITC]	10.2	10.1	9.94	10.2
	(0.04)	(0.03)	(0.04)	(0.04)
ln [SNAP]	10.7	10.6	10.4	10.6
	(0.04)	(0.04)	(0.04)	(0.05)
ln [State UI]	10.8	10.9	10.5	11.0
	(0.04)	(0.03)	(0.04)	(0.04)
Net Log Migration	0.37	0.38	0.38	0.37
	(0.00)	(0.00)	(0.00)	(0.00)
Net Log Low Wage Commuting	0.00	0.02	0.05	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
ln [Employment]	8.79	8.69	8.45	8.77
	(0.02)	(0.02)	(0.02)	(0.02)
ln [Avg Monthly Earnings]	7.57	7.59	7.56	7.60
	(0.00)	(0.00)	(0.00)	(0.00)
Counties	3,137	862	280	582

US Contiguous Counties, 2000-2018. Sample counties are only those with a policy difference at the border. Weighted by 2000 County Population. Never treated counties never enact a state EITC program; Ever Treated enact a state EITC during the sample period. There are 567 sample counties that at some point do not have a state EITC enacted.

Table 3 – Effect of State EITC Programs on Federal EITC Returns

Model:	TWFE	SBPFE	SBRDD:L	SBRDD:Q
Panel A: (Annual)				
ln [Max St.EITC]	0.11	0.15	0.50	0.62
	(0.10)	(0.07)	(0.20)	(0.22)
Observations	15,104	32,886	14,996	14,996
Panel B: (Annual)				
ln [Max St.EITC]	0.55	0.56	0.64	0.63
	(0.10)	(0.08)	(0.12)	(0.12)
ln [Max St.EITC] × %Unm.Mom	-4.8	-5.0	-3.8	-3.7
	(0.48)	(0.56)	(0.50)	(0.39)
Observations	15,104	32,886	14,996	14,996
Panel C: (Annual)				
ln [Max St.EITC]	-1.2	-1.3	-1.2	-1.2
	(0.33)	(0.35)	(0.35)	(0.45)
ln [Max St.EITC] × LFP:Unm.Mom	1.7	2.0	2.1	2.1
	(0.39)	(0.48)	(0.49)	(0.59)
Observations	15,092	32,862	14,984	14,984
Panel D: (Annual)				
St.EITC Prog	-0.004	0.01	-0.005	-0.0006
	(0.02)	(0.01)	(0.04)	(0.001)
Observations	15,104	32,886	14,996	14,996
Panel E: (Annual)				
St.EITC Prog	0.08	0.10	-0.00	-0.0003
	(0.04)	(0.03)	(0.05)	(0.001)
St.EITC Prog × %Unm.Mom	-0.83	-0.90	-0.04	-0.15
	(0.46)	(0.27)	(0.35)	(0.44)
Observations	15,104	32,886	14,996	14,996
Panel F: (Annual)				
St.EITC Prog	-0.23	-0.24	-0.17	0.00
	(0.10)	(0.07)	(0.10)	(0.00)
St.EITC Prog × LFP:Unm.Mom	0.32	0.35	0.24	0.09
	(0.13)	(0.09)	(0.13)	(0.07)
Observations	15,092	32,862	14,984	14,984
Cluster	State	State Border	State Border	State Border

Clustered standard errors in parentheses; there are 45 states for the TWFE, 78 state borders for the SBFE, and 70 state borders for the SBRDDs. All regressions weighted county population in 2000. Controls: log of total county returns and log of state real GDP.

Table 4 – Effect of State EITC Programs on Various Outcomes

Model:	TWFE (a)	SBPFE (b)	SBRDD:L (c)	SBRDD:Q (d)
Panel A: (Annual)	ln [SNAP Dollars]			
ln [Max State EITC]	0.37	0.13	-0.09	0.41
	(0.36)	(0.20)	(0.47)	(0.38)
Observations	14,628	31,776	13,440	13,440
Panel B: (Annual)	ln [State UI Dollars]			
ln [Max State EITC]	1.10	0.86	2.10	2.00
	(0.46)	(0.27)	(0.54)	(0.61)
Observations	15,877	34,376	14,623	14,623
Panel C: (Annual)	ln [In Migration]			
ln [Max State EITC]	0.13	-0.038	0.053	0.38
	(0.14)	(0.14)	(0.36)	(0.43)
Observations	13,994	30,273	13,896	13,896
Panel D: (Annual)	ln [Out Migration]			
ln [Max State EITC]	0.10	-0.11	-0.21	-0.078
	(0.14)	(0.079)	(0.15)	(0.13)
Observations	14,897	32,012	14,788	14,788
Panel E: (Annual)	ln [Net Low Wage Communiting Same State]			
ln [Max State EITC]	-0.063	0.096	-0.07	0.39
	(0.078)	(0.11)	(0.30)	(0.31)
Observations	13,257	28,296	13,170	13,170
Panel F: (Annual)	ln [Net Low Wage Communiting Neighbor State]			
ln [Max State EITC]	-0.42	0.36	-0.75	-0.61
	(0.85)	(0.49)	(0.57)	(0.69)
Observations	13,216	28,206	13,130	13,130
Panel G: (Quarterly)	ln [Total Employment: Less HS]			
ln [Max State EITC]	0.08	0.006	-0.29	-0.49
	(0.07)	(0.10)	(0.37)	(0.40)
Observations	62,941	136,455	62,678	62,678
Panel H: (Quarterly)	ln [Avg Earnings: Less HS]			
ln [Max State EITC]	-0.008	-0.10	-0.21	-0.17
	(0.06)	(0.05)	(0.10)	(0.11)
Observations	62,049	134,719	61,786	61,786
Cluster	State	State Border	State Border	State Border

Clustered standard errors in parentheses; there are 45 states for the TWFE, 78 state borders for the SBFE, and 70 state borders for the SBRDDs. All regressions weighted county population in 2000. Controls: log of total county population and log of state real GDP.

Table 5 – Effect of State EITC Programs
on QWI Employment and Earnings

Model:	TWFE	SBPFE	SBRDD:L	SBRDD:Q
	(a)	(b)	(c)	(d)
	DV: ln [Total Employment: Less HS]			
Panel A				
ln [Max St.EITC]	-0.057	-0.14	-0.32	-0.46
	(0.098)	(0.13)	(0.35)	(0.43)
ln [Max St.EITC] × %Unm.Mom	2.49	2.28	1.19	1.42
	(0.60)	(0.74)	(0.41)	(0.52)
Observations	64,313	136,680	62,274	62,274
Panel B				
ln [Max St.EITC]	0.33	0.55	-0.093	-0.13
	(0.33)	(0.30)	(0.29)	(0.42)
ln [Max St.EITC] × LFP:Unm.Mom	-0.28	-0.74	-0.24	-0.39
	(0.46)	(0.45)	(0.34)	(0.26)
Observations	64,310	136,674	62,271	62,271
	DV: ln [Avg Earnings: Less HS]			
Panel C				
ln [Max St.EITC]	0.006	-0.14	-0.19	-0.21
	(0.071)	(0.065)	(0.096)	(0.13)
ln [Max St.EITC] × %Unm.Mom	0.11	0.56	-0.26	-0.52
	(0.20)	(0.33)	(0.36)	(0.35)
Observations	63,399	136,680	61,377	61,377
Panel D				
ln [Max St.EITC]	0.058	0.10	0.056	0.027
	(0.15)	(0.11)	(0.10)	(0.099)
ln [Max St.EITC] × LFP:Unm.Mom	-0.048	-0.28	-0.32	-0.27
	(0.20)	(0.16)	(0.10)	(0.11)
Observations	63,393	136,674	61,371	61,371
Cluster	State	State Border	State Border	State Border

Clustered standard errors in parentheses; there are 45 states for the TWFE, 78 state borders for the SBFE, and 70 state borders for the SBRDDs. All regressions weighted county population in 2000. Controls: log of total county population and log state real GDP.

Table 6 – Effect of State EITC Programs on Industry Employment

Model:	TWFE (a)	SBPFE (b)	SBRDD:L (c)	SBRDD:Q (d)
	DV: ln [Employment]			
Panel A				
ln [Max State EITC]	-0.21	-0.11	-0.053	-0.098
	(0.12)	(0.062)	(0.23)	(0.18)
Observations	229,712	389,924	222,841	222,841
Panel B				
ln [Max St.EITC]	0.16	0.09	0.13	0.021
	(0.067)	(0.077)	(0.23)	(0.23)
ln [Max St.EITC] $\times$ %Women$_{cj}$	-0.38	-0.18	-0.36	-0.36
	(0.13)	(0.56)	(0.12)	(0.13)
Observations	217,705	376,852	211,204	211,204
Panel C				
ln [Max St.EITC]	-0.066	-0.044	-0.051	-0.15
	(0.082)	(0.083)	(0.28)	(0.28)
ln [Max St.EITC] $\times$ %of Women$_{cj}$	0.54	0.63	0.66	0.66
	(0.087)	(0.11)	(0.077)	(0.077)
Observations	217,723	376,852	211,222	211,222
Cluster	State	State Border	State Border	State Border

Clustered standard errors in parentheses; there are 45 states for the TWFE, 78 state borders for the SBFE, and 70 state borders for the SBRDDs. All regressions weighted county population in 2000. Controls: log of total county population and a two-digit industry fixed effect.

A Additional Data Sources

In Table 7 I list additional information about State EITC returns and expenditures. Most of this information comes from annual state tax expenditure reports. Some values are estimates, some are listed as exact data, and others are not described in the reports. Several reports state that EITC claims are a high quality data item compared with other items in the reports.

[Table 7 about here.]

B Additional Results

Here, I present additional empirical results. Tables 8-12 are based on additional outcomes variables that were not presented in the main text. Figures 7-9 use the DID_M of De Chaisemartin and d'Haultfoeuille (2020).

These additional results for the most part match the results in the main text with one exception. The QWI earnings results in the main text are negative and statistically significant; whereas, in Figure 8 (b) the event study graph shows a positive but statistically insignificant trend.

B.1 Event Study Graphs

In Figures 4-6, I plot event study version of the returns, employment, and earnings variables. To estimate these plots, I replace the treatment variable as:

$$\gamma^T T_{st} = \sum_{r=\underline{\mathrm{t}}, r\neq -1}^{\bar{\mathrm{t}}} \beta_r 1\left[(t - \text{Start}_s) = r\right], \tag{12}$$

where r is the event time variable, Start_s is the first year the state EITC program is active, $\underline{\mathrm{t}}$ is the earliest period in the event time, and $\bar{\mathrm{t}}$ is the last period in the event time. If a state never has an EITC program, then set $1\left[(t - \text{Start}_s) = r\right] = 0$ for all values of r. The β_r coefficients with $r \geq 0$ capture the dynamic treatment effect of the policy while for $r \leq 0$ serve as a placebo test for pre-existing trends.

Figure 4 shows positive treatment effects for the TWFE and SBFE graphs but the SBRD linear and quadratic graphs show an initially negative effect that wanes. This matches the estimates in Table 3.

Even though the returns estimates do not seem conclusive, the employment effects in Figure 5 shows positive treatment effects all the estimators. Figure 6 show negative trends before the state programs and then show mixed results for the treatment effects. The TWFE, SBFE, and SBRD linear models show positive effects, which contradicts Table 4 Panel H, while the SBRD quadratic has negative effects.

[Figure 4 about here.]

[Figure 5 about here.]

[Figure 6 about here.]

B.2 Additional Outcomes

[Table 8 about here.]

[Table 9 about here.]

[Table 10 about here.]

[Table 11 about here.]

[Table 12 about here.]

B.3 Event Study Plots

[Figure 7 about here.]

[Figure 8 about here.]

[Figure 9 about here.]

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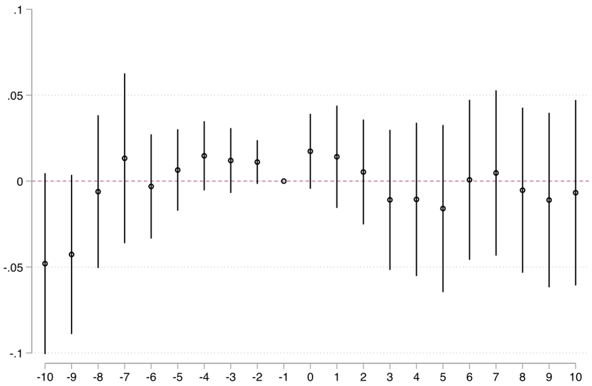
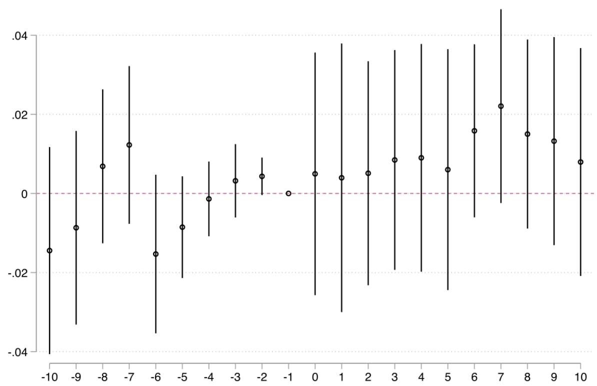
Figure 4 – Log Returns with EITC

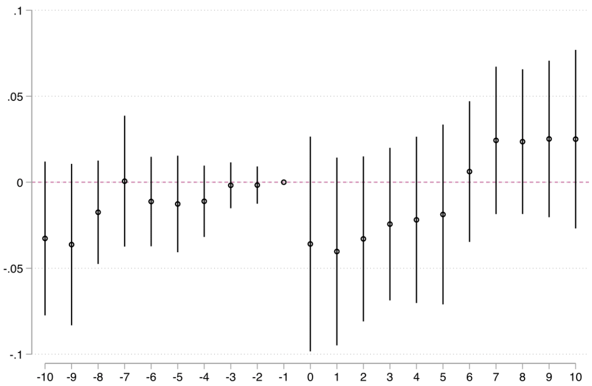
(a) TWFE



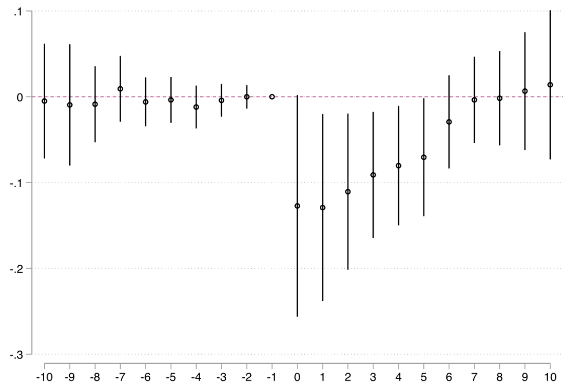
(b) SBFE



(c) SBRD: Linear

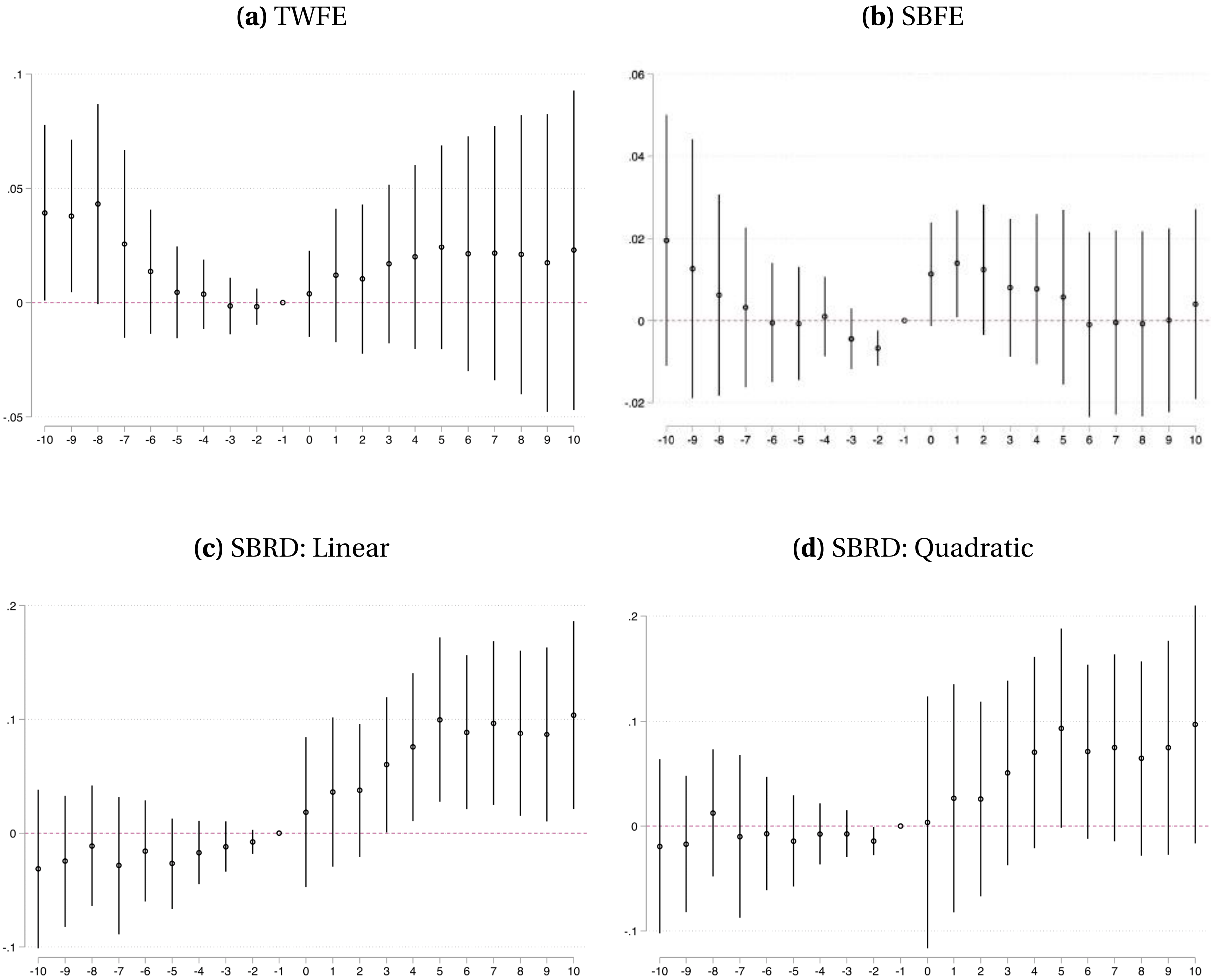


(d) SBRD: Quadratic



Data: IRS SOI.

Figure 5 – Log Employment

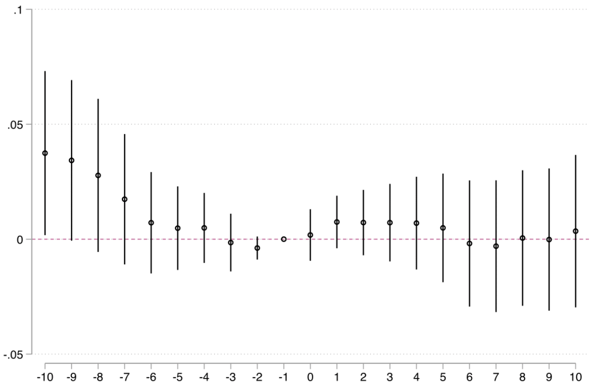


(a) TWFE **(b)** SBFE **(c)** SBRD: Linear **(d)** SBRD: Quadratic

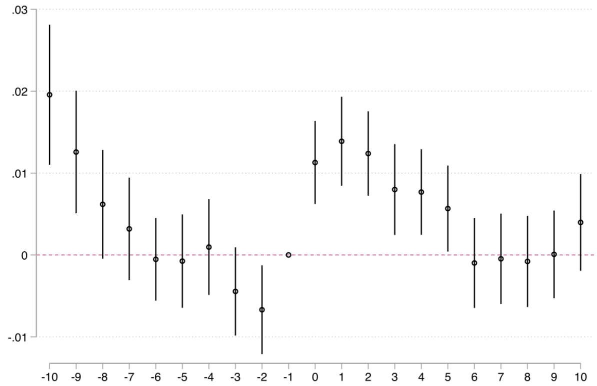
Data: QWI, Women with Less than HS Degree.

Figure 6 – Log Earnings

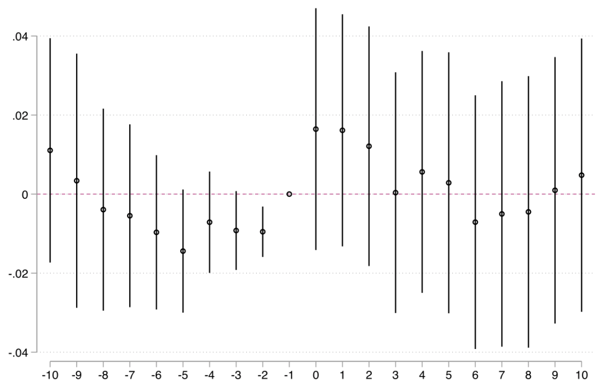
(a) TWFE



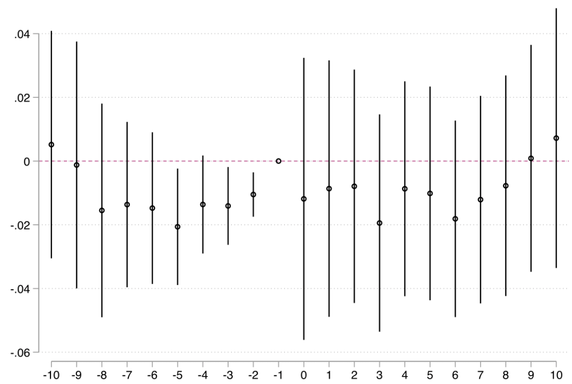
(b) SBFE



(c) SBRD: Linear

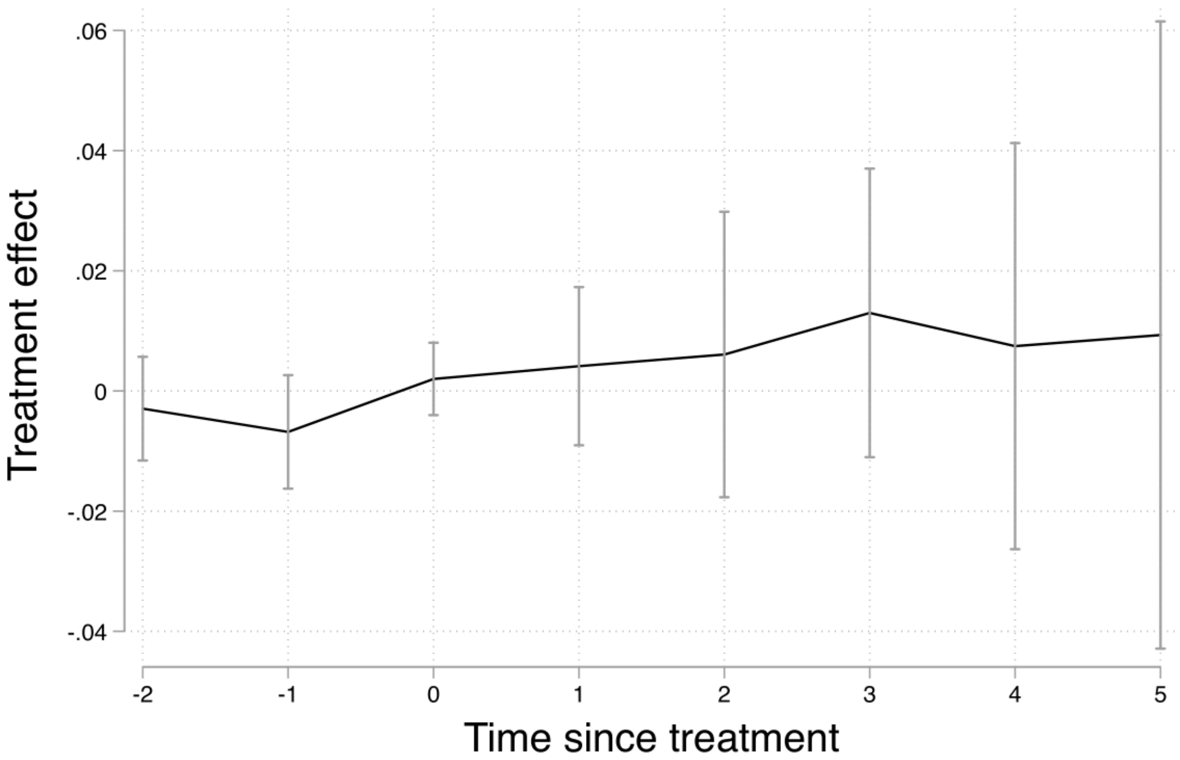


(d) SBRD: Quadratic



Data: QWI, Women with Less than HS Degree.

Figure 7 – Outcome: Log EITC Returns DID_M

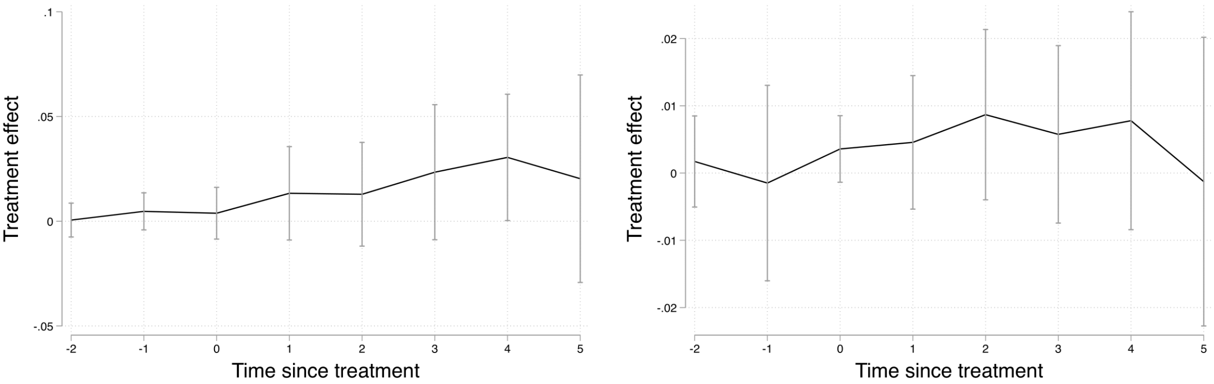


Data: IRS SOI

Figure 8 – Employment and Earnings

(a) Outcome: Log Employment
Less than HS: DID_M

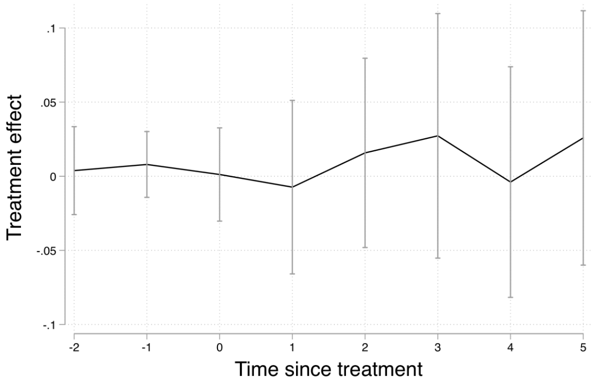
(b) Outcome: Log Avg Earnings
Less than HS: DID_M



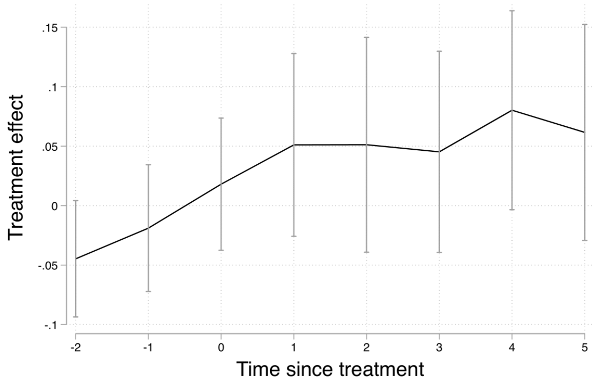
Data: QWI

Figure 9 – Industry Employment conditional on
1^{st} or 5^{th} Quintile

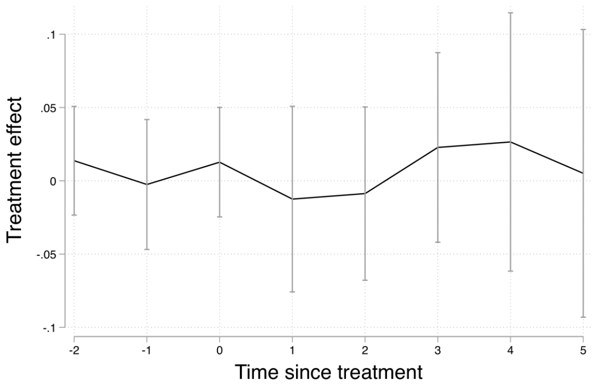
(a) Outcome: Log Employment
5^{th} Quintile Percent of Women: DID_M



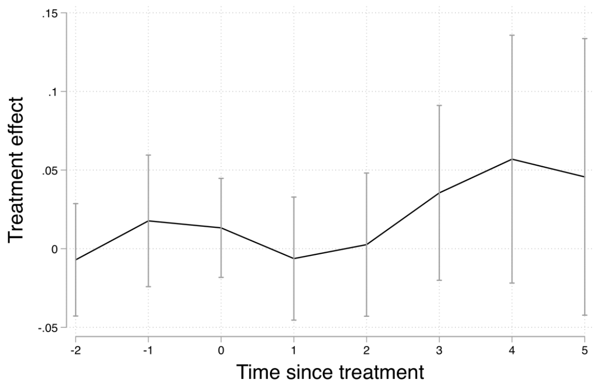
(b) Outcome: Log Avg Earnings
5^{th} Quintile Percent Women: DID_M



(c) Outcome: Log Employment
1^{st} Quintile Percent of Women: DID_M



(d) Outcome: Log Avg Earnings
1^{st} Quintile Percent Women: DID_M



Data: QCEW. Note: these plots conditions on being on the 1^{st} or 5^{th} quintile of the variable percent of women or percent women in the industry.

List of Tables

Table 7 – State EITC Returns and Amounts Sources

State (a)	Year (b)	URL (c)	Notes (d)
CA	2018	http://www.dof.ca.gov/Forecasting/Economics/Tax_Expenditure_Reports/documents/Tax_ExpenditureReport_2019-20_B.png	Forecast 1 billion in 2020
CO	2017	www.colorado.gov/pacific/sites/default/files/2019_Annual_Report_1.png	
CT	2018	portal.ct.gov/-/media/DRS/Research/annualreport/DRS-FY19-Annual-Report.png?la=en	
DC	ty 2020	cfo.dc.gov/node/1456456	Estimate
DE	fy 2020	finance.delaware.gov/financial-reports/tax-preference-report/	
HI	ty 2018	files.hawaii.gov/tax/stats/stats/act107_2017/act107_earnedincome_txcredit_2018.png	
IL	ty2017	www2.illinois.gov/rev/research/taxstats/IndIncomeStratifications/Documents/2017-IIT-1040ILReturn-Final.png	
IN	fy 2018	www.in.gov/sba/files/Tax%20Expenditure%20Report%20FY%202018-2021%20Final%20GW.png	Estimate
IA	fy 2018	tax.iowa.gov/sites/default/files/2019-08/Individual%20Income%20Tax%20Report%202017.png	Partial Estimate
KS	ty 2017	www.ksrevenue.org/png/ar19complete.png	
LA	fy 2018	lla.la.gov/PublicReports.nsf/8F85E9838E24E5308625831B00524FF5/$FILE/0001A8EC.png	
ME	fy 2018	www.maine.gov/revenue/research/tax_expenditure_report_17.png	Estimate
MD	fy 2018	dbm.maryland.gov/budget/Documents/operbudget/FiscalYear2018Tax%20ExpenditureReport.png	Includes Montgomery county
MA	fy 2018	www.mass.gov/doc/2020-tax-expenditure-budget/download	
MI	fy 2018	sigma.michigan.gov/EI360TransparencyApp/files/Tax%20Expenditure%20Reports/Tax%20Expenditure%20Report%202018.png	
MN	ty 2017	www.revenue.state.mn.us/minnesota-income-tax-statistics-county	Estimate
MT			No Information
NE	ty 2018	revenue.nebraska.gov/research/statistics/nebraska-statistics-income	Table F2
NJ	ty 2019	www.nj.gov/treasury/taxation/png/taxexpenditurereport2020.png	
NM	ty 2017	realfile.tax.newmexico.gov/ 2018%20NMTRD%20Tax%20Expenditure%20Report.png	
NY	ty 2018	www.tax.ny.gov/research/stats/stat_pit/earned_income_tax_credit/earned_income_tax_credit_analysis_of_credit_claims_open_data_short2.htm	NYS + NYC EITC
OH	ty 2018	www.tax.ohio.gov/tax_analysis/tax_data_series/individual_income/publications_tds_individual/Y1TY18.aspx	
OK	ty 2017	www.ok.gov/tax/documents/Tax%20Expenditure%20Report%202017-2018.png	
OR	ty 2017	www.oregon.gov/dor/programs/gov-research/Pages/research-personal.aspx	Returns are partial year
RI	ty 2018	digitalcommons.uri.edu/cgi/viewcontent.cgi?article=1774&context=srhonorsprog	Estimate
SC	ty 2018	dor.sc.gov/resources-site/publications/Publications/2018-2019_AnnualReport.png	
VT	ty 2018	tax.vermont.gov/sites/tax/files/documents/income_stats_2018_state.png	
VA	2019	www.tax.virginia.gov/sites/default/files/inline-files/2019-annual-report.png	
WI	ty 2018	www.revenue.wi.gov/Pages/RA/IIT-RefundableCredits.aspx	

Year descriptions are either Tax Year, Fiscal Year, or is ambiguous based on language of the state tax agency. I include when the agency declares that values are estimates, but this may not be comprehensive.

Table 8 – Effect of State EITC Programs on Federal EITC Returns

Model:	TWFE	SBPFE	SBRDD:L	SBRDD:Q
Panel A:		ln [Total Fed EITC Dollars]		
ln [Max State EITC]	0.07	0.13	0.62	0.82
	(0.12)	(0.09)	(0.23)	(0.33)
Panel B:		ln [Total Fed EITC Dollars]		
State EITC Indicator	-0.003	0.007	0.004	-0.11
	(0.02)	(0.02)	(0.03)	(0.05)
Observations	15,104	32,886	14,996	14,996
Cluster	State	State Border	State Border	State Border

Clustered standard errors in parentheses; there are 47 states for the TWFE, 74 state borders for the SBFE, and 65 state borders for the SBRDDs. All regressions weighted county population in 2000. Controls: log of total county returns (Panels A, B) and log of state real GDP.

Table 9 – Effect of State EITC Programs on Transfer Income

Model:	TWFE	SBPFE	SBRDD:L	SBRDD:Q
Panel A:		ln [Total SSI Dollars]		
ln [Max State EITC]	-0.06	-0.03	0.15	0.18
	(0.11)	(0.13)	(0.21)	(0.20)
Panel D:		ln [Total SSI Dollars]		
State EITC Indicator	-0.03	-0.05	-0.06	-0.27
	(0.03)	(0.03)	(0.05)	(0.21)
Observations	14,601	31,713	13,413	13,413
Cluster	State	State Border	State Border	State Border

Clustered standard errors in parentheses; there are 47 states for the TWFE, 74 state borders for the SBFE, and 65 state borders for the SBRDDs. All regressions weighted county population in 2000. Controls: log of state real GDP.

Table 10 – Effect of State EITC Programs on Migration

Model:	TWFE	SBPFE	SBRDD:L	SBRDD:Q
Panel A:		ln [Same State In Migrants]		
ln [Max State EITC]	0.29	0.10	0.007	0.17
	(0.18)	(0.21)	(0.33)	(0.38)
Panel B:		ln [Same State In Migrants]		
State EITC Indicator	0.00	0.04	-0.08	-0.16
	(0.03)	(0.03)	(0.05)	(0.12)
Observations	13,853	30,038	13,761	13,761
Panel C:		ln [Dif State In Migrants]		
ln [Max State EITC]	-0.05	-0.09	-0.04	0.13
	(0.19)	(0.20)	(0.57)	(0.65)
Panel D:		ln [Dif State In Migrants]		
State EITC Indicator	-0.03	-0.004	-0.16	0.03
	(0.03)	(0.03)	(0.08)	(0.19)
Observations	10,814	24,250	10,736	10,736
Panel E:		ln [Same State Out Migrants]		
ln [Max State EITC]	0.08	-0.01	-0.16	0.06
	(0.20)	(0.11)	(0.31)	(0.30)
Panel F:		ln [Same State Out Migrants]		
State EITC Indicator	-0.02	0.009	-0.11	-0.08
	(0.03)	(0.02)	(0.05)	(0.07)
Observations	14,123	30,650	14,026	14,026
Panel G:		ln [Dif State Out Migrants]		
ln [Max State EITC]	0.32	-0.17	-0.47	-0.40
	(0.18)	(0.13)	(0.30)	(0.25)
Panel H:		ln [Dif State In Migrants]		
State EITC Indicator	-0.02	-0.03	-0.07	-0.16
	(0.04)	(0.02)	(0.10)	(0.12)
Observations	11,003	24,707	10,919	10,919
Cluster	State	State Border	State Border	State Border

Clustered standard errors in parentheses; there are 47 states for the TWFE, 74 state borders for the SBFE, and 65 state borders for the SBRDDs. All regressions weighted county population in 2000. Controls: log of total county population and log of state real GDP.

Table 11 – Effect of State EITC Programs on Low Wage Commuting

Model:	TWFE	SBPFE	SBRDD:L	SBRDD:Q
Panel A:		ln [Same State In Commuters]		
ln [Max State EITC]	-0.21	-0.07	-0.41	-0.20
	(0.16)	(0.12)	(0.30)	(0.21)
Panel B:		ln [Same State In Commuters]		
State EITC Indicator	-0.03	-0.004	-0.12	-0.34
	(0.02)	(0.02)	(0.15)	(0.29)
Observations	13,257	28,782	13,170	13,170
Panel C:		ln [Dif State In Commuters]		
ln [Max State EITC]	-0.28	0.08	-0.22	-0.25
	(0.19)	(0.18)	(0.42)	(0.43)
Panel D:		ln [Dif State In Commuters]		
State EITC Indicator	0.005	0.04	0.05	-0.16
	(0.02)	(0.04)	(0.09)	(0.15)
Observation	13,236	28,764	13,149	13,149
Panel E:		ln [Same State Out Commuters]		
ln [Max State EITC]	-0.15	-0.16	-0.35	-0.50
	(0.18)	(0.13)	(0.19)	(0.20)
Panel F:		ln [Same State Out Commuters]		
State EITC Indicator	-0.03	-0.02	-0.05	-0.32
	(0.02)	(0.02)	(0.06)	(0.25)
Observations	13,258	28,784	13,171	13,171
Panel G:		ln [Dif State Out Commuters]		
ln [Max State EITC]	0.14	-0.29	0.49	0.26
	(0.81)	(0.39)	(0.31)	(0.34)
Panel H:		ln [Dif State In Commuters]		
State EITC Indicator	0.05	-0.04	-0.004	-0.18
	(0.06)	(0.04)	(0.08)	(0.15)
Observations	13,244	28,785	13,158	13,158
Cluster	State	State Border	State Border	State Border

Clustered standard errors in parentheses; there are 46 states for the TWFE, 79 state borders for the SBFE, and 70 state borders for the SBRDDs. All regressions weighted county population in 2000. Controls: log of total county population and log of state real GDP.

Table 12 – Effect of State EITC Programs on Women's Employment

Model:	TWFE	SBPFE	SBRDD:L	SBRDD:Q
Panel A:		ln [Employment: Total]		
ln [Max State EITC]	0.06	0.06	-0.25	-0.40
	(0.04)	(0.07)	(0.30)	(0.35)
Panel B:		ln [Employment: Total]		
State EITC Indicator	0.007	0.03	0.02	0.14
	(0.009)	(0.005)	(0.02)	(0.04)
Observations	63,308	137,331	63,045	63,045
Panel C:		ln [Avg Earnings: Total]		
ln [Max State EITC]	0.03	-0.04	-0.07	-0.009
	(0.03)	(0.03)	(0.12)	(0.15)
Panel D:		ln [Avg Earnings: Total]		
State EITC Indicator	0.006	0.007	0.01	0.14
	(0.006)	(0.005)	(0.02)	(0.06)
Observation	62,158	135,108	61,895	61,895
Panel E:		ln [Employment: Less HS]		
ln [Max State EITC]	0.08	0.006	-0.29	-0.49
	(0.07)	(0.10)	(0.37)	(0.40)
Panel F:		ln [Employment: Less HS]		
State EITC Indicator	0.002	0.04	0.06	0.18
	(0.01)	(0.01)	(0.04)	(0.10)
Observations	62,941	136,455	62,678	62,678
Panel G:		ln [Avg Earnings: Less HS]		
ln [Max State EITC]	-0.008	-0.10	-0.21	-0.17
	(0.06)	(0.05)	(0.10)	(0.11)
Panel G:		ln [Avg Earnings: Less HS]		
State EITC Indicator	0.005	0.01	0.008	0.16
	(0.007)	(0.009)	(0.02)	(0.08)
Observation	62,049	134,719	61,786	61,786
Cluster	State	State Border	State Border	State Border

Clustered standard errors in parentheses; there are 45 states for the TWFE, 78 state borders for the SBFE, and 70 state borders for the SBRDDs. All regressions weighted county population in 2000. Controls: log of total county population and log of state real GDP.