

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

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Preliminary Work

Abstract

Twenty eight states spend \$4 billion to supplement the federal Earned Income Tax Credit, with several justifying the tax expenditure as a pro-work incentive. Yet no systematic evaluation of these supplements exists. I use state border policy variation to identify state supplements effects. I first document that subsidy rates are greater when a state's neighbor already has a supplement. Next, I assess whether supplements affect county level EITC take-up, migration, commuting, employment, and earnings. Estimates are sensitive to the estimation design and sample used. While supplements increase benefits to low-income workers, results fail to provide robust evidence of increased economic activity.

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

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1 Introduction

Twenty eight states spend over \$4 billion annually to supplement the federal Earned Income Tax Credit.¹ In tax expenditure reports, several states explicitly justify the supplements as a pro-work incentive, while others justify their programs using an anti-poverty rationale. Yet, there is much we do not know about these state level programs. Do they increase federal EITC take up? Do they cause workers to migrate or commute across borders? Do they spur labor supply and employment? Do they pay for themselves?

While previous analyses have used state EITC 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

I evaluate these questions at the county level using two empirical designs that exploit policy variation across state borders. First, I use a state border pair fixed effect design (SBFE) that generalizes a case-study approach while controlling for local economic conditions, similar to Holmes (1998); Huang (2008); Dube, Lester and Reich (2010). Second, I use a state border distance regression discontinuity design (SBRD) that accounts for the degree a county's economic activity occurs near a state border, similar to Dieterle, Bartalotti and Brummet (2020).

I first describe the EITC policy variation across states along with suggestive evidence of strategic subsidy competition between states. Second, I describe a model that yields a measure of the fiscal externality of the state policies in terms of estimable elasticities that can be used for economic evaluation of the programs. The model is based on Monte et al. (2018) and allows for migration, commuting, and an extensive labor supply choice. Finally, I conduct and report my empirical findings, which I briefly summarize below.

For my outcome variables, I use data from the IRS Statistics of Income (EITC take up and migration), the Census Longitudinal Origin and Destination Statistics (commuting and employment), and the Census Quarterly Workforce Indicators (employment and earnings). Like previous studies, I use state maximum credit amounts and state expansion timing to measure state policy variation.

A novel fact that I document is that states that border other states which have already implemented an EITC supplement tend to themselves implement more generous

¹This is based on state tax return and tax expenditure reports from tax years 2017 to 2019—see Table 5—and, as far as I am aware, this fact has not been documented given the decentralized nature of state tax expenditures.

²For 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.

³Specifically, 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.

⁴Buhlmann 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.

supplements to match their neighbor's incumbent program. I find that these second-mover states make their state EITC subsidy rates on average 7 percentage points more generous, which is over 50% more generous than states that do not have a neighboring incumbent program. Additionally, I present suggestive evidence that the states which have already implemented supplements tend to make their supplements roughly 2 percentage points more generous the five years after their neighbor implements a supplement.

This could imply that state supplement variation is subject to underlying trends in near-by states that is also correlated with labor market variables in the expansion state. This threat to identification of causal EITC effects has not been explored previously, as far as I am aware.

Given the above, I separate the results by comparing all state border policy variation and the subset of borders where only side of the border has a state supplement (one-sided borders). I find that the results are highly dependent on empirical strategy and the sample used. When pooling all possible state borders, results are typically larger in magnitude and estimate signs are consistent with the EITC boosting labor market activity. However, when using the subset of borders with only one state supplement and more recent state programs, results are often smaller in magnitude and/or opposite sign as the pooled results.

For example, using the SBFE strategy, I find that the elasticity (and its robust standard error) between county Federal EITC returns and maximum state EITC amount is 0.16 (0.05) using all borders, 0.11 (0.09) using only one-sided borders, and 0.01 (0.13) using one-sided borders with supplements enacted after 2000, where state-border clustered standard errors are in parentheses. Using the SBRD strategy for the same three subsets, the elasticity is 0.43 (0.17), 0.23 (0.39), and -0.17 (0.28), respectively. When I use an event-study approach, I find that one-sided borders appear to dynamic treatments effects centered around zero.

In aggregate, my results suggest a modest increase in federal EITC take-up, no effect on migration or commuting, and an inconclusive effect on employment and earnings. 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 terms of labor market effects. This result implies that state EITC supplements do not fulfill the economic development justification of some states for their implementation. 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

Currently, 28 states, the District of Columbia, and two municipalities have implemented supplements to the federal EITC. Two justifications for EITC programs as that they pro-

vide economic stimulus benefits and/or provide economic relief to low income workers. Michigan justifies its program using the former: “*The earned income tax credit, at both the federal and state levels, is intended to increase work effort and attachment to the labor force and is a good example of a tax expenditure designed to influence taxpayer behavior*” (Executive Budget Appendix on Tax Credits, Deductions, and Exemptions).” While California includes the latter justification in the text of the law itself: “...*The purpose of the California Earned Income Tax Credit is to reduce poverty among California’s poorest working families and individuals*” (CA Rev & Tax Code §17052.12, 2018).⁵

Table 1 reports state and federal tax returns with EITC claims and expenditure amounts, along with the statutory state subsidy rate, the percent of state claims over federal, and the percent of state expenditure over federal. The table shows that number of state EITC claim roughly matches federal claims; the average of column (f) is roughly 90%. However, the tax expenditure of each state is typically much lower given that state supplement rates are bounded between 0% and 40% across states. The average of column (h) is 15.7% which is slightly lower than the average state supplement rate in column (b), 17.1%.

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.⁶ 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 also 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 Midwest. 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.⁷ 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 plots policy variation at state borders due to state supplements across several dimensions. All three plots are plotted in ‘event-time’ of a state EITC implementation that has occurred after 2000. Figure (2.a) shows the average impact of a state supplement on the real max state credit across all state borders. The average benefit difference across state borders is \$466 or a 9.5% increase in generosity, which is roughly a 1-2% increase in annual gross earnings for a single tax-filer with one qualifying dependent in the max-credit region.

Figure (2.b) compares the state supplement rates between state borders that are one-sided, meaning that there is no incumbent program on either side of the state border, versus two-sided, meaning that there is an incumbent program already in effect on the other side of the border already has a program implemented. The plot shows that states

⁵For eight states, I find justifications of state EITC from laws, tax expenditure reports, or other official documents—specifically: CA, CO, LA, ME, MI, NJ, NM, and VY. Oregon tax expenditure reports explicitly state a lack of official purpose from the legislature; I have found official statements for other programs.

⁶States in red supplement the Fed EITC but do so using a non-standard supplement schedule; i.e., do not use a ‘top-up’ rule. In the regression specifications, I include these states by finding the maximum state credit associated with their state policy.

⁷The 2000 county level distribution of unmarried mothers in the labor force is not available.

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

State	Subsidy Rate (%)	Fed Claims (1000s)	Fed Amount (\$millions)	State Claims (1000)	State Amount (\$millions)	State % of Fed Claims	State % of Fed Amount
(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
CA	–	2,820	6,600	2,046	388	72.5	5.9
CO	10.0	332	729	343	74	103.3	10.2
CT	27.5	216	485	193	95	89.5	19.7
DE	20.0	72	174	–	14	–	7.8
DC	40.0	49	115	63	79	127.6	68.5
HI	20.0	94	205	56	15	59.2	7.5
IL	18.0	922	2,300	914	316	99.1	13.7
IN	9.0	510	1,200	–	104	–	8.7
IA	15.0	194	451	208	69	107.5	15.2
KS	17.0	195	469	197	79	100.9	16.9
LA	3.5	488	1,400	–	49	–	3.5
ME	5.0	95	200	100	10	105.3	5.1
MD	28.0	394	922	–	166	–	18.0
MA	23.0	382	811	–	205	–	25.2
MI	6.0	743	1,900	–	118	–	6.2
MN	–	316	699	315	244	99.8	34.9
MT	3.0	74	161	–	–	–	–
NE	10.0	126	301	120	29	95.0	9.5
NJ	37.0	583	1,400	–	440	–	31.5
NM	10.0	199	493	198	50	99.4	10.1
NY	30.0	1,620	3,800	2,332	1,082	143.9	28.5
OH	10.0	887	2,200	783	179	88.3	8.2
OK	5.0	322	830	300	16	93.3	1.9
OR	8.0	257	542	247	49	96.0	9.0
RI	15.0	80	183	93	28	116.0	15.5
SC	20.1	468	1,200	60	21	12.7	1.8
VT	36.0	41	80	40	27	96.6	34.2
VA	20.0	583	1,400	347	136	59.5	9.7
WI	15.0	351	795	239	93	68.0	11.8

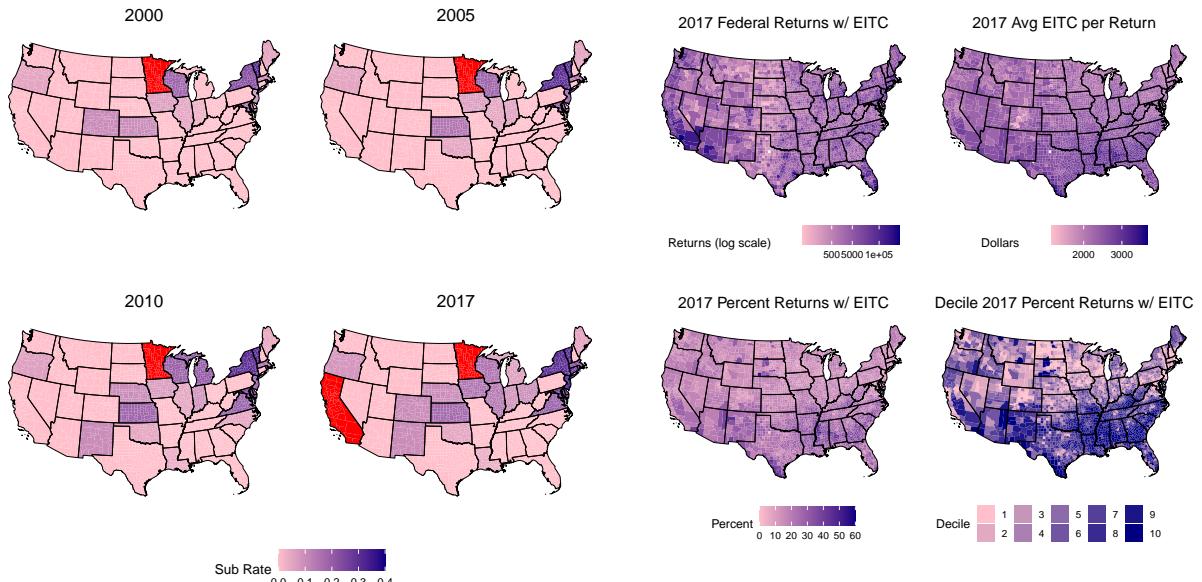
State EITC returns and amounts data accessed from individual state websites; typically state tax expenditure reports. Most recent value is reported. Federal EITC returns and amounts from tax year 2018 (IRS SOI). The New York number of returns uses both NY state and NY city EITC programs and likely double counts the number claims. CA and MN have non-standard programs that do not map into a single subsidy rate. MT implemented its program for tax year 2019 and has not released expenditure reports. Sources for table in Appendix A.

implement more generous subsidies when their neighbor already has a state program; on average 7 percentage points more generous than one-sided borders.

Figure (2.c) plots the incumbent state's policy reaction to their neighbor's new supplement. Specifically, the plot shows whether the incumbent's subsidy rate in each period is statistically different from the rate the year before the new program is implemented. The result is suggestive that incumbents make their programs on average 2 percentage points more generous in the five years after their neighbor's implementation.

Figures (2.b-c) imply that state borders where both sides have state supplements may not be setting their state supplement rate completely exogenously, which may limit what can be learned from expansions along these borders.

Figure 1 – EITC Policy and Use Variation



Note: maps state supplement rates over four years, where pink indicates no supplement, darker blues indicate more generous subsidies, and red indicates a non-standard supplement; maps county level IRS tax return data for tax year 2017.

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 microfound a sufficient statistic approach to evaluate state EITC supplements.⁸

Let \mathcal{S} be the set possible locations – ‘counties’ – in the economy, and the counties in \mathcal{S} can be partitioned into M ‘states,’ $\mathcal{S} = \{S_1, S_2, \dots, S_M\}$. 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:

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

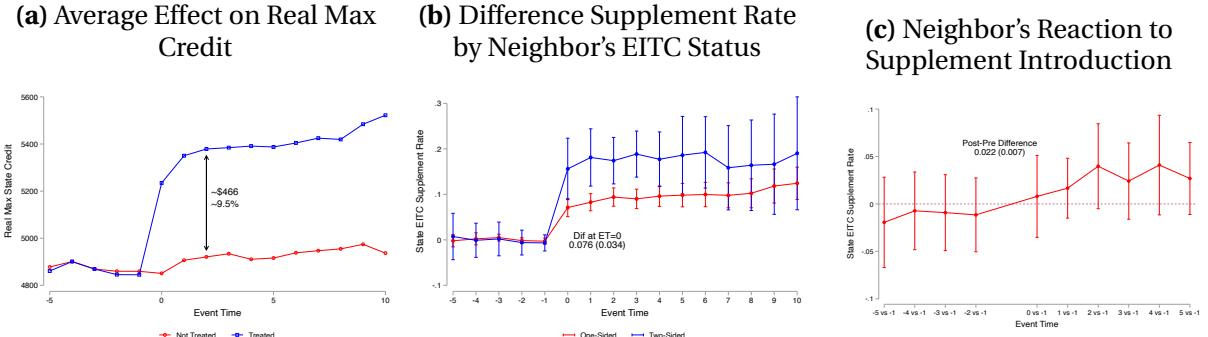
That is, individuals have a two-stage decision process such that they first choose a residence location, $o \in \mathcal{S}$, and then a work choice $d \in \{\mathcal{S} \cup \{\text{Unemployment}\}\}$ based on some (potentially endogenous) indirect utility value; e.g., a residence-location specific amenity plus post-tax earnings. I denote agent i ’s choice bundle as (o^i, d^i) .⁹

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

⁸The 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.

⁹Such 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.

Figure 2 – Effect of State Supplement Implementation



Note: (a) plots the average change in real max credit across all state supplements introduced after 2000; (b) plots regression coefficients of state supplement rates on event-time indicators interacted with whether a state's neighbor also has a state supplement controlling for year FEs with state-border clustered standard errors; (c) plots regression coefficients of the incumbent neighbor's supplement rate on event-time indicators controlling for year FEs with White standard errors (due to two few clusters).

government s uses residence based income taxation¹⁰ with origin-destination specific tax rates, $R^s = \sum_{o \in s} R^o = \sum_{o \in s} (\sum_{d \in S} t_d^o w_d^o \pi_{d|o} \pi_o)$, then the first-order¹¹ fiscal externality as a proportion of initial revenue is (where $\hat{x} = dx/x$):

$$\frac{FE^s}{R^s} = \sum_{o \in s} \frac{R^o}{R^s} \left(\underbrace{\hat{\pi}_o}_{\text{Migration}} + \underbrace{\frac{R^o}{R^s} (\hat{w}_o^o + \hat{\pi}_{o|o})}_{\text{Own Employment}} + \underbrace{\sum_{d \in S \setminus o} \frac{R_d^o}{R^s} \cdot (\hat{w}_d^o + \hat{\pi}_{d|o})}_{\text{Commuting}} \right). \quad (2)$$

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 ($g^i = 1$): $\frac{dW/d\theta}{\mu}|_{g^i=1} = FE$ (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.

Ultimately, this study only estimates the causal change in real economic variables and does not attempt a welfare evaluation. The estimated elasticities, reported below in the next section, do not capture the local heterogeneity of behavioral responses implied by the model, but do give a hint towards their magnitude in order to assess the fiscal externality.

4 Empirical Designs

I use two empirical designs on the set of counties that are at state borders with a policy difference. The first is a state-border fixed effect (SBFE) design that removes common

¹⁰If 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.

¹¹That is, assuming multiplicative terms are negligible: $\hat{x} \cdot \hat{y} \approx 0$.

time-varying shocks between each border county pair. The second 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 C be the maximum combined state and federal EITC credit in the state, and let T be an indicator equal to one if the state's program is active. For all the regressions, I control for log population (or log tax returns), log real state GDP, county fixed effects, and either county-pair-year fixed effects or state-border-year fixed effects interacted with distance to the state border. In addition, I weight all regressions by county population in 2000. I explain each design in more detail separately.

4.1 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 interpreted causally.

The regression equation is:

$$y_{cpst} = X_{cpst}\beta + \gamma^C \ln [C_{st}] + \lambda_c + \lambda_{pt} + u_{cpst}, \quad (3)$$

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.

4.2 State Border Regression Discontinuity

The SBRD design takes seriously the idea of a spatial discontinuity in policy at the state border by modeling the difference in expected outcome as a function of 'economic distance' to the border. Holmes (1998) provides these distance measures.

An ideal study would use as fine a local geography as possible, such as census blocks, to take full advantage using distance to the border. Dieterle et al. (2020) note that counties are not ideal for this analysis since counties are a political jurisdiction rather than an economic market area and county land area varies greatly by state.¹² I use a global polynomial method because the implied measurement error from using counties forces the use of a parametric method rather than a non-parametric local method.

The regression equation is:

$$\begin{aligned} y_{csbt} = & X_{csbt}\beta + \gamma^C \ln [C_{st}] + \lambda_c + \lambda_{bt} \\ & + D_{bt} \cdot \left[1 + \sum_k \theta_{bt}^{0;k} (1 - T_{st}) \cdot m_{csb}^k + \sum_k \theta_{bt}^{1;k} T_{st} \cdot m_{csb}^k \right] + u_{cst}, \end{aligned} \quad (4)$$

¹²These authors use census block employment weighted county centroids, while my analysis uses population weighted.

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.¹³ For inference, I cluster standard errors at the state-border level.

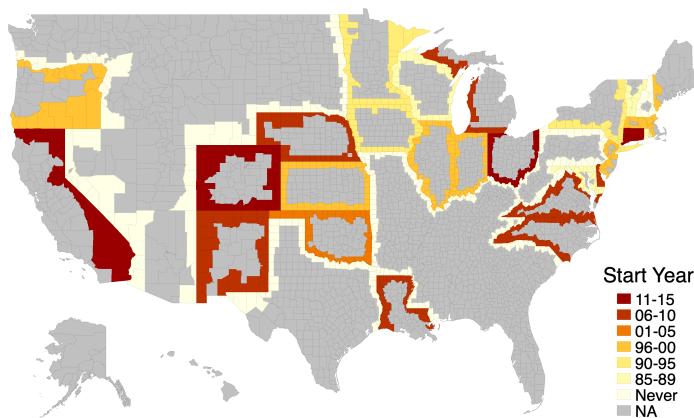
5 Data

The data used in the analysis is based on the contiguous border counties in the United States. There are 3,144 county equivalents (including DC) in the US of which 1,184 share a border segment with another county, but only 905 have a policy discontinuity due to a state EITC program at some point in time. 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 2018.¹⁴ I focus on the period starting in 2000 to avoid using variation from the 1994 OBRA expansion and welfare reform in the late 1990s. Figure 3 shows the specific counties used in the paper by state supplement start.¹⁵

The tax return data is from the IRS Statistics of Income.¹⁶ The migration data is also based on the IRS Statistics of Income County to County Flows.¹⁷ The commuting 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.

For the migration and commuting data, I calculate the net migration / commuting percent as the difference of entrants minus exiters over an initial local value. Specif-

Figure 3 – Border Counties by Treatment Status



Note: maps the counties used in the empirical section by state supplement program implementation groups, where darker colors are more recent and grey counties are either in a state's interior or non-continental states (AK and HI).

¹³This 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.

¹⁴Because I use a continuous variable as the treatment, log max state credit, all border counties provide identifying variation even if both states have an EITC program. In supplemental analysis where I use treatment timing for policy variation, I ‘stack’ the state borders in event time, which ensures only one state is treated in the estimation window.

¹⁵CO had a short-lived program from 1999-2001 that I have omitted; SC’s program started in 2018.

¹⁶I accessed the 2000 to 2010 EITC returns data from the Brookings Institute via Cecile Murray.

¹⁷The years 1990 to 2000 are adapted from pre-formatted files from Hauer (2019).

ically, the net migrants percent is entrants minus exiters divided by the start of year county residents, while the net commuting percent is the difference between in-commuters and out-commuters divided by employed county residents (equal to the out-commuters plus non-commuting workers).

I collect state EITC parameters from the supplementary information for NBER's Taxsim (Feenber and Coutts, 1993).¹⁸ 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.

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 905 contiguous border counties that I use in the estimation. Counties that are never-treated (c) appear to differ relatively more from all counties in column (a) than the ever-treated counties (d).

Table 2 – Summary Statistics

Counties:	All Counties	Full Sample	Never Treated	Ever Treated
	(a)	(b)	(c)	(d)
State EITC Program	40.4% (0.70)	62.2% (1.01)	0.0% (0.00)	81.5% (0.90)
ln [Max State EITC]	8.38 (0.00)	8.42 (0.00)	8.32 (0.00)	8.45 (0.01)
State EITC Supplement Rate	6.3% (0.13)	11.3% (0.27)	0.00 (0.00)	14.9% (0.32)
ln [County Population]	12.8 (0.03)	12.8 (0.01)	12.3 (0.01)	13.0 (0.01)
ln [Real State GDP]	13.1 (0.02)	12.9 (0.02)	12.5 (0.03)	13.1 (0.02)
ln [Fed Tax Returns w/ EITC]	10.2 (0.04)	10.2 (0.05)	9.8 (0.05)	10.4 (0.06)
Net Migration Percent	-0.03% (0.018)	0.063% (0.031)	0.16% (0.041)	0.011% (0.042)
Net Low+Mid Wage Commuting Percent	-0.45% (0.37)	4.65% (0.98)	2.93% (0.63)	5.16% (1.26)
ln [Employment]	8.79 (0.02)	9.79 (0.01)	9.31 (0.01)	9.94 (0.01)
ln [Avg Monthly Earnings]	7.49 (0.00)	7.89 (0.00)	7.81 (0.00)	7.91 (0.00)
Counties	3,137	905	294	611

US Contiguous Counties, 2000-2018. Columns b-d use border-counties 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.

¹⁸I have also manually checked the values by going to the various state websites.

6 Results

Given the patterns shown in Figure 2, I present three sets of results. First, I present results that use all possible state borders with at least one year of a policy discontinuity. Second, I focus on the one-sided state borders where only one state has a EITC supplement for the whole sample period. Each of these use variation in the maximum credit available in the state based on the state's subsidy rate.

In the third set of results, I use variation in state policy timing rather than max state credit to estimate state EITC effects. I present these results using stacked event study estimates, separating results by whether the border is one-sided or two-sided. I describe this approach in greater detail below.

6.1 All Borders

Table 3 displays the results using all borders. The table presents either elasticities (returns, employment, earnings) or semi-elasticities (net migration percent, net commuting percent). Recall, on average a state program increases EITC generosity by about 9.5 percent.

Panel A shows that a 1% increase in EITC generosity induced between [0.16, 0.43] percent additional Federal EITC returns for the county relative to counties across the state border. Each elasticity is statistically significantly different from zero. This result implies that state supplements induce greater take-up of the federal EITC either due to greater awareness or increasing earnings to require filing a tax return.

Panels B and C display a semi-elasticity estimate; i.e., a one-percent change in generosity implies a $\beta/100$ percentage point change in the net migration / commuting. Neither set of estimates is statistically different from zero. The migration coefficient estimates are between [0.003, 0.018] from a mean of 0.0063, which translates to an elasticity between [0.47, 2.86] at the mean. The commuting coefficient estimates are between [-0.59; -0.23] from a mean of 0.0465, which translates to an elasticity between [-12.7, -4.9] at the mean. While the migration elasticities seem plausible, taken literally the commuting elasticities would imply huge economic effects given that generosity increased by 10 percent.

Panels D and E display employment and earning elasticities. The estimated employment elasticity is between [-0.35, -0.08], which would imply that state supplements decrease the number of workers in a county relative to counties across the border; although, none of the estimates are statistically different from zero. The estimated earnings elasticity is between [-0.14, -0.11], which would imply that state supplements decrease the workers' earnings in a county relative to counties across the border. These estimates are each statistically different from zero. Assuming the employment effect is weakly negative, the negative earnings effect could be the result of workers reducing their hours (an income effect) or potential subsidy capture by employers.¹⁹

¹⁹Income effects due to the EITC are typically assumed to be small or non-existent; incidence effects of the EITC are explored in Leigh (2010); Rothstein (2010); Watson (2020).

Table 3 – Effect of State EITC Programs: All Borders

Model:	SBPFE (a)	SBRDD:L (b)	SBRDD:Q (c)
Panel A: (Annual)	ln [Fed EITC Returns]		
ln [Max St.EITC]	0.16 (0.05)	0.25 (0.19)	0.043 (0.19)
Observations	34,790	18,606	18,606
Panel B: (Annual)	Net Migration Percent		
ln [Max State EITC]	0.003 (0.006)	0.005 (0.011)	0.018 (0.014)
Observations	34,812	18,612	18,612
Panel C: (Annual)	Net Low+Mid Wage Commuting Percent		
ln [Max State EITC]	-0.23 (0.43)	-0.59 (0.43)	-0.28 (0.42)
Observations	32,878	17,578	17,578
Panel D: (Quarterly)	ln [Total Employment: Women, Less HS]		
ln [Max State EITC]	-0.08 (0.09)	-0.21 (0.19)	-0.35 (0.22)
Observations	141,129	75,234	75,234
Panel E: (Quarterly)	ln [Avg Earnings: Women, Less HS]		
ln [Max State EITC]	-0.11 (0.05)	-0.14 (0.04)	-0.12 (0.05)
Observations	139,518	65,394	65,394
Cluster	State Border	State Border	State Border

Clustered standard errors in parentheses; 78 clusters. Regressions weighted county population in 2000. Controls: log of county population (log total returns in Panel A) and log of state real GDP.

6.2 One-Sided Borders

Table 4 displays the results using only one-sided borders borders with state implementations after 2000. This subsample mirrors the event study analysis presented in the next subsection but uses maximum credit variation as in Table 3.

On balance, these results fail to provide evidence of recent state EITC supplements affecting labor market outcome. In Panel A, instead of being positive and statistically different from zero, the new returns elasticity is near-zero for the SBFE and negative for the SBRD. In Panel B, the migration semi-elasticities are similar in magnitude as before and are still statistically indistinguishable from zero. In Panel C, the commuting semi-elasticities magnitudes vary by three orders of magnitude depending on the design. In

Panel D, the employment elasticities are now all positive rather than negative. In Panel E, two of the earnings elasticities are now also positive.

The inconsistency in the results stems from two sources. First, the subsample uses fewer state-borders and thus many fewer observations. Second, the states borders used in subsample could have different properties than states with older programs which could reflect different underlying trends.

Table 4 – Effect of State EITC Programs : One-Sided State Borders

Model:	SBPFE (a)	SBRDD:L (b)	SBRDD:Q (c)
Panel A: (Annual)		ln [Fed EITC Returns]	
ln [Max St.EITC]	0.01 (0.13)	-0.08 (0.28)	-0.31 (0.47)
Observations	11,974	6,366	6,366
Panel B: (Annual)		Net Migration Percent	
ln [Max State EITC]	-0.007 (0.013)	-0.013 (0.016)	0.005 (0.057)
Observations	11,998	6,372	6,372
Panel C: (Annual)		Net Low+Mid Wage Commuting Percent	
ln [Max State EITC]	0.03 (0.14)	0.16 (0.28)	1.84 (1.00)
Observations	11,196	5,949	5,949
Panel D: (Quarterly)	ln [Total Employment: Women, Less HS]		
ln [Max State EITC]	0.26 (0.16)	0.60 (0.59)	0.16 (1.46)
Observations	47,672	25,298	25,298
Panel E: (Quarterly)		ln [Avg Earnings: Women, Less HS]	
ln [Max State EITC]	-0.05 (0.11)	0.23 (0.28)	0.61 (0.65)
Observations	47,075	24,941	24,941
Cluster	State Border	State Border	State Border

Clustered standard errors in parentheses; 27 clusters. Regressions weighted county population in 2000. Controls: log of county population (log total returns in Panel A) and log of state real GDP.

6.3 Stacked Event Studies

To probe the differences between Table 3 and 4, I perform event study analyses that use variation in state program implementation timing.

Let D_s be an indicator variable for the state along a given border that implements a state supplement and let T_{ps} be the year that a stat EITC program is implemented along a state border. The specifications I estimate is the following:

$$y_{cpst} = X_{cpst}\beta + \sum_{v \in V} \gamma_v \cdot 1[t - T_{ps} = v] \cdot 1[D_s = 1] + \lambda_c + \lambda_{pt} + u_{cpst} \quad (5)$$

$$\begin{aligned} y_{csbt} &= X_{csbt}\beta + \sum_{v \in V} \gamma_v \cdot 1[t - T_{ps} = v] \cdot 1[D_s = 1] + \lambda_c + \lambda_{bt} \\ &\quad + D_{bt} \cdot [1 + \theta_{bt}^0(1 - T_{st}) \cdot m_{csb} + \theta_{bt}^1 T_{st} \cdot m_{csb}] + u_{cst}, \end{aligned} \quad (6)$$

where $V = \{-5, -4, \dots, 10\} \setminus \{-1\}$ is the event-time values. Note, for the SBRD design I only use the linear specification. The $\{\gamma_v\}$ terms are the estimates of the dynamic treatment effects of the policy pooled across each state implementation.

I am able to split the analysis by one- and two-sided borders and to inspect pre-trends and anticipation effects. The pooled results correspond to the results in Table 3 and the one-sided results correspond to Table 4. Almost all the estimates are not statistically different from zero, which again fails to provide evidence that state supplements affect labor market outcomes. Generally, the pooled and one-sided sample pre-treatment periods are centered around zero implying no pre-trends; however, the two-sided sample results appear to have pre-trends that raise concerns about the treatment effects.

For the returns plots, using the pooled or two-sided results indicate positive treatment effects, but the one-sided results indicate essentially no effect. The employment plots show strong employment effects that grow over time. However, unlike in the other plots, the one-sided sample estimates do appear to have pre-trends which casts doubt on the results. Finally, the earnings results are near zero for all specifications.

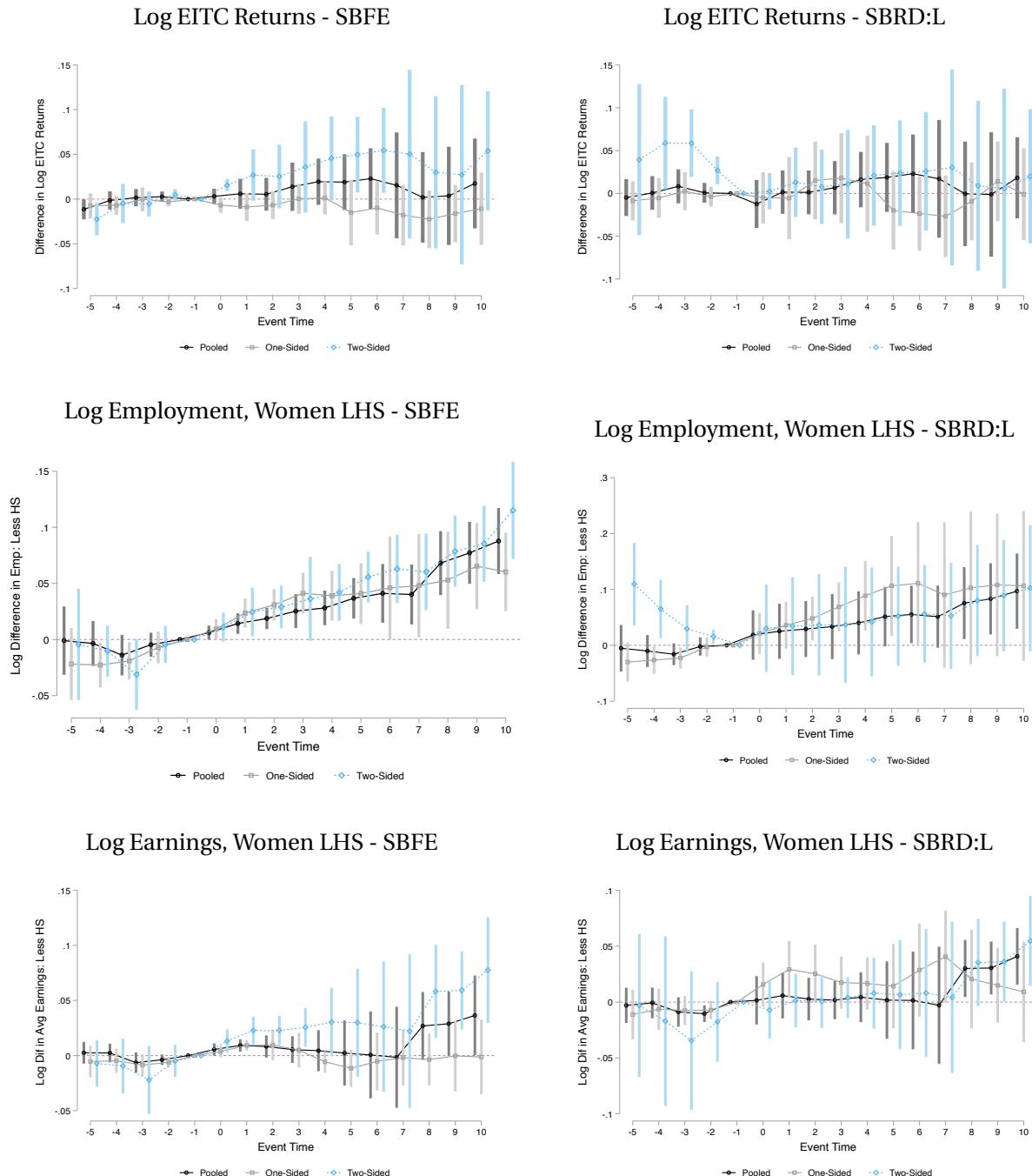
7 Conclusion

The Earned Income Tax Credit is one of the largest anti-poverty programs in the United States and is increasingly supplemented by the states. Several states explicitly justify their programs as an economic development tax expenditure meant to increase labor force participation. I documented variation in state EITC policies and test this claim using two empirical designs that use variation at state borders. I test for effects in federal EITC take up, county migration, county commuting, and employment and earnings for women with less than a high school degree.

I find that estimates are highly dependent on the empirical design and sample used. If I use all possible state policies and borders, then I find that state EITC supplements increase take-up of the federal EITC, do not affect migration or commuting, and either decrease or have no affect on low educated women's employment and earnings. When I limit the sample to 'one-sided' borders where a state supplemented was implemented after 2000, I find mixed results that are all statistically insignificant.

Overall, 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.

Figure 4 – Stacked Event Study Plots



Note: Plots of event study coefficients for SBFE and SBRD designs with state-border robust standard errors for three different samples: pooled, one-sided, and two-sided. The pooled uses all possible state borders, the one-sided uses only borders where the new state program is the first program on the border, and the two-sided uses only borders where the new state program is the second program. Each coefficient is the difference in outcome variable for the state implementing the program.

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A Additional Data Sources

In Table 5 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 5 – State EITC Returns and Amounts Sources

State	Year	URL	Notes
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	efo.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/E1360TransparencyApp/files/Tax%20Expenditure%20Reports/Tax%20Expenditure%20Report%202018.png	
MN	TY 2017	www.revenue.state.mn.us/minnesota-income-tax-statistics-county	
MT			
NE	TY 2018	revenue.nebraska.gov/research/statistics/nebraska-statistics-income	
NJ	TY 2019	www.nj.gov/treasury/taxation/png/taxexpenditurereport2020.png	Table F2
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/2018-2019AnnualReport.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.

B Additional Results

B.1 State Border Regression Results

Table 6 displays the predicted state supplement rates—the *margins*—from the following regression:

$$y_{sbt} = \alpha + \sum_{v \in V} \gamma_v^a \cdot 1[t - T_{sb} = v] + \sum_{v \in V} \gamma_v^b \cdot 1[t - T_{sb} = v] \cdot 1[\text{Two-Sided}] + D_t \beta^a + D_t \cdot 1[\text{Two-Sided}] \beta^b + u_{sbt}, \quad (7)$$

where y is the state supplement rate for the implemented program, T_{sb} is the year the state supplemented is implemented for the border, $1[\text{Two-Sided}]$ is an indicator for whether along a given state border there is an incumbent program, and D_t are year indicators. The year controls absorb a general positive trend in state supplement rates. Using the results of this regression, I calculate the margins by the border-status, which is what is

plotted in Figure 2 (b). This continues to hold time trends fixed by averaging over years as well as show the levels of thee subsidies rather than only their relative difference. The standard errors in the regression are clustered by state-borders.

To plot the reation function for Figure 2 (c), I use only the state borders where there is an incumbent program and look at how the. incumbent program ‘reacts’ when its neighbor state implements a program. Table 7 displays the coefficients from the following regression:

$$y_{sbt} = \alpha + \sum_{v \in V} \gamma_v \cdot 1[t - T_{sb} = v] + \lambda_t + \lambda_s + u_{sbt}, \quad (8)$$

where y is the state supplement rate for the incumbent program, T_{sb} is the year the *new* state supplemented is implemented for the border, λ_t and λ_s are year and state FEs respectively. The year and state FEs absorb a general positive trend in state supplement rates by time and age of incumbent programs. Figure 2 (c) plots the coefficients $\{\gamma_v\}$ and their White standard errors.

B.2 Event Study Regression Results

The following tables underlie the plots in Figure 4. Specifically, they are ‘stacked’ event studies of state EITC supplement introductions between 2000 and 2018. For each empirical design, SBFE or SBRD, I present three samples: pooled, one-sided, and two-sided. The pooled sample includes all state borders with a state supplement introduced; the one-sided are only those state borders where there is no incumbent program one side of the border; the two-sided are those where there is an incumbent program when the supplement is introduced.

The regression equations are described in the main text with the figures. Note that the standard errors are clustered by state borders, but the number of clusters starts at 36 and goes to 9 in the two-sided sample. This is generally considered to be too few leading to SEs to be too small (not conservative enough). However, even if the SEs are too small, the majority of estimates are still not statistically different from zero. In light of this, I do not attempt a more formal treatment of the standard errors—such as an analytic bias correction in the variance matrix or an appropriate bootstrap procedure—and instead advise an interested reader to follow the simple advice of Cameron and Miller (2015) and use a T distribution with degrees of freedom equal to the number of clusters.

Table 6 – State Supplement Rates by Border Status: One- vs Two-sided Borders

Event Time	Margins of State Supplement Rate	
	One-Sided	Two-Sided
-5	0.00 (0.00)	0.01 (0.00)
-4	0.00 (0.01)	0.00 (0.02)
-3	0.00 (0.01)	0.00 (0.02)
-2	0.00 (0.00)	-0.01 (0.02)
-1	0.00 (0.01)	-0.01 (0.02)
0	0.07 (0.00)	0.16 (0.02)
1	0.08 (0.01)	0.18 (0.03)
2	0.09 (0.01)	0.17 (0.03)
3	0.09 (0.01)	0.19 (0.03)
4	0.10 (0.01)	0.18 (0.03)
5	0.10 (0.01)	0.19 (0.03)
6	0.10 (0.01)	0.19 (0.04)
7	0.10 (0.01)	0.16 (0.04)
8	0.10 (0.01)	0.16 (0.04)
9	0.12 (0.01)	0.17 (0.05)
10	0.12 (0.02)	0.19 (0.06)
N	597	

Both columns show predicted values by border-status from the same regression. State-border clustered standard errors parentheses. Controls: year by border-status indicators. Event time is relative to the year that a state implements a supplement, where the omitted base year that is normalized to zero is the year before implementation. Samples is all state-borders where the implementing states at least 10 years apart, the implemented supplement activates between 2000 and 2018, and the implementation is not reversed.

Table 7 – State Supplement Rates by Border Status: One- vs Two-sided Borders

Event Time	Incumbent Reaction
-5	-0.02 (0.00)
-4	-0.02 (0.02)
-3	-0.01 (0.02)
-2	-0.01 (0.01)
0	0.00 (0.01)
1	0.01 (0.01)
2	0.03 (0.01)
3	0.02 (0.01)
4	0.02 (0.02)
5	0.03 (0.01)
N	110

White standard errors parentheses. Controls: year and state FEs. Event time is relative to the year that the new state supplement is implemented, where the omitted base year that is normalized to zero is the year before implementation. Samples is all state-borders with an incumbent state program when there is a supplement activated between 2000 and 2018, at least 10 after the incumbent's program starts, and the implementation is not reversed.

Table 8 – Stacked Event Studies :
Log Annual EITC Returns in County

	DV: Log EITC Returns					
	SBFE			SBRD:L		
Event Time	Pooled	One-Sided	Two-Sided	Pooled	One-Sided	Two-Sided
-5	-0.01 (0.01)	-0.01 (0.01)	-0.02 (0.01)	0.00 (0.01)	-0.01 (0.01)	0.04 (0.04)
-4	0.00 (0.01)	-0.01 (0.01)	0.00 (0.01)	0.00 (0.01)	-0.01 (0.01)	0.06 (0.02)
-3	0.00 (0.00)	0.00 (0.01)	-0.01 (0.01)	0.01 (0.01)	0.00 (0.01)	0.06 (0.02)
-2	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.01)	0.00 (0.01)	0.03 (0.01)
0	0.00 (0.00)	-0.01 (0.00)	0.02 (0.00)	-0.01 (0.01)	-0.01 (0.01)	0.00 (0.01)
1	0.01 (0.01)	-0.01 (0.01)	0.03 (0.01)	0.00 (0.01)	-0.01 (0.02)	0.01 (0.02)
2	0.01 (0.01)	-0.01 (0.01)	0.03 (0.02)	0.00 (0.01)	0.02 (0.02)	0.01 (0.02)
3	0.01 (0.01)	0.00 (0.01)	0.04 (0.02)	0.01 (0.02)	0.02 (0.03)	0.01 (0.03)
4	0.02 (0.01)	0.00 (0.01)	0.05 (0.02)	0.02 (0.02)	0.01 (0.03)	0.02 (0.03)
5	0.02 (0.02)	-0.02 (0.02)	0.05 (0.02)	0.02 (0.02)	-0.02 (0.02)	0.02 (0.03)
6	0.02 (0.02)	-0.01 (0.01)	0.05 (0.02)	0.02 (0.02)	-0.02 (0.02)	0.03 (0.03)
7	0.02 (0.03)	-0.02 (0.02)	0.05 (0.04)	0.02 (0.03)	-0.03 (0.02)	0.03 (0.05)
8	0.00 (0.02)	-0.02 (0.02)	0.03 (0.04)	0.00 (0.03)	-0.01 (0.02)	0.01 (0.04)
9	0.00 (0.03)	-0.02 (0.02)	0.03 (0.04)	0.00 (0.04)	0.01 (0.02)	0.01 (0.05)
10	0.02 (0.02)	-0.01 (0.02)	0.05 (0.03)	0.02 (0.02)	0.00 (0.03)	0.02 (0.03)
Counties	457	348	115	457	348	115
Obs	11,886	8,880	3,006	6,325	4,715	1,610
Clusters	36	27	9	36	27	9

State-border clustered standard errors parentheses. Regressions weighted county population in 2000. Controls: log of county population (log total returns in Panel A), log of state real GDP, and design specific FEs. Event time is relative to the year that a state implements a supplement, where the omitted base year that is normalized to zero is the year before implementation. Samples are based on the whether at the time of implementation of a given state supplement for a given state border there is an incumbent program.

Table 9 – Stacked Event Studies :
Log Quarterly Employment: Women, LessHS in County

	DV: Log Employment: Women, LessHS					
	SBFE			SBRD:L		
Event Time	Pooled	One-Sided	Two-Sided	Pooled	One-Sided	Two-Sided
-5	0.00 (0.01)	-0.02 (0.02)	0.00 (0.02)	-0.01 (0.02)	-0.03 (0.02)	0.11 (0.03)
-4	0.00 (0.01)	-0.02 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.03 (0.01)	0.06 (0.02)
-3	-0.01 (0.00)	-0.02 (0.01)	-0.03 (0.01)	-0.02 (0.01)	-0.02 (0.01)	0.03 (0.02)
-2	0.00 (0.00)	-0.01 (0.01)	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	0.02 (0.01)
0	0.01 (0.00)	0.01 (0.00)	0.01 (0.01)	0.02 (0.02)	0.02 (0.02)	0.03 (0.03)
1	0.01 (0.01)	0.02 (0.01)	0.02 (0.01)	0.03 (0.02)	0.04 (0.02)	0.03 (0.04)
2	0.02 (0.01)	0.03 (0.01)	0.03 (0.01)	0.03 (0.02)	0.05 (0.02)	0.04 (0.04)
3	0.03 (0.01)	0.04 (0.01)	0.04 (0.02)	0.03 (0.03)	0.07 (0.02)	0.04 (0.05)
4	0.03 (0.01)	0.04 (0.01)	0.04 (0.01)	0.04 (0.03)	0.09 (0.03)	0.04 (0.04)
5	0.04 (0.02)	0.04 (0.01)	0.06 (0.01)	0.05 (0.03)	0.11 (0.04)	0.05 (0.04)
6	0.04 (0.02)	0.05 (0.02)	0.06 (0.01)	0.06 (0.03)	0.11 (0.05)	0.06 (0.04)
7	0.04 (0.03)	0.05 (0.02)	0.06 (0.01)	0.05 (0.03)	0.09 (0.06)	0.05 (0.04)
8	0.07 (0.02)	0.05 (0.02)	0.08 (0.01)	0.08 (0.03)	0.10 (0.07)	0.08 (0.04)
9	0.08 (0.03)	0.07 (0.02)	0.09 (0.01)	0.08 (0.03)	0.11 (0.06)	0.09 (0.04)
10	0.09 (0.02)	0.06 (0.02)	0.12 (0.02)	0.10 (0.03)	0.11 (0.07)	0.10 (0.05)
Counties	475	366	114	475	366	114
N	48,649	36,218	12,431	25,824	19,192	6,632
CL	37	28	9	37	28	9

State-border clustered standard errors parentheses. Regressions weighted county population in 2000. Controls: log of county population (log total returns in Panel A), log of state real GDP, and design specific FEs. Event time is relative to the year that a state implements a supplement, where the omitted base year that is normalized to zero is the year before implementation. Samples are based on the whether at the time of implementation of a given state supplement for a given state border there is an incumbent program.

Table 10 – Stacked Event Studies :
Log Quarterly Avg Monthly Earnings: Women, LessHS in County

	DV: Log Avg Monthly Earnings: Women, LessHS					
	SBFE			SBRD:L		
Event Time	Pooled	One-Sided	Two-Sided	Pooled	One-Sided	Two-Sided
-5	0.00 (0.00)	-0.01 (0.01)	-0.01 (0.01)	0.00 (0.01)	-0.01 (0.01)	0.00 (0.03)
-4	0.00 (0.00)	0.00 (0.01)	-0.01 (0.01)	0.00 (0.01)	-0.01 (0.01)	-0.02 (0.03)
-3	-0.01 (0.00)	-0.01 (0.00)	-0.02 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.03 (0.03)
-2	0.00 (0.00)	-0.01 (0.00)	0.00 (0.01)	-0.01 (0.00)	-0.01 (0.00)	-0.02 (0.02)
0	0.01 (0.00)	0.00 (0.00)	0.01 (0.00)	0.00 (0.01)	0.02 (0.01)	-0.01 (0.01)
1	0.01 (0.00)	0.01 (0.00)	0.02 (0.01)	0.01 (0.01)	0.03 (0.01)	0.00 (0.01)
2	0.01 (0.00)	0.01 (0.01)	0.02 (0.01)	0.00 (0.01)	0.03 (0.01)	0.00 (0.01)
3	0.01 (0.01)	0.00 (0.01)	0.03 (0.01)	0.00 (0.01)	0.02 (0.01)	0.00 (0.01)
4	0.00 (0.01)	-0.01 (0.00)	0.03 (0.01)	0.00 (0.01)	0.02 (0.01)	0.01 (0.01)
5	0.00 (0.01)	-0.01 (0.01)	0.03 (0.02)	0.00 (0.02)	0.01 (0.02)	0.01 (0.02)
6	0.00 (0.02)	-0.01 (0.01)	0.03 (0.03)	0.00 (0.02)	0.03 (0.02)	0.01 (0.02)
7	0.00 (0.02)	0.00 (0.01)	0.02 (0.03)	0.00 (0.03)	0.04 (0.02)	0.00 (0.03)
8	0.03 (0.02)	0.00 (0.01)	0.06 (0.02)	0.03 (0.01)	0.02 (0.02)	0.04 (0.02)
9	0.03 (0.01)	0.00 (0.02)	0.06 (0.02)	0.03 (0.01)	0.01 (0.02)	0.04 (0.02)
10	0.04 (0.02)	0.00 (0.02)	0.08 (0.02)	0.04 (0.01)	0.01 (0.02)	0.05 (0.02)
Counties	473	364	114	472	363	114
N	48,150	35,758	12,392	25,516	18,909	6,607
CL	37	28	9	36	28	9

State-border clustered standard errors parentheses. Regressions weighted county population in 2000. Controls: log of county population (log total returns in Panel A), log of state real GDP, and design specific FEs. Event time is relative to the year that a state implements a supplement, where the omitted base year that is normalized to zero is the year before implementation. Samples are based on the whether at the time of implementation of a given state supplement for a given state border there is an incumbent program.