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

C. Luke Watson*

August 6, 2021

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

*Watson: Department of Economics, Michigan State University, email: watso220@msu.edu. I am grateful for the advice and support from my advisors John D. Wilson, Leslie Papke, and Oren Ziv. I thank participants at the 2020 National Tax Association for constructive comments. All mistakes are my own.

Contents

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?

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). These designs allow me to control for local macroeconomic shocks that previous EITC studies have not controlled, which would bias results if present.[5]

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

[1] This 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.

[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.

[5] The primary reason is that state supplement rates vary at the state-year level, thus at most state-linear-trends could be used.

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 that have already implemented an EITC supplement tend to themselves implement more generous 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 that 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 are 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 one 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 semi-elasticity (and its robust standard error) between county federal EITC returns and state supplement rates is 0.16 (0.05) using all borders but 0.07 (0.12) using only one-sided borders, where state-border clustered standard errors are in parentheses. Using the SBRD strategy for the same three subsets, the elasticity is 0.23 (0.17) and -0.06 (0.25), respectively. When I use an event-study approach, I find that the dynamic treatment effects for one-sided borders appear centered around zero implying no short- or long-run effects.

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 supplements 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 State EITC Supplements

Currently, 28 states, the District of Columbia, and two municipalities have implemented supplements to the federal EITC. Collectively, these governments spend $4 billion in tax expenditures.Collectively, these governments spend $4 billion in tax expenditures.[6] For some context, the state share of medicaid expenditure for these states (and DC) is $138 billion (Kaiser Family Foundation, 2021). State medicaid and CHIP expenditures represent about 16% of state budgets (Medicaid and CHIP Payment and Access Commission, 2021), while state EITC are roughly 0.4% of state budgets. Nevertheless, the pro-work incentives of state EITC supplements may cause them to be more politically popular to tout than medicaid expenditures when discussing aid to low income families.

Two justifications for EITC programs are that they provide 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)."[7]

Table 1 reports several policy features of state supplements and usage. Columns (b)-(e) report state supplement rates, the tax year 2020 average[8] maximum credit in the state (equal to the supplement rate times the average federal max credit), whether the state supplement is refundable, and how the state supplement treats non-resident workers. States that make the EITC refundable effectively can make average state tax rates negative, while non-refundability reduces the salience and effect of a state supplement.[9] Most states make nonresidents ineligible for state credits; however, seven make them available at a prorated rate equal to the portion of 'state AGI' to 'total AGI' and four place no limit on the credit (though none of these are refundable).

Columns (f)-(i) report total state EITC claims, state expenditures, and these values as a fraction of federal EITC usage in the states. The table shows that number of state EITC claim roughly matches federal claims. New York and the District of Columbia have claims above the federal amount while Hawaii, Virginia, Wisconsin, and South Carolina have much fewer claims than the federal program.[10] 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 (i) is 15.7% which is slightly lower the average state supplement rate in column (b), 17.1%, because (i) incorporates differential take-up of state EITCs.

[6]For more discussion on state EITC supplements, see Waxman and Legendre (2021).

[7]For 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.

[8]For this average, I use a constant weighting of 0.4 for single qualifying child credit, 0.4 for two children, and 0.2 for three plus children.

[9]While refundability should make the state supplement more salient and beneficial to workers, in unreported results I find no differential effect of state supplements.

[10]South Carolina's program was enacted in 2018 and is relatively new, so this low number may be due to salience issues.

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

State	Subsidy Rate (%)	State Max ($)	Refundable (Y/N)	Non-Resident Treatment	State Claims (1000s)	State Amount ($millions)	State % of Fed Claims	State % of Fed Amount
(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)	(i)
CA	–	–	Y	Inelig	2,046	388	72.5	5.9
CO	10	513	Y	Inelig	343	74	103.3	10.2
CT	27.5	1412	Y	Inelig	193	95	89.5	19.7
DE	20	1027	N	Inelig	–	14	–	7.8
DC	40	2053	Y	Inelig	63	79	127.6	68.5
HI	20	1027	N	Frac	56	15	59.2	7.5
IL	18	924	Y	Frac	914	316	99.1	13.7
IN	9	462	Y	Frac	–	104	–	8.7
IA	15	770	Y	Frac	208	69	107.5	15.2
KS	17	873	Y	Inelig	197	79	100.9	16.9
LA	3.5	180	Y	Inelig	–	49	–	3.5
ME	5	257	Y	Frac, NRf	100	10	105.3	5.1
MD	28	1437	Y	Inelig	–	166	–	18
MA	23	1181	Y	Inelig	–	205	–	25.2
MI	6	308	Y	Elig, NRf	–	118	–	6.2
MN	–	–	Y	Inelig	315	244	99.8	34.9
MT	3	154	Y	Inelig	–	–	–	–
NE	10	513	Y	Inelig	120	29	95	9.5
NJ	37	1899	Y	Inelig	–	440	–	31.5
NM	10	513	Y	Inelig	198	50	99.4	10.1
NY	30	1540	Y	Elig, NRf	2,332	1,082	143.9	28.5
OH	10	513	N	Elig	783	179	88.3	8.2
OK	5	257	N	Inelig	300	16	93.3	1.9
OR	8	411	Y	Frac	247	49	96	9
RI	15	770	Y	Frac	93	28	116	15.5
SC	20.1	1032	N	Inelig	60	21	12.7	1.8
VT	36	1848	Y	Inelig	40	27	96.6	34.2
VA	20	1027	N	Elig	347	136	59.5	9.7
WI	15	770	Y	Inelig	239	93	68	11.8

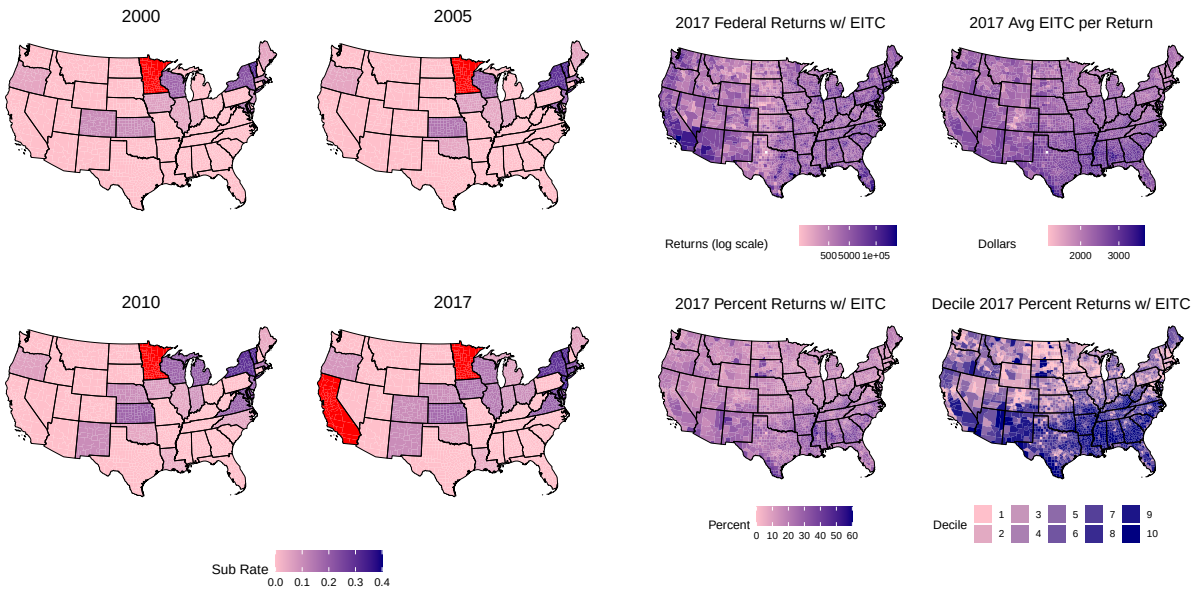
State EITC returns and amounts data accessed from individual state websites; typically state tax expenditure reports. Most recent value is reported. Federal EITC returns and amounts from tax year 2018 (IRS SOI). The New York number of returns uses both NY state and NY city EITC programs and likely double 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.

Next, 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.[11] Interestingly, there seems to be some spatial correlation in State EITC spread, where most states with a program border another state with a program.

[11] States 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.

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 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.[12] There appears to be some negative correlation between state EITC programs and the average Federal EITC amounts but positive correlation with the labor force participation of mothers.

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.

2.1 Across State EITC Policy Coordination

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 change in max state credit (federal plus state EITC) when a state implements a supplement across all state borders. On average, this change 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.

As Figure 1 shows, some state borders have only one state supplement (e.g., Virginia and Kentucky) while others have supplements on both sides (e.g., Virginia and Maryland). I call borders with only one supplement 'one-sided' and borders with two supplements 'two-sided.' In the case of two-sided borders, the older program is the 'incumbent' and the newer program is the 'implementing' program. For this paper, I focus

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

on state supplements introduced after 2000, so all incumbents have programs initiated before 2000 and implementing programs after 2000.

Figure (2.b) compares the state supplement rates between state borders that are one-sided versus two-sided borders. The plot shows that states implement more generous subsidies when their neighbor already has a state program. On average, implementing states make their supplement 7 percentage points more generous than implementing states without a neighboring incumbent program.

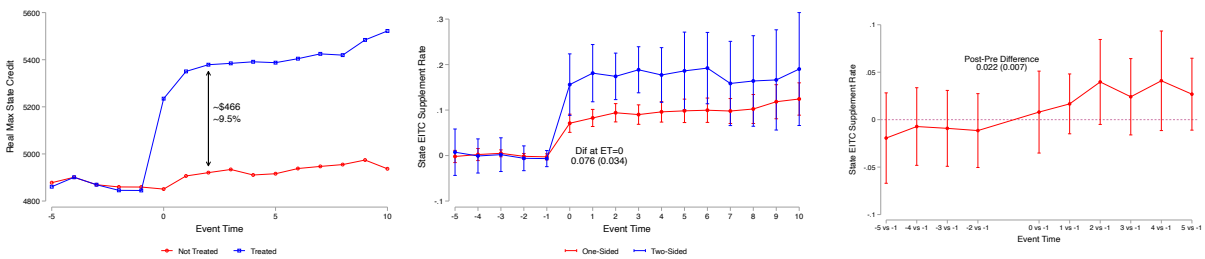
Figure (2.c) plots the incumbent state's policy reaction to their neighbor's new supplement. Specifically, the plot shows whether the incumbent's subsidy rate in each period is statistically different from the rate the year before the new program is implemented. The result suggests that incumbents make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

Figure 2 – Effect of State Supplement Implementation

(a) Average Effect on Real Max Credit

(b) Supplement Rates by Neighbor's EITC Status

(c) Neighbor's Reaction to Supplement Introduction



Note: (2.a) plots the average change in real max credit across all state supplements introduced after 2000; (2.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; (2.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 few clusters).

2.1.1 Implications of Coordination

Overall, 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.

For two states, $s \in \{1, 2\}$ along a given border segment, b, let r_{sbt} be the state EITC supplement rate. The results in Figure 2 tell us that $\text{Cov}(r_{1bt}, r_{2bt}) \neq 0$. This is not obviously a concern.

Suppose y_{sbt} is an outcome of interest, determined by the following equation:

$$y_{sbt} = \alpha_s + \lambda_{bt} + \gamma r_{sbt} + u_{sbt}. \tag{1}$$

If a neighbor's policy is uncorrelated with unobservable trends in the outcome variable, then the OLS regression estimate of γ is unbiased despite the policy coordination:

$$\mathsf{Cov}(r_{1bt}, u_{2bt}) = 0 \implies \mathsf{E}[\hat{\gamma}^{\mathsf{OLS}}] = \gamma. \tag{2}$$

However, if the variables are correlated, then the policy coordination will bias the estimate: $\mathsf{Cov}(r_{1bt}, r_{2bt}) \neq 0 \wedge \mathsf{Cov}(r_{1bt}, u_{2bt}) \neq 0 \implies \mathsf{E}[\hat{\gamma}^{\mathsf{OLS}}] \neq \gamma$. Examining this theoretical relationship is beyond the scope of this chapter, but would be a fruitful future project.

To deal with this issue empirically, I will look at the full-sample results and results where only one side of the border has a state EITC program. For these borders, because only one side has a state supplement, then mechanically $\mathsf{Cov}(r_{1bt}, r_{2bt}) = 0$ as $r_{2bt} = 0$.

3 Evaluating State EITC Supplements

To justify the labor market outcomes that I use below, I formalize a simple model of location and work choice that explains how the tax policy variation interacts with labor market choices to affect state budgets.[13] The change in the state budget constraint due to the behavioral responses to the policy change is a way to apply a dollar amount to the 'unintended effect' of the policy change and can be used a measure of economic welfare change (Hendren, 2016; Kleven, 2020).

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:

$$\underbrace{\Pr((\mathsf{o}^i, \mathsf{d}^i) = (o, d))}_{:=\pi_{o,d}} = \underbrace{\Pr(\mathsf{d}^i = d \mid \mathsf{o}^i = o) \cdot \Pr(\mathsf{o}^i = o)}_{:=\pi_{d|o} \cdot \pi_o}. \tag{3}$$

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 $(\mathsf{o}^i, \mathsf{d}^i)$.[14]

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[15] 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[16] fiscal externality as a proportion of initial revenue is (where $\hat{x} = \mathsf{d}x/x$):

[13] 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.

[14] 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.

[15] 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.

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

$$\frac{\mathsf{FE}^s}{R^s} = \sum_{o \in s} \frac{R^o}{R^s} \left(\underbrace{\hat{\pi}_o}_{\text{Migration}} + \underbrace{\frac{R^o_o}{R^o}(\hat{w}^o_o + \hat{\pi}_{o|o})}_{\text{Own Employment}} + \underbrace{\sum_{d \in \mathcal{S} \setminus o} \frac{R^o_d}{R^o} \cdot (\hat{w}^o_d + \hat{\pi}_{d|o})}_{\text{Commuting}} \right). \tag{4}$$

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

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 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.

These designs allow for me to control for local macroeconomic trends. For the SBFE these are county-pair trends and for SBRD these are state-border trends. These specifications use the counties across the border as a counterfactual if the states did not implement a supplement. The assumption is that these counties face similar economic forces that are not limited by state borders except for the EITC policy change. If macroeconomic trends do spillover across state borders, then not including the border controls will lead to biased estimates.

For all the designs below, let y be the log of some outcome variable, let X be controls, let r be the state supplement rate, and let T be an indicator equal to one if the state's program is in-effect. 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 Max State Credit Variation

My primary independent variable of interest is the state EITC supplement rate, r_{st}, as discussed above. This variable directly represents the state generosity and is the specific policy tool used by the states.

Previous studies[17] have used the '(log) state maximum credit' where the state maximum credit is constructed as a dependent-size weighted maximum credit, where the weights represent the number of families with 1, 2, or 3+ dependents. Literally, these studies calculate this as $C_{st} = 0.4C^1_{st} + 0.4C^2_{st} + 0.2C^{3+}_{st}$, where C^i_{st} is the max state credit for i dependents. As most states programs use a 'top up' formula, each C^i_{st} term is calculated as $(1 + r_{st}) \cdot C^i_t$, where C^i_t is the federal max credit for i dependents.[18] Combining these two facts, the real max state credit is one plus the state supplement rate times a weighted average of the federal max credits for dependents: $C_{st} = (1+r_{st}) \cdot (0.4C^1_t + 0.4C^2_t + 0.2C^{3+}_t)$.

A regression of C_{st} or $\ln[C_{st}]$ on the supplement rate and year indicators, $\{r_{st}, D_t\}$, will absorb all the variation in the state max credit variable and yield an R^2 value of 1.[19] Thus the state max credit variable is econometrically equivalent to using the state subsidy rate and year indicators. One cannot separately identify the effect of the level of EITC on an outcome variable from common year effects captured by year indicator variables; rather, one can only identify the relative differences between states within a given year. Since nearly all work on the EITC includes year indicators as control variables, any prior work that claimed to identify the effect of 'dollars of additional EITC' misstated their actual finding.

Given the above and my use of year-location indicator variables, I directly use the state supplement rates to assess the causal impact of the state EITC programs. I interpret coefficients as the given change in the outcome variable in terms of additional percentage point in the state supplement. This usage makes the identifying variation more transparent and interpretation more reliable.[20]

Finally, for states with a non-standard supplement–California and Minnesota–I find the family-size weighted maximum credit based on the non-standard supplement and then divide this by the federal maximum EITC credit for the effective state supplement rate.

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 interpreted causally.

[17] Three prominent examples include Leigh (2010); Kasy (2017); Bastian (forthcoming).

[18] One exception to this is for Wisconsin that has dependent specific subsidy rates, so for this state $C^i_{st} = (1 + r^i_{st}) \cdot C^i_t$; however, this is not enough variation for identification on a national scale.

[19] For example, using the log max state credit the regression is the exact specification of the variable's definition: $\ln[C_{st}] = \ln[1 + r_{st}] + \ln[C_t]$.

[20] If one wants to interpret the effects in terms of dollars, then one could multiply the current real federal EITC max credit and multiply this by 0.01 to find the dollar value of a one percentage point increase in credit amount.

The regression equation is:

$$y_{cpst} = X_{cpst}\beta + \gamma^r \ln[C_{st}] + \lambda_c + \lambda_{pt} + u_{cpst}, \tag{5}$$

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 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 of using distance to the border as an identification strategy. 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.[21] 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 (Dieterle et al., 2020).

The regression equation is:

$$\begin{aligned} y_{csbt} = {} & X_{csbt}\beta + \gamma^C \ln[C_{st}] + \lambda_c + \lambda_{bt} \\ & + D_{bt} \cdot \left[1 + \sum_k \theta_{bt}^{0;k}(1 - T_{st}) \cdot m_{csb}^k + \sum_k \theta_{bt}^{1;k} T_{st} \cdot m_{csb}^k \right] + u_{cst}, \end{aligned} \tag{6}$$

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.[22] For inference, I cluster standard errors at the state-border level.

5 Data

The data used in the analysis are 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.[23] I focus on the period starting in 2000 to avoid

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

[22] 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.

[23] Because 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.

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.[24]

The tax return data are from the IRS Statistics of Income.[25] The migration data are also based on the IRS Statistics of Income County to County Flows.[26] The commuting data are 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. Specifically, 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 (Feenberg and Coutts, 1993).[27] 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).

6 Results

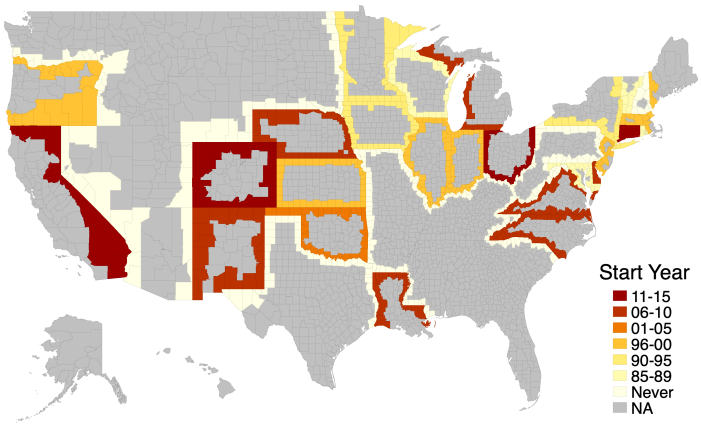
Given the patterns shown in Figure 2, I present three sets of results. First, I present results that use all possible state borders with at least one year of a policy discontinuity. Second, I focus on the one-sided state borders where only one state has a EITC supplement for the whole sample period.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

I use the state supplement rate as the treatment variable. When the outcome is a log variable, then then the estimate is a a semi-elasticity interpreted as *a one percentage point increase in the subsidy causes a* $100 \cdot (e^{\gamma} - 1)$ *percent change in the outcome.* For context, recall that the average state supplement rate is 9.5 percent of the federal EITC.

In the third set of results, I use variation in state policy timing rather than state supplement rate 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.

[24] CO had a short-lived program from 1999-2001 that I have omitted; SC's program started in 2018.
[25] I accessed the 2000 to 2010 EITC returns data from the Brookings Institute via Cecile Murray.
[26] The years 1990 to 2000 are adapted from pre-formated files from Hauer (2019).
[27] I have also manually checked the values by going to the various state websites.

Figure 3 – Border Counties by Treatment Status



This figure 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).

6.1 All Borders

Table 3 displays the results using all borders. The table presents either semi-elasticities (returns, employment, earnings) or level changes (net migration percent, net commuting percent). Recall, on average a state supplement increases EITC generosity by about 10 percentage points from a federal max credit of $4,870 in 2017, the final year in the sample.[28]

Panel A shows that a one percentage point increase in EITC generosity induces between $[0.16, 0.44]$ percent additional Federal EITC returns for the county relative to counties across the state border. Each semi-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 estimates of a one percentage point increase in the state supplement implies a γ-percentage point change in the net migration / commuting. Neither set of estimates is statistically different from zero. The migration coefficient estimates are between $[0.003, 0.017]$ from a mean of 0.004. The commuting coefficient estimates are between $[-0.54; -0.23]$ from a mean of 0.18. While the migration change estimates seem plausible, taken literally the commuting changes imply huge economic effects given that supplements increased by 10 percentage points.

Panels D and E display employment and earnings semi-elasticities, similar to Panel A. The estimated employment semi-elasticity is between $[-0.26, -0.07]$, which would imply that state supplements decrease the number of workers in a county relative to counties across the border. None of the estimates is statistically different from zero. The estimated earnings elasticity is between $[-0.12, -0.09]$, 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 employ-

[28] In January 2020 terms, the amount is $5,138. Note, the federal EITC is adjusted annually for inflation based on the Consumer Price Index before 2017 and now the Personal Consumption Expenditure index.

Table 2 – Summary Statistics

	All Counties (a)	Full Sample (b)	Never Treated (c)	Ever Treated (d)
State EITC Program	37.4%	52.0%	0.0%	77.1%
	(0.21)	(0.39)	(0.00)	(0.40)
State EITC Supplement Rate	5.0%	7.4%	0.00	10.9%
	(0.04)	(0.07)	(0.00)	(0.09)
County Returns (1000s)	43.6	50.9	37.3	57.42
	(0.59)	(1.08)	(1.17)	(1.49)
County Population (1000s)	98.1	108.6	83.9	120.5
	(1.27)	(1.13)	(1.25)	(1.56)
Real State GDP (1000s)	413.0	376.8	371.3	379.5
	(1.81)	(3.03)	(5.86)	(3.50)
Fed Tax EITC Returns	7,790	8,519	6,887	9,304
	(116)	(208)	(239)	(286)
Net Migration Percent	0.20%	0.37%	0.14%	0.48%
	(0.10)	(0.35)	(0.02)	(0.51)
Net Low+Mid Wage Commuting Percent	16.4%	17.9%	16.4%	18.6%
	(0.06)	(0.23)	(0.43)	(0.27)
Employment	1,969	2,063	1,587	2,290
	(18.9)	(28.5)	(32.7)	(39.1)
Avg Monthly Earnings	1,574	1,590	1,579	1,595
	(0.87)	(1.63)	(2.81)	(2.01)
Counties	3,137	905	294	611

US Contiguous Counties, 2000-2018. Columns b-d use border-counties with a policy difference at the border. Never treated counties never enact a state EITC program; Ever Treated enact a state EITC during the sample period.

ment 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.[29]

6.2 One-Sided Borders

Table 4 displays the results using only one-sided 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 outcomes. 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

[29]Income 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	SBRDD:L	SBRDD:Q
	(a)	(b)	(c)
Panel A: (Annual)	ln [Fed EITC Returns]		
State Supp. Rate	0.15	0.21	0.37
	(0.05)	(0.17)	(0.14)
Observations	34,790	18,606	18,606
Panel B: (Annual)	Net Migration Percent		
State Supp. Rate	0.003	0.006	0.017
	(0.005)	(0.010)	(0.012)
Observations	34,812	18,612	18,612
Panel C: (Annual)	Net Low+Mid Wage Commuting Percent		
State Supp. Rate	-0.28	-0.54	-0.23
	(0.38)	(0.39)	(0.35)
Observations	32,878	17,578	17,578
Panel D: (Quarterly)	ln [Total Employment: Women, Less HS]		
State Supp. Rate	-0.08	-0.20	-0.31
	(0.08)	(0.16)	(0.17)
Observations	141,129	75,234	75,234
Panel E: (Quarterly)	ln [Avg Earnings: Women, Less HS]		
State Supp. Rate	-0.11	-0.12	-0.10
	(0.04)	(0.03)	(0.04)
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.

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	SBRDD:L	SBRDD:Q
	(a)	(b)	(c)
Panel A: (Annual)	ln [Fed EITC Returns]		
State Supp. Rate	0.07	-0.07	-0.30
	(0.12)	(0.25)	(0.43)
Observations	11,974	6,366	6,366
Panel B: (Annual)	Net Migration Percent		
State Supp. Rate	-0.006	-0.012	0.005
	(0.012)	(0.015)	(0.054)
Observations	11,998	6,372	6,372
Panel C: (Annual)	Net Low+Mid Wage Commuting Percent		
State Supp. Rate	0.03	0.15	1.73
	(0.13)	(0.26)	(0.93)
Observations	11,196	5,949	5,949
Panel D: (Quarterly)	ln [Total Employment: Women, Less HS]		
State Supp. Rate	0.23	0.54	0.13
	(0.15)	(0.55)	(1.37)
Observations	47,672	25,298	25,298
Panel E: (Quarterly)	ln [Avg Earnings: Women, Less HS]		
State Supp. Rate	-0.04	0.21	0.56
	(0.10)	(0.26)	(0.61)
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 state EITC program is implemented along

a state border. The specifications I estimate is the following:

$$y_{cpst} = X_{cpst}\beta + \sum_{v \in \mathsf{V}} \gamma_v \cdot 1[t - T_{ps} = v] \cdot 1[D_s = 1] + \lambda_c + \lambda_{pt} + u_{cpst} \tag{7}$$

$$\begin{aligned} y_{csbt} = X_{csbt}\beta &+ \sum_{v \in \mathsf{V}} \gamma_v \cdot 1[t - T_{ps} = v] \cdot 1[D_s = 1] + \lambda_c + \lambda_{bt} \\ &+ D_{bt} \cdot \left[1 + \theta^0_{bt}(1 - T_{st}) \cdot m_{csb} + \theta^1_{bt} T_{st} \cdot m_{csb}\right] + u_{cst}, \end{aligned} \tag{8}$$

where $\mathsf{V} = \{-5, -4, \ldots, 10\} \setminus \{-1\}$ 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.

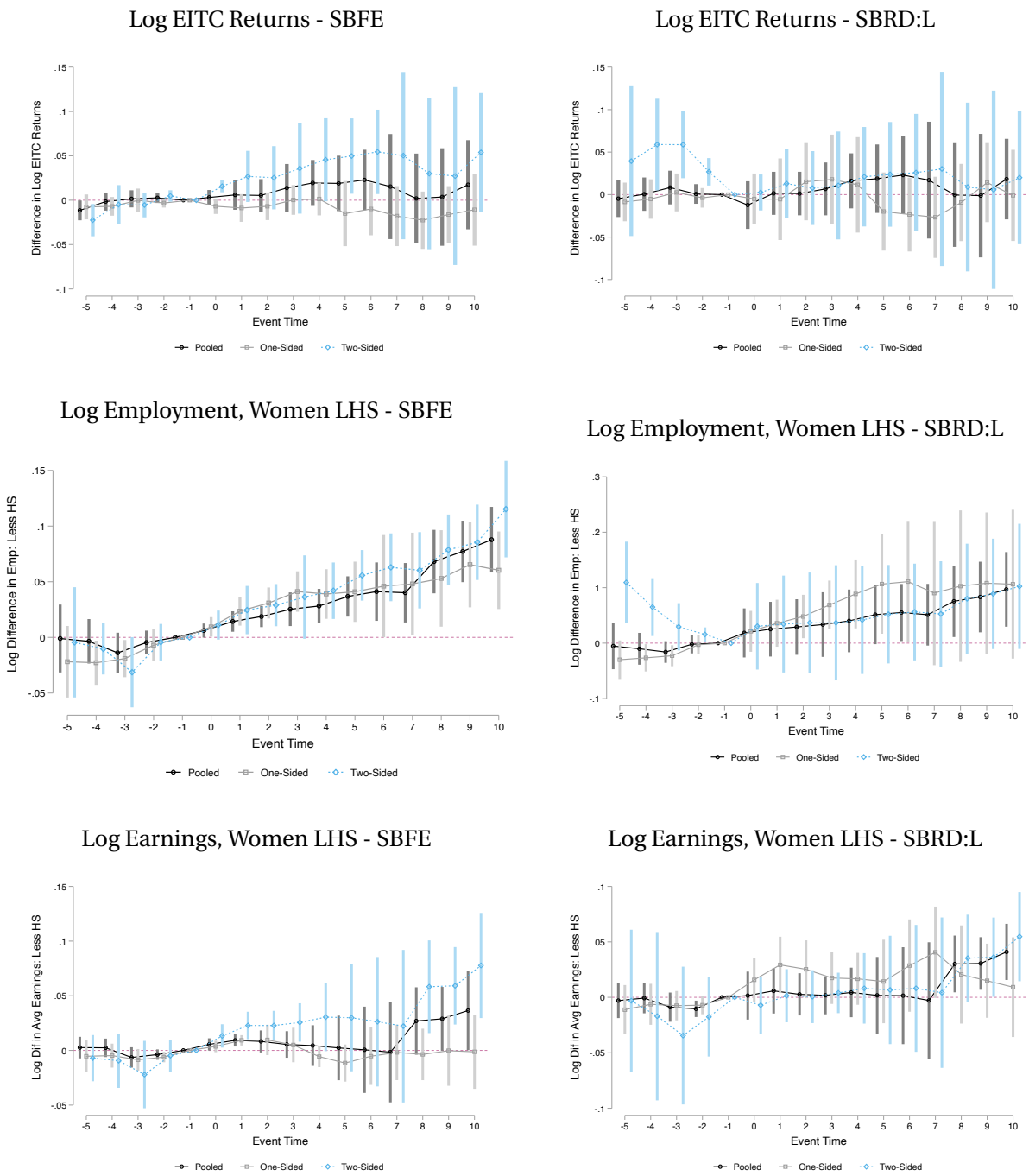
7 Conclusion

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 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.

References

Agrawal, David R, and William H Hoyt. 2018. "Commuting and taxes: Theory, empirics and welfare implications." *The Economic Journal*, 128(616): 2969–3007.

Bastian, Jacob. forthcoming. "The rise of working mothers and the 1975 earned income tax credit." *American Economic Journal: Economic Policy*.

Buhlmann, Florian, Benjamin Elsner, and Andreas Peichl. 2018. "Tax refunds and income manipulation: evidence from the EITC." *International Tax and Public Finance*, 25 1490–1518.

Cameron, A Colin, and Douglas L Miller. 2015. "A practitioner's guide to cluster-robust inference." *Journal of human resources*, 50(2): 317–372.

Dieterle, Steven, Otávio Bartalotti, and Quentin Brummet. 2020. "Revisiting the Effects of Unemployment Insurance Extensions on Unemployment: A Measurement-Error-Corrected Regression Discontinuity Approach." *American Economic Journal: Economic Policy*, 12(2): 84–114.

Dube, Arindrajit, T William Lester, and Michael Reich. 2010. "Minimum wage effects across state borders: Estimates using contiguous counties." *The review of economics and statistics*, 92 945–964.

Feenberg, Daniel, and Elizabeth Coutts. 1993. "An Introduction to the TAXSIM Model." *Journal of Policy Analysis and Management*, 12 189–194.

Finkelstein, Amy, and Nathaniel Hendren. 2020. "Welfare Analysis Meets Causal Inference." *Journal of Economic Perspectives*, 34(4): 146–67.

Hauer, Mathew E. 2019. "IRS SOI County to County Flows: Formatted [dataset]." *Open Science Framework*.

Hendren, Nathaniel. 2016. "The policy elasticity." *Tax Policy and the Economy*, 30(1): 51–89.

Holmes, Thomas J. 1998. "The effect of state policies on the location of manufacturing: Evidence from state borders." *Journal of political Economy*, 106 667–705.

Huang, Rocco R. 2008. "Evaluating the real effect of bank branching deregulation: Comparing contiguous counties across US state borders." *Journal of Financial Economics*, 87 678–705.

Kaiser Family Foundation. 2021. "Federal and State Share of Medicaid Spending FY2019." URL: https://www.kff.org/medicaid/state-indicator/federalstate-share-of-spending/.

Kasy, Maximilian. 2017. "Who wins, who loses? Identification of the welfare impact of changing wages."

Kleven, Henrik. 2019. "The EITC and the Extensive Margin: A Reappraisal."

Kleven, Henrik. 2020. "Sufficient statistics revisited."Technical report, National Bureau of Economic Research.

Leigh, Andrew. 2010. "Who Benefits from the Earned Income Tax Credit? Incidence among recipients, coworkers and firms." *The B.E. Journal of Economic Analysis & Policy*, 10 1–43.

Medicaid and CHIP Payment and Access Commission. 2021. "Medicaid's share of state budgets." URL: https://www.macpac.gov/subtopic/medicaids-share-of-state-budgets/.

Monte, Ferdinando, Stephen J. Redding, and Esteban Rossi-Hansberg. 2018. "Commuting, Migration, and Local Employment Elasticities." *American Economic Review*, 108(12): .

Neumark, David, and Peter Shirley. 2017. "The Long-Run Effects of the Earned Income Tax Credit on Women's Earnings."

Neumark, David, and Katherine E Williams. 2016. "Do state earned income tax credits increase participation in the federal eitc."

Rothstein, Jesse. 2010. "Is the EITC as Good as an NIT? Conditional Cash Transfers and Tax Incidence." *American Economic Journal: Economic Policy*, 2 177–208.

Watson, C. Luke. 2020. "The General Equilibrium Incidence of the Earned Income Tax Credit."

Waxman, Samantha, and Juliette Legendre. 2021. "States Can Adopt or Expand Earned Income Tax Credits to Build Equitable, Inclusive Communities and Economies." URL: https://www.cbpp.org/research/state-budget-and-tax/states-can-adopt-or-expand-earned-income-tax-credits-to-build.

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	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			Not Yet in Effect
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.

B Additional Results

B.1 Alternate Specifications

In Table 6 I report coefficient estimates for alternative specifications for log total federal EITC returns and employment for women with less than a high school degree, using the SBFE and SBRD:L specifications. In column (a), I reproduce the main results from Table 3. For column (b), I do not weight the regressions, which changes the interpretation from an individual policy effect to a county policy effect. For column (c), I omit the the state GDP control, which was included as the previous literature finds that state supplement rates are correlated with the variable (Leigh, 2010). Finally, column (d) adds county-specific linear-trends, which is the most aggressive specification.

Ultimately, the results of the alternative specifications emphasize how sensitive the estimates are to specification changes.

Table 6 – Alternate Specifications: Fed Returns and Employment

	Main (a)	Unweight (b)	No State GDP (c)	County Trends (d)
		FULL SAMPLE - SBFE		
		DV: ln[Total Fed EITC Claims]		
γ	0.15	0.07	0.18	0.04
se	(0.05)	(0.05)	(0.05)	(0.03)
		DV: ln[Employment, LHS Women]		
γ	-0.06	0.10	-0.19	0.02
(se)	(0.08)	(0.08)	(0.13)	(0.05)
		ONE-SIDE SAMPLE - SBFE		
		DV: ln[Total Fed EITC Claims]		
γ	0.07	0.18	0.07	0.02
(se)	(0.12)	(0.09)	(0.11)	(0.07)
		DV: ln[Employment, LHS Women]		
γ	0.11	0.15	0.12	0.15
(se)	(0.15)	(0.14)	(0.14)	(0.09)
		FULL SAMPLE - SBRD:L		
		DV: ln[Total Fed EITC Claims]		
γ	0.21	0.08	0.32	0.04
(se)	(0.17)	(0.07)	(0.23)	(0.07)
		DV: ln[Employment, LHS Women]		
γ	-0.20	0.12	-0.40	-0.03
(se)	(0.16)	(0.14)	(0.27)	(0.08)
		ONE-SIDE SAMPLE - SBRD:L		
		DV: ln[Total Fed EITC Claims]		
γ	-0.07	-0.13	-0.07	-0.08
(se)	(0.25)	(0.11)	(0.25)	(0.11)
		DV: ln[Employment, LHS Women]		
γ	0.54	-0.13	0.42	0.31
(se)	(0.55)	(0.34)	(0.52)	(0.36)

State-border clustered standard errors parentheses. Controls always include year by pair or border-status indicators and either log total county returns or population.

B.2 State Border Regression Results

Table 7 displays the predicted state supplement rates from the following regression:

$$y_{sbt} = \alpha + \sum_{v \in \mathsf{V}} \gamma_v^a \cdot 1[t - T_{sb} = v] + \sum_{v \in \mathsf{V}} \gamma_v^b \cdot 1[t - T_{sb} = v] \cdot 1[\text{Two-Sided}] \\ + D_t \beta^a + D_t \cdot 1[\text{Two-Sided}] \beta^b + u_{sbt}, \tag{9}$$

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 an incumbent program is along the border, and D_t are year indicators. I include the year indicators to absorb the general positive trend in state supplement rates.

I use the predicted values rather than coefficients to highlight the difference in magnitude of the one- and two-sided borders over time and compared to each other. This is the same as displaying the coefficients $\{\gamma_v^a\}$ for the one-sided and $\{\gamma_v^a + \gamma_v^b\}$ for the two-sided borders. These are the values (and their clustered standard errors) plotted in Figure 2.b in the main text.

To plot the reaction 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 8 displays the coefficients from the following regression:

$$y_{sbt} = \alpha + \sum_{v \in \mathsf{V}} \gamma_v \cdot 1[t - T_{sb} = v] + \lambda_t + \lambda_s + u_{sbt}, \tag{10}$$

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.3 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 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 clusters that causes the standard errors to be too small (not conservative enough). However, even if the standard errors 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

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

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

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

Table 8 – 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 state implementation year, where the omitted base year is the year before implementation. Samples is all state-borders where the implementing states at least 10 years apart, the implemented supplement activates between 2000-2018, and the implementation is not reversed.

advice of Cameron and Miller (2015) and use a T distribution with degrees of freedom equal to the number of clusters.

Table 9 – Stacked Event Studies : Log EITC Returns

	DV: Log EITC Returns					
	SBFE			SBRD:L		
Event Time	Pooled	One-Sided	Two-Sided	Pooled	One-Sided	Two-Sided
-5	-0.01	-0.01	-0.02	0.00	-0.01	0.04
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.04)
-4	0.00	-0.01	0.00	0.00	-0.01	0.06
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
-3	0.00	0.00	-0.01	0.01	0.00	0.06
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
-2	0.00	0.00	0.00	0.00	0.00	0.03
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
0	0.00	-0.01	0.02	-0.01	-0.01	0.00
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
1	0.01	-0.01	0.03	0.00	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
2	0.01	-0.01	0.03	0.00	0.02	0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
3	0.01	0.00	0.04	0.01	0.02	0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)
4	0.02	0.00	0.05	0.02	0.01	0.02
	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)
5	0.02	-0.02	0.05	0.02	-0.02	0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
6	0.02	-0.01	0.05	0.02	-0.02	0.03
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)
7	0.02	-0.02	0.05	0.02	-0.03	0.03
	(0.03)	(0.02)	(0.04)	(0.03)	(0.02)	(0.05)
8	0.00	-0.02	0.03	0.00	-0.01	0.01
	(0.02)	(0.02)	(0.04)	(0.03)	(0.02)	(0.04)
9	0.00	-0.02	0.03	0.00	0.01	0.01
	(0.03)	(0.02)	(0.04)	(0.04)	(0.02)	(0.05)
10	0.02	-0.01	0.05	0.02	0.00	0.02
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(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 or total returns, log of state real GDP, and design specific FEs. Event time is relative to the state implementation year, where the omitted base year 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 Employment: Women, LessHS

	DV: Log Employment: Women, LessHS					
	SBFE			SBRD:L		
Event Time	Pooled	One-Sided	Two-Sided	Pooled	One-Sided	Two-Sided
-5	0.00	-0.02	0.00	-0.01	-0.03	0.11
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
-4	0.00	-0.02	-0.01	-0.01	-0.03	0.06
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
-3	-0.01	-0.02	-0.03	-0.02	-0.02	0.03
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
-2	0.00	-0.01	0.00	0.00	0.00	0.02
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
0	0.01	0.01	0.01	0.02	0.02	0.03
	(0.00)	(0.00)	(0.01)	(0.02)	(0.02)	(0.03)
1	0.01	0.02	0.02	0.03	0.04	0.03
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.04)
2	0.02	0.03	0.03	0.03	0.05	0.04
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.04)
3	0.03	0.04	0.04	0.03	0.07	0.04
	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)	(0.05)
4	0.03	0.04	0.04	0.04	0.09	0.04
	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.04)
5	0.04	0.04	0.06	0.05	0.11	0.05
	(0.02)	(0.01)	(0.01)	(0.03)	(0.04)	(0.04)
6	0.04	0.05	0.06	0.06	0.11	0.06
	(0.02)	(0.02)	(0.01)	(0.03)	(0.05)	(0.04)
7	0.04	0.05	0.06	0.05	0.09	0.05
	(0.03)	(0.02)	(0.01)	(0.03)	(0.06)	(0.04)
8	0.07	0.05	0.08	0.08	0.10	0.08
	(0.02)	(0.02)	(0.01)	(0.03)	(0.07)	(0.04)
9	0.08	0.07	0.09	0.08	0.11	0.09
	(0.03)	(0.02)	(0.01)	(0.03)	(0.06)	(0.04)
10	0.09	0.06	0.12	0.10	0.11	0.10
	(0.02)	(0.02)	(0.02)	(0.03)	(0.07)	(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 or total returns, log of state real GDP, and design specific FEs. Event time is relative to the state implementation year, where the omitted base year 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 11 – Stacked Event Studies : Log Avg Monthly Earnings: Women, LessHS

	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	-0.01	-0.01	0.00	-0.01	0.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)
-4	0.00	0.00	-0.01	0.00	-0.01	-0.02
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)
-3	-0.01	-0.01	-0.02	-0.01	-0.01	-0.03
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.03)
-2	0.00	-0.01	0.00	-0.01	-0.01	-0.02
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.02)
0	0.01	0.00	0.01	0.00	0.02	-0.01
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
1	0.01	0.01	0.02	0.01	0.03	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
2	0.01	0.01	0.02	0.00	0.03	0.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
3	0.01	0.00	0.03	0.00	0.02	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
4	0.00	-0.01	0.03	0.00	0.02	0.01
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
5	0.00	-0.01	0.03	0.00	0.01	0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
6	0.00	-0.01	0.03	0.00	0.03	0.01
	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)
7	0.00	0.00	0.02	0.00	0.04	0.00
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.03)
8	0.03	0.00	0.06	0.03	0.02	0.04
	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
9	0.03	0.00	0.06	0.03	0.01	0.04
	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
10	0.04	0.00	0.08	0.04	0.01	0.05
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(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 or total returns, log of state real GDP, and design specific FEs. Event time is relative to the state implementation year, where the omitted base year 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.