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DISCUSSION PAPER SERIES

IZA DP No. 11033

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ABSTRACT

Tax Refunds and Income Manipulation Evidence from the EITC*

Welfare programs are important for reducing poverty but create incentives for recipients to maximize their income by either reducing labor supply or manipulating taxable income. In this paper, we quantify the extent of such behavioral responses for the Earned Income Tax Credit (EITC) in the US. We exploit that US states can set top-up rates, which means that, at a given point in time, workers with the same income receive different tax refunds in different states. Using event studies as well as a border pair design, we document that a raise in the state-EITC leads to more bunching of self-employed tax filers at the first kink point of the tax schedule. While we document a strong relationship up until the Great Recession in 2007, we find no effect thereafter. These findings point to important behavioral responses to what is the largest welfare program in the US.

JEL Classification: H20, H24

Keywords: EITC, tax refunds, income manipulation

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1 Introduction

Assessing the responsiveness of individuals to policy changes is of key importance for the (optimal) design of tax-benefit systems and for predicting the effects of policy reforms. Labor supply and taxable income responses have been studied extensively in the literature (see, e.g., Blundell and MaCurdy (1999), Meghir and Phillips (2008), Keane (2011), Saez et al. (2010) and Bargain and Peichl (2016) for surveys). An important insight of this literature is that welfare programs aimed at reducing poverty can trigger behavioral responses from recipients, who can maximize their welfare receipt by reducing labor supply or manipulating their taxable income. Because some responses — especially income manipulation — are costly to the taxpayer, effective policy design requires knowledge of the strength of these responses. One way to measure such behavioral responses is the degree of bunching at eligibility thresholds or kink points in the tax schedule (Saez, 2010; Chetty et al., 2013; Bastani and Seli, 2014).

In this paper, we document and quantify behavioral responses for the Earned Income Tax Credit (EITC), the largest welfare program in the United States. We exploit the discretion of each state in topping up the federal EITC, whereby recipients with the same taxable income receive higher tax refunds in some states than in others, leading to substantial variation in top-up rates across states and over time. Using event studies and a border pair design, we analyze to what extent tax filers manipulate their taxable income in response to a change in the state top-up rate. To measure income manipulation, we use data by Chetty et al. (2013) on the share of self-employed tax filers within a county who bunch around the first kink point of the EITC schedule.

In theory, one would expect that higher top-up rates lead to more bunching at the kink point because they give income manipulation a higher pay-off. Figure 1, which illustrates the main finding of our analysis, suggests that the theory is confirmed by the data. Here we compare counties located at a state border in a state with a raise in the top-up rate to control counties on the other side of the border, located in states without a raise. After taking out time trends, bunching in both groups follows a similar pattern before the raise but diverges thereafter. In states without a raise, it follows the same downward trend, while in states with a raise, bunching significantly increases after the raise.

While this figure provides prima facie evidence of a significant behavioral response, there are several endogeneity concerns that prevent us from interpreting this relationship as causal. One important concern is that states set top-up rates with behavioral responses in mind. A state that expects a strong response may be reluctant to raise the top-up than a state that expects no or very little response. Alternatively, as shown by Neumark and Williams (2016), states may raise the top-up rate to encourage people to participate in the federal EITC, thereby increasing the inflow of federal EITC dollars into the state. Using a border pair design with multiple combinations of fixed effects, we address several important sources of endogeneity. In this research design, we compare the level of bunching in counties on opposite sides of a state border. In this setting, tax filers in treated counties receive for the same income a higher tax

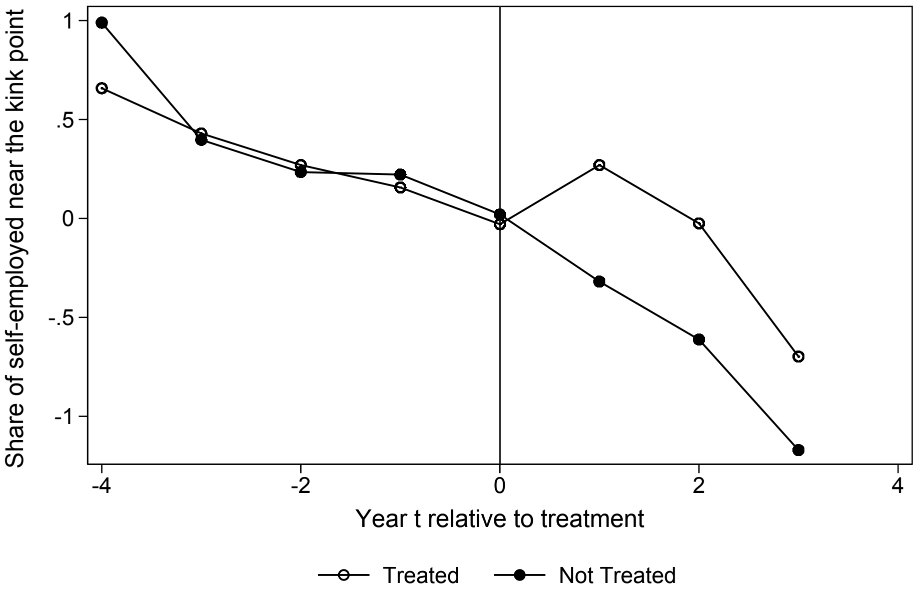


Figure 1: Bunching of self-employed workers near the kink point in counties with and without a raise in the top-up rate.

Notes: This figure compares the level of bunching before and after a raise in the top-up rate in the treatment counties — located in a state with a raise in $t = 0$ — to that in a neighboring control county located in a state without a raise. To make the counties comparable across years, year fixed effects and border pair fixed effects have been controlled for.

refund compared to those living in the control county across the state border.

Our estimates confirm the behavioral responses to a raise in the top-up rate observed in Figure 1. We consistently find a positive effect of the EITC top-up rate on the level of bunching at the kink point. In our preferred specification, an increase in the top-up rate by one within-county-pair standard deviation leads to an increase in bunching by about 10% of a standard deviation. To put this result in perspective, suppose that the average top-up rate would be raised from currently 3 percent by one standard deviation to 10 percent, which would be equivalent to raising the annual refund from \$180 to \$570. In this case, our estimates predict an increase in the degree of bunching by 2.6 percentage points. Across the US, in absolute numbers, this corresponds to an additional 930,000 EITC claimants, of which 250,000 would additionally bunch at the kink point.

We also document a change in the response to the EITC top-up rate during the Great Recession in 2008/09. While we observe a strong positive response up until 2007, we find small and statistically insignificant effects from 2008 onwards. This result appears to be driven by an overall higher number of self-employed workers claiming the EITC during the crisis. Because our outcome variable is the ratio of self-employed whose income is close to the kink point over all self-employed EITC claimants, the ratio remains unchanged when both the numerator and denominator are affected by the current economic situation.

Our results suggest that tax filers significantly respond to changes in the EITC schedule by manipulating their taxable income, either through changes in labor supply or through incorrect reporting of income. Moreover, the response in the total number of EITC claimants point to knowledge effects as well as labor supply responses. Seemingly, when a state introduces a top-up rate, self-employed people become more aware of the EITC, leading to more people claiming it as well as more people claiming an amount close to the revenue-maximizing kink point. An alternative explanation for this effect is that the EITC induces people to shift income from employment to self-employment, in which case income manipulation is easier.

This paper adds to the growing literature on the economic and social impact of the EITC.[1] Several studies show that the EITC substantially improves the lives of low-income families in the United States. Positive effects are found for example on infant health (Hoynes et al., 2015), maternal employment (Bastian, 2016), children's education outcomes (Bastian and Michelmore, 2017), the likelihood to get married (Anderberg, 2008; Bastian, 2017), as well as poverty reduction (Hoynes and Patel, 2015). Other studies emphasize the distortive nature of the EITC by showing that the kink points in the tax schedule provide an important incentive to manipulate taxable income to maximize one's tax refund (Saez, 2010; Chetty et al., 2013). This manifests itself through a visible degree of bunching of taxable incomes around this kink point, although it remains unclear whether this response is driven by income misreporting or an actual labor supply response.[2] While theses studies have documented and provided a rationale for bunching at the kink point, the contribution of our paper is to quantify the extent to which income manipulation responds to changes in the refund rates. Our results are important for assessing the effectiveness of the EITC and can inform policymakers about potential adverse responses of future increases in top-up rates.

More broadly, this paper contributes to the literature on behavioral responses to incentives provided by design features of public policies. A vast literature analyzes labor supply responses, especially to taxation, and numerous surveys and handbook articles have been written on this topic.[3] However, the variation in the magnitude of labor supply elasticities found in the literature is substantial (see Evers et al. (2008), Bargain et al. (2014)), and there is little agreement among economists on the size of the elasticity that should be used in economic policy analyses (Fuchs et al., 1998). Heim (2007) and Blau and Kahn (2007) show that married women's wage elasticities have strongly declined over time in the USA. A possible explanation for this finding is that a more stable attachment of women to the labor market is responsible for modest participation

[1] For surveys, see Hotz and Scholz (2003), Eissa and Hoynes (2006), Meyer (2010) and Nichols and Rothstein (2016).

[2] A key result of the existing literature on labor supply reactions to the EITC is that there are positive effects at the extensive margin (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Grogger, 2003; Hotz and Scholz, 2006; Gelber and Mitchell, 2012). The latter result which was found primarily for single mothers does not hold true for secondary wage earners, for whom Eissa and Hoynes (2004) find a decrease in participation. In contrast to these findings, previous research suggests that there are none or only small effects at the intensive margin (Rothstein, 2010; Chetty and Saez, 2013; Lin and Tong, 2017). Using data from Finland, Harju and Matikka (2016) provide evidence for substantial income shifting among high-wage earners.

[3] See, e.g., Hausman (1985); Pencavel (1986), Killingsworth and Heckman (1986), Heckman (1993), Blundell and MaCurdy (1999), Meghir and Phillips (2008), Keane (2011), Keane and Rogerson (2012), McClelland and Mok (2012), Bargain et al. (2014).

responses to financial incentives in the recent period. In addition to labor supply, a more recent literature has investigated the elasticity of taxable income, following the seminal contributions by Feldstein (1995, 1999).[4] There is also evidence that gross income is less responsive to tax changes than taxable income (Saez et al. (2010); Kleven and Schultz (2014)). Our paper shows that such incentives are also at play for the EITC, and tax filers significantly respond to them.

In the remainder of the paper, we first provide detailed information on the institutional background of the EITC (Section 2). In Section 3, we explain how we measure income manipulation, describe the construction of the dataset and present descriptive evidence. In Section 4, we describe the empirical strategy. In Section 5, we present the main estimation results. Section 6 concludes.

2 Institutional Background

We begin by providing information about the federal EITC and the state-specific tax credits (state EITC). We show that EITCs considerably vary across states, such that workers with the same income receive higher tax refunds in some states than in others. We further describe bunching at the first EITC kink point, our outcome of interest, and provide a theoretical discussion why one would expect bunching to increase after a raise in the State EITC.

2.1 The EITC

With 26.7 million workers receiving 63 billion dollars per year, The Earned Income Tax Credit (EITC) is arguably the largest and most important welfare program in the US (Nichols and Rothstein, 2016). Its aim is to supplement a person's labor income and reduce the income tax burden of low-wage earners while providing incentives to work. The eligibility for the EITC and the amount of tax credit depends on the number of children as well as one's taxable income. To claim the EITC, eligible tax payers have to file a federal tax return. Their income tax liability is then reduced by the amount of the EITC. If the tax credit exceeds the tax liability, the taxpayer receives a tax refund. Taxes are in general paid in the state where the income is earned, although some states have reciprocity agreements that allow taxpayers to file their tax returns in their state of residence (Agrawal and Hoyt, 2016).

Figure 2 illustrates the EITC tax schedule in 2009, the last year in our sample period, for families with one and two children as a function of annual earned income. The EITC schedule consists of three parts. In a phase-in region, starting at earnings of zero, the marginal refund increases with every additional dollar of labor income. At the plateau, for a range of annual earnings the tax credit remains constant, while it gets phased out above a certain threshold. For families with one child, for example, the tax credit is phased in at a rate of 34% starting from the first dollar of labor income, and reaches the plateau at an annual income of $8,950. Above the second kink point at $16,420, the tax credit is phased out at 16%. The maximum tax credit

[4] See Meghir and Phillips (2008) and Saez et al. (2010), for surveys, and Dörrenberg et al. (2015) for theory and evidence.

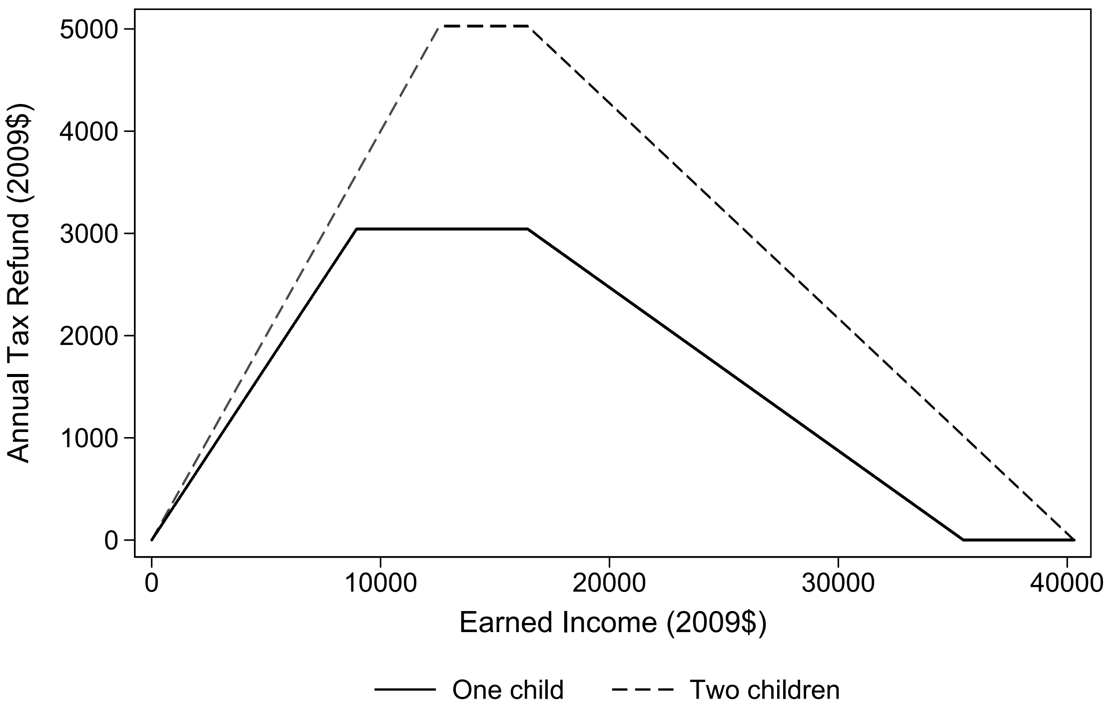


Figure 2: The EITC schedule in 2009

Notes: This graph displays the relationship between the tax refund and household labor income according to the 2009 federal EITC schedule. Tax units with adjusted gross income above the earned income threshold are not eligibile. First EITC kink point for families with one child: $8,950; for families with two children $12,570. Second kink point at $16,420.

for a family with one child is \$3,043, which they receive when their annual income lies between both kink points. If it lies above or below the kink points, the tax credit gets reduced.[5] For workers without children, the maximum tax credit is very small (\$457).

2.2 State-specific tax credits

In our analysis, we exploit the variation in state-specific top-up rates over time. Besides the federal EITC, which is common to all eligible workers in the US, each state can decide to top up the federal tax credit by a certain percentage. As argued by Neumark and Williams (2016), states have good reasons to top up the EITC. Besides improving the economic situation of poor families, a higher EITC may increase employment, states need to spend less on unemployment benefits. In addition, more EITC claimants means that more federal EITC dollars flow into the state, which may benefit the local economy.

The total tax credit in a given state is computed as

$$\text{total tax credit} = \text{federal EITC} \times (1 + \text{top-up rate}).$$

In some states, for example Minnesota and Wisconsin, the top-up rate depends on the number of children; the top-up is only granted to families with children, or families with children receive higher top-up rates than singles or childless couples.[6] Moreover, some states refund the tax credit if the tax liability becomes negative while others have a top-up of zero for negative tax liability. Over the years, the number of states with a top-up rate steadily increased. While in 1996 six states granted a top-up, in 2009, the end of our sample period, it were 20 states. As shown in Figure 4, the top-up rates considerably vary across states. It is zero in some states and as high as 40% in the District of Columbia (DC).[7]

EITC claimants in states with a low top-up rate are granted a significantly lower tax credit compared to claimants *with the same pre-tax income* in states with a high top-up rate. Figure 3 illustrates the difference in tax credit for EITC claimants with one child in a state with zero top-up and a state with a top-up rate of 40 percent. A claimant with an income at the first kink point would receive a tax credit of \$3,043 in a state without a top-up, and \$4,260 in DC, which has the highest top-up rate in the US. In both states, the kink points of the EITC schedule are the same, although the phase-in and phase-out region are steeper in the state with the high top-up rate. Therefore, in 2009, a family with one child receiving the maximum credit would receive an additional tax credit of \$30 from a one percentage point increase in the top-up rate. The same family would gain \$960 through moving from Cheshire county in New Hampshire to neighboring Windham county in Vermont. In 2009, New Hampshire and Vermont are the bordering US states with the largest difference in top-up rates (32 percentage points).

[5] For families with two children, the kink points 2009 are at \$12,570 and \$16,420. The maximum tax credit is \$5028, which results in steeper phase-in and phase-out regions compared to the schedule for families with one child.

[6] Wisconsin has a top-up rate of zero for childless people, but top-up rates of 4%, 14%, and 43% for families with one, two, and three and more children, respectively.

[7] We are aware that, technically, DC is technically not a state. However, it has its own EITC.

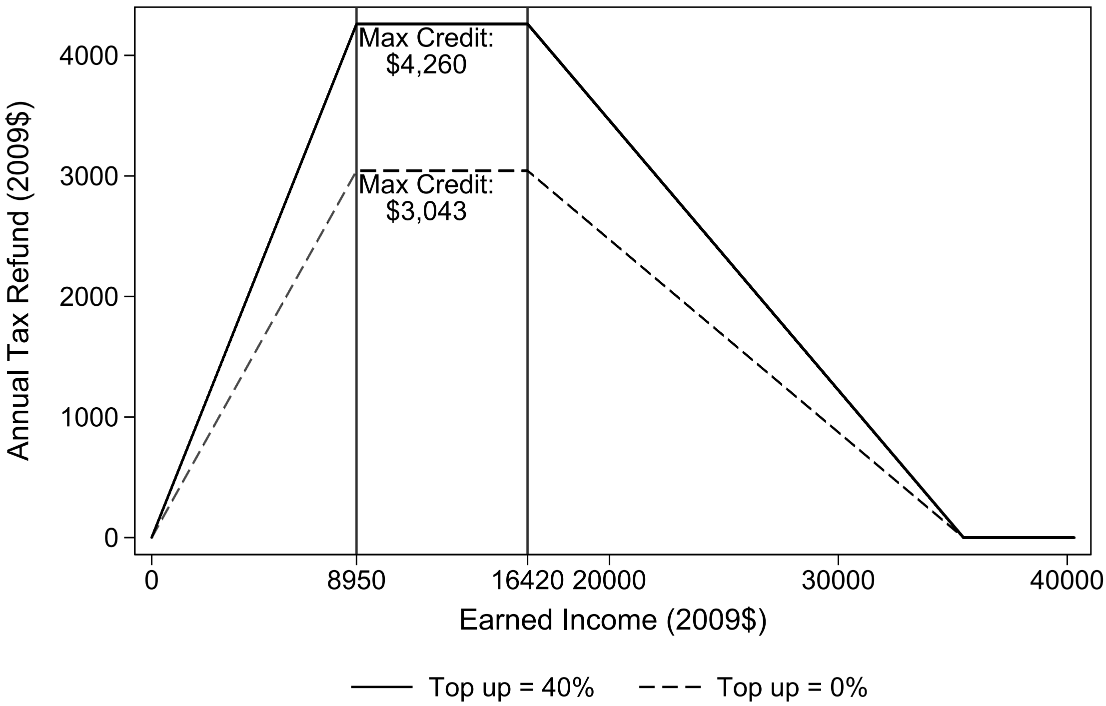


Figure 3: Tax credit in states with high and low top-up rates

Notes: This figure displays the EITC schedule for claimants with one child in a state with zero top-up and a state with a top-up rate of 40 percent in 2009. The vertical lines mark the first and the second kink point. Tax units with adjusted gross income above the earned income threshold are not eligible. Families with unearned income may be ineligible.

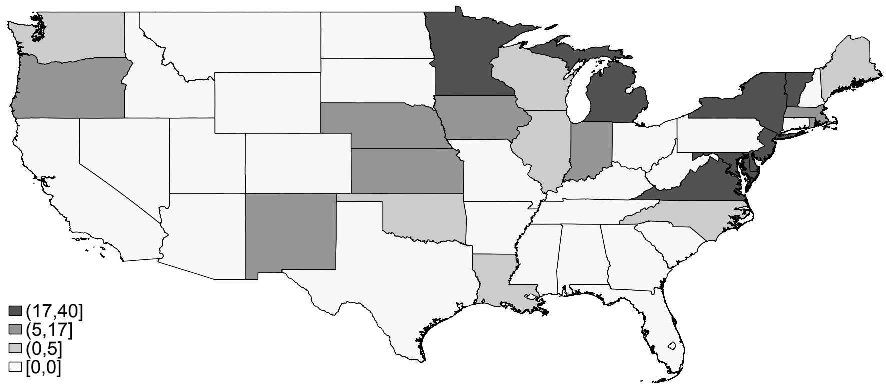


Figure 4: State-specific top-up rates in 2009

Notes: This Figure shows the variation in top up rates across states in 2009. Darker colors refer to higher top-up rates.

2.3 Bunching as a measure of income manipulation

With its two kink points, the EITC schedule provides incentives for recipients to manipulate their taxable income. For tax filers whose income is close to one of the kink points, it is optimal to manipulate their income to be exactly at the kink point. At the first kink point, the marginal tax credit switches from a high positive value to zero, such that every additional dollar in earnings above the threshold does not result in higher tax credits. On the other hand, the tax liability increases with every dollar earned, regardless of the tax credit.[8]

There are several margins along which EITC claimants can manipulate their taxable Income, namely labor supply, income shifting and tax evasion. A legal margin is adjusting one's labor supply; for example, workers may decide to work fewer hours, thereby decreasing their annual earnings while increasing their tax refund. Another way to adjust one's labor supply and manipulate taxable income, especially for self-employed workers, is to smooth the stream of income over time. For self-employed workers whose income is close the first kink point, it could pay off to postpone projects to the following year, thereby maximizing the tax credit in the present year. A further — yet illegal — margin of income manipulation is incorrectly declaring one's income in the annual tax return.

Such manipulations manifest themselves in a noticeable degree of bunching around the first kink point of the EITC schedule, as documented by Saez (2010) and Chetty et al. (2013). In the absence of income manipulation, one would expect the income distribution to be smooth. Instead, however, a large number of EITC claimants report an income that is very close to the first kink point, resulting in a spike in the earnings distribution.

Some groups of workers have a much greater scope for income manipulation than others. As shown by Saez (2010), pure wage earners — i.e. regularly employed workers — display no bunching at the kink point, because their taxable income gets directly reported to the Internal Revenue Service (IRS) by their employer, limiting the scope for incorrectly declaring one's income. In addition, work hours are usually fixed in a work contract, making it difficult to adjust one's labor supply. Self-employed workers, by contrast, have a much greater scope of manipulating their taxable income, as they report the taxable income to the financial authorities themselves, and they are free to choose how much they work.[9]

A raise in the top-up rate provides people with a higher payoff for income manipulation. Therefore, we would expect bunching to increase following a raise in the top-up rate, although we would only expect this effect for self-employed tax filers. Likewise, would not expect any effect for tax filers without children, because their federal EITC is very small in the first place.

[8] For a theory of optimal income transfers with a non-linear tax schedule, see Kaplow (2007).

[9] Additional evidence from Denmark by Daniel and Bertel (2013) suggests that half of the bunching response among self-employed is due to inter-temporal income shifting.

3 Data and Descriptive Evidence

In this section, we describe the construction of the dataset and provide descriptive statistics for the main variables. In addition, we produce event study graphs that provide descriptive evidence on an increase in bunching following a raise in the State EITC.

3.1 Data

We construct our dataset by linking county-level data on tax filing with state-level institutional data on the EITC, as well as county-level demographic data.

County-level data on tax filing. Our main outcome of interest is the bunching of self-employed workers around the first kink point of the EITC schedule. We use the data compiled by Chetty et al. (2013) for our analysis. Bunching is measured as the share of self-employed EITC-claimants in an area whose income falls within a window of $500 around the first EITC kink point. The denominator of this share is the total number of self-employed EITC claimants in that area. In 2009, this represents about 600,000 people. From Chetty et al. (2013), this measure is available for all 3-digit zip codes from 1996 to 2009. In Appendix A, we explain how we convert zip-code-level information to the county-level.

In additional regressions, we consider three outcome variables representing the absolute number of EITC claimants, namely the number of self-employed claimants near the kink point (the numerator of the main outcome), the total number of self-employed EITC claimants (the denominator) as well as the total number of non-self-employed claimants.

Institutional data. We combine the county-level data with institutional data on the State EITC from 1996 to 2009, as well as institutional features such as refunds not being granted to workers without children, or negative tax credits not being paid out. We take this data from the NBER TAXSIM database.[10]

County-level demographic data. To run balancing tests, as well as to control for pre-treatment characteristics of counties, we use county-level data on population, employment as well as average wages. Data on employment and wages are taken from the Quarterly Census of Employment and Wages (QCEW), whereas population data are taken from the county-level population statistics provided by the Bureau of Labor Statistics.

3.2 Descriptive statistics

Table 1 reports descriptive statistics for the main variables of interest. Because in one of our research designs we only use counties that straddle a state border, we separately report statistics for border counties.

[10] See Feenberg and Coutts (1993) for a documentation.

Overall, the outcome variables as well as the regressors of interest strongly increase over time. The first two panels show the evolution of the State EITC. We first consider a dummy that equals unity if a county is located in a state with a top-up rate, and zero otherwise. Over the sample period, the share of counties in states with top-up rates increased from 11.5% to 44%. Likewise, the average top-up rate across all counties increased over the same period. Due to the large share of zeros, it only amounted to 1.6% in 1996, whereas it increased to over 5% in 2009.

Panels 3)-5) display the mean and standard deviation of our outcome variables. The share of self-employed EITC claimants near the kink point corresponds to the bunching measure used in Chetty et al. (2013). The variables displayed in Panels 4) and 5) represent the denominator and numerator, respectively, of the bunching measure. In addition, Panel 6) reports the total number of EITC claimants per county.

To compare border counties with all counties, we additionally report population and labor market statistics for the year 2004. According to these statistics, border counties do not differ in their demographic and economic structure from non-border counties. From 1,184 border counties, we construct a dataset of 1,308 border county pairs, whereby a county that straddles multiple counties in a neighboring state is part of multiple county pairs.

3.3 Descriptive evidence on top-up rates and income manipulation

The descriptive statistics in Table 1 show that both the top-up rate as well as the extent of bunching increases over the sample period. In a next step, we provide evidence on how both are related. We use the sample of border pairs and pay particular attention to the timing of raises in the top-up rate. We exclude from the sample the few county pairs in which the top-up rate decreased (55 pairs).[11] In addition, if a county pair experiences several changes over the sample period, we only include the first change.

As in Figure 1 in the introduction, we are interested in the time trends in bunching in counties that experience a raise in the EITC compared to those where the EITC remains constant. Within each pair, we consider as treated the county that is located in a state with a change in the top-up rate and as control the county located in a state without a change. If top-up rates were to have an effect on income manipulation, following a raise in the State EITC in the treatment group, we would expect to see an increase in bunching in the treatment but not in the control counties.

To provide more systematic evidence for a response in bunching, we estimate an event study equation of the form

$$y_{cpst} = \sum_{k=-4}^{3} \alpha_k \times \mathbb{1}_{[t=t^*+k]} + \sum_{k=-4}^{3} \beta_k \text{ treat}_s \times \mathbb{1}_{[t=t^*+k]} + \boldsymbol{X'_{st}}\gamma + \delta_t + \varepsilon_{cpst}, \quad (1)$$

whereby we consider the period beginning 4 years before the raise and running until two years

[11] In our main analysis in Section 5, these county pairs will be included.

Table 1: Descriptive statistics

	All counties		Border Counties	
	Mean	SD	Mean	SD
1 Top-up dummy (1 if state has a top-up rate, in percent)				
1996	11.5	32.0	13.1	33.7
2000	22.8	42.0	25.7	43.7
2004	26.3	44.0	29.5	45.6
2009	43.8	49.6	46.6	49.9
2 Top-up rate (in percent)				
1996	1.60	5.94	2.17	7.58
2000	2.59	6.03	3.00	6.48
2004	3.14	6.99	3.71	7.61
2009	5.51	8.34	6.03	8.77
3 Share of self-employed EITC claimants near the kink point				
1996	5.04	1.55	5.00	1.61
2000	7.18	2.99	7.08	3.13
2004	8.50	3.98	8.29	3.96
2009	9.27	4.68	8.97	4.53
4 Self-employed EITC claimants				
1996	817	2,755	753	2,149
2000	866	3,235	826	2,957
2004	1,187	4,309	1,108	3,982
2009	1,434	5,004	1,326	4,782
5 Self-employed EITC claimants near the kink point				
1996	54	328	52	264
2000	91	572	99	702
2004	143	751	138	773
2009	194	902	178	904
6 Non-self-employed EITC claimants				
1996	4,714	13,244	4,458	12,659
2000	4,734	13,430	4,507	13,054
2004	5,006	13,135	4,736	12,768
2009	5,371	13,336	5,054	12,895
Population, 2004	93,320	302,015	93,581	260,604
Unemp rate, 2004	5.69	1.82	5.67	1.87
Empl rate, 2004	94.31	1.82	94.33	1.87
Average wage, 2004	28,805	6,141	28,909	6,219
Counties	3141		1184	
County pairs	NA		1308	
States	51		49	

Notes: This table reports descriptive statistics for the main variables of interest for selected years. The top-up dummy equals one if a county lies in a state with a top-up rate. The column on the left reports the statistics for all counties in the US, while the column on the right only reports the statistics for counties that straddle a state border.

after. The subscripts c, p, s and t refer to county, pair, state and time respectively. We choose as base period the year before the raise, i.e. $t^* = -1$. Our coefficients of interest are β_k, which represent differential changes in bunching between the treated and untreated counties within a pair p relative to the base year. To control for time trends that are common to all counties, we include two distinct sets of fixed effects. The first set, $\mathbb{1}_{[t=t^*+k]}$, controls for average time trends before and after a raise in the top-up rate, regardless of the year in which the raise occurred. Because within our sample period of 14 years the raises occur in different calendar years, we additionally control for year fixed effects δ_t.[12] The year fixed effects ensure that the response to a raise in 1996 receives the same weight in the estimate of β_k as the response in, say, 2008. We also control for time-varying features of the tax code (X_{st}), namely whether the refund depends on the number of children, and whether a positive refund is given if a person's tax credit exceeds his/her tax liability. The error term ε_{cpst} captures all determinants of the outcome that are not explained by the regressors in the above estimating equation.

Figure 5 displays the estimates for β_k. Before the raise in the top-up rate, the estimates are close to zero and statistically insignificant. This is consistent with the parallel pre-trends shown in Figure 1. After the raise, we find significant positive effects on bunching in the treatment relative to the control counties. A raise in the top-up rate increases the degree of bunching by half a percentage point, which amounts to 5% of the mean in 2009.

While these results provide strong evidence of tax filers responding to changes in top-up rates, there are endogeneity concerns that prevent us from interpreting these results as causal. The same economic factors that affect a state's decision to raise its top-up rate could also directly influence bunching. Despite the parallel pre-trends, we may not be able to appropriately control for these factors in the above regression. In the following sections, we address such endogeneity concerns by using a border pair design. In addition, we define here an event as a raise in the top-up rate, such that our estimates reflect the impact of an average raise. In the next Section, we are able to quantify the marginal effect of raising the top-up rate by 1 percentage point.

[12] This approach — controlling for leads and lags as well as year fixed effects — is similar to the one used by Jäger (2016).

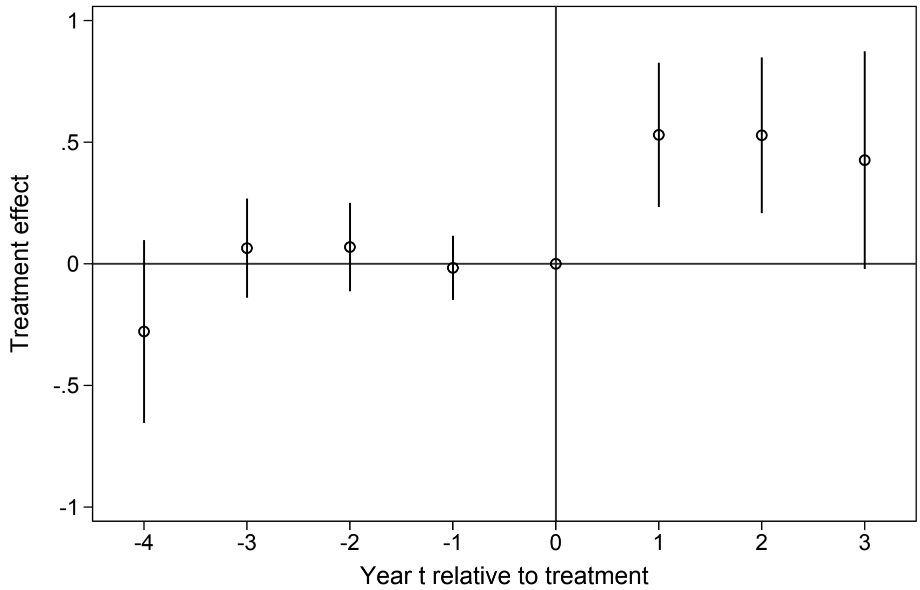


Figure 5: Bunching before and after a raise in the top-up rate.

Notes: This graph displays the coefficient estimates of β_k in Equation (1). The specification includes year fixed effects and controls and is estimated on a sample restricted to counties straddling a same state border. The reference category is the year before treatment. The vertical line represents the period zero, i.e. the year before treatment.

4 Main Analysis - Empirical Strategy

While the event study shows an increase in income manipulation following a raise of the state top-up rate, there are several endogeneity concerns preventing us from interpreting these estimates as causal. In this section, we describe our identification strategy, which relies on a comparison of neighboring counties that are exposed to different EITC top-up rates.

4.1 Empirical model

To quantify the effect of the EITC top-up rates on income manipulation, we consider an empirical model of the form

$$y_{cpst} = \alpha + \beta \text{ top-up}_{st} + \boldsymbol{X'_{st}}\gamma + FE(p, s, t) + \varepsilon_{cpst}. \tag{2}$$

The outcome y in county c, which is located in pair p and state s, at time t is regressed on the top-up rate in state s at time t. We control for time-varying state-level features of the EITC ($\boldsymbol{X_{st}}$), namely whether the refund depends on the number of children, and whether a positive refund is given if a person's tax credit exceeds his/her tax liability. In addition, we condition on fixed effects along several dimensions — pair, state, time, as well as combinations of these dimensions.

The error term ε_{cpst} captures all the remaining determinants of the outcome. To account for serial correlation as well as cross-sectional correlation in the error term, we cluster the standard errors at the county- as well as the pair-level. In addition, we asses our inference through permutation tests.

4.2 Identification

Given that the top-up rates are not randomly assigned to states but chosen by state governments, we cannot immediately interpret the estimate of β as causal. A causal interpretation requires that there be no correlation of the top-up rate with the error term conditional on controls and fixed effects,

$$cov(\text{top-up}_{st}, \varepsilon_{cpst} | \boldsymbol{X_{st}}, FE(p, s, t)) = 0. \tag{3}$$

There are at least three challenges to a causal interpretation. First, top-up rates may be set endogenously. A state government that expects a strong reaction of taxpayers to a raise in the top-up rate may choose a lower top-up rate than a state expecting a weak reaction. A second problem is economic shocks that affect EITC eligibility as well as the choice of top-up rate. A state that is hit by a negative economic shock may decide to raise the top-up rate to alleviate the consequences for low-income families. At the same time the shock may lower incomes and, thus, increase the number of households eligible for the EITC. Therefore, an economic shock can result in a spurious relationship between tax refunds and income manipulation.

A third challenge is differential time trends in income manipulation and top-up rates. As shown by Chetty et al. (2013), knowledge about the EITC schedule substantially varies across areas and over time. Initially, in some areas, tax filers seem to have no knowledge about the first kink point being income-maximizing, while in other areas there is a high concentration of tax filers with a taxable income around the kink point. Over time, as the knowledge of the EITC spreads, areas with initially zero bunching eventually catch up with those areas with a high degree of bunching from the outset. Unless appropriately controlled for, the estimated effect of top-up rates on income manipulation may reflect those differential time trends rather than a causal effect.

Border pair design. To circumvent these challenges, we apply a border pair design, whereby we compare neighboring counties that straddle a state border.[13] Taxpayers with the same pre-tax income are eligible for different top-up rates on either side of the border. This setting has quasi-experimental character, as it allows us to compare the change in income manipulation in treated counties that experience a raise in top-up rates to changes in very similar control counties where the top-up rate remains unchanged. The border pair design differs from a conventional panel estimator in the definition of the control group. In the panel estimator, the control group is a weighted average of all other counties, whereas in the border pair design, each treated county

[13] Similar approaches have been used by Dube et al. (2010) to evaluate changes in minimum wages in the US, and by Lichter et al. (2015) to estimate the impact of government surveillance in East Germany.

is assigned its neighbor as a control county. To the extent that neighboring counties are more similar than a particular county and the weighted average of all other counties, the neighboring counties provide a more suitable control group.

We implement the border pair design with three distinct sets of fixed effects.

Pair and year fixed effects, $FE(p,s,t) = \delta_p + \delta_t$**.** In the first model, we condition on year and pair fixed effects, which restrict the identifying variation to within pairs over time. A positive estimate of β indicates that a widening of the gap in top-up rates within a county pair leads to a widening of the gap in the outcome. These fixed effects help us to overcome the first of the three challenges. The pair fixed effects control for the average top-up-rate differential in each pair and, thus, absorb any variation in states' differential setting of top-up rates.

Pair and year fixed effects and pair-specific time trends. While useful as a starting point, the two-way fixed effect model with pair and year fixed effect can yield biased estimates if county pairs diverge in their time trends, which have been shown to be present for bunching (Chetty et al., 2013). To address this challenge, we additionally include pair-specific time trends in the regression. In that case, the coefficient β is identified off deviations from the time trend within a pair.

Pair-by-year fixed effects, $FE(p,s,t) = \delta_{pt}$**.** In a more demanding specification, we include pair-by-year fixed effects, which absorb all average differences in observable and unobservable characteristics between years within each county pair. Restricting the variation in that way is useful to exclude that the estimation of β is confounded by local economic shocks or differential time trends between pairs. Take, for example, a pair that is hit by a negative shock that coincides with a change in top-up rate in one of the counties and directly affects the level of bunching. Neither the pair nor the year fixed effects would account for that shock. However, the pair-by-year fixed effects absorb such shocks, which raises the plausibility that the identifying assumption (3) holds.

To understand how β can be identified on top of pair-by-year fixed effects, it is instructive to use as reference point a model with separate time and pair fixed effects. In that model, we exploit variation in top-up rates within pairs over time. A slightly more restrictive model would be one with pair-specific time trends, which exploits variation within pairs over time on top of the time trends. Our model with pair-by-year fixed effects goes yet another step further and allows for year-pair-specific economic shocks. It is possible to identify this model because the top-up rates as well as the outcomes vary *within* each pair. In the fixed-effect estimator for β, each pair-year combination receives equal weight. We no longer use variation within pairs over time, but rather use variation within and across pairs *after differencing out any pair-specific shocks.*

Identifying variation Table 2 displays the amount of variation, measured by the standard deviation, in the most important variables for different samples as well as for different fixed effect specifications. Column (1) displays the variation for all counties, whereas Columns (2)-(4) display the variation for border counties only. In the border pair sample, some counties appear more than once if they have more than one neighbor in a different state. Going from left to right, one can see that the amount of variation gets reduced as more fixed effects are added. However, even after controlling for pair-by-year fixed effects, there is still substantial variation in top-up rates as well as the outcome variables. Figure 8 in Appendix B illustrate the relationship between top-up rates and bunching for the border pair sample, after pair-by-year fixed effects and state-specific characteristics of the EITC have been controlled for. The graph points to a significant positive relationship, which we will further explore in the following section.

Table 2: Variation in key variables

	(1) All Counties	(2) Border Counties	(3) Border Counties	(4) Border Counties
Top-up rates				
SD	6.86	7.56	5.43	4.88
Top-up dummy				
SD	0.44	0.45	0.33	0.29
Share of self-employed near the kink point				
SD	3.83	3.75	1.89	1.42
EITC claimants, self-employed				
SD	3956.62	3391.67	2299.95	2175.05
EITC claimants, non-self-employed				
SD	13245.24	13029.56	8284.27	8238.79
Self-employed claimants near the kink				
SD	684.01	686.86	504.16	460.22
Controls:				
County FE	No	No	No	No
Year FE	No	No	Yes	No
Pair FE	No	No	Yes	No
Pair × Year FE	No	No	No	Yes
N	43967	36616	36616	36616

This table displays the variation,measured by the standard deviation, in the main variables with various sets of fixed effect. The all county data set comprises of all counties in the US. The border county da-taset consists of counties straddling a state borders only. Column (1) -(2) display the raw standard deviations. Column (3) shows the residual variation after a transformation of separate year and pair fixed effects. Column (4) shows the residual variation after a transformation of year-by-pair fixed effects

5 Results

In the following, we present our estimates for the impact of the state EITC along several behavioral margins. We first present our main results for the border pair design, using different fixed effect specifications. In a further step, we analyze whether the response changed during the Great Recession in 2008/9. In both analyses, inference relies on parametric assumptions about the spatial and serial correlation of standard errors. To assess the robustness of our inference, we perform permutation tests, which confirm our main conclusions.

5.1 EITC refund rates and income manipulation

Table 3 presents OLS estimation results based on the regression model in Equation (2). We consider three fixed-effect specifications, four outcome variables, and two treatment definitions. Each entry is the result of a separate regression of the outcomes listed in Panels A)-D) on the top-up dummy or rate. In Columns (1)-(3), the regressor of interest is a binary variable that equals unity if a state has a top-up rate, whereas in Columns (4)-(6), the regressor of interest is the top-up rate in percentage points (zero for counties located in states without a top-up rate).

Our main measure for income manipulation is the bunching of self-employed EITC claimants within a \$500-interval around the first kink point of the EITC schedule. For each county, this measure is computed as the number of self-employed EITC claimants within this interval divided by the total number of self-employed EITC claimants. In Panels B and C, we separately estimate the impact of the top-up rate on both components that make up the bunching measure. This allows us to study whether the overall effect is driven by changes in the number of people around the kink point (numerator) or in the overall number of tax filers (denominator). Finally, in Panel D, we also consider as outcome the number of non-self-employed claimants. If we found an effect of the top-up rate on this variable, this would be indicative of knowledge effects and labor supply responses rather than manipulation of taxable income.

Effect of the state EITC on bunching. In Columns (1)-(3), we only consider changes in the top-up rate along the extensive margin. The coefficient $\widehat{\beta} = 0.365$ in Panel A, Column (1), means that when a state introduces a top-up rate, bunching increases in a treated county in that state by 0.365 percentage points relative to the neighboring county in a different state, where the top-up dummy remains unchanged. This effect amounts to 4.4% of the mean level of bunching in 2004, as well as 19% of a within-pair standard deviation in bunching. The estimated coefficient is statistically significant at the 10%-level. In Column (2), when we condition on pair-specific time trends, we find a similar point estimate, although the estimate is less precise and no longer statistically significant. In Column (3), our most conservative specification, we condition on pair-by-year fixed effects, based on which we obtain an even larger point estimate of $\widehat{\beta} = 0.492$, significant at the 10%-level. These results suggest that tax filers respond to the introduction of a state EITC with a higher share declaring an income closer to the revenue-maximizing kink point.

While these results provide a first indication of an effect, it should be noted that the effect is driven by changes in a limited number of states. Over the sample period, only 14 states introduced a top-up rate. Within a county pair, the identification comes from switches in the dummy from zero to one, which can only happen once per county over the sample period. In Columns (4)-(6), in contrast, we identify the effect off changes in the top-up rate along both the extensive and the intensive margin.

In the model with separate pair and year fixed effects, shown in Column (4), we find no statistically significant effect of an increase in the top-up rate on bunching. However, once we condition on pair-specific time trends or pair-by-year fixed effects, the effect is large and statistically significant. For a within-pair standard deviation in the top-up rate ($sd = 5.43$), bunching increases by $5.43 \times 0.023 = 0.12$ percentage points, which is around 6.6 percent of a within-pair standard deviation in bunching.

Effect on the number of self-employed claimants. The results shown in Panel A represent the effect of an increase in the top-up rate on the *share* of EITC claimants whose income is close to the EITC kink point. This share consists of two components, namely in the numerator the number of self-employed tax filers close to the kink point and in the denominator the total number of self-employed tax filers. A positive effect in Panel A indicates that the numerator increases more than the denominator, leading to a higher share. To assess the relative contributions of both, we separately consider the effects of the EITC in Panels B and C. In Column (1), we find that the introduction of a top-up rate increases the number of tax filers near the kink point by 222, which is larger than the mean number in 2004 or 2009. At the same time, it leads to an increase in the total number of self-employed EITC claimants by 893, which is around 75% of the mean in 2004. In Column (4), we estimate that a one-percentage-point increase in the state EITC raises the number of self-employed claimants near the kink point by 8.6 (1.7% of a within-pair standard deviation) and increases the total number of self-employed claimants by 36.5 (1.6% of a within-pair standard deviation). With both regressors, the effect size increases when we condition on pair-by-year fixed effects. To sum up, the top-up rate increases both the numerator and the denominator with the former increasing more than the latter.

Effect on non-self-employed EITC claimants. Finally, in Panel D, we estimate the impact of the EITC on the number of non-self-employed claimants. This group is interesting because it has little scope for manipulating their declared taxable income. Rather, any effect here is indicative of a change in labor supply. The evidence on this channel is mixed. We find large and statistically significant results when we use the top-up dummy as regressor, but small and statistically insignificant results when we use the continuous measure of the top-up rate. These results provide suggestive evidence for labor supply effects, although the marginal effect of an increase in the top-up rate on bunching appears to be driven by other channels. This is not surprising, given that, in general, it is (more) difficult to adjust labor supply at the intensive margin — i.e. the number of hours worked — due to frictions in the labor market. Yet, it is possible that a higher state EITC increases labor supply at the extensive margin which we cannot rule out but also not directly test with our data. An alternative explanation for this effect could be knowledge effects (Chetty et al., 2013). The introduction of a state EITC is a salient event that triggers public discussions. Therefore, taxpayers may be more aware of the introduction of a state EITC compared to the raise of an already existing state EITC.

5.2 The impact of top-up rates before and during the great recession

While bunching had been steadily increasing up until 2007, there has been a significant drop in 2008 and 2009, while at the same time the average top-up rate continued its upward trend. A possible reason for these developments is the Great Recession in 2008/09, during which states expanded their EITC top-up rates, while the increase in unemployment decreased the number of eligible households (see Figure 6).

To assess whether the impact of the top-up rate changes with the Great Recession, we estimate a regression with a full interaction of the top-up dummy or rate with dummies for the pre- and post-Great-Recession period.

$$y_{cpst} = \beta_1 \text{ top-up}_{st} \times \mathbb{1}_{[t<2008]} + \beta_2 \text{ top-up}_{st} \times \mathbb{1}_{[t>=2008]} + \boldsymbol{X'_{st}}\gamma + \delta_{pt} + \varepsilon_{cpst}. \quad (4)$$

The first term is an interaction between the top-up rate and a dummy that equals one in the pre-crisis years, while the second term is an interaction with a dummy that equals one from 2008 onwards.[14] Our results point to a large and significant effect before 2008, although while we find no consistent effects in 2008/9. In Column (1), the effect on bunching in 2008/9 is negative, which is the case because the denominator — the total number of self-employed claimants — reacts more than the number of claimants close to the kink point.

5.3 Assessing inference through permutation tests

While the border design facilitates the estimation of a causal effect by providing clear treatment and control counties, it also complicates statistical inference. The error terms can be correlated across space as well as within counties over time, which can lead to an underestimation of standard errors, and an under-rejection of the null hypothesis of no effect (Bertrand et al., 2010). Moreover, in the border pair design, some counties are part of multiple pairs, such that their errors are mechanically correlated. As a first step, to account for correlations in the error term, we applied to all estimates a two-way clustering procedure at the county- as well as pair-level. However, clustering may not eliminate all systematic correlations of the error terms.

To assess the statistical significance of our estimates without relying on assumptions about clustering, we additionally perform permutation tests for the four main outcomes. In these tests, we first obtain an empirical placebo distribution of estimates that would occur under the null hypothesis of there being no effect. In a second step, we compare our estimates to the placebo

[14] While these two dummies are multicollinear, it is possible to include these interactions in the regression because we do not include the dummies on their own.

Table 3: The Effects of Top-up rates on Bunching

	(1) Top-up Dummy	(2) Top-up Dummy	(3) Top-up Dummy	(4) Top-up Rate	(5) Top-up Rate	(6) Top-up Rate
A. Share of self-employed near the kink point						
Top-up	0.365*	0.310	0.492*	0.010	0.023**	0.029**
	(0.220)	(0.244)	(0.284)	(0.009)	(0.011)	(0.013)
B. Number of self-employed EITC claimants near the kink point						
Top-up	222.665**	237.897**	295.758**	8.587**	9.125**	11.439**
	(96.409)	(105.777)	(126.710)	(4.284)	(4.545)	(5.468)
C. Total number of self-employed EITC claimants						
Top-up	892.661**	951.592**	1168.111**	36.507**	35.294**	43.317**
	(376.278)	(410.242)	(492.554)	(17.155)	(17.257)	(20.765)
D. Total number of non-self-employed EITC claimants						
Top-up	1930.364*	2151.488*	2633.806*	58.175	61.947	77.751
	(1095.349)	(1210.460)	(1424.684)	(42.224)	(46.721)	(55.923)
Controls:						
Year FE	Yes	Yes	No	Yes	Yes	No
Pair FE	Yes	Yes	No	Yes	Yes	No
Pair-spec Time tr.	No	Yes	No	No	Yes	No
Pair × Year FE	No	No	Yes	No	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	36608	36608	36592	36608	36608	36592

This table displays results of separate OLS regressions of our outcome variables on a dummy for having a top-up rate Column (1) – Column (2) and on top-up rates Column (3) – Column (4) , as well as the controls and fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at county and pair level, are reported in parentheses.

Table 4: Top-up rates and bunching before and after the Great Recession.

	(1) Top-up Rate	(2) Top-up Rate	(3) Top-up Rate
A. Share of self-employed near the kink point			
Top-up before 2008	0.019*	0.022*	0.035***
	(0.010)	(0.011)	(0.013)
Top-up 2008, 2009	-0.022***	0.026***	0.004
	(0.008)	(0.009)	(0.010)
B. Number of self-employed EITC claimants near the kink point			
Top-up before 2008	9.783**	10.341**	12.786**
	(4.621)	(4.898)	(5.753)
Top-up 2008, 2009	4.254	3.437	5.419
	(3.311)	(3.150)	(4.609)
C. Total number of self-employed EITC claimants			
Top-up before 2008	38.208**	38.659**	48.186**
	(17.277)	(17.859)	(21.389)
Top-up 2008, 2009	30.349*	19.552	21.548
	(17.339)	(15.167)	(19.960)
D. Total number of non-self-employed EITC claimants			
Top-up before 2008	65.071	68.770	90.632
	(44.377)	(48.234)	(58.560)
Top-up 2008, 2009	33.206	30.033	20.166
	(36.788)	(42.030)	(51.956)
Controls:			
Year FE	Yes	Yes	No
Pair FE	Yes	Yes	No
Pair-spec Time tr.	No	Yes	No
Pair × Year FE	No	No	Yes
Controls	Yes	Yes	Yes
N	36608	36608	36592

This table displays results of separate OLS regressions of our outcome variables on top-up rates, as well as the controls and fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at county and pair level, are reported in parentheses.

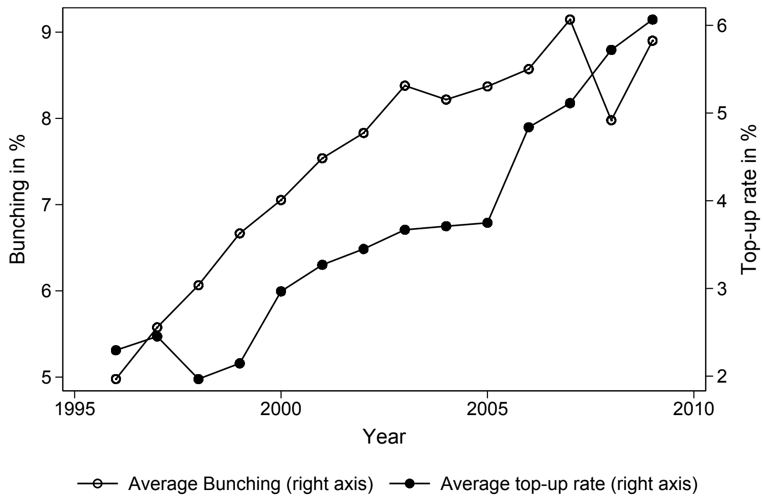


Figure 6: Top-up rates and bunching, 1996-2009

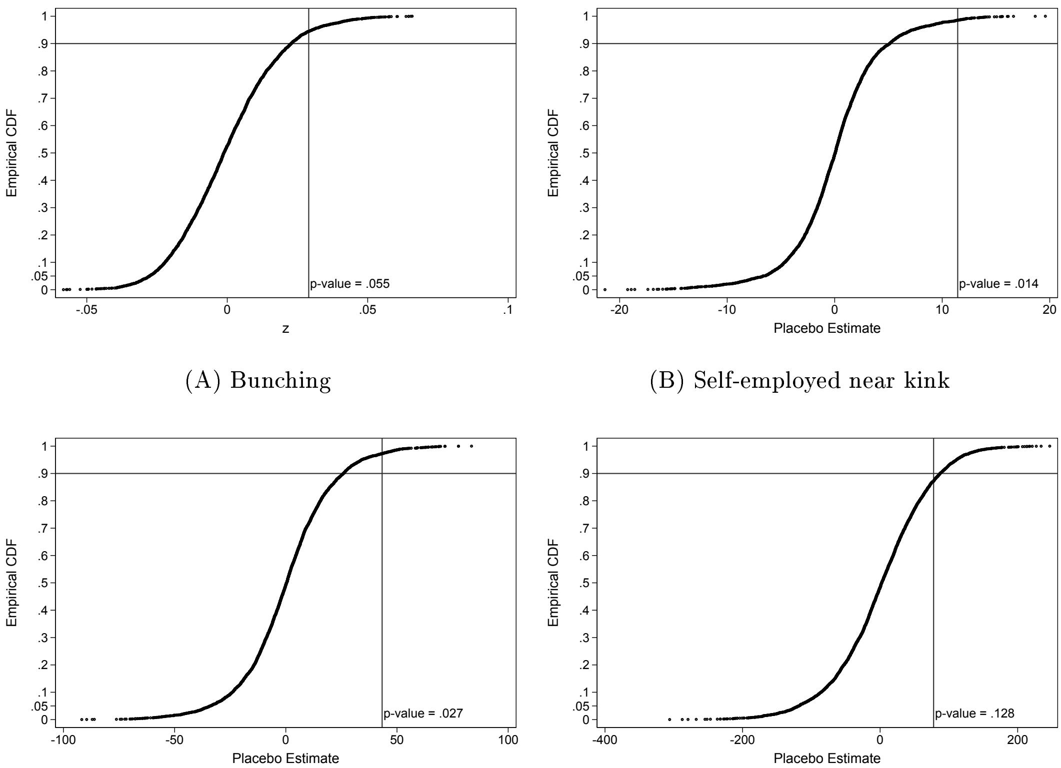
Notes: This figure shows the average level of bunching in percent (left axis), as well as the average top-up rate. Each dot represents the average across all counties within a given year.

distribution and obtain a empirical p-value that describes the probability of obtaining a result that is at least as extreme as ours.[15] In a conventional case, namely one in which a treatment is assigned once, the placebo distribution is obtained by repeatedly randomizing the treatment across observations and estimating the same model in each replication. The complication in our case is that top-up rates within states are path-dependent. States do not randomly set a top-up rate every year, but rather adjust the rate of the previous year. To account for path-dependency, we therefore randomize over 14-year paths in top-up rates. In each replication, we randomly assign each state a path for its top-up rate and estimate the model.

Figure 7 displays the cdfs of the placebo distributions based on 5,000 replications, as well as the z-scores of our estimates (vertical lines) from Column (6) in Table 3. The horizontal lines describe the 90-th percentile of the placebo distribution. Statistical significance at the 10%-level requires that the intersection of both lines be located South-East of the placebo distribution. This is the case for the outcomes displayed in Panels A-C, where the empirical p-values are 0.055, 0.014, and 0.027, respectively. For the outcome in Panel D, namely the total number of non-self-employed claimants, the p-value is 0.128, which means that this estimate is not statistically significant at the 10%-level.

These results confirm the inference drawn from the two-way clustering approach in Table 3. Raises in the top-up rate significantly increase bunching near the kink point, which is the result of an overproportional increase in the number of claimants with an income close to the kink point. As before, we find no statistically significant effect on the total number of non-self-employed EITC claimants.

[15] This procedure follows Kennedy (1995) and Chetty et al. (2009).



(A) Bunching

(B) Self-employed near kink

(C) Total nr of self-employed

(D) Total nr of non-self-employed

Figure 7: Permutation tests

Note: Each panel displays the cumulative density function (cdf) of the empirical distribution of the estimates based on Equation (2) with pair-by-year fixed effects under the null hypothesis of a zero effect. Each distribution is based on 5,000 replications. The empirical p-values indicate the likelihood of obtaining an estimate that is at least as extreme as the one in our main analysis under the null hypothesis of no effect. The smaller the p-value, the less likely a result is to emerge by chance.

5.4 Discussion

Overall, our results support the hypothesis that higher tax refunds create a greater incentive for income manipulation and, therefore, can trigger behavioral responses along several margins. While our data do not allow us to fully distinguish between false declaration of taxable income and labor supply responses at the extensive or intensive margin, our results suggest that both mechanisms are important. Our finding that a raise in the top-up rate increases the extent of bunching at the kink point suggests that there are adverse responses to the state EITC. If the effect was exclusively explained by labor supply responses — especially at the extensive margin — it would be unlikely that we find an effect on bunching. For labor supply responses along the extensive margin, we would rather expect that the numerator and denominator are similarly affected, i.e. the additional number of claimants near the kink point is proportional to the total additional number of claimants. In contrast, the positive effect on bunching suggests that the additional number of claimants at the kink point is much larger relative to the additional number of claimants. While not a proof, these over-proportional changes at the kink point to false declarations of taxable income and potentially to labor supply adjustments at the intensive margin.

Nonetheless, the effects on the total number of self-employed EITC claimants suggests that not all behavioral responses to the state EITC can be classified as adverse. One of the central aims of the EITC is to provide recipients with an incentive to work. The results in Panel Cof Tables 3 and 4 and to some degree also the results for non-self-employed workers in Panel D suggest that these incentives work. A higher top-up rate induces more people to work, and this additional labor supply appears to be spread out along the income distribution rather than concentrated at the kink point.

6 Conclusion

Virtually all public policies trigger behavioral responses by their recipients. In this paper, we document and quantify such behavioral responses for the Earned Income Tax Credit, the largest welfare program in the US. Using data on the extent of bunching at the first kink point of the EITC schedule, and exploiting variation in state-specific tax refunds over time, we find significant behavioral responses along several margins.

First, we document that higher EITC top-up rates increase the number of self-employed people who claim the EITC. This effect can either represent an increase in (self-employed) labor supply, or a change in tax filing behavior. LaLumia (2009), for example, shows that raises in the tax refund increase the likelihood that potential recipients declare their self-employed income.

Second, we show that a raise in the EITC top-up rate leads to an over-proportional increase in the number of self-employed claimants who declare an income close to the income-maximizing first kink point of the EITC schedule. The increase in this number is considerably larger than that of the total number of self-employed EITC claimants, in turn leading to more bunching at the kink point. This result points to a significant behavioral response, namely that tax filers choose to declare their taxable income or their labor supply or both in a way that maximizes their EITC receipt.

These results suggest that the EITC, as any other welfare program, triggers behavioral responses. To policymakers, some of these — for example labor supply at the extensive margin — are desirable, while adverse responses, such as false declaration of taxable income, are not. While our results for the effect on bunching suggest that income manipulation is an important response, we would require more detailed data to fully disentangle labor supply effects from manipulation of taxable income through false declaration. For future work, we are hoping that such data become available.

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A Converting zip-code-level data to county-level data

The dataset by Chetty et al. (2013) provides data at the level of 3-digit zip codes. Because the border pair design requires information at the county-level, we convert the data from the zip-code to the county level. The dataset mainly consists of absolute numbers, such as the number of EITC claimants in a given zip code. If a zip code comprises more than one county, we divide the absolute numbers evenly across all counties within a ZIP code. For example, if there are 1000 claimants in zip-code A and A consists of two counties we assign each county 500 claimants. If, on the other hand, a county is part of more than one zip code, we assign this county the sum of the absolute numbers. If the zip code that cuts through a county also covers another county, we split the absolute numbers between these countries before adding up within counties. For example, if zip codes A (1,000 claimants) and B (500 claimants) are completely contained in county X, we assign county X 1,500 claimants. If, however, zip code A also covers another county while B is fully contained in X, we assign county X 500 claimants from A and 500 claimants from B.

For the 3,141 counties in our dataset, we apply the first method — split the numbers between counties within a zip code — to 1,179 counties. For another 1,960 counties, we apply both methods, namely we split numbers between counties as well as aggregate numbers within counties. The remaining two counties coincide with the zip codes.

B More on identifying variation

Figure 8 illustrates the relationship between state-specific top-up rate (horizontal axis) and the degree of bunching (vertical axis) in a binned scatter with ten equally sized bins on each axis. The graph controls for state-specific characteristics of the EITC, as well as pair-by-year fixed effects. The regression line corresponds to the regression coefficient in Table 3, Panel A), Column (4).

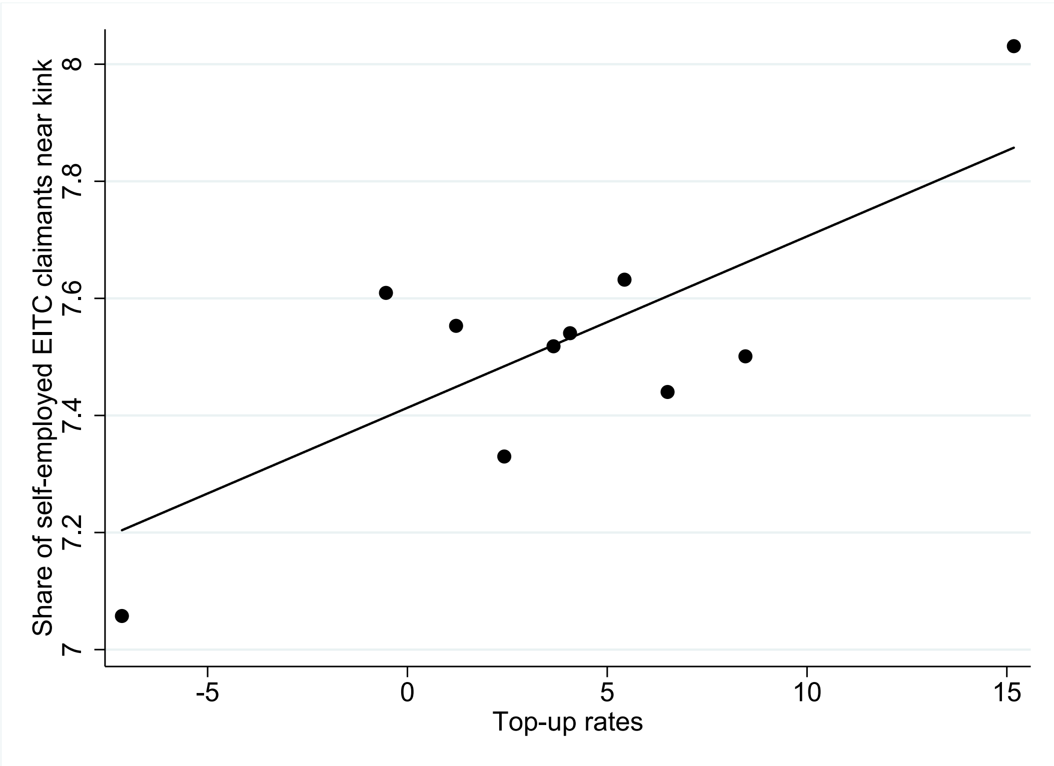

Figure 8: Bunching vs. top-up rates

Notes: This graph displays the relationship between the share of self-employed at the first kink point of the EITC and the state specific top-up rates in a binned scatter, whereby each variable is divided in ten equally sized bins. Both variables have been demeaned by pair-by-year fixed effects, and we control for state-level features of the EITC.