How Does the Earned Income Tax Credit Work? Exploring the Role of Commuting and Personal Transportation

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Although the Earned Income Tax Credit is one of the largest and most extensively studied programs affecting low-income workers in the U.S., relatively little is known about the mechanisms for its labor-supply effects observed in the literature. This study examines a pathway that has yet to be formally studied despite ample circumstantial evidence in its favor: transportation and commuting. Prior work shows that EITC recipients devote a substantial share of their tax refunds to purchasing and maintaining personal vehicles necessary for job search and commuting. I use a simulated instrument approach leveraging metro-level variation in EITC exposure to compare labor-supply responses to policy changes between areas with varying levels of dependence on cars for commuting workers. The main results give strong support to the transportation mechanism: labor-supply effects are roughly 20% smaller in cities with abundant public transportation and 20% larger in highly car-dependent areas. These results find no support in supplemental analyses into the seasonality of estimated EITC coefficients and the effect of policy changes on reports of transportation difficulties. The paper also presents new results using the simulated instrument approach to address two outstanding questions in the literature: the effects of state EITC supplements and the impact of the 2009 EITC expansion.

The Earned Income Tax Credit (EITC) is the largest needs-tested cash assistance program in the U.S. and a pillar of the nation's anti-poverty policy. In 2022, EITC refunds worth $64 billion bolstered the household finances of 31 million families. Literature on the labor-market impacts of the program forms a nearly unanimous consensus that the EITC's refundable tax credits increase employment and labor force participation, particularly among single mothers, fulfilling the policy's basic aims.

Yet there remains uncertainty regarding the pathways linking the EITC to higher labor supply. According to an extensive 2016 review of the literature, "studies on participation are generally silent on the specific mechanism for the observed changes" (Nichols and Rothstein, 2016, 191). In static labor supply models, the EITC provides fully informed eligible individuals an unambiguous incentive to work. With potential benefits for tax year 2023 totaling more than \$7,000 on earnings of as little as \$16,510, these incentives are economically meaningful. In spite of these large dollar amounts, survey-based work finds that potential recipients often lack basic knowledge about EITC eligibility and face substantial uncertainty regarding their eventual tax refunds. Motivated by the disconnect between the informational requirements of canonical labor supply models and the informational frictions faced by low-income workers, some researchers have raised serious doubts about the scholarly consensus around the EITC (Mead, 2014; Kleven, 2023).

This paper explores a particular mechanism for the EITC's observed effects: the purchase and maintenance of vehicles used for commuting. An extensive literature surveyed in Section 1.3 points to transportation spending as a key use of EITC benefits. Although some suggestive evidence supports the EITC-transportation pathway (Barr, Eggleston and Smith, 2022), this is the first study to examine the mechanism in depth. One notable feature of the transportation pathway is that it relies on the liquidity effects of the EITC rather than on the informational channels that have been criticized as empirically problematic and behaviorally implausible.

To motivate how this pathway could produce the employment effects estimated in prior research, I construct an equilibrium search model with heterogeneity by post-tax wage in the rate of job separations. The EITC operates in this environment by providing lower-wage workers the opportunity to make larger investments in the sorts of social and physical capital that bolster employment stability. The model represents a step towards better theorizing the impacts of the EITC in frictional labor markets in which employers enjoy some degree of monopsony power. With this model in mind, I present an identification framework that speaks to the question of information versus liquidity as driving labor supply responses to the EITC, a question often overlooked in the EITC literature.

In the empirical application I build on the methodological approach used in much of the literature surrounding the EITC and labor supply: March Current Population Survey (CPS) data analyzed in a generalized difference-in-differences framework leveraging plausible exogenous policy variation over time and between family sizes. To isolate the hypothesized transportation pathway, I test for heterogeneous impacts of the EITC on respondents in metropolitan areas with differing local commuting characteristics, i.e., high versus low access to public transportation or high versus low dependence on cars for commuting. In doing so, however, it is necessary also to take into account variation between different areas in exposure to changes in EITC policy. Intuitively, larger EITC responses should be observed in states and metropolitan areas with a higher share of workers with incomes low enough to qualify for the EITC. Because high-public-transit areas also tend to exhibit higher average wages, failure to take differential EITC exposure into account will give rise to misleading results when interacting EITC measures with indicators for local commuting characteristics.

To capture underlying variation between local areas in EITC exposure I create a simulated instrument based on data collected before the large federal EITC expansions in the 1990s used to estimate the main effects. The simulated instrument projects metropolitan-area income and family characteristics over time to simulate EITC refunds that would be observed based on exogenous policy variation occurring outside the time frame from which the underlying sample for the instrument is drawn. This creates measures immune to behavioral changes that are endogenous to policy shifts. Although prior work has also constructed simulated instruments for the EITC (Bastian and Jones, 2021; Michelmore and Pilkauskas, 2021), this paper is the first to build local variation into such an instrument.

My main results find that the estimated effects of the EITC are significantly lower in areas with access to abundant public transportation and, by the same token, are significantly higher in areas where residents depend heavily on cars for commuting. These results are robust to a range of specifications and support the transportation pathway of the EITC. Supplemental tests of the hypothesis, however, provide little additional support for the main results. I do not find any patterns in the seasonality of EITC employment effects suggestive of liquidity or transportation playing an obvious role. I also find paradoxical results using a CPS survey question asking directly about transportation issues as a reason for being out of the labor force. While not invalidating the EITC-transport pathway, these supplemental tests point to the need for additional research to examine this mechanism.

In addition to providing a better understanding of the familiar extensive-margin effects of the EITC, this study also presents new evidence on outstanding questions relating to the policy. Turning to the 2009 federal EITC expansion for parents of three or more children, I find modest evidence for positive effects on employment and labor force participation when using the simulated instrument and accounting for regional heterogeneity. I also estimate the effects of state EITCs on the labor supply of single women using a strategy exploiting the intra-state regional variation conveyed in my simulated instrument. Running state-specific regressions for states that introduced large EITC supplements, I find positive coefficient estimates roughly in line with those estimated for the federal EITC, though with substantial variation in magnitude and significance.

This study makes several contributions to the already sizeable EITC literature. In presenting new estimates of the effect of the EITC on labor supply outcomes, it broadens our understanding of one of the keystone policies in the U.S. anti-poverty toolkit. It also lays the groundwork for exploring the liquidity effects of the EITC as distinct from pathways that rely on challenging assumptions about individual knowledge of the tax code and future earnings trajectories. Finally, I present evidence that the EITC's effects on employment depend in part on household access to transportation options. While the importance of personal vehicles for commuting in the U.S. is well-established, it is worth pondering the opportunity costs of EITC refunds spent on unreliable used cars with unfavorable financing terms. The smaller effects of the EITC in areas with ample public transit speak to the value of public commuting infrastructure.

1 Background

1.1 History and program design

The Earned Income Tax Credit was born out of the Great Society expansion of the welfare state in the late 1960s and the political pushback it engendered. Lawmakers concerned about work incentives conceived the policy as a "workfare" or "work bonus" program that would head off the negative income tax proposals then gaining traction (Hotz and Scholz, 2003). The policy Congress eventually introduced in 1975 provided refundable tax credits whose value phased in at a rate of 10% of earned income. The maximum credit was $400 ($709 in 2023 dollars), with eligibility limited to families making less than $8,000 annually ($14,179).

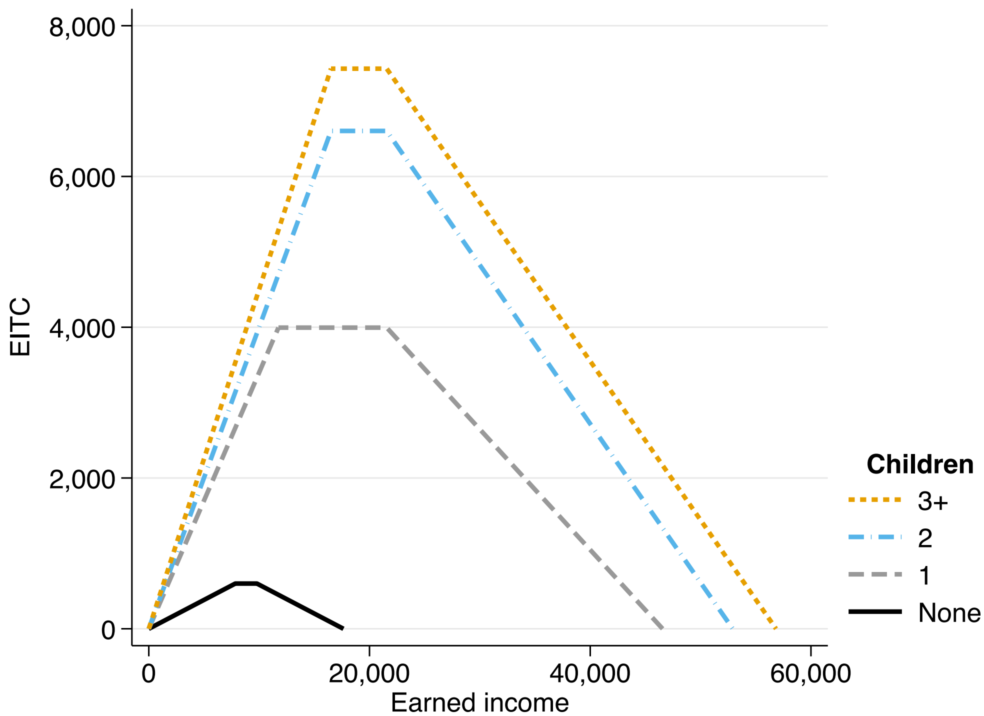
Congress augmented the EITC repeatedly since 1975, with notable expansions in 1986, 1990, 1993 and 2009 turning the program into a central pillar of federal anti-poverty policy. Among other changes, these reforms indexed the EITC to inflation, increased phase-in rates and maximum benefits, extended the upper earnings thresholds, enlarged the refunds available to households with two or more children (1993) and three or more children (2009), and extended a small credit to childless workers (see Figure 2). The EITC grew more rapidly than any other welfare program in U.S. history, with real outlays on the EITC more than tripling in the 1990s (Hotz and Scholz, 2003). In 2022, 31 million workers and families received the EITC, with benefits totalling $64 billion (Internal Revenue Service, 2023). The majority of filers are single adults with children, a group that receives about three-quarters of all EITC benefits (Hoynes and Patel, 2018).

The EITC is structured to incentivize work on the extensive margin while minimizing intensive-margin distortions in its phase-out range. The amount of the credit depends on family structure and is calculated on a sliding scale that rises alongside earnings to its first "kink" where it plateaus, then phases out as household earnings continue to rise. The result is the trapezoid-shaped schedule depicted in Figure 1. Within the plateau region of earnings, recipients are eligible for a constant dollar amount of tax relief. In 2023, a single tax filer with two children is eligible to earn a maximum tax credit of $6,604 if they earn between $16,510 and $21,560. For each dollar of adjusted gross income (AGI) above $16,510, the tax credit is phased out up to $52,918.[1] The maximum credit for a childless adult between the ages of 25 and 65 is $600, phasing out fully at an AGI of $17,640. Married taxpayers filing jointly face EITC schedules that are identical to those of single filers except that the threshold at which the benefit begins phasing out is higher, as is the point at which benefits fully phase out (Tax Policy Center, 2023).

In addition to the federal EITC program, 28 states and the District of Columbia offer supplemental EITCs, 23 of which are refundable. Most states with an EITC add-on simply add a

[1] The phase-in rate and plateau parameters apply to earned income, which comprises wages, tips, and other compensation, as well as net self-employment income. The phase-out rate applies to AGI, which includes investment income and retirement distributions. Workers with investment income above a certain threshold ($11,000 in 2023) are ineligible.

Figure 1: Federal EITC eligibility and benefits schedule, 2023



Source: Tax Policy Center (2023)

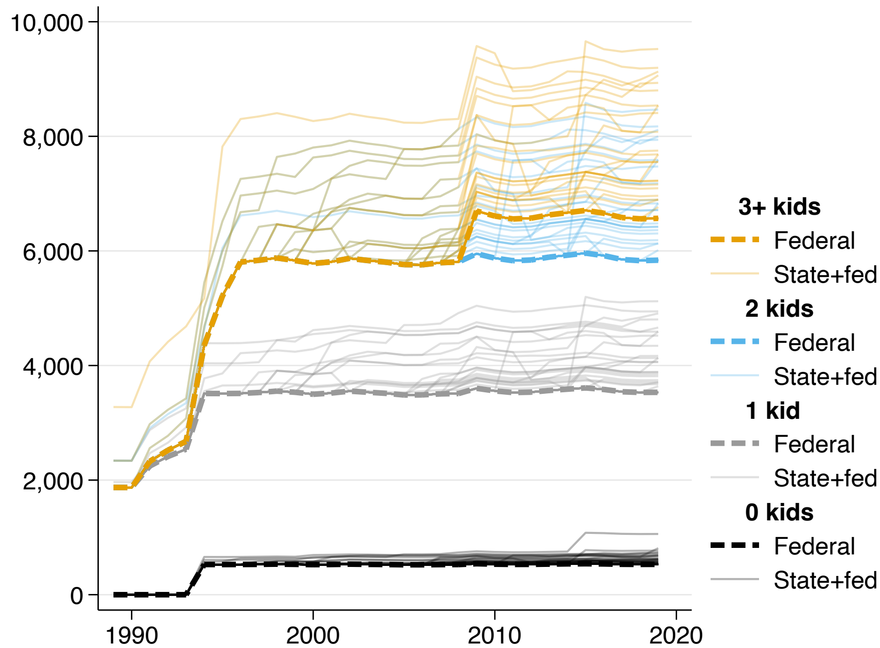
fixed percentage to the federal EITC; taxpayers entitled to a federal credit multiply that amount by the applicable state percentage to determine the state benefit. A few states, like California and Minnesota, have more complicated state EITC formulas. Figure 2 shows total maximum federal and federal-plus-state EITC benefits by number of children, 1989–2019.

Because it is a refundable tax credit, the EITC takes the form of cash payments for those whose income tax liability is low or non-existent. This includes the majority of recipients. The IRS begins disbursing refunds to filers on February 15 and the bulk of EITC payments arrive in February and March (Wilson, 2020). Most EITC recipients file their taxes with for-profit tax preparers such as H&R Block and a thriving market exists for high-interest "refund anticipation loans" (Chetty, Friedman and Saez, 2013; Nichols and Rothstein, 2016).

1.2 Research on employment effects

A wide range of empirical research finds substantial extensive margin effects of the EITC. According to the comprehensive review of Nichols and Rothstein (2016), the literature forms an "overwhelming consensus" that the EITC boosts the employment and labor force participation of single mothers. Much of this evidence derives from the large 1993 expansion, which was

Figure 2: Maximum real federal and state-plus-federal EITC benefits

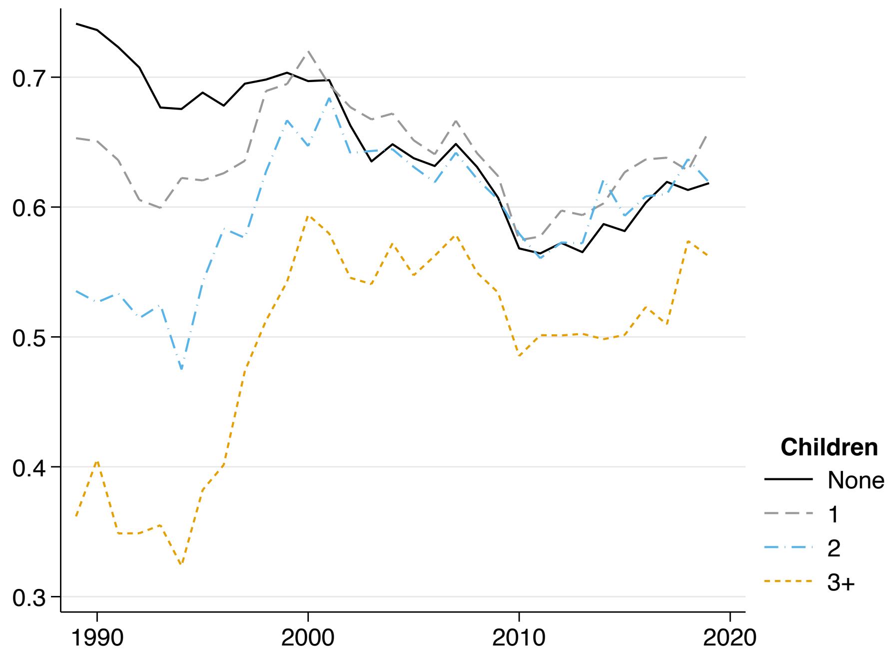


Note: State EITCs include only those state tax credits that are fully refundable. State EITC histories from Komro et al. (2020) and Shapiro (2019). Amounts expressed in 2019 dollars.

followed by a sharp increase in employment among unmarried mothers, as Figure 3 illustrates. Employment rose especially briskly for those with two or more children, for whom the 1993 expansion was largest.

Much of the literature on the EITC and the extensive margin uses March CPS data and some form of difference-in-differences empirical strategy. An early and influential example of this approach is Eissa and Liebman (1996), who found that the 1986 EITC expansion increased labor force participation among single mothers by 2.8 percentage points. Meyer and Rosenbaum (2001) applied a structural labor supply model incorporating changes in welfare program generosity; their estimates attributed about 60% of the increase in single mothers' employment during the 1990s to the EITC expansions of that decade. Hotz, Mullin and Scholz (2006) used administrative panel data from California in a fixed-effects design and concluded that the 1990s EITC expansions boosted both employment and EITC claiming. Hoynes and Patel (2018) used difference-in-differences to estimate the total direct and indirect effects of the EITC on poverty, finding that the 1993 expansion reduced post-tax poverty rates by 7 percentage points and led to a roughly 6 percentage point increase in employment among single mothers. Bastian (2020) explored the 1975 introduction of the EITC and estimated that it increased maternal labor supply by 6%. Using CPS data linked to administrative tax records, Bastian and Jones (2021) found positive employment and earnings effects and correspondingly lower welfare program usage stemming from EITC expansions between 1990 and 2017. Schanzenbach and Strain (2021) provided evidence for extensive margin effects in the 1975, 1986, 1990 and 1993 expansions. Although most studies examining the extensive margin effects of the EITC focus on single mothers (for whom the EITC provides an unambiguous incentive to work), Eissa and Hoynes (2004) analyzed EITC expansions between 1984 and 1996 and concluded that these reduced labor force participation among married women by about 1 percentage point.

Figure 3: Employment rates of low-education single women, 1989–2019



Note: CPS ASEC 1989–2019. Sample limited to unmarried women ages 20-50 with educational attainment of a high school degree or less. Children includes all dependents in the household ages 18 or younger.

A dissenting view of the EITC's effects highlights the informational and psychological frictions that prevent workers from internalizing tax incentives. A wide gulf separates the considerable informational requirements of structural labor supply models (e.g., Eissa, Kleven and Kreiner, 2008) and the evidently low level of EITC awareness among the target population. Estimates of EITC take-up find that 20-25% of eligible workers fail to claim EITC benefits (Nichols and Rothstein, 2016; Internal Revenue Service, 2022). Low information appears to play a major role in failure to claim. Eissa and Liebman (1996, 634) described interviews with potential recipients who displayed "virtually no awareness of the credit," while Liebman's experience as an IRS tax preparation volunteer "revealed that even past recipients were unaware of the credit." Surveys conducted by Bhargava and Manoli (2015) found that just 54% of adults using nonprofit tax filing centers claimed to be aware of the EITC and one-third of those who were likely eligible claimed not to be. A similar survey by Caldwell, Nelson and Waldinger (2023) found that while respondents held accurate refund expectations in the aggregate, one-quarter of filers were "not at all certain" that their refunds would fall within $1,000 of their best guess. Having conducted interviews with 60 welfare officials in Wisconsin in 1995, Mead (2014) attested that not one of these sources pointed to the EITC expansion as a contributor to falling caseloads in the 1990s. These well-documented information deficits mean "the EITC is not an *a priori* likely candidate for finding large effects on labor supply" Kleven (2023, 5).

The alternative explanation for the rise in single mothers' employment in the 1990s points to the confounding effects of a strong economy and deep reforms to welfare. The major expansions of the EITC in the 1990s took place amid a broad shift in anti-poverty policy from direct cash assistance to tax credits and in-kind support. President Bill Clinton famously campaigned on a promise to "end welfare as we know it" and later characterized the 1993 EITC expansion as "a cornerstone of our effort to reform the welfare system and make work pay" (Clinton, 1994). The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA) transformed the welfare state, replacing the Aid to Families with Dependent Children (AFDC) program with Temporary Assistance for Needy Families (TANF) and imposing stricter time limits on the receipt of benefits. In the years leading up to PRWORA, the federal government granted dozens of waivers to states allowing them to tighten rules around AFDC eligibility. The literature studying the impacts of these reforms generally finds positive employment impacts for single mothers, though estimates of the magnitudes vary widely (Ziliak, 2016).

Although virtually all studies on the effects of EITC expansions control for state and federal welfare reforms, there remain concerns about the impact of these policy changes on estimates of EITC effects. Kleven (2023) argued that empirical patterns in this period make more sense in light of welfare reforms than EITC expansions. Employment gains among single women in the 1990s exhibit a "fanning-out" pattern by number of children, with larger employment increases among mothers of three or more children than those of two children. Because the 1993 expansion treated all parents of two or more children equally, Kleven argues this pattern is better explained by welfare reforms. Yet Bastian and Jones (2021) argued that the "fanning-out" pattern is mitigated after taking into account the age of the youngest child and indicators of fertility.[2]

1.3 Mechanisms

Although myriad outcomes have been studied in connection to the EITC,[3] relatively little research has probed the *mechanisms* by which the EITC increases employment. According to Nichols and Rothstein (2016, 191), "it seems plausible given general ignorance about tax

[2] See also Schanzenbach and Strain (2021) for a response to the initial 2019 version of Kleven's working paper.

[3] Examples include academic achievement (Dahl and Lochner, 2012), infant health (Hoynes, Miller and Simon, 2015), maternal health (Evans and Garthwaite, 2014), maternal mental health (Schmidt, Shore-Sheppard and Watson, 2023), adult outcomes of EITC-exposed infants (Barr, Eggleston and Smith, 2022) and attitudes towards female employment (Bastian, 2020).

policy that impacts on net income are realized after the fact and influence subsequent behavior, keeping many single mothers in the labor force who otherwise would have exited." Rather than prompting prospective workers on the sidelines to join the labor force, the refunds may work in large part by reducing exit from the labor force. For households with low potential earnings, federal tax rebates can amount to more than one-third of annual income (and nearly one-half in a few states with large EITC supplements). Spending out of this windfall, particularly for those who are credit-constrained and illiquid, can help maintain the physical and social capital required for working parents to remain working.[4]

Taking up the question of EITC and exit, Wilson (2020) explored the hypothesis that by raising the opportunity cost of dropping out, the EITC helps keep workers facing adverse shocks on the job. Using linked CPS monthly observations, she found a positive association between months worked across a four-month stretch and the EITC eligible to *receive* as a refund in the survey year (rather than the benefits eligible to *earn* this year and receive the next year). Probing monthly data for insights into whether this result stemmed learning about the EITC or increased liquidity among recipients, Wilson concluded that the information channel was better supported by the data since no seasonal bump in employment stability arose around tax time.

A few authors have studied how the increased liquidity provided by EITC recipiency affects employment. Michelmore and Pilkauskas (2021) used a simulated measure of the EITC in a difference-in-differences framework to show that employment increases associated with EITC expansions are driven by mothers with the youngest children. Turning to data from the Survey of Income and Program Participation, they estimated that for a \$1,000 increase in the EITC, mothers of children under three are 23 percentage points more likely to obtain child care, mostly coming from informal arrangements. This is in line with qualitative work from Bellisle (2022) in which working mothers detailed how they sustained informal child care arrangements in part through gifts out of their EITC refunds.

This paper explores the hypothesis that EITC refunds encourage employment by providing the liquidity necessary to repair and purchase automobiles used for commuting and job search. More than three-quarters of U.S. workers drove alone to their jobs in 2019 (U.S. Census Bureau, 2023) and research has shown that car ownership boosts employment by allowing workers to access more job opportunities (Baum, 2009; Bastiaanssen, Johnson and Lucas, 2020).[5] Yet the transportation pathway has been addressed only glancingly in the literature on employment and the EITC. The most relevant evidence comes from the working paper of Barr, Eggleston and Smith (2022), who use birthdays around January 1 as a source of variation in EITC and other

[4]Although EITC recipients face deep uncertainty about future credits, they often treat refunds as a form of forced savings earmarked for particular purposes, especially big-ticket items. This tendency is well explained by a behavioral life cycle savings model with framing and mental accounting features (Romich and Weisner, 2000).

[5]As Goodman-Bacon and McGranahan (2008) note, welfare and income-support programs acknowledge the link between cars and employment. Most state TANF policies exempt the value of one or more vehicles from asset limits used to determine eligibility, as does the federal Supplemental Security Income program.

benefits in a regression discontinuity design.[6] They found that EITC and other cash assistance in the first year of a child's life is associated with higher earnings and educational attainment years later. Notably, they found strong evidence for heterogeneity in these effects across metro areas with higher or lower access to public transportation. Estimated treatment effects were significantly smaller in areas characterized by abundant public transit, a pattern attributed to the fact that a large part of EITC refunds go towards automobiles that keep parents better attached to the labor market.

While research exploring the transportation mechanism of EITC's extensive margin effects is lacking, a wide range of evidence both qualitative and quantitative emphasizes the importance of automobile spending out of EITC benefits. Sociological work on the EITC is particularly illuminating. Drawing on 115 in-depth interviews with EITC recipients, Sykes et al. (2015) noted the frequency of unprompted responses linking EITC refunds to upward mobility through the purchase and maintenance of assets, particularly cars. For poor households living paycheck to paycheck in their sample, "building assets—investing in durable goods and building up savings—is usually possible only at tax time" (Sykes et al., 2015, p. 3). Similarly, the book-length study of Halpern-Meekin et al. (2015) found that many of their interviewees linked refunds to upward mobility and "getting ahead," with vehicle purchase and maintenance constituting a major use of EITC checks. Two of the working mothers they interviewed secured refund anticipation loans from H&R Block in order to take advantage of used-car sales. Another informant had for three years running used her refund to buy a used car that had to be replaced the following year at tax time. Notably, almost none of the subjects interviewed in Halpern-Meekin et al. described altering their employment decisions or hours worked in order to maximize tax refunds, a fact attributable to both the complexity of the tax code and the unpredictability of employment (Halpern-Meekin et al., 2015, 85).

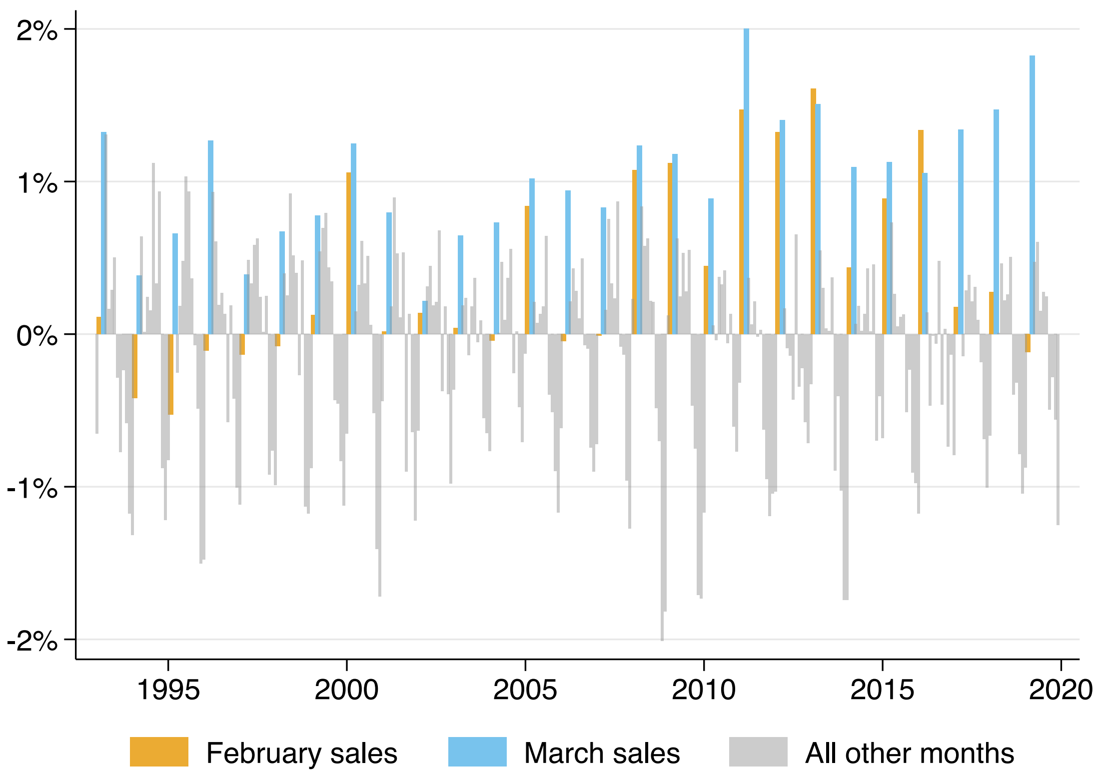
Surveys of low-income tax filers often find transportation-related expenses to be a large part of planned or realized EITC usage. Romich and Weisner (2000) found that over one-quarter of respondents planned to use their credit on a vehicle, while the corresponding figure for Smeeding, Phillips and O'Connor (2000) was 22%. Seven percent of those surveyed by Linnenbrink et al. (2008) planned to make vehicle purchase the *main* use of their refunds. In a survey of rural low-income workers by Mammen and Lawrence (2006), 35% of those who collected refunds reported spending on transportation-related items, making it the second-highest category of EITC use after paying bills and loans (some of which may be auto loans). The eventual spending that recipients devote towards automobiles likely exceeds planned spending. Mendenhall et al. (2012) found that while just 12% of sampled households planned to spend money on car-related outlays, nearly three times that share ended up doing so. This highlights the unpredictability

[6] A low-earning working family with a child born December 31 can claim EITC benefits during tax season two months later, while those with a child born January 1 have to wait more than a year to receive these benefits. This sharp discontinuity provides a source of exogenous variation which can be used to estimate the effect of cash assistance on various outcomes.

of automobile breakdowns. Among the EITC recipients surveyed in Despard et al. (2015), 42% faced major car repairs within six months of tax filing.

Data on consumption expenditures further underscores EITC-related spending on vehicles. Exploiting seasonal variation in refund timing, Barrow and McGranahan (2000) found increased spending on durables around tax refund season and Goodman-Bacon and McGranahan (2008) estimated that the EITC was responsible for 35% more monthly spending on vehicles in February for EITC recipients relative to non-recipients. Fisher and Rehkopf (2022) reached similar conclusions. Adams, Einav and Levin (2009) used data from a large used-car dealer and found that sales were concentrated in tax-rebate season, driven by subprime borrowers who face down-payment constraints. Auto-industry analysts point to tax refunds as a major driver of the used-car market (DuPlessis, 2022; Grieve, 2023) and some dealerships even offer tax preparation services for customers hoping to make down payments (Tompor, 2019).

Figure 4: Retail sales at used car retailers, monthly share



Note: Bars depict each month's share of real annual used-car sales. For visual clarity, 1/12 is subtracted from each observation so that bars represent deviations from the average monthly share of sales. Data: U.S. Census Bureau Monthly Retail Trade Survey 1993-2019. Retrieved from FRED, Federal Reserve Bank of St. Louis.

Suggestive evidence of the EITC's effects on used car sales is apparent in national aggregates. Figure 4 depicts monthly used-car sales as a share of annual sales (1/12 is subtracted from each observation so that bars represent deviations from the average monthly share). March used-car sales have been above average in every year since 1993 and March was the highest-sales month in 60% of the years over this period. Although February was often a month of disproportionately high car sales, it ceased to be an outlier beginning in 2017. That year marked the beginning of an anti-fraud initiative at the IRS that pushed back the date of earliest refund receipt to at least February 15, two weeks later than refunds typically began going out. It is possible (though highly speculative) that the drop in February's share of used care sales in 2017 and thereafter reflects this change in IRS policy.

A final source of evidence on the mechanisms of the EITC comes from employers. Although research into employers' views of the EITC is scarce to nonexistent, it is the job-stability pathway that employer-facing organizations have highlighted when discussing the EITC's effects on employment. National Enrollment Services, a company that helps businesses sign their employees up for government benefits including the EITC, advertises several advantages of the EITC to employers, including "lowering employee turnover," and "incentive to work which stabilizes employee retention while growing a business's bottom line" (National Enrollment Services, 2023). In a similar vein, a 2007 report from the Institute for a Competitive Workforce, an affiliate of the U.S. Chamber of Commerce, described how "the credit helps workers to keep working and care for themselves at no cost to the business itself" (Institute for a Competitive Workforce, 2007). The report listed four expenses the EITC helps cover that in turn reduce turnover: transportation costs, keeping a car in working order, education and training, and child care.

2 The EITC in frictional labor markets

Researchers have typically theorized the effects of the EITC with reference to static labor supply models in which the choice to work depends on the costs of working net of foregone welfare benefits weighed against the returns to employment net of taxes and tax credits (Meyer and Rosenbaum, 2001; Eissa, Kleven and Kreiner, 2008). Alternatively, studies into the tax incidence of the EITC (i.e., to what extent employers capture EITC benefits through lower wages) have specified market-level partial-equilibrium models in which the EITC moves the labor supply curve outward (Leigh, 2010; Rothstein, 2010). Both types of models assume that workers are fully informed about EITC program specifics and that they face no barriers to securing or maintaining employment once the participation decision is made.

An alternate view of the low-income labor market where the EITC operates takes into account the frictions and constraints that complicate the standard labor supply model. In this view, workers experience adverse job-ending shocks and search takes time. Factors such as worker mobility, liquidity, household assets and social capital play a role in workers' attachment to the labor market. Rather than operating purely through expectations and incentives, the EITC can affect employment indirectly by providing liquidity to credit-constrained households vulnerable to shocks such as loss of child care, vehicle breakdown, injury, or illness. As a result, EITC recipients experience fewer job separations and low-wage businesses enjoy lower turnover.

I formalize the idea of the EITC acting to reduce worker exit using an equilibrium search model in the style of Burdett and Mortensen (1998, henceforth BM). In the model, as in BM, homogeneous workers search for new job opportunities on the job and while unemployed. Workers face the risk of job destruction. Firms homogeneous in their productivities seek to maximize flow profits by posting wages paid uniformly to all workers. Equilibrium is determined as a game in which each employer sets its wage taking the offers of all other employers as given.

I generalize BM by specifying the rate of job destruction as a function $\delta(w)$ of the post-tax wage w with $\delta'(w) < 0$ and $\delta''(w) > 0$. This contrasts with BM and most other search models, which specify a constant rate of job destruction δ either overall or within each of a finite number of worker types. The form of $\delta(w)$ captures the idea that higher post-tax incomes allow workers to invest in social and physical capital—especially care-giving arrangements and automobiles—that reduce the incidence of job-ending shocks. As a result, the separation rate is inversely related to the post-tax wage. The enactment or expansion of an EITC serves to increase the post-tax wage and reduce the separation rate for workers in the EITC-eligible earnings range.[7]

Appendix B develops the full model and its solution, which must be worked out using numerical methods. As in BM, the model gives rise to a continuous distribution of wage offers and an equilibrium employment rate. The expected wage is marked down from productivity by an amount that depends on the rate of job destruction and the abundance of job offers, among other factors. An expansion of the EITC leads to an unambiguously higher equilibrium employment rate though an ambiguous change in the expected wage. This ambiguity stems from the fact that while a reduced separation rate at the low end of the wage distribution allows lower-wage employers to maintain a larger workforce than they could otherwise, workers can also climb the wage ladder with fewer interruptions. In this way, the model does not reduce to a shift of the labor supply curve in a standard neoclassical partial-equilibrium model. Two other important differences are worth emphasizing. First, the model requires no awareness of the EITC on the part of workers, nor do changing work incentives play any role in the outcomes (although these factors could easily be accommodated by the model, as detailed in Appendix B). Second, the search model specifies the mechanism through which the EITC raises employment: reduced job destruction at the lower end of the wage distribution.

[7]For tractability, the model assumes that all workers work identical hours. This assumption could be relaxed following Shephard (2017).

3 Methodology

3.1 Identification

In the empirical literature on the employment effects of the EITC, the often implicit assumption that the credit works primarily through the information channel simplifies the causal pathways under consideration. It is generally taken for granted that the EITC schedule of the current tax year—the EITC eligible to *earn* in the survey year—is the primary explanatory variable.[8] Yet taking the liquidity channel seriously introduces additional modeling questions. If households are affected by the receipt of refunds and not (only) the expectation of receiving them, then it will be necessary to include an EITC variable representing the tax year prior to the current year, or the EITC eligible to *receive*.

To provide a rough and necessarily incomplete illustration of the identification challenge, Figure 5 depicts a causal graphical model summarizing the hypothetical causal pathways under consideration. For visual clarity, the diagram abstracts from demographic factors and unobserved variables, which would almost certainly influence each of the nodes depicted. Dashed arrows represent causal pathways carried by information or expectations. The outcome of interest is Emp_t, or current-year employment. It is affected via informational pathways by both the EITC eligible to earn in the current year, $EITC_t$, and the refund eligible to receive which was earned in the prior year, $EITC_{t-1}$. Knowledge of the current-year EITC schedule provides an inducement to work, while receipt of prior-year EITC increases EITC awareness. The causal path $EITC_{t-1} \rightarrow EITC_t$ captures the correlation between policies of neighboring years.

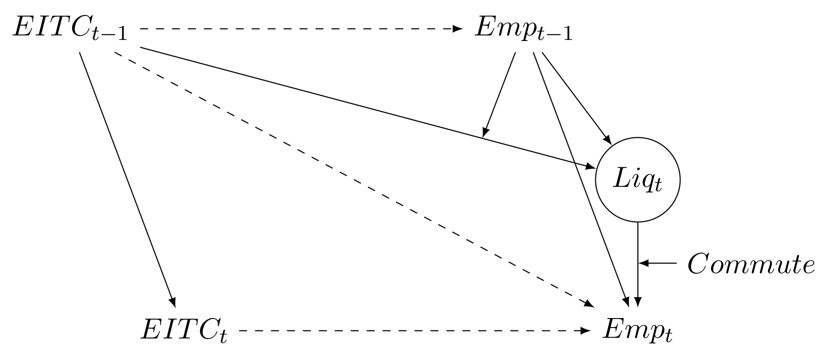
The causal picture is complicated by both prior-year employment Emp_{t-1} and household liquidity Liq_t (the latter of which is placed in a circle to denote that it is unobserved). Prior-year employment depends on the prior-year tax schedule $EITC_{t-1}$ through expectations. It affects current-period employment through persistence on the job, contributes directly to liquidity, and moderates the effect of $EITC_{t-1}$ on liquidity since the EITC refund eligible to receive this year depends on earnings in the prior year. Liquidity affects Emp_t through the child care and transport pathways discussed above.[9]

Reduced-form empirical work on the EITC typically fails to distinguish between the information and liquidity pathways or current- vs prior-year EITC schedules (a notable exception being Wilson, 2020). Estimates of the treatment effect $EITC_t \rightarrow Emp_t$ are thus biased by the omission of $EITC_{t-1}$. In practice this may not matter all that much, since the effects are same-signed and because including both $EITC_t$ and $EITC_{t-1}$ in the regression equation introduces significant multicollinearity.

[8] In any case, authors typically accept that it takes households some time to learn about changes to the EITC, which explains why the effect size of a change in EITC generosity grows over the course of several years in event-study designs (Bastian and Jones, 2021).

[9] Income effects might also play a role, at least in the short run. LaLumia (2013) found that unemployment spells among likely-EITC-eligible adults last longer when they begin in February (just before refund receipt) than in other months.

Figure 5: Causal graphical model of pathways between EITC and employment



Because household liquidity is unobserved in the available data, it is impossible to fully distinguish between liquidity and information effects of the EITC. Yet it is possible to test for the presence of liquidity effects by observing that the effect of EITC receipt on transportation (and thus employment) is moderated by the availability of public transportation for prospective commuters.[10] The variable *Commute* in the diagram is a moderator between Liq_t and Emp_t capturing the commuting characteristics of metropolitan area or region of residence of the respondent. In practice this means using an interaction to estimate the effects of $EITC_{t-1}$ simultaneously for high- and low-public transit areas (or for areas of high or low car-dependence). The estimated coefficient on $EITC_{t-1}$ will still represent a mixture of liquidity and information channels, but the interactions will provide insights into the contribution of liquidity alone.

The empirical methodology taken up in this paper follows the widely adopted strategy of using exogenous changes to federal EITC policy to identify its effects on the target population. This approach relies on two sources of variation to identify treatment effects: variation over time and variation between families in EITC generosity. I build on the prior literature by using two additional sources of variation: local exposure to federal EITC policy changes and differences in commuting characteristics. Underlying differences in wage levels between cities and regions means that for a given change to the EITC schedule, different shares of the working population will experience changes to their post-tax income. There will also be different share of the population in the plateau region of the EITC versus the phase-out region. The result is substantial variation in exposure to changes in the EITC. IRS statistics show that in Mississippi, the state with the lowest median income, 12% of the population claimed the EITC and the average refund was $2,962. In high-income New Hampshire, 5.1% of the population received refunds averaging $1,966 (Internal Revenue Service, 2023).

[10] An especially apt illustration of the interactions between the EITC, public transportation, and automobile use comes from a March 1995 news article on the effects of a strike that shut down SEPTA, Philadelphia's public transit system. "John Cifaldi, owner of Vista Motors at 7418 Frankford Ave., said he sold two inexpensive cars yesterday morning to SEPTA riders. Cifaldi, who sells cars in the $5,000-to-$6,000 range, said the strike 'couldn't have happened at a better time' from a used-car dealer's standpoint because tax refunds will provide some customers with money for a down payment" (Stets, 1995).

Incorporating local variation in underlying EITC exposure is necessitated by the inclusion of interactions of the EITC variable with indicators for high public transit or high car dependence at the metropolitan or regional level (construction of these indicators is detailed below). Without taking local EITC exposure into account, the interaction of the EITC variable with a high-public-transit indicator will be biased by the fact that high-public-transit metro areas such as New York City and San Francisco also have higher housing costs and, consequently, higher wages for lower-educated workers.[11] Failure to control for local variation leaves the EITC-commuting interaction liable to pick up not only the intended effect but also the degree of local exposure to EITC expansions. This kind of local variation motivates Fitzpatrick and Thompson (2010), who hypothesized that workers in higher-cost, higher-wage areas benefited less from nationally uniform changes in federal EITC generosity. They estimated heterogeneous impacts of the 1993 EITC expansion by cost-of-living categories measured with housing costs, finding higher labor-supply responses in the lowest-cost areas.

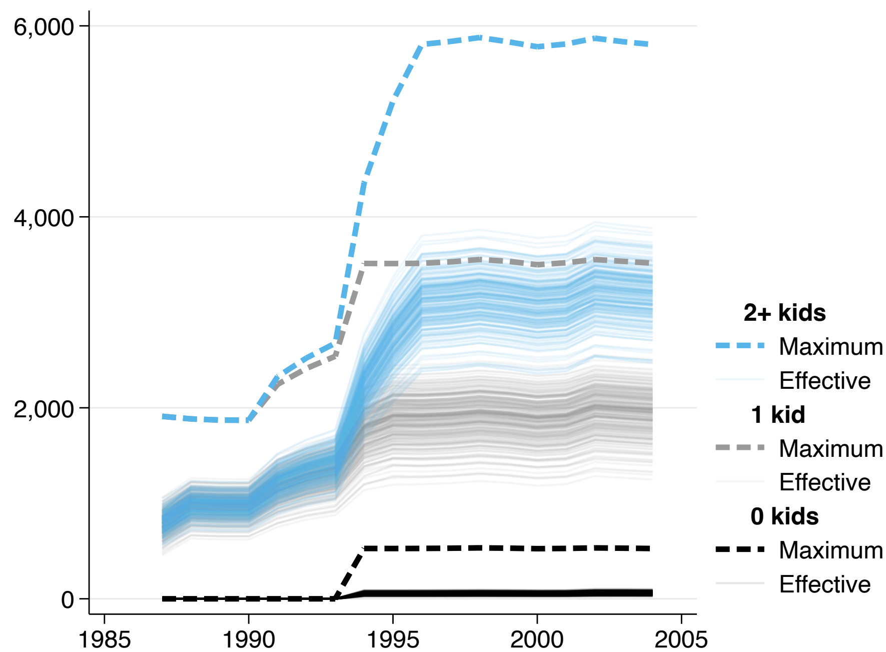
In order to capture local variation in EITC exposure, I use a simulated instrument approach similar to those employed in Michelmore and Pilkauskas (2021) and Bastian and Jones (2021). The idea is to build an instrument which uses a sample of household data to simulate policy-related measures that are not affected by decision-making endogenous to the policy changes (Currie and Gruber, 1996). To do so, I first draw the full sample of unmarried female respondents with a high school education or less from the 5% sample of Census data via IPUMS (Ruggles et al., 2023). I then copy this set of households to each year from 1989–2019 and inflate future earnings using CPI. This simulated sample represents a counterfactual population whose employment, earnings, and fertility decisions are unaffected by changes in the EITC after 1989 (the 1990 survey asks about labor market outcomes of the previous year). I then use the National Bureau of Economic Research (NBER) Taxsim program to calculate state and federal taxes for each household across all years. I collapse the simulated tax data (including federal and state EITC refunds) to MSA $\times$ year $\times$ age group $\times$ family-size cells where family size is defined by the number of children (0, 1, 2 or 3+) and age groups are 20–34 and 35–50. Finally, these simulated EITC measures are inflation-adjusted to 2019 dollars to produce the $SimEITC$ variable. The result can be thought of as something like an "effective" EITC, in that it conveys a notion of the expected EITC benefit that workers in various MSA-demographic cells would receive.

Exploiting local, within-state heterogeneity in EITC exposure requires the construction of a set of metropolitan-area identifiers that can be linked from the Census 5% sample to CPS and across varying MSA definitions from 1989 through 2019. For those living outside defined metro areas or whose metro areas do not link to the full sample of CPS, I create residual non-

[11] This observation is typical in the urban economics literature. In the model of Black, Kolesnikova and Taylor (2009), for instance, workers differ by their education and willingness to pay for location-specific amenities. When the value of amenities is capitalized into housing prices, lower-educated workers must receive higher wages to induce them to live and work in a high-amenity, high-cost city.

metro areas defined at the state level. For example, respondents residing in Autauga County, Alabama belong to the Montgomery MSA. Residents of Baldwin County, Alabama are assigned to non-metro Alabama area since the statistical area to which they belong does not correspond to a MSA recorded in CPS data for the entire sample period. For simplicity, I will refer to both these residual regions and officially defined metro areas collectively as MSAs. MSAs spanning multiple states are defined at the MSA-state level where available. The final data set contains 226 metropolitan areas and residual non-metro state areas.[12]

Figure 6: Maximum and simulated real EITC measures, 1989–2004



Note: Graph shows both the maximum federal EITC eligible to earn by family size (heavy dashed lines) and simulated EITC measures that incorporate local variation in projected earnings (thin solid lines). Both expressed in 2019 dollars. For clarity, the two age groups (20–34 and 35–50) have been averaged.

The resulting data set captures changes over time to EITC policy variables for different family types as well as underlying levels of exposure to the EITC due to pre-expansion local variation in incomes. Figure 6 illustrates the high degree of variation between MSAs. The simulated EITC in 2019 dollars for mothers of two in 1996 (averaged over the two age groups) ranges from \$2,381 in the Maryland suburbs of Washington, D.C., to \$3,803 in Ocala, Florida. The figure also demonstrates that the maximum available EITC refund (heavy dashed lines) significantly

[12]For smaller MSAs, some MSA-demographic cells have low sample size in the Census 5% sample (for instance, mothers aged 35-50 with three or more children in Gainesville, Florida). In these cases I draw same-state donor individuals either from the non-metro part of that state or from same-state MSAs no larger than twice the recipient MSA's population. I draw enough donors to ensure a sample size of at least 50 for calculation of each MSA-demographic cell's simulated instrument. Only 0.3% of the eventual sample used to create the simulated instrument consists of observations copied from one MSA to another.

overstates the average EITC benefits that members of a family type are likely to receive based on projected earnings, especially for childless single women. While past EITC studies have utilized the simulated instrument strategy (Michelmore and Pilkauskas, 2021; Bastian and Jones, 2021) and other work on the EITC has exploited state- or metro-level variation (Fitzpatrick and Thompson, 2010; Neumark and Williams, 2020; Aladangady et al., 2022), this paper is the first to combine these approaches.

Equation 1 describes the econometric model for the OLS regressions used to estimate the effects of the EITC on labor market outcomes Y_{ijst} for individual i in MSA j, state s and time t. The coefficients of interest β_1 capture the interaction of $SimEITC_{g(i,s)t}$—which is calculated for each year t as a function $g(i,s)$ of individual family and demographic characteristics and state—with $Commute_j$, which is a dichotomous indicator capturing the commuting characteristics of the respondent's place of residence (e.g., whether it has abundant public transportation). X_{ist} contains demographic and state controls including a cubic in age, race (white non-Hispanic, Black non-Hispanic, Hispanic or other), and indicators for the number of children 18 or younger in the household as well as the presence of children under five and under one year old. State controls include GDP growth, unemployment rate, state minimum wage, state average tax rate for higher-income families, and indicators for six state welfare waivers. All specifications also include state-MSA and year fixed effects γ_{js} and γ_t.

$$Y_{ijst} = \beta_0 + \beta_1 SimEITC_{g(i,j),t} \times Commute_j + \beta_2 X_{ist} + \gamma_{js} + \gamma_t + \varepsilon_{ijst} \tag{1}$$

Identification rests on three main assumptions. First, since the study uses repeated cross-sections, the composition of the sample must not change along any unobservable features that also correlate with the outcomes. Second, there cannot be divergent trends in employment between groups with different exposures to the EITC, for example, single mothers of one child versus single mothers of two or more children in the 1990s. Parallel trends tests conducted in prior studies help to alleviate this concern for the 1990s and 2009 expansions (Meyer and Rosenbaum, 2001; Hoynes, Miller and Simon, 2015; Bastian and Jones, 2021). Finally, this strategy requires that the local variation in pre-1989 earnings used to define the simulated instrument is exogenous to subsequent employment trends by family type. This might not be the case, for instance, if income convergence between low- and high-income regions brought about disproportionately higher labor supply in the initially low-income areas.

3.2 Data

I use data from the Annual Social and Economic Supplement of the Current Population Survey (ASEC) via IPUMS (Flood et al., 2022). Also known as the March CPS, the survey collects data on more than 75,000 households every March and has long been the principal source of empirical estimates relating to the EITC due to its substantial demographic detail and national coverage. The primary sample is unmarried women ages 20 to 50 with a high school education or less, 1989–2004. ASEC person weights are used in all regressions and summary statistics. In some supplemental analyses I use monthly CPS data as well. A drawback of the monthly CPS is that metropolitan identifiers become available only in 1994.

The ASEC data captures age and relationships for all members of a household, allowing me to link parents and children (some may be foster or stepchildren). Because ages are given only in year increments, I treat children younger than one as being born in the calendar year prior to the March survey date. I also construct indicators for whether the respondent has any children younger than five or born within the last year.

Outcomes of interest are weekly labor force participation, weekly employment, annual employment, and annual weeks worked. The latter variables are retrospective: respondents are asked about their work experiences in the prior calendar year. When annual variables serve as outcomes, I adjust other variables that depend on the calendar year to reflect their values for the prior year. This includes the age band used to define the sample, which is changed to those ages 21–51 at survey time when using the annual employment outcome. Since I explore the use of both current-year and prior-year EITC measures as explanatory variables, it is necessary to adjust the number of children used for EITC eligibility. For the current-year outcomes, both the current-year EITC schedule and the prior-year (lagged) EITC schedule are based on the number of children born by the survey date, since I treat children younger than one year as being born by December 31 of the prior year. But when the outcome is annual employment and the explanatory variable is a lagged EITC, I adjust the number of children used in calculating EITC benefits to exclude children younger than 1 at the survey date, since these children could not have been born early enough to factor into tax filing for EITC received in the prior year.

The main regressions include a number of state-level controls, including state-level unemployment rates, real GDP growth, minimum wages, income tax rates, and six indicators for the presence of federal welfare waivers. State minimum wages are from the Vaghul and Zipperer (2016) data set. The state income tax measure is the average of state tax rates faced by married couples with zero or two children at twice the median national income; this is calculated using NBER Taxsim. State welfare waivers are drawn from Table B in Department of Health and Human Services (1999). When the maximum available EITC is used as the explanatory variable in robustness exercises, federal EITCs are drawn from Tax Policy Center (2023) and state EITCs are from Komro et al. (2020) cross-checked with Shapiro (2019).

To test for differential responses to EITC increases by local commuting characteristics, I create indicators capturing the frequency of commuting by public transit and automobile. I construct these variables using the 1990 5% Census sample (Ruggles et al., 2023), which asks working respondents how they commute to work. I restrict the sample to those with a high school education or less and group respondents into the MSAs defined above. In the reported

Table 1: Summary statistics

	All		0 kids		1 kid		2+ kids	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Age	33.04	9.06	33.27	10.10	33.20	8.82	32.47	6.79
Black non-Hispanic	0.26	0.44	0.21	0.41	0.28	0.45	0.37	0.48
Hispanic	0.17	0.38	0.15	0.36	0.17	0.37	0.21	0.41
Child younger than 1	0.04	0.20	0.00	0.00	0.06	0.24	0.11	0.31
Child younger than 5	0.20	0.40	0.00	0.00	0.33	0.47	0.46	0.50
Prior-year earnings, 000s	10.93	14.15	12.11	14.31	11.27	14.29	8.34	13.36
Maximum EITC, 000s	1.97	2.01	0.32	0.26	3.00	0.70	4.35	1.71
Simulated EITC, 000s	0.83	0.98	0.02	0.02	1.35	0.37	1.99	0.82
In metropolitan area	0.81	0.39	0.82	0.39	0.80	0.40	0.80	0.40
In high-public-transit locale	0.31	0.46	0.32	0.47	0.29	0.45	0.30	0.46
In high-automobile locale	0.24	0.43	0.23	0.42	0.25	0.43	0.25	0.43
Employed last week	0.64	0.48	0.69	0.46	0.65	0.48	0.53	0.50
In labor force last week	0.72	0.45	0.76	0.43	0.73	0.44	0.63	0.48
Employed at all last year	0.73	0.44	0.77	0.42	0.76	0.43	0.65	0.48
Observations	108,913		54,318		24,882		29,713	

Note: CPS ASEC. Sample consists of unmarried women ages 20-50 with educational attainment of high school or less, 1989–2004. Earnings includes wages and salaries and includes zeroes for those who did not work in the prior year. Simulated EITC uses the simulated effective EITC by state-year-family size, as described in Section 3. All dollar amounts are 2019 dollars. *High-public-transit* and *high-automobile* indicators reflect whether the respondent's place of residence is in the top quartile of MSAs by commute type.

models, the variable *high public* indicates whether the place of residence is in the top quartile of MSAs by public transportation commuting. Variables *high auto* and *low auto* similarly identify areas in the top or bottom quartiles of commuting by automobile, respectively. I do not define a corresponding *low public* variable because it is not very informative. This is apparent in Appendix Figure A1, which shows the distribution of commuting shares by public transit and automobile among lower-educated workers in the 1990 5% Census sample, weighted by population.

4 Results

4.1 Main results

As a preamble to the main specifications, Table 2 reports the results of regressions using four separate versions of the explanatory variable: either maximum EITC or simulated EITC, each in either its current-year or lagged form (these regressions use the full controls described in Table 3). As noted above, liquidity effects of the EITC should show up with a lag, since refunds sent out in calendar year t follow the EITC schedule for tax year $t-1$. For both the maximum EITC and the simulated EITC, and for both outcomes listed, lagged versions of the explanatory variable are more precisely estimated and produce higher model likelihoods. The same can be said of results for the simulated EITC relative to the maximum EITC. For both outcomes, weekly labor force participation and weekly employment, the best-fit models are those using the simulated EITC at a one-year lag. Similar results hold for the annual outcomes, employment and weeks worked, reported in Appendix Table A1.

Table 2: Effect of the EITC on labor supply outcomes for different EITC specifications

	Weekly labor force participation				**Weekly employment**			
	Max	Max, lag	Sim	Sim, lag	Max	Max, lag	Sim	Sim, lag
EITC	0.0395^{***}	0.0453^{***}	0.0639^{***}	0.0718^{***}	0.0359^{***}	0.0412^{***}	0.0572^{***}	0.0646^{***}
	(9.66)	(11.01)	(10.72)	(11.95)	(8.54)	(10.43)	(9.47)	(11.10)
R-squared	0.0731	0.0738	0.0735	0.0741	0.0847	0.0852	0.0849	0.0854
Log-likelihood	-63,688	-63,650	-63,668	-63,632	-69,747	-69,718	-69,734	-69,708
Observations	108,972	108,972	108,972	108,972	108,972	108,972	108,972	108,972

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: CPS ASEC, 1989–2004. Sample consists of unmarried women ages 20-50 with educational attainment of high school or less. Table shows coefficient estimates on different versions of the treatment variable for outcomes listed above. Columns labeled *Max* use the maximum federal EITC benefit by year and family size. Those labeled *Sim* use the *SimEITC* as described in Section 3. Columns 2 and 4 use lagged values of the respective EITC variables. EITC is measured in 1,000s of 2019 dollars. All models use full controls described in Table 3.

Although the results presented in Table 2 build confidence in the choice of lagged *SimEITC* as the explanatory variable, they should not be construed as a formal test of the EITC's causal pathways. Since event-study designs have found that EITC effects grow over time, we should expect that a lagged EITC variable would perform well. It should also not be surprising that coefficients on the simulated EITC are more precisely estimated than the maximum EITC given the additional variation present in the simulated variable. That said, it is reassuring that the additional variation in the simulated EITC leads to a better fit, which would not be the case if adjusting the EITC variable for underlying exposure to policy changes merely added noise. The fact that the coefficient estimates on the simulated versions of the EITC variable are roughly 60% larger than the maximum EITC estimates should also not be over-interpreted; the average simulated EITC value over the whole sample is less than half that of the maximum EITC.

Table 3 reports results of regressions without EITC-by-commuting interactions building up the the preferred specification, which uses the set of "full controls" listed in column 5. Across specifications and outcomes the coefficients on *SimEITC* are positive and statistically significant. In the full-control specification, a $1,000 increase in the effective federal EITC raises labor force participation by 7.2 percentage points and employment by 6.5 percentage points.

The estimates survive a large number of controls, including regressors intended to capture the effects of welfare reform. The regressions with full controls (column 5) include family and demographic factors (a cubic in age, four race categories, number of children fixed effects and indicators for having a child under five a child under one); state controls (minimum wage, real GDP growth, state tax rates on higher incomes); state linear trends; and reform-kids fixed effects, which interact number of children with presence of each of six welfare reform waivers.

Table 3: The effect of the EITC on labor supply outcomes: Various controls

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Weekly labor force participation						
SimEITC	0.0871***	0.0823***	0.0825***	0.0838***	0.0718***	0.0808***
	(0.00597)	(0.00571)	(0.00571)	(0.00583)	(0.00601)	(0.00591)
R-squared	0.0631	0.0718	0.0723	0.0735	0.0741	0.0740
Observations	108,972	108,972	108,972	108,972	108,972	108,972
Panel B: Weekly employment						
SimEITC	0.0827***	0.0785***	0.0783***	0.0794***	0.0646***	0.0776***
	(0.00556)	(0.00528)	(0.00523)	(0.00533)	(0.00582)	(0.00614)
R-squared	0.0687	0.0831	0.0838	0.0846	0.0854	0.0850
Observations	108,972	108,972	108,972	108,972	108,972	108,972
Panel C: Annual employment						
SimEITC	0.0914***	0.0861***	0.0861***	0.0873***	0.0767***	0.0818***
	(0.00624)	(0.00585)	(0.00585)	(0.00603)	(0.00560)	(0.00598)
R-squared	0.0619	0.0740	0.0746	0.0758	0.0765	0.0766
Observations	105,138	105,138	105,138	105,138	105,138	105,138
Panel D: Annual weeks worked						
SimEITC	4.219***	4.075***	4.064***	4.112***	3.587***	4.001***
	(0.268)	(0.253)	(0.252)	(0.255)	(0.280)	(0.283)
R-squared	0.0813	0.0971	0.0976	0.0986	0.0993	0.0991
Observations	105,138	105,138	105,138	105,138	105,138	105,138
Demographics		✓	✓	✓	✓	✓
State controls			✓	✓	✓	✓
State trends				✓	✓	✓
Reform-kids FE					✓	
Kid-state controls						✓

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: CPS ASEC, 1989–2004. Sample is unmarried women ages 20-50 with at most a high school diploma. *SimEITC* is lagged one year and measured in 1,000s of 2019 dollars (see Section 3). Demographic controls are a cubic in age, race, fixed effects for number of children and indicators for presence of children less than five and less than one. State controls are GDP growth, unemployment rate, state minimum wage, state average tax rate for higher-income families, and indicators for six state welfare waivers. Reform-kids fixed effects interact number of children 18 or younger with welfare reform waivers. Kid-state controls interact number of children with state controls listed above. All models include state-MSA and year fixed effects. Controls included in column 5 are "full controls."

The EITC coefficient on each outcome shrinks when the reform-kids controls are introduced, consistent with the idea that the welfare reforms of the 1990s confound estimates of the EITC's effects on labor supply, at least to a moderate degree. Robustness exercises discussed below provide further assurance that the EITC boosted labor supply independent of welfare reforms.

Having established the basic structure of the econometric model, the main results are presented in Table 4. In addition to the full controls from column 5 of Table 3, these models interact MSA commuting characteristics with *SimEITC* in order to probe for differential responses. The results of Table 4 broadly support the hypothesized transport mechanism of the EITC's extensive-margin effects.

Table 4: Effects of the EITC on labor supply outcomes by local commuting characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Weekly labor force			**Weekly employment**		
SimEITC	0.0730***	0.0732***	0.0673***	0.0659***	0.0661***	0.0607***
	(0.00666)	(0.00680)	(0.00633)	(0.00617)	(0.00626)	(0.00577)
SimEITC × high public	-0.0115			-0.0121*		
	(0.00670)			(0.00551)		
SimEITC × low auto		-0.0138			-0.0151**	
		(0.00734)			(0.00561)	
SimEITC × high auto			0.0112*			0.00976
			(0.00493)			(0.00534)
Observations	108,972	108,972	108,972	108,972	108,972	108,972
	Annual employment			**Annual weeks worked**		
SimEITC	0.0793***	0.0793***	0.0717***	3.679***	3.677***	3.405***
	(0.00712)	(0.00731)	(0.00569)	(0.315)	(0.320)	(0.285)
SimEITC × high public	-0.0213**			-0.782*		
	(0.00663)			(0.314)		
SimEITC × low auto		-0.0234**			-0.828*	
		(0.00703)			(0.335)	
SimEITC × high auto			0.0130*			0.472
			(0.00518)			(0.285)
Observations	105,138	105,138	105,138	105,138	105,138	105,138

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: CPS ASEC, 1989–2004. Sample consists of unmarried women ages 20-50 with educational attainment of high school or less. All models use lagged *SimEITC* and full controls as described in Table 3. The *high public* and *high (low) auto* indicators reflect whether the respondent's place of residence is in the top (bottom) quartile of commuting by public transport or automobile.

The estimated effects of the EITC on weekly labor supply measures is 15-25% smaller in MSAs with high public transit access relative to those without (recall that *high public* indicates the MSA is in the top quartile by commuting via public transit). Thus a $1,000 increase in the *SimEITC* boosts weekly employment by 6.6 percentage points in most of the country, but only about 5.5 percentage points in those metropolitan areas with the most abundant public transportation. On the other hand, the treatment effect on the weekly labor supply measures is significantly smaller in areas with low auto dependence and greater in highly car-dependent areas, again with differences falling in the 15-25% range. The estimated effect of the EITC on labor force participation is roughly 7.0 percentage points in high-auto MSAs versus roughly 6.1 percentage points elsewhere. The heterogeneity by commuting characteristics is even greater for the outcomes measured annually. Notably, these estimates are mostly significant even after controlling for state-MSA fixed effects and several state-level economic and policy controls.

4.2 Sensitivity

The pattern of results reflected in Table 4 is robust to a number of different specifications. First, to ensure that the choice of cutoffs for defining the commuting characteristics indicators do not drive the results, Appendix Table A2 repeats the main regressions but with *high public* indicating that the MSA in the top decile by public transit rather than top quartile; *low auto* and *high auto* are defined equivalently. The results remain largely consistent with the main results, though they are less precisely estimated. This is not surprising given that fewer MSAs are included the *high public* and *high auto* designations when using decile rather than quartile cutoffs.

Another possibility is that the novel choice of MSA-level simulated EITC variable drives the results. To ensure this is not the case I repeat the main regressions using maximum EITC (*MaxEITC*) as the explanatory variable, as is more typical in the literature. To capture exposure to the 1993 EITC expansion, I create a dichotomous variable *HighExp* that indicates whether a state is in the top half of states by the share of unmarried high-school-educated women whose income is within the plateau region of the 1993 EITC schedule or below in years 1990-1992 (measured using ASEC data). This variable captures some of the variation in EITC exposure contained in $SimEITC$, though with much greater aggregation and less detail. I estimate baseline models interacting $MaxEITC \times HighExp$ as well as models testing heterogeneity by commuting characteristics, which test the three-way interactions $MaxEITC \times HighExp \times HighComm$, where $HighComm$ is either high public transit or high automobile use. In order to account for some of the local heterogeneity, I also report results controlling for an indicator of metropolitan status and this indicator interacted with $MaxEITC$.

Appendix Tables A3 and A4 report the results of the maximum EITC–exposure–commuting interactions for the weekly and annual outcomes, respectively. The first column shows significantly larger coefficients on $MaxEITC$ in high-exposure states, as expected. The triple interactions with high-public-transit indicators (columns 2-3) show that among low-exposure states, high-public-transit MSAs exhibit a smaller combined EITC effect. Heterogeneity by commuting characteristics also holds in high-exposure states, though the difference is not statistically significant for all outcomes. Looking at the triple interaction with high-auto MSAs (columns 4-5), low-exposure states again show significant heterogeneity in EITC effects by commuting type. For high exposure states, however, high- and low-auto MSAs have similar interaction coefficients. Overall, the results echo the main set of results though with less consistency across models. This likely reflects the choice of a more aggregated explanatory variable.

The next two sets of results address the possibility that welfare reforms in the 1990s drive the results. In Appendix Table A5 I repeat the main regressions but exclude states that ever instituted a welfare waiver, as in Schanzenbach and Strain (2021). Despite the sample being less than one-third the size of the main sample—only 17 states went without welfare waivers in the mid-1990s—the coefficients on $SimEITC$ remain statistically significant across the board. Heterogeneities in the treatment effect by commuting characteristics follow the same pattern as in the main results.

A separate concern is that the national welfare reform legislation passed in the 1996 bill PRWORA drives the results. Appendix Table A6 reports results for a shortened sample, 1989–1995, as in Bastian and Jones (2021). Since $SimEITC$ is lagged one year, this sample leaves out increases in the EITC that continued for parents of two or more children in 1995 and 1996. Yet it still includes four years of EITC variation, including the post-1990 increases and the first year of the post-1993 increase. The results largely mirror the main results, though the coefficients on $SimEITC$ are slightly smaller than in the full sample for the weekly outcomes. The same pattern of heterogeneity by commuting characteristics holds in the shorter sample, though the high-auto interactions are statistically significant only for the regression with annual employment outcome. As a final check for welfare-reform-related effects, Appendix Table A7 combines both of the sample exclusions reported above, using only states without welfare waivers and limiting the time period to 1989–1995. The same pattern of results again holds.

4.3 Additional tests

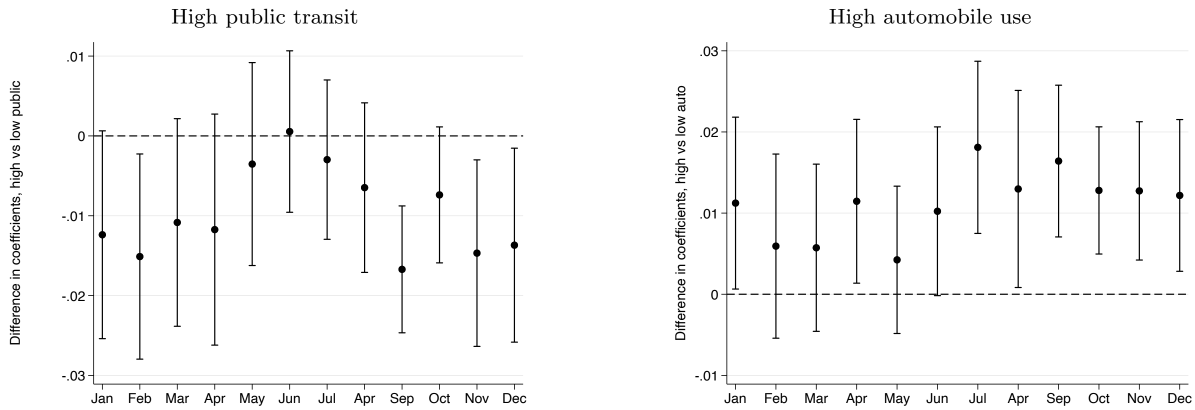
Although the results so far fit well with a liquidity-based EITC mechanism acting through access to transportation, they do not rule out other pathways. This section reports the results of two further tests of the transportation-pathway hypothesis.

The first test is to use CPS monthly data to probe for seasonality in the EITC effect. The CPS monthly sample is defined equivalently to the ASEC sample except that it begins in 1994 rather than 1989 due to availability of geographic identifiers for defining the local commuting indicators. If the EITC acted through increased liquidity and vehicle purchase, we might expect to see higher coefficients on $SimEITC$ in the early months of the year, particularly for those MSAs with higher rates of car usage. This assumes, however, that the increased labor supply from a newly purchased or repaired car is concentrated in the months around EITC receipt, which may not be the case. For instance, EITC recipients may use their refunds to replace or repair vehicles that were still functional but would have broken down later in the year. Another factor potentially cutting against the seasonality in labor supply responses to EITC receipt is that the liquidity provided by the EITC could allow recently unemployed workers to search longer, as found in LaLumia (2013).

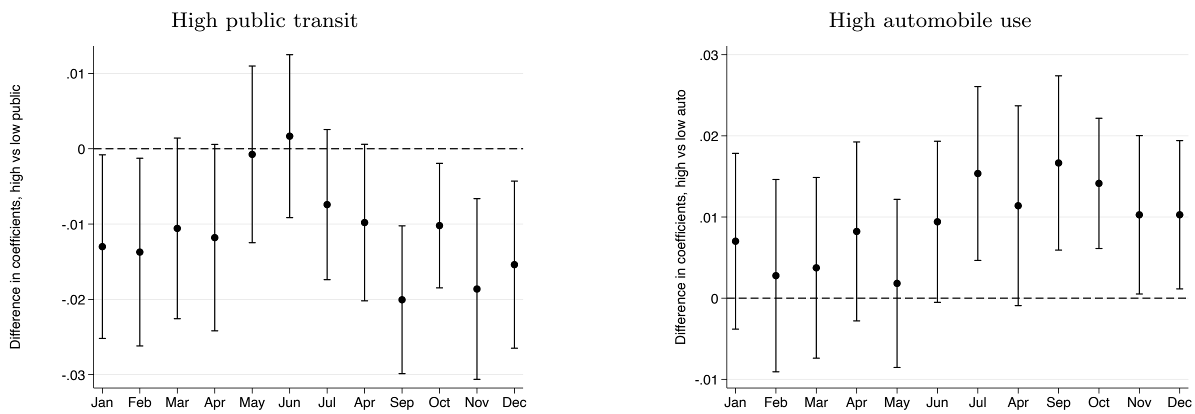
Figure 7 displays the results of regressions using the triple interaction of $SimEITC$, commuting characteristics and calendar month. The estimates shown are differences between commuting types in the EITC-by-month-by-commuting type coefficients (Appendix Figure A2 depicts the individual coefficient estimates). In other words, each point reflects estimates as to how much more or less an increase in the EITC affects a high-public MSA relative to a low-public MSA

Figure 7: Differences by local commuting characteristics in month-specific effects of EITC on labor force outcomes

Panel A: Weekly labor force participation



Panel B: Weekly employment



Note: CPS 1994–2004. Sample is unmarried women 20–50 with at most a high school education. Graphs show differences between MSAs of different commuting characteristics in coefficient estimates and 95% confidence intervals of the interaction commuting-type × month × *SimEITC*. Regressions use the full set of controls (see Table 4) as well as controls for (any school-age child) × month.

in that month (and likewise for high and low auto). The regressions also includes fixed effects for the interaction of calendar month and presence of a school-age child (ages 5–18), capturing influences of the K-12 calendar on female labor force participation (Price and Wasserman, 2023).

For the high-public transit side, the results seem to indicate that the differences in EITC responses are present in all but the summer months. This apparent school-year effect arises despite the inclusion of month-by-(school-age child) fixed effects. One interpretation could be that low-public areas differ from high-public ones except for during summer break, when parents everywhere need to find child care arrangements, a cost defrayed by higher EITC benefits. There is a slightly different picture when high-auto areas are compared to low-auto areas in the second column of Figure 7. Differences in EITC responses are minimal in the early part of the year when EITC benefits are received, grow throughout the summer, and remain elevated throughout the rest of the year. This is not the pattern that would be expected if EITC-enabled vehicle investments produced immediate labor-supply effects. Nor do the individual (undifferenced) EITC-by-month-by-commuting type estimates in Appendix Figure A2 exhibit any February or March uptick in employment associated with higher EITC benefits. The lack of obvious seasonality around EITC receipt echoes the findings in Wilson (2020).

Another test of the EITC-commuting hypothesis makes use of the survey question included in CPS monthly samples 1994 and thereafter which asks discouraged workers—those out of the labor force who want a job—why they did not look for work in the previous week. Possible responses include "transportation problems" as well as two responses related to child care and family responsibilities. I construct an indicator for those out of work due to transport or family responsibilities and use this as the outcome in regressions with $SimEITC$ and heterogeneity by commuting characteristics. Appendix Figure A3 plots trends in transportation and child-care problems by number of children among those in the sample. The figure indicates that family responsibilities are far more commonly reported than transport issues in the sample and that both declined rapidly in the latter half of the 1990s.

The results of the regressions using transportation problems as the outcome, reported in the first two columns of Table 5, complicate the transport-pathway hypothesis. Although in both specifications the EITC is associated with reductions in the share of respondents out of the labor force due to transportation issues, reductions are *greater* in high-public-transit and *smaller* in highly car-dependent MSAs. This pattern is a mirror image of the main results, in which the effects of the EITC on employment and labor force participation were muted in high-public-transit areas and larger in car-dependent areas. The commuting area–specific coefficients for the effect of the EITC on family responsibilities are not statistically significant, though they do match the signs in the baseline results.

There are reasons to treat these results with caution, however. The first issue has to do with selection: the question about transport issues is asked only of those who already report being out of the labor force but who want a job. For instance, someone who is unemployed and looking for work will not be asked this question, even if their lack of reliable transportation prevents them from searching more widely. Another issue is the relatively small number of cases in which respondents cite lack of transportation as the reason for not being in the labor force: just 0.28% of the 1994–2004 CPS monthly sample (for comparison, 1.67% of the entire sample is out of the labor force due to child care or family responsibilities). Finally, it is unclear how the structure of the survey question may affect how respondents in different metro areas respond. Since the data does not support multiple answers, regional differences in the salience of transportation challenges versus other work-related costs, especially child care, could influence the results. As Edin and Lein (1997) find in their interviews with single-mother welfare recipients, decisions to incur the costs of working are complex and multi-dimensional.

Table 5: Effects of EITC on share reporting transportation problems and family responsibilities

	(1)	(2)	(3)	(4)
	Transportation problems		Family responsibilities	
SimEITC	-0.00128*	-0.00157**	-0.0168***	-0.0169***
	(0.000504)	(0.000510)	(0.00182)	(0.00187)
SimEITC × public	-0.000861***		0.000944	
	(0.000170)		(0.000782)	
SimEITC × auto		0.000646**		-0.000111
		(0.000229)		(0.000622)
Observations	662,025	662,025	662,025	662,025

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Monthly CPS 1989–2004. Sample consists of unmarried women ages 20-50 with educational attainment of high school or less. All models use lagged *SimEITC* and full controls as described in Table 3. The *high public* and *high auto* indicators reflect whether the respondent's place of residence is in the top quartile of commuting by public transport or automobile. Outcomes reflect whether respondents listed transportation problems or family responsibilities as the reason for not looking for work, if out of the labor force but wanting to work.

Taken together, the tests for direct effects of the EITC on labor supply seasonality and on transportation difficulties provide no support for the main hypothesis of this paper. Although for the reasons listed above these supplemental tests are not conclusive, the results leave open the possibility that unobserved MSA-specific factors correlated with commuting characteristics drive the results. It will take further research, ideally work with access to high-frequency consumer spending and employment data, to quantify the importance of the EITC's liquidity and transport pathway.

5 The 2009 EITC Expansion and State EITC Supplements

Having established the viability of my simulated instrument approach, it is worthwhile to examine further applications of this methodology to the EITC. The following sections explore two open questions in the EITC research: the effects of the 2009 federal expansion targeting parents of three or more children and the impacts of state supplements to the federal program.

5.1 The 2009 federal EITC expansion

The 2009 EITC expansion raised benefits only for families with 3 or more children. Given the relatively limited target population, empirical analyses of the 2009 expansions have not yet reached a consensus. A few studies have found moderate employment effects (Bastian and Jones, 2021; Bastian and Lochner, 2022). Diverging from those contributions, event-study approaches in both Kleven (2023) and Schanzenbach and Strain (2021) failed to find any effect on the labor supply of mothers of three or more children from the 2009 expansion. By contrast, the event-study results presented in Bastian and Jones (2021) showed an increase in employment for mothers of three or more children relative to other mothers (the authors also furnished evidence for parallel pre-trends leading up to the 2009 expansion). In another of their tests of the 2009 expansion's employment effects, Bastian and Jones (2021) interacted $MaxEITC$ with an indicator for post–2005 using their full 1990-2017 sample. This resulted in a highly significant and positive coefficient on the post–2005 EITC, a finding that I am able to replicate. Yet when the sample is restricted to 2005 forward, the EITC coefficient turns slightly negative and is no longer statistically different from zero.[13] The choice of sample period has a major effect on estimates of the 2009 reform.

To examine the 2009 EITC expansion, I first re-calculate $SimEITC$ for the later period, drawing the donor households from the 2000 5% Census sample and otherwise following the same process outlined in Section 3 to project incomes forward and calculate the MSA-specific $SimEITC$ measure. I also recalculate the indicators for MSA commuting characteristics. Using these updated variables I repeat the main regressions for the period 2002–2019. Given the relatively small size of the treated group for the 2009 expansion, I estimate this model using the substantially larger monthly CPS data set. The regression use the full controls outlined in Table 3, but with two exceptions: I substitute monthly date fixed effects for year fixed effects and replace the welfare-waiver-by-number-of-kids fixed effects with number-of-kids fixed effects (welfare waivers cease to be relevant in the later period). I report results for the full sample—unmarried women 20–50 with at most a high school degree—as well as for a sample restricted to mothers, in line with previous studies (Bastian and Lochner, 2022; Kleven, 2023).

The results of these regressions, reported in Table 6, provide qualified support for a positive labor supply effect in the most automobile-dependent areas. The baseline Monthly CPS results without EITC-commuting interactions are positive though insignificant. Allowing for heterogeneity by high public transit or high car dependence leads to a pattern of coefficients that has the same signs as the corresponding results in the earlier-period regressions. The coefficient estimates for $EITC \times high\ auto$ are significant for both employment and labor force participation estimated using CPS. Yet the sum of coefficients $EITC + EITC \times high\ auto$ is significant at only the 10% level and only for the samples that exclude women without children. These results suggest that the labor-supply effects of the 2009 expansion were felt, if at all, in those areas where lower-educated single mothers depend the most on commuting by car. The fact that the broad pattern of estimates matches those of the models examining the 1990s EITC expansion—despite the different time period and updated versions of the key explanatory variables—lends additional credence to the transport-pathway hypothesis.

[13] Results available upon request. Note that my sample and covariates differ to some degree from Bastian and Jones (2021).

Table 6: Effects of the 2009 EITC expansion on labor supply outcomes by local commuting characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Weekly labor force participation			
Sample:	0+ kids	1+ kids	0+ kids	1+ kids	0+ kids	1+ kids
SimEITC	0.00740	0.00636	0.00477	0.00869	0.000479	0.00322
	(0.00628)	(0.00878)	(0.00617)	(0.00879)	(0.00565)	(0.00887)
SimEITC × high public			-0.00413	-0.0104		
			(0.00244)	(0.00536)		
SimEITC × high auto					0.00736***	0.0133*
					(0.00219)	(0.00540)
Sum of coefficients						
SimEITC + EITC × high auto					0.00784	0.0165
					(0.00580)	(0.00953)
Observations	1,011,748	457,026	1,011,748	457,026	1,011,748	457,026
			Panel B: Weekly employment			
Sample:	0+ kids	1+ kids	0+ kids	1+ kids	0+ kids	1+ kids
SimEITC	0.00545	0.00877	0.00216	0.0108	-0.00202	0.00576
	(0.00744)	(0.00868)	(0.00714)	(0.00861)	(0.00644)	(0.00879)
SimEITC × high public			-0.00517*	-0.00928		
			(0.00254)	(0.00589)		
SimEITC × high auto					0.00795**	0.0128*
					(0.00243)	(0.00573)
Sum of coefficients						
SimEITC + EITC × high auto					0.00593	0.0185
					(0.00685)	(0.00953)
Observations	1,011,748	457,026	1,011,748	457,026	1,011,748	457,026

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Monthly CPS, 2002–2019. Sample consists of single women ages 20-50 with educational attainment of high school or less. In columns 2, 4 and 6 the sample is restricted to mothers. All models use lagged simulated EITC and full set of controls as described in Table 3 with modifications noted in text. The *high public* and *high auto* indicators reflect whether the respondent's place of residence is in the top quartile of commuting by public transport or automobile.

5.2 State EITCs

States have implemented supplements to the federal EITC since the 1980s, yet there remains some debate regarding their impact and identification. Both Leigh (2010) and Bastian and Jones (2021) presented results that suggest state EITC implementations suffer from endogeneity issues related to political and economic influences; Bastian and Lochner (2022) comes to the opposite conclusion for expansions 2003–2018. Because states face tighter budget constraints than the federal government, EITC supplements may depend on changes to state tax codes or ongoing economic performance. Estimation strategies relying on difference-in-differences approaches may also be biased by well-known issues relating to the staggered implementation of treatments with dynamic and heterogeneous effects (De Chaisemartin and d'Haultfoeuille, 2022).

Despite potential endogeneity concerns, Bastian and Jones (2021) found that state EITCs were associated with higher labor supply outcomes while Neumark and Williams (2020) provided modest evidence that state EITCs boost federal EITC program participation. Kleven (2023) failed to find any effect of state EITC implementation in a synthetic-control event-study framework.[14] In Wilson (2020), including state EITCs in a total-EITC measure altered the interpretation of key results. In both Bastian and Jones (2021) and Schanzenbach and Strain (2021), estimates of labor supply effects fell relative to the benchmark federal-only results when state-plus-federal EITC was used as the explanatory variable. While not remarked upon in those papers, this could reflect omitted-variable bias in the estimates obtained from using only the federal EITC. For states offering EITC top-ups, federal expansions feed through directly to higher state benefits and, presumably, to labor supply responses. Thus the state EITC is positively correlated both with the outcomes of interest and the explanatory variable, which should bias the federal-only coefficient estimate upwards.

Table 7 reports the results of regressions using both federal and state EITCs as well as interactions of these with commuting indicators.[15] As before, I lag the EITC variable by one year so that it reflects the total EITC refunds eligible to receive in the survey year. In line with prior literature, the coefficient estimates for the total EITC are smaller than for the federal EITC alone. Otherwise the pattern of coefficients is broadly consistent with the main results, including the significant heterogeneity in effect size by MSA commuting types.

An alternate approach to examining the effects of state EITCs is to leverage the local variation in EITC exposure that exists *within* states to run state-by-state regressions for those states that implemented EITC supplements. This strategy mitigates to some degree concerns over policy endogeneity since it does not use as a control group outside states that are not subject to unobserved factors that are potentially endogenous to the state introducing an EITC add-on. By assumption, anything endogenous to a state EITC implementation affects all groups within the state equally. An obvious drawback to estimating state EITC effects independently for different states is the significant reduction in sample size. Yet this strategy is made more viable by the use of within-state regional variation: not only is there variation between family sizes in state EITC receipt, but also between MSAs with varying levels of exposure to the statewide expansion. Exploiting this source of variation marks an improvement over event-study strategies that treat state EITCs as dichotomous events.

In choosing which state EITC expansions to explore, I focus only on those occurring in 2000 and afterwards in order to avoid contamination from the major federal EITC expansion of the 1990s. I also include only those implementations that offered refundable tax credits and whose supplements rose to at least 10% of the federal EITC within five years. This leaves

[14] Kleven (2023) estimated synthetic control states for each state EITC expansion then ran stacked event studies using the treatment and synthetic control states together.

[15] Because I am concerned with the liquidity channel of the EITC, I include only fully refundable state EITCs in the measures for state EITC.

Table 7: Effects of state-plus-federal EITC on labor supply outcomes by local commuting characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Weekly labor force			**Weekly employment**		
TotEITC	0.0628***	0.0654***	0.0585***	0.0558***	0.0585***	0.0520***
	(0.00559)	(0.00593)	(0.00582)	(0.00556)	(0.00562)	(0.00548)
TotEITC × high public		-0.0131			-0.0136*	
		(0.00693)			(0.00574)	
TotEITC × high auto			0.0139**			0.0124*
			(0.00517)			(0.00554)
Observations	108,972	108,972	108,972	108,972	108,972	108,972
	Annual employment			**Annual weeks worked**		
TotEITC	0.0677***	0.0721***	0.0628***	3.118***	3.291***	2.929***
	(0.00483)	(0.00654)	(0.00491)	(0.280)	(0.293)	(0.284)
TotEITC × high public		-0.0224***			-0.858**	
		(0.00660)			(0.328)	
TotEITC × high auto			0.0157**			0.614*
			(0.00544)			(0.297)
Observations	105,138	105,138	105,138	105,138	105,138	105,138

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

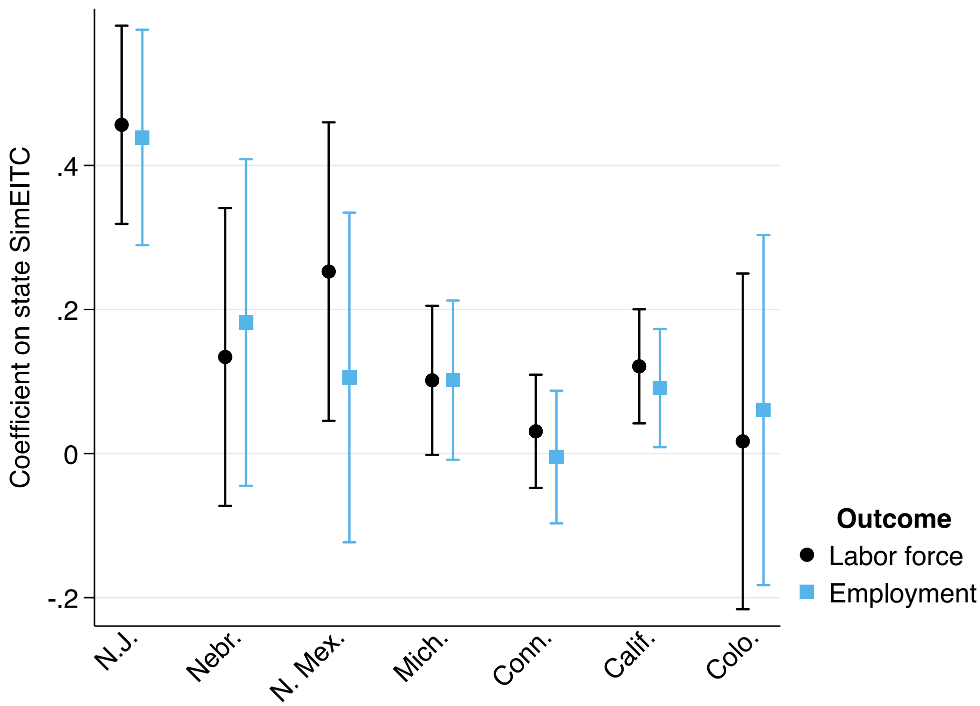
Note: CPS ASEC, 1989–2004. Sample consists of unmarried women ages 20-50 with educational attainment of high school or less. All models use lagged simulated EITCs and full set of controls as described in Table 3. *TotEITC* is the sum of simulated federal and refundable state EITCs. The *public* and *auto* indicators reflect whether the respondent's place of residence is in the top quartile of commuting by public transport or automobile.

seven expansions: New Jersey (2000), Nebraska (2006), New Mexico (2007), Michigan (2008), Connecticut (2011), California (2015) and Colorado (2015).[16] For each state expansion the sample includes five years prior to state EITC expansion and seven years after, or six years of state EITC receipt and six years prior to it. As before, I limit the sample to unmarried women with education of a high school degree of less. I use the monthly CPS for its larger sample size. Regressions include the same set of full controls as in the baseline results and outlined in Table 3, excluding state-level controls since they would be perfectly collinear in a single-state sample. In line with my approach to the 2009 expansion, the EITC variable is the simulated state EITC with donors for the simulation drawn from the 2000 5% Census sample.

Figure 8 plots the coefficient estimates on state *SimEITC* for each of the state-level regressions and for both outcomes (weekly labor force participation and weekly employment). In nearly all the regressions the coefficient on the state EITC variables is positive and at times significant at the 95% level. Although the coefficient estimates for the 2000 New Jersey expansion are unusually high, the rest of the estimates fall broadly within the range of the baseline estimates.

[16]The 2000 District of Columbia expansion is excluded since D.C. has only one metro area.

Figure 8: Coefficient estimates for state-specific EITC expansion regressions



Note: Monthly CPS, varying years. Coefficient estimates and 95% confidence intervals associated with state-level *SimEITC*. Regressions control for family and demographic variables listed in Table 3 as well as monthly date and MSA fixed effects. Samples span from five years prior to state EITC implementation to seven years after. Standard errors clustered on individuals.

A further test of both state EITC implementation and the transportation pathway is to interact state-level *SimEITC* with commuting characteristics for the four states home to high-public-transit MSAs among those with large post-2000 EITC implementations: New Jersey, Connecticut, California and Colorado. This allows testing the local commuting-type heterogeneity explored in previous models, albeit with far smaller sample sizes. Coefficient estimates from these regressions are depicted in Appendix Figure A4. While high-public-transit MSAs exhibit lower point estimates for all but two of the regressions, most of the differences are not statistically significant.

6 Conclusion

This study presents evidence that the well-documented effects of the EITC on the labor supply of single mothers act in part through the transportation pathway, the hypothesis that recipients of EITC benefits use the additional spending power they provide to buy and maintain vehicles that bring them to work and facilitate job search. This finding helps bridge two large though heretofore disconnected literatures on the EITC, the first documenting how EITC recipients view and conduct EITC-related spending, the second quantifying the effects of the EITC on labor supply.

In my preferred set of estimates, I find that the a \$1,000 increase in the effective EITC eligible to receive boosts weekly employment by 6.6 percentage points in most metropolitan areas, but only 5.5 percentage points in those areas with the most abundant public transportation—a statistically significant 18% difference in effect size. Similarly, the employment effect of the EITC is roughly 16% higher in those metropolitan areas and rural regions with the greatest dependence on cars (7.0 percentage points vs 6.1 percentage points). These estimates survive a large array of controls and account for substantial amount of metro-level heterogeneity.

The key methodological innovation in this study is a simulated instrument that captures the wide variation in exposure to EITC expansions stemming from regional heterogeneity in incomes across the target population. The simulated instruments I construct—which capture policy variation over time and between metropolitan areas, family types and age groups—have two main benefits. First, they make it possible to estimate heterogeneous effects of the EITC between MSAs with different types of commuting patterns without picking up the confounding effects of underlying income differences between MSAs (and thus differences in exposure to the EITC). These simulated instruments also allow for more precise estimates of the EITC's effect across the U.S. and within states.

The empirical strategy I develop also sheds new light on two open questions in the EITC literature: the effects of the 2009 EITC expansion and of state EITC supplements. In both cases I find moderate support for positive labor-supply effects, and in the case of the 2009 expansion I document a pattern of heterogeneity by local commuting characteristics that echoes the main results. Although these results are not as precisely estimated as in the main analyses, they provide suggestive evidence in line with the labor-supply effects found in the EITC literature as well as the transport-pathway hypothesis.

Yet some caution is warranted in interpreting the findings. The results rest on assumptions regarding parallel employment trends among different family types that, while commonly adopted in the EITC literature, remain untestable. Moreover, the main results find no support in supplemental analyses into phenomena that would presumably accompany the transportation pathway. I fail to find any obvious seasonality in employment responses to increased EITC benefits that would correspond to increased liquidity around tax season helping recipients secure transportation and get to work. I also find contradictory results when examining the EITC's impacts on the share of single women reporting that they are out of the labor force due to transportation issues. Here the EITC effect appears larger for high-public-transport areas, reversing the pattern of the main results. Although there are conceptual and data-related weaknesses with both of these supplemental analyses, they point to the need for additional research into the mechanisms underlying the EITC.

This study informs future work exploring the pathways behind the labor supply effects of the EITC observed in the literature. An outstanding question is the one raised by Nichols and Rothstein (2016) regarding the extent to which the EITC's measured impacts reflect reduced exit among recipients or discouraged workers newly joining the labor market. That is, does the EITC primarily operate by keeping workers from being sidelined, or by bringing them in off the sidelines? While this study does not look at turnover or separations explicitly, future work could do so.[17]

This research also carries potential policy implications. One way of interpreting the relatively muted effects of EITC expansions in high-public-transit areas is that these areas have less room to improve—public transportation is doing its job. Facilitating employment stability is an important component in the returns on investment into public transportation. On the other hand, the larger estimated labor supply improvements that highly car-dependent areas experience from EITC expansions may be transmitted through a somewhat costly cycle of recipients buying used cars until they ultimately (and unexpectedly) break down.

Finally, the theoretical underpinnings of this study have implications beyond the EITC. By making explicit the inverse relationship between wages and separations—a pattern that is conspicuous in actual labor markets but virtually absent in the job search literature—the model advanced here uncovers another pathway by which cash benefits to workers encourage employment. This could prove a useful perspective in comparing the effects of the negative income taxes and the EITC, as in Rothstein (2010), or in analyzing the potential impacts of universal basic income proposals.

[17] An earlier line of inquiry in this research project used the U.S. Census Bureau's public-use Quarterly Workforce Indicators (QWI) data set to examine the EITC's effects on turnover directly. Given the level of aggregation in that data, particularly its lack of family size measures, it became clear that the QWI is not well-suited to the task. Future work on the turnover question would benefit from using the administrative Longitudinal Employer-Household Dynamics data set, the source of the public-use QWI.

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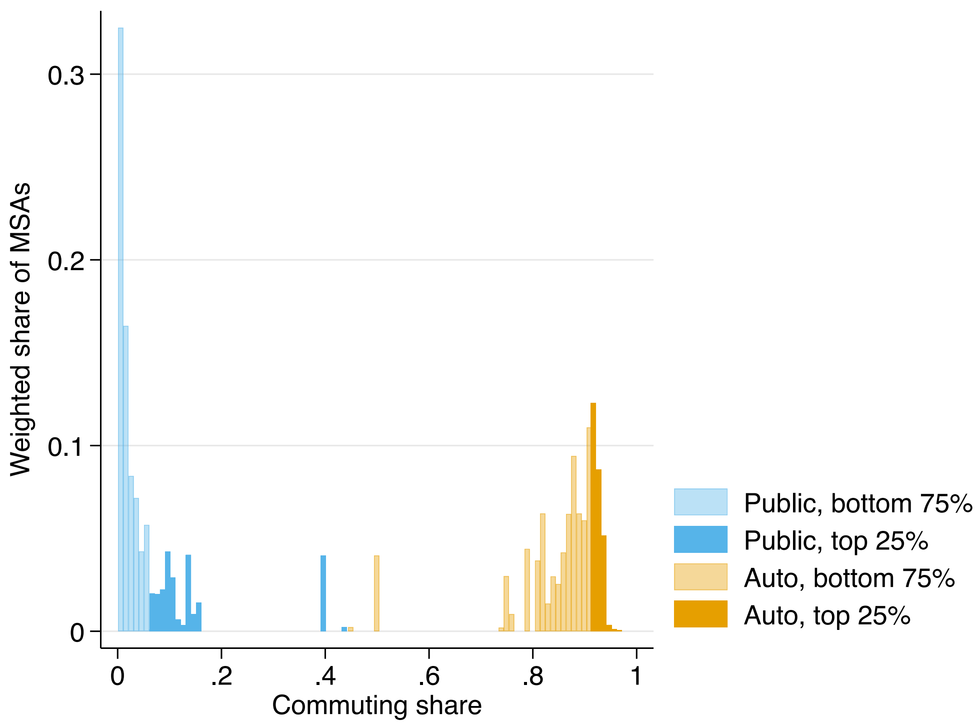
A Additional Figures and Tables

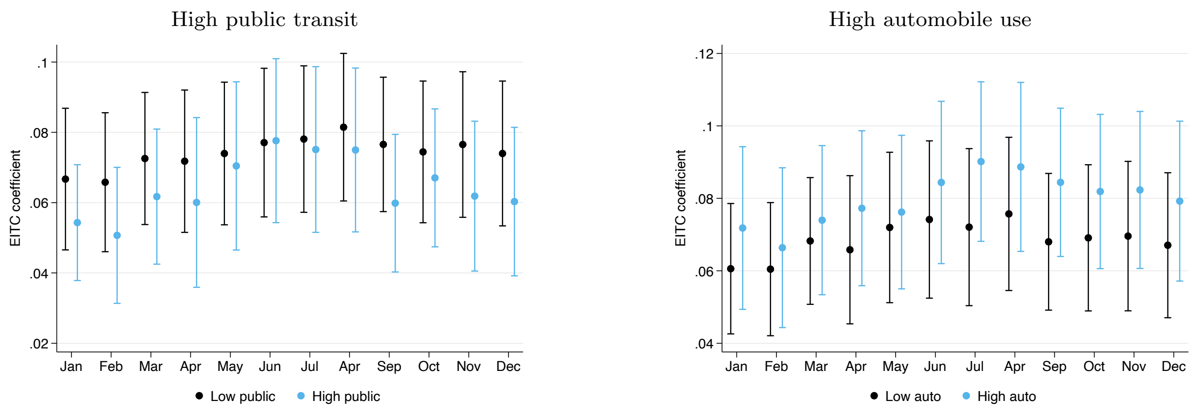
Figure A1: Distribution of MSAs by commuting share



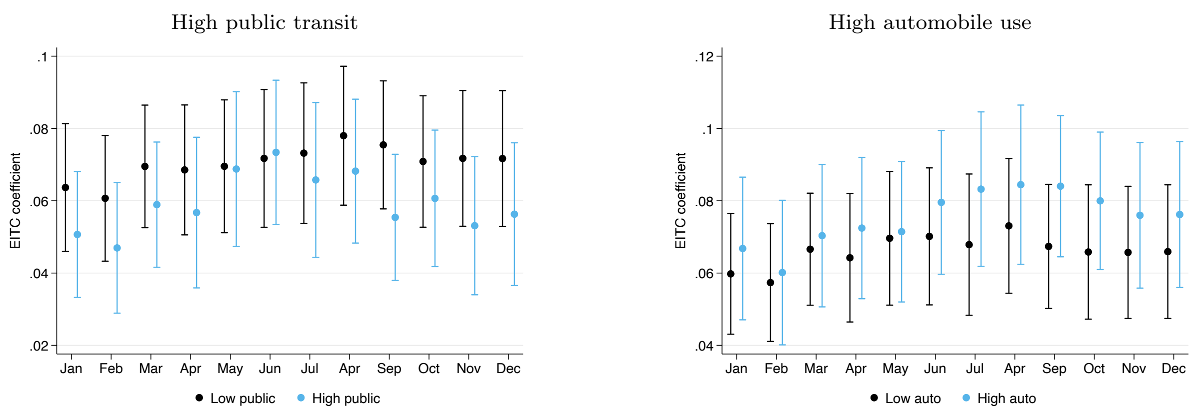
1990 5% Census sample, IPUMS. Histograms depict the share of MSAs in each bin of commuting share by type of commute. Commuting shares calculated as the share of workers with at most a high school education who either commute by public transit or by automobile. MSAs weighted by working high-school-educated population.

Figure A2: Effect of EITC on labor force outcomes by month and local commuting characteristics

Panel A: Weekly labor force participation

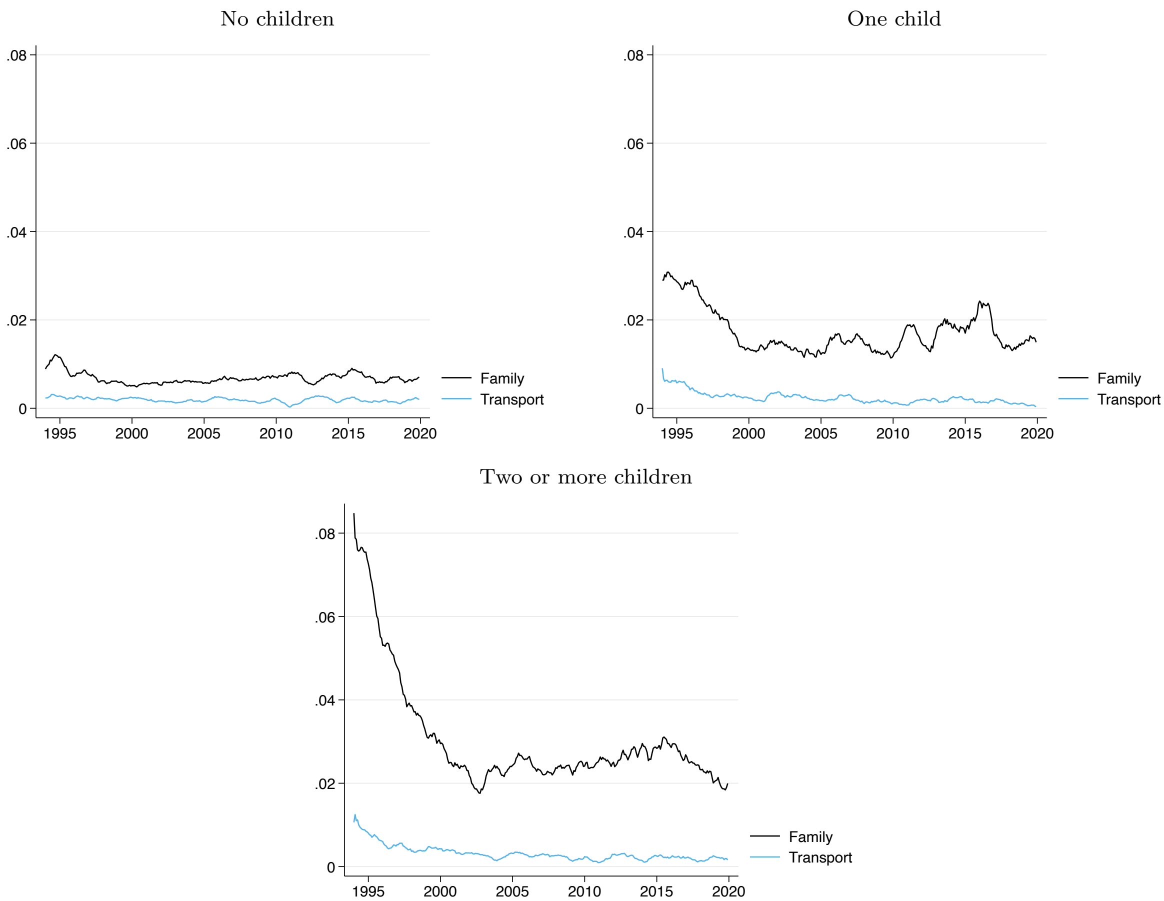


Panel B: Weekly employment



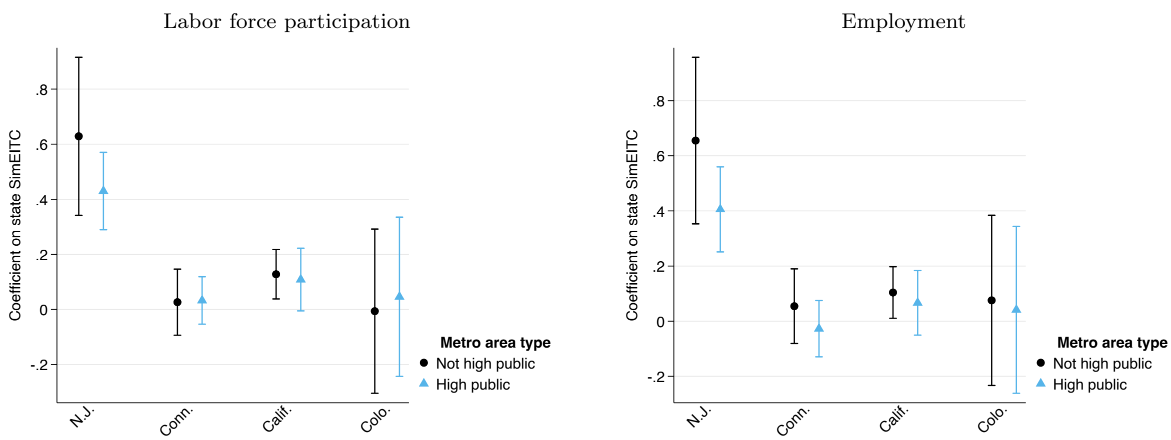
Note: CPS 1994–2004. Sample is unmarried women 20–50 with at most a high school education. Graphs show coefficient estimates and 95% confidence intervals coefficients on EITC and commuting-type × month × EITC. Regressions use the full set of controls (see Table 4) as well as controls for (any school-age child) × month.

Figure A3: Incidence of out-of-labor-force due to transportation problems or family responsibilities



Note: CPS 1994–2019. Sample is unmarried women 20–50 with at most a high school education. Outcomes reflect whether respondents listed transportation problems or family responsibilities as the reason for not looking for work, if out of the labor force but wanting to work.

Figure A4: Coefficient estimates for state-specific EITC expansion regressions by local commuting characteristics



Monthly CPS, varying years. Coefficient estimates and 95% confidence intervals associated with state-level $SimEITC$ interacted with the indicator for high local public transportation access. Regressions control for family and demographic variables listed in Table 3 as well as monthly date and MSA fixed effects. Samples span from five years prior to state EITC implementation to seven years after. Standard errors clustered on individuals.

Table A1: Effect of the EITC on labor supply outcomes for different EITC specifications, annual outcomes

	Annual employment				**Annual weeks worked**			
	Max	Max, lag	Sim	Sim, lag	Max	Max, lag	Sim	Sim, lag
EITC	0.0461***	0.0479***	0.0739***	0.0759***	2.127***	2.224***	3.409***	3.503***
	(11.07)	(12.71)	(13.10)	(13.72)	(9.71)	(10.99)	(11.49)	(12.29)
R-squared	0.0768	0.0771	0.0772	0.0775	0.0991	0.0994	0.0994	0.0997
Log-likelihood	-59,105	-59,087	-59,083	-59,067	-472,951	-472,935	-472,934	-472,921
Observations	105,138	105,138	105,138	105,138	105,138	105,138	105,138	105,138

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: CPS ASEC, 1989–2004. Sample consists of unmarried women ages 20-50 with educational attainment of high school or less. Table shows coefficient estimates on different versions of the treatment variable for outcomes listed above. Columns labeled *Max* use the maximum federal EITC benefit by year and family size. Those labeled *Sim* use the *SimEITC* as described in Section 3. Columns 2 and 4 use lagged values of the respective EITC variables. EITC is measured in 1,000s of 2019 dollars. All models use full controls described in Table 3.

Table A2: The effect of the EITC on labor supply outcomes by local commuting characteristics—alternate commuting indicator definition

	(1)	(2)	(3)	(4)	(5)	(6)
	Weekly labor force			**Weekly employment**		
SimEITC	0.0723***	0.0723***	0.0692***	0.0652***	0.0653***	0.0630***
	(0.00663)	(0.00659)	(0.00623)	(0.00629)	(0.00624)	(0.00577)
SimEITC × high public	-0.0114			-0.0146		
	(0.0114)			(0.00802)		
SimEITC × low auto		-0.0103			-0.0131	
		(0.0111)			(0.00802)	
SimEITC × high auto			0.0106			0.00650
			(0.00587)			(0.00718)
Observations	108,972	108,972	108,972	108,972	108,972	108,972
	Annual employment			**Annual weeks worked**		
SimEITC	0.0778***	0.0779***	0.0743***	3.630***	3.631***	3.542***
	(0.00726)	(0.00718)	(0.00554)	(0.323)	(0.319)	(0.274)
SimEITC × high public	-0.0202			-0.820		
	(0.0119)			(0.555)		
SimEITC × low auto		-0.0188			-0.744	
		(0.0117)			(0.547)	
SimEITC × high auto			0.00993			0.184
			(0.00659)			(0.390)
Observations	105,138	105,138	105,138	105,138	105,138	105,138

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: CPS ASEC, 1989–2004. Sample consists of unmarried women ages 20-50 with educational attainment of high school or less. All models use lagged simulated EITC and full set of controls as described in Table 3. The *public* and *auto* indicators reflect whether the respondent's place of residence is in the top or bottom decile of commuting by public transport or automobile (rather than top or bottom quartile as in Table 4). *Metro controls* include an indicator for residing in a metropolitan area and an interaction of this indicator with the EITC variable.

Table A3: Effect of maximum EITC on labor supply by local commuting characteristics and binary exposure, weekly outcomes

	(1)	(2)	(3)	(4)	(5)
Panel A: Weekly labor force participation					
	Baseline	Public		Auto	
MaxEITC	0.0387***	0.0419***	0.0431***	0.0383***	0.0403***
	(0.00489)	(0.00486)	(0.00544)	(0.00493)	(0.00549)
MaxEITC × HighExp	0.0119**				
	(0.00372)				
MaxEITC × LowExp × HighComm		-0.00675	-0.00643	0.00885	0.00839
		(0.00373)	(0.00374)	(0.00502)	(0.00485)
MaxEITC × HighExp × LowComm		0.00944**	0.00925**	0.0120**	0.0117**
		(0.00308)	(0.00307)	(0.00433)	(0.00423)
MaxEITC × HighExp × HighComm		0.00587	0.00628	0.0142***	0.0136***
		(0.00471)	(0.00469)	(0.00407)	(0.00386)
Metro controls			✓		✓
Observations	108,972	108,972	108,972	108,972	108,972
Panel B: Weekly employment					
	Baseline	Public		Auto	
MaxEITC	0.0334***	0.0368***	0.0385***	0.0331***	0.0358***
	(0.00421)	(0.00435)	(0.00515)	(0.00425)	(0.00514)
MaxEITC × HighExp	0.0139***				
	(0.00344)				
MaxEITC × LowExp × HighComm		-0.00706*	-0.00659*	0.00697	0.00632
		(0.00274)	(0.00279)	(0.00522)	(0.00515)
MaxEITC × HighExp × LowComm		0.0110**	0.0107**	0.0147***	0.0144***
		(0.00333)	(0.00332)	(0.00432)	(0.00427)
MaxEITC × HighExp × HighComm		0.0103	0.0109*	0.0149***	0.0140***
		(0.00546)	(0.00552)	(0.00394)	(0.00378)
Metro controls			✓		✓
Observations	108,972	108,972	108,972	108,972	108,972

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: CPS ASEC, 1989–2004. Unmarried women ages 20-50 with high school diploma or less. Models use lagged maximum EITC and full set of controls (see Table 3). *HighExp* indicates residence in state with high exposure to the 1990s EITC reforms, as described in text. The *HighComm* and *LowComm* indicators reflect whether the respondent's place of residence is in the top 25% or bottom 75%, respectively, of commuting by public transport or automobile, as indicated in column labels *Public* and *Auto*. *Metro controls* include an indicator for residing in a metropolitan area and an interaction of this indicator with EITC.

Table A4: Effect of maximum EITC on labor supply by local commuting characteristics and binary exposure, annual outcomes

	(1)	(2)	(3)	(4)	(5)
Panel A: Weekly labor force participation					
	Baseline	Public		Auto	
MaxEITC	0.0423***	0.0483***	0.0508***	0.0417***	0.0463***
	(0.00417)	(0.00495)	(0.00537)	(0.00419)	(0.00488)
MaxEITC × HighExp	0.0126**				
	(0.00411)				
MaxEITC × LowExp × HighComm		-0.0127**	-0.0120**	0.0126**	0.0113*
		(0.00389)	(0.00397)	(0.00458)	(0.00451)
MaxEITC × HighExp × LowComm		0.00818*	0.00777*	0.0133**	0.0126**
		(0.00325)	(0.00318)	(0.00492)	(0.00473)
MaxEITC × HighExp × HighComm		0.000516	0.00138	0.0154***	0.0140***
		(0.00733)	(0.00734)	(0.00448)	(0.00417)
Metro controls			✓		✓
Observations	105,138	105,138	105,138	105,138	105,138
Panel C: Annual weeks worked					
	Baseline	Public		Auto	
MaxEITC	1.939***	2.151***	2.319***	1.915***	2.169***
	(0.227)	(0.232)	(0.259)	(0.226)	(0.249)
MaxEITC × HighExp	0.674***				
	(0.201)				
MaxEITC × LowExp × HighComm		-0.449*	-0.403*	0.456	0.390
		(0.177)	(0.180)	(0.285)	(0.283)
MaxEITC × HighExp × LowComm		0.518**	0.491**	0.744**	0.705**
		(0.181)	(0.177)	(0.254)	(0.244)
MaxEITC × HighExp × HighComm		0.231	0.288	0.725***	0.646**
		(0.362)	(0.364)	(0.212)	(0.194)
Metro controls			✓		✓
Observations	105,138	105,138	105,138	105,138	105,138

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: CPS ASEC, 1989–2004. Unmarried women ages 20-50 with high school diploma or less. Models use lagged maximum EITC and full set of controls (see Table 3). *HighExp* indicates residence in state with high exposure to the 1990s EITC reforms, as described in text. The *HighComm* and *LowComm* indicators reflect whether the respondent's place of residence is in the top 25% or bottom 75%, respectively, of commuting by public transport or automobile, as indicated in column labels *Public* and *Auto*. *Metro controls* include an indicator for residing in a metropolitan area and an interaction of this indicator with EITC.

Table A5: Effect of the EITC on labor supply outcomes by local commuting characteristics—no–waiver states

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Weekly labor force				**Weekly employment**			
SimEITC	0.0916***	0.0940***	0.0939***	0.0833***	0.0783***	0.0818***	0.0813***	0.0692***
	(0.00988)	(0.0112)	(0.0111)	(0.0111)	(0.00842)	(0.00928)	(0.00921)	(0.00924)
SimEITC × high public		-0.0153				-0.0221**		
		(0.0121)				(0.00799)		
SimEITC × low auto			-0.0171				-0.0220**	
			(0.0120)				(0.00790)	
SimEITC × high auto				0.0246**				0.0270***
				(0.00716)				(0.00744)
Observations	32,815	32,815	32,815	32,815	32,815	32,815	32,815	32,815
	Annual employment				**Annual weeks worked**			
SimEITC	0.0932***	0.0987***	0.0980***	0.0836***	3.841***	4.089***	4.050***	3.367***
	(0.0104)	(0.0135)	(0.0133)	(0.0112)	(0.443)	(0.523)	(0.517)	(0.517)
SimEITC × high public		-0.0310**				-1.377**		
		(0.0101)				(0.497)		
SimEITC × low auto			-0.0304**				-1.306*	
			(0.0104)				(0.513)	
SimEITC × high auto				0.0299***				1.485***
				(0.00855)				(0.401)
Observations	31,860	31,860	31,860	31,860	31,860	31,860	31,860	31,860

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

CPS ASEC, 1989–2004. Sample consists of unmarried women ages 20-50 with educational attainment of high school or less. Sample is further limited to states that did not receive welfare waivers in the 1990s. All models use lagged simulated EITC and full set of controls as described in Table 3. The *public* and *auto* indicators reflect whether the respondent's place of residence is in the top or bottom quartile of commuting by public transport or automobile.

Table A6: Effect of the EITC on labor supply outcomes by local commuting characteristics—pre–1996 sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Weekly labor force				**Weekly employment**			
SimEITC	0.0473^{**}	0.0473^{**}	0.0469^{**}	0.0343^{*}	0.0349^{*}	0.0349^{*}	0.0344^{*}	0.0221
	(0.0162)	(0.0167)	(0.0166)	(0.0171)	(0.0154)	(0.0157)	(0.0157)	(0.0163)
SimEITC × high public		-0.0348^{*}				-0.0374^{**}		
		(0.0175)				(0.0126)		
SimEITC × low auto			-0.0378^{*}				-0.0434^{***}	
			(0.0187)				(0.0123)	
SimEITC × high auto				0.0255				0.0249
				(0.0143)				(0.0135)
Observations	47,936	47,936	47,936	47,936	47,936	47,936	47,936	47,936
	Annual employment				**Annual weeks worked**			
SimEITC	0.0926^{**}	0.0804^{**}	0.0806^{**}	0.0695^{**}	4.563^{***}	4.159^{***}	4.181^{***}	3.666^{**}
	(0.0283)	(0.0251)	(0.0249)	(0.0262)	(1.300)	(1.228)	(1.221)	(1.251)
SimEITC × high public		-0.0577^{**}				-1.915^{*}		
		(0.0182)				(0.765)		
SimEITC × low auto			-0.0615^{**}				-1.964^{*}	
			(0.0190)				(0.805)	
SimEITC × high auto				0.0350^{*}				1.363
				(0.0149)				(0.773)
Observations	46,282	46,282	46,282	46,282	46,282	46,282	46,282	46,282

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: CPS ASEC, 1989–1995. Sample consists of single mothers ages 20-50 with educational attainment of high school or less. All models use lagged simulated EITC and full set of controls as described in Table 3. The *high public* and *high auto* indicators reflect whether the respondent's place of residence is in the top quartile of commuting by public transport or automobile.

Table A7: Effect of the EITC on labor supply outcomes by local commuting characteristics—no-waiver states, pre–1996 sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Weekly labor force				**Weekly employment**			
SimEITC	0.0925***	0.0954***	0.0934***	0.0540*	0.0796**	0.0821**	0.0803**	0.0407
	(0.0249)	(0.0258)	(0.0258)	(0.0243)	(0.0246)	(0.0249)	(0.0247)	(0.0231)
SimEITC × high public		-0.0596*				-0.0503**		
		(0.0266)				(0.0189)		
SimEITC × low auto			-0.0673*				-0.0509**	
			(0.0257)				(0.0186)	
SimEITC × high auto				0.0844***				0.0855***
				(0.0231)				(0.0181)
Observations	13,851	13,851	13,851	13,851	13,851	13,851	13,851	13,851
	Annual employment				**Annual weeks worked**			
SimEITC	0.197**	0.166**	0.161**	0.131**	9.263***	8.051**	7.934**	6.114**
	(0.0581)	(0.0486)	(0.0487)	(0.0477)	(2.554)	(2.379)	(2.412)	(2.062)
SimEITC × high public		-0.100***				-3.889***		
		(0.0225)				(1.083)		
SimEITC × low auto			-0.104***				-3.838***	
			(0.0219)				(1.104)	
SimEITC × high auto				0.104***				4.943***
				(0.0213)				(1.121)
Observations	13,442	13,442	13,442	13,442	13,442	13,442	13,442	13,442

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: CPS ASEC, 1989–1995. Sample consists of single mothers ages 20-50 with educational attainment of high school or less. All models use lagged simulated EITC and full set of controls as described in Table 3. The *high public* and *high auto* indicators reflect whether the respondent's place of residence is in the top quartile of commuting by public transport or automobile.

Table A8: Effects of state and federal EITC on labor supply outcomes by local commuting characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: ASEC 1989-2004								
Outcome:	Weekly labor force			Weekly employment			Annual employment		
FedEITC	0.0738***	0.0742***	0.0694***	0.0671***	0.0675***	0.0637***	0.0793***	0.0807***	0.0744***
	(0.00670)	(0.00704)	(0.00705)	(0.00648)	(0.00659)	(0.00651)	(0.00669)	(0.00722)	(0.00675)
StEITC	-0.0389	-0.0507	-0.0320	-0.0486	-0.0531	-0.0457	-0.0552*	-0.0640	-0.0454
	(0.0225)	(0.0307)	(0.0227)	(0.0258)	(0.0370)	(0.0267)	(0.0257)	(0.0378)	(0.0259)
FedEITC × public		-0.0125			-0.0123*			-0.0227**	
		(0.00746)			(0.00615)			(0.00786)	
StEITC × public		0.0511			0.0362			0.0676	
		(0.0430)			(0.0494)			(0.0492)	
FedEITC × auto			0.00970			0.00757			0.0115*
			(0.00500)			(0.00540)			(0.00522)
StEITC × auto			0.127			0.191*			0.0771
			(0.0684)			(0.0747)			(0.0437)
Observations	108,972	108,972	108,972	108,972	108,972	108,972	105,138	105,138	105,138
	Panel B: ASEC 1989-2019								
Outcome:	Weekly labor force			Weekly employment			Annual employment		
FedEITC	0.0756***	0.0759***	0.0755***	0.0742***	0.0745***	0.0739***	0.0772***	0.0776***	0.0766***
	(0.00582)	(0.00577)	(0.00584)	(0.00511)	(0.00513)	(0.00511)	(0.00598)	(0.00601)	(0.00586)
StEITC	0.0156	-0.0114	0.0143	0.00495	-0.0161	0.00156	0.00962	-0.0118	0.0107
	(0.0143)	(0.0148)	(0.0162)	(0.0149)	(0.0168)	(0.0171)	(0.0130)	(0.0177)	(0.0143)
FedEITC × public		-0.00295			-0.00507			-0.00845	
		(0.00433)			(0.00391)			(0.00455)	
StEITC × public		0.0581*			0.0515			0.0595*	
		(0.0265)			(0.0299)			(0.0299)	
FedEITC × auto			-0.00000210			0.000135			0.00118
			(0.00277)			(0.00308)			(0.00305)
StEITC × auto			0.0302			0.0808			0.00325
			(0.0436)			(0.0514)			(0.0531)
Observations	216,856	216,856	216,856	216,856	216,856	216,856	210,094	210,094	210,094

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

CPS ASEC, 1989–2019. Sample consists of unmarried women ages 20-50 with educational attainment of high school or less. All models use lagged simulated EITCs and full set of controls as described in Table 3. *StEITC* is limited to state credits that are fully refundable. The *public* and *auto* indicators reflect whether the respondent's place of residence is in the top quartile of commuting by public transport or automobile. *Metro controls* include an indicator for residing in a metropolitan area and interactions of this indicator with each EITC variable.

B Equilibrium job search with heterogeneous job destruction

B.1 Model

The model is a variant of the classic Burdett and Mortensen (1998) (BM) model. Homogeneous workers search for new job opportunities whether employed or unemployed and face the risk of job destruction while working. Firms homogeneous in their productivities seek to maximize flow profits by posting wages which they pay uniformly to all workers. Equilibrium is determined as a game in which each employer sets its wage taking the offers of all other employers as given.

Workers are either unemployed with value function V_U and flow utility of non-employment b or employed at wage w with value function $V_E(w)$. Wages are taxed at a rate $\tau(w)$. For simplicity of exposition, it is assumed that the labor market under consideration consists entirely of workers who owe no income taxes and may qualify for refundable tax credit so that $\tau(w) \leq 0$. An extension of the model along the lines of Shephard (2017) could include a number of worker types—e.g., single and married, male and female, multiple family sizes—who face different marginal tax rates depending on their household structure.

Unemployed and employed workers encounter new job offers at rates λ_U and λ_E, respectively. Jobs are dissolved at a rate $\delta(w;\tau)$ which depends on the post-tax wage, i.e., wages plus EITC benefits; henceforth this will be written $\delta(w)$. The assumption of a non-constant rate of job destruction—motivated in section 1.3—constitutes the main difference between this model and the canonical BM model. I assume that $\delta(w)$ is a convex and monotonically decreasing function of w, with $\delta'(w) < 0$ and $\delta''(w) > 0$.[18] Workers discount the future at a rate ρ.

The value functions for employed and unemployed workers are:

$$\rho V_U = b + \lambda_U \Big[\int_{\underline{w}}^{\bar{w}} \max\{V_E(x), V_U\} dx - V_U \Big] \tag{B.1}$$

$$\begin{aligned} \rho V_E(w) = (1-\tau(w))w + \lambda_E \Big[\int_{\underline{w}}^{\bar{w}} \max\{V_E(x), V_E(w)\} dx - V_E(w) \Big] \\ + \delta(w) \Big[V_U - V_E(w) \Big] \end{aligned} \tag{B.2}$$

An (implicit) expression for the reservation wage w^* for which $V_E(w^*) = V_U$ can be formulated from equations B.1 and B.2. In what follows, however, I will assume that the statutory minimum wage $\underline{w}$ exceeds the reservation wage and unemployed workers thus accept any job offered to them.

The wage distribution $G(w)$ can be characterized by observing that in a steady state, the

[18] The underlying dynamics of the function $\delta(w)$ is left undefined in this model but could be motivated with an investment function in which investments in household capital are made stochastically at a rate depending on the post-tax wage, along the lines of Flinn, Gemici and Laufer (2017).

number of workers exiting jobs paying w or less must equal the number entering these positions.

$$(1-u)\lambda_E \bar{F}(w) + (1-u)\int_{\underline{w}}^{w} \delta(x)dG(x) = u\lambda_U F(w) \tag{B.3}$$

Taking the derivative with respect to w (using Leibniz's rule) and rearranging yields:

$$\big(\tfrac{u}{1-u}\lambda_U + \lambda_E G(w)\big)f(w) + \lambda_E g(w)F(w) = g(w)\big(\delta(w) + \lambda_E\big) \tag{B.4}$$

Equation B.4 describes a first-order differential equation which can be solved for the offer distribution $F(w)$. The solution is:

$$F(w) = \frac{\lambda_E G(w) + \int_{\underline{w}}^{w} \delta(x)dG(x) + C}{\lambda_E G(w) + \frac{u}{1-u}\lambda_U} \tag{B.5}$$

where C is a constant of integration.

At the maximum offered wage, $\bar{w}$, equation B.3 evaluates to:

$$(1-u)\int_{\underline{w}}^{\bar{w}} \delta(x)dG(x) = u\lambda_U \tag{B.6}$$

$$\implies \frac{u}{1-u} = \frac{\bar{\delta}}{\lambda_U} \tag{B.7}$$

where $\bar{\delta} \equiv \int_{\underline{w}}^{\bar{w}} \delta(x)dG(x)$ is the average rate of job destruction over all wages. Equation B.7, together with equation B.5 evaluated at $w = \bar{w}$, implies that $C = 0$ and thus:

$$F(w) = \frac{\lambda_E G(w) + \int_{\underline{w}}^{w} \delta(x)dG(x)}{\lambda_E G(w) + \int_{\underline{w}}^{\bar{w}} \delta(x)dG(x)} = \frac{\lambda_E G(w) + \int_{\underline{w}}^{w} \delta(x)dG(x)}{\lambda_E G(w) + \bar{\delta}} \tag{B.8}$$

Equation B.8 is a generalization of the BM wage offer distribution. If the rate of job destruction is a constant $\delta(w) = \delta_0$, equation B.8 can be rearranged to form $G(w) = \delta_0 F(w)/(\delta_0 + \lambda_E \bar{F}(w))$, where $\bar{F}(w) \equiv 1 - F(w)$. This is the canonical BM wage distribution. When $\delta(w)$ is instead a function of w, the integrals in equation B.8 prevent a comparable closed-form expression for $G(w)$.

Firms maximize flow profits $\pi(w;F) = (p-w)\ell(w;F)$, where p represents productivity (assumed to be uniform across firms) and $\ell(w;F)$ denotes the steady-state employment level at a firm paying w. Higher wages attract a larger labor force $\ell(w;F)$ at the expense of reducing profits per worker $(p-w)$. As Burdett and Mortensen (1998) showed, the distribution $F(w)$ is devoid of mass points and firms offer an atomless continuum of wages. The steady-state level of employment at a firm paying w is thus:

$$\ell(w;F) = (1-u)\frac{dG(w)}{dF(w)} \tag{B.9}$$

$$= \frac{\lambda_U}{\bar{\delta}+\lambda_U} \times \frac{(\lambda_E G(w)+\bar{\delta})^2}{\delta(w)(\lambda_E G(w)+\bar{\delta}) + \lambda_E(\bar{\delta} - \int_{\underline{w}}^{w} \delta(x)dG(x))} \tag{B.10}$$

Firm employment $\ell(w)$ is expressed as a function of $G(w)$ and $\bar{\delta}$, which is endogenous to $G(w)$. Using equation B.7, the profit function is thus:

$$\pi(w) = (p-w)\frac{\lambda_U}{\bar{\delta}+\lambda_U} \times \frac{(\lambda_E G(w)+\bar{\delta})^2}{\delta(w)(\lambda_E G(w)+\bar{\delta}) + \lambda_E(\bar{\delta} - \int_{\underline{w}}^{w} \delta(x)dG(x))} \tag{B.11}$$

In equilibrium, profits are equalized across firms. At the minimum wage $\underline{w}$ the profit level is:

$$\pi(\underline{w}) = (p-\underline{w}) \times \frac{\lambda_U}{\bar{\delta}+\lambda_U} \times \frac{\bar{\delta}}{\lambda_E+\delta(\underline{w})} \tag{B.12}$$

Thus,

$$(p-\underline{w})\frac{\bar{\delta}}{\lambda_E+\delta(\underline{w})} = (p-w)\frac{(\lambda_E G(w)+\bar{\delta})^2}{\delta(w)(\lambda_E G(w)+\bar{\delta}) + \lambda_E(\bar{\delta} - \int_{\underline{w}}^{w} \delta(x)dG(x))} \tag{B.13}$$

which, after rearranging, provides an implicit equation for the wage offer at every point in the wage distribution:

$$w = p - \frac{(p-\underline{w})\bar{\delta}}{\lambda_E+\delta(\underline{w})} \times \frac{\delta(w)(\lambda_E G(w)+\bar{\delta}) + \lambda_E(\bar{\delta} - \int_{\underline{w}}^{w} \delta(x)dG(x))}{(\lambda_E G(w)+\bar{\delta})^2} \tag{B.14}$$

It follows that the maximum offered wage is

$$\bar{w} = p - (p-\underline{w})\frac{\delta(\bar{w})\bar{\delta}}{(\lambda_E+\delta(\underline{w}))(\lambda_E+\bar{\delta})} \tag{B.15}$$

Although the model does not yield closed-form expressions for the objects of interest—$F(w)$, $G(w)$, and $\bar{w}$—these can be computed using numerical methods given equations B.14 and B.15 as well as values for p, $\underline{w}$, λ_E and the function $\delta(w)$.

For a numerical solution of the model, I first provide a candidate maximum wage $\hat{\bar{w}}$ and a discrete approximation of $g(w)$, denoted $\hat{\mathbf{g}} \equiv [\hat{g}_1 \ldots \hat{g}_N]$. The N points of $\hat{\mathbf{g}}$ correspond to the length-N vector $W \equiv [\underline{w}, \ldots, \hat{\bar{w}}]$ of possible wages. I calculate equation B.15 and N instances of equation B.14, one for each of the N points in the wage distribution. Together, these express $N+1$ unknowns in $N+1$ equations. Note that $\bar{\delta} \equiv \int_{\underline{w}}^{\bar{w}} \delta(x)dG(x)$ depends on $\bar{w}$ as well as $G(w)$

and thus must be separately calculated for each guess of $[\hat{\bar{w}}, \hat{\mathbf{g}}]$. I use a Newton search algorithm to compute the values of the endogenous variables that minimize residuals for equations B.14 and B.15.

To test the validity of the procedure outlined above, I compute solutions of the model in which the job dissolution function $\delta(w) = \delta_0$, which, as noted, leads to the BM result and thus closed-form solutions for $\bar{w}$ and $G(w)$. The algorithm is capable of matching the BM results to an arbitrary degree of precision, even when starting from an initial value of $\hat{\bar{w}} = p$ and a naive initial candidate wage distribution $\hat{g}_i \sim \text{Uniform}(\underline{w}, p)$. Additional assurance comes from the fact that the procedure arrives at distributions $\hat{\mathbf{g}}$ for which $\sum^N \hat{g}_i \approx 1$ despite the fact that the algorithm does not enforce normalization.

I calibrate the model using parameters for λ_U and λ_E as estimated in Bowlus and Seitz (2000) for a basic BM model. I set $\underline{w} = 7.0$ to approximate the U.S. federal minimum wage. I estimate the model for two sets of parameters, scenarios A and B, which vary by productivity p (which acts as a wage ceiling since $\bar{w} < p$) as well as the parameters governing the job dissolution function $\delta(w)$. This function takes the form $\delta(w) = c \cdot Y(w; EITC)^{-d}$, where $c > 0$, $d \geq 0$ and $Y(w; EITC)$ converts the hourly wage w to a post-tax annual wage according to the 2023 EITC schedule, relying on the somewhat unrealistic assumption of full-time work schedules for all workers. In scenario A, $c = 1.85$, $d = 2$ and $p = 16$. In scenario B, $c = 0.9$, $d = 1$ and $p = 12$. Scenario B features a narrower range of potential wages with a dissolution rate that falls less steeply with the wage.

Results from computing the model for both no-EITC and EITC settings for each scenario are listed in Table B.1. As is evident in the first row, the values governing $\delta(w)$ have been chosen to yield $\bar{\delta} \approx 0.01$, or a monthly job dissolution rate of 1%. The EITC reduces the dissolution rate by 11% and 14% in scenarios A and B, respectively. The unemployment rate u is reduced by 10% (from 9.1% to 8.2%) in scenario A and by 16% (from 9.5% to 8.0%) in scenario B. The models differ in how the wage distribution responds. The highest offered wage falls slightly in scenario A while it rises slightly in scenario B. The expected wage in scenario A also falls, from \$14.68 to \$13.90, a 5% reduction. In scenario B the expected wage rises 1% from \$8.63 to \$8.73.

Table B.1: Results of search model computation

	Scenario A		Scenario B	
	No EITC	EITC	No EITC	EITC
$\bar{\delta}$	0.00998	0.00889	0.0105	0.00864
u	0.0907	0.0816	0.0949	0.0796
$\bar{w}$	15.3	15.1	9.35	9.45
$E(w)$	14.7	13.9	8.63	8.73

In these models, the EITC unambiguously reduces job destruction and unemployment but has ambiguous effects on the expected wage. Wages may fall, as in scenario A, analogous to the situation of a shifting supply curve in the standard neoclassical model. Yet wages may also hold constant or even rise, as in scenario B. The outcome depends on the balance of two opposing forces. On one hand, reducing the dissolution rate at the bottom of the wage distribution allows employers in that wage range to hold onto more employees, reducing the expected wage. But diminished job destruction also allows workers to climb the job ladder, increasing the expected wage. In reality, of course, numerous other factors are at play. Yet this model highlights how a plausible mechanism of the EITC can generate outcomes broadly consistent with empirical observations.

B.2 Extensions

So far I have abstracted away from workers' decision to participate in the labor market. Instead, I have shown that the equilibrium effects of the EITC can arise absent *any* extensive margin effects, that is, without any increase in the size of the labor force in response to changes in the tax code. Refundable tax credits can give rise to higher employment and lower pre-tax wages simply through changes in turnover patterns and steady-state wage offer strategies.

Yet this setup almost certainly overstates the degree of informational frictions potential labor market entrants face regarding the availability of EITC benefits. Extensive margin effects may be incorporated within the search model elucidated above by assuming that workers are heterogeneous in the degree to which they value non-participation, denoted V_O. Workers will enter the labor market only when the expected value of search exceeds the costs associated with looking for work.

If workers are uniform in their non-participation value V_O (as I have assumed so far of their flow value of unemployment b), then labor supply responses will be degenerate. That is, either all workers will choose to participate or none will. Allowing for heterogeneity in V_O, as in Flinn (2006), provides a way for the aggregate labor supply to vary with the value of search, which includes the value of receiving EITC refunds.[19]

The model can be further extended to endogenize contact rates λ_U and λ_E using a standard matching technology (Flinn, 2011). The number of matches $M = M(S, V)$ is a function (homogeneous of degree 1) of the effective number of job searchers S and the number of job vacancies V. The effective size of the pool of searchers $S = U + \nu E$ is made up of the set of unemployed workers U and employed workers E whose search effort is some fraction ν that of unemployed workers.

[19] Survey evidence shows that even those who have worked throughout the year and expect to receive tax refunds are highly uncertain about how much they will receive during tax season, although they are broadly correct on average (Caldwell, Nelson and Waldinger, 2023). An alternative specification could incorporate this uncertainty into the model by allowing households to learn over time about the parameters of the distribution governing potential refunds.

Denoting labor market tightness $\theta \equiv \frac{V}{S}$, the contact rate for a searcher is:

$$\lambda(S,V) = \frac{M(S,V)}{S} = \frac{V}{S}M(\frac{S}{V},1) = \theta q(\theta) \tag{B.16}$$

where the function $q(\theta) \equiv M(\frac{1}{\theta},1)$ relates labor market tightness to the rate at which vacancies are filled. Contact rates for employed and unemployed workers are $\lambda_U = \lambda(\theta)$ and $\lambda_E = \nu\lambda(\theta)$. To illustrate the mechanics of the matching function, consider the case of a Cobb-Douglas matching function $M(S,V) = AS^{\alpha}V^{1-\alpha}$, where $\alpha \in (0,1)$, the contact rate is $\lambda(\theta) = A\theta^{1-\alpha}$. Intuitively, a tighter labor market means a higher contact rate for searching workers.

With endogenous contact rates and heterogeneity in the value of participation, an increase in the EITC leads to an increase in the participation rate—since EITC raises the value of search—and thus a decrease in the tightness of the labor market. A looser labor market leads to lower contact rates for both employed and unemployed workers. Following equation B.14, a lower value of λ_E means a lower offered wage at every point in $G(w)$ and a diminished expected wage. The overall effect is analogous to a shifting outward of the neoclassical labor supply curve. In assessing the labor market impacts of the EITC, it would be important to account for both the typical extensive margin effects, which rely on some knowledge of the EITC and its parameters, as well as employment stability effects, which do not. Both pathways contribute to higher employment and (potentially) lower equilibrium wages, but only the employment stability channel reduces the rate of job separations workers and firms experience.[20]

Other extensions of this model could be explored. Currently the only parameter affected by the post-tax wage is the rate of separations. Yet it is plausible that enhanced liquidity from the EITC could also spur more effective job search as workers are able to expand the geographic scope of their job search. Specifying the function $\lambda_E(w;\tau)$ in lieu of the constant λ_E could capture workers' improved on-the-job search due to changes in the tax code.[21] The functional form of $\delta(w)$ could also be applied to the framework of Shephard (2017), one of very few search models to tackle the impact of refundable tax credits (in that case, the empirical application is the U.K.'s version of the EITC). Shephard's model builds on Bontemps, Robin and van den Berg (1999), which features heterogeneity in firm productivity and workers' values of non-employment b. Changes in the tax code induce more labor supply through the extensive-margin pathway: workers place a higher value on search because they are fully informed about the nature of the EITC. It would be informative to augment such a model with the liquidity-driven job-stability pathway conveyed in the functipon $\delta(w)$.

[20] This assumes that workers are homogeneous in their rate of job dissolution given the wage. Yet workers may differ systematically in $\delta(w)$. Extensive margin labor force growth may bring into the labor market workers who tend to be more susceptible to job-ending shocks at a given wage, mitigating the turnover-reducing effects of the employment stability channel.

[21] Although it is plausible that EITC-induced liquidity also enhances search efficiency for unemployed workers, it would be challenging to specify a function like $\lambda_U(w)$ since unemployed workers by definition earn no wages. Such a function would have to refer back to the previous year's earnings, substantially complicating the model.