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

Owen F. Davis*

New York City Independent Budget Office

owdavis@ibo.nyc.gov

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Despite ample evidence that the Earned Income Tax Credit (EITC) boosts labor supply, its underlying mechanisms remain underexplored. This study examines the hypothesis that the EITC supports employment by helping recipients afford personal transportation. I build a simulated instrument leveraging metro-level variation in EITC exposure to compare responses to policy changes between areas with varying commuting characteristics. Estimated employment effects are roughly 20% smaller where public transportation is abundant and 20% larger in car-dependent areas. Additional analyses show significant associations between EITC expansion and car ownership. The paper also presents new evidence on state-level EITCs and the 2009 EITC expansion.

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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 2024, EITC refunds worth $64 billion bolstered the household finances of 23 million families. Literature on the labor-market impacts of the program forms a nearly unanimous consensus that the program'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 2024 totaling nearly $9,000 on earnings of $17,400, these incentives are economically meaningful. Yet surveys find that potential recipients often lack basic knowledge about EITC eligibility and face substantial uncertainty regarding their eventual tax refunds. Motivated by evidence of informational frictions faced by low-income workers, some researchers have raised doubts over the scholarly consensus around the EITC (Mead, 2014; Kleven, 2024).

This paper explores a particular mechanism for the EITC's observed effects: the purchase and maintenance of automobiles used for commuting. Numerous surveys and analyses of consumer spending point to vehicle investment as a key use of EITC benefits. Although some suggestive evidence supports the EITC-transportation channel (Barr, Eggleston and Smith, 2022), this is the first study to examine the mechanism in depth. The central hypothesis is that the EITC operates at least in part by providing lower-wage workers the opportunity to make larger investments in assets that bolster employment stability. A notable feature of the transportation pathway is that it stems from the liquidity impacts of the EITC rather than on the informational channels that have been characterized as behaviorally implausible.

The empirical methodology broadly follows a standard approach, analyzing March Current Population Survey (CPS) data in a framework leveraging exogenous tax-policy variation over time and between family sizes. To isolate the hypothesized transportation mechanism, I test for heterogeneous impacts of the EITC on respondents in communities with differing local commuting characteristics, i.e., high versus low access to public transportation or dependence on cars for commuting. This strategy requires accounting for variation between different areas in exposure to changes in EITC policy. Intuitively, larger EITC responses should be observed where a greater share of workers earn incomes low enough to qualify for the EITC. Because high-public-transit areas also tend to have higher wages, failure to take differential EITC exposure into account might introduce bias when interacting EITC measures with indicators for local commuting characteristics.

To capture underlying geographical variation in EITC exposure I create a simulated instrument based on data collected before the large federal EITC expansions in the 1990s. The instrument projects community-level 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.

The main results show that the estimated effects of the EITC vary significantly according to the abundance of public transportation and local dependence on cars for commuting. Employment responses are roughly 20% smaller in high–public transportation areas and 20% larger in highly car-dependent areas, with similar patterns for hours and weeks worked. These results are robust to a range of specifications. By contrast, I do not find any patterns in the seasonality of EITC employment effects suggestive of immediate liquidity playing an obvious role.

To further probe the EITC-transportation channel I make use of data from the Survey of Income and Program Participation (SIPP) covering vehicle ownership over the time period of interest. Applying the same simulated instrument approach as before, I find a strong and significant association between the EITC and household car ownership: a $1,000 increase in the simulated EITC is associated with a 4.6 percentage point greater probability of household car ownership for unmarried less-educated women, a relationship that varies robustly by local commuting characteristics in the same way as before. Estimates for labor supply outcomes using the SIPP data replicate the results found using CPS, including substantial commuting-related heterogeneity.

In addition to providing a better understanding of familiar extensive-margin effects, this study also presents new evidence on outstanding questions relating to the EITC. 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 community-level heterogeneity. I also estimate the effects of state EITCs on the labor supply of unmarried women using a strategy exploiting the local variation embedded in the simulated instrument. State-specific regressions for states that introduced large EITC supplements yield coefficient estimates roughly in line with those estimated for the federal EITC.

This study contributes to an already sizable EITC literature around the extensive margin effects of the EITC. In their comprehensive review of the literature, Nichols and Rothstein (2016) describe an "overwhelming consensus" that the EITC boosts the employment and labor force participation of single mothers. An early and influential example is Eissa and Liebman (1996), who found that the 1986 EITC expansion increased labor force participation among unmarried mothers by 2.8 percentage points. In a structural model incorporating changes in welfare program generosity, Meyer and Rosenbaum (2001) attributed about 60% of the increase in unmarried mothers' employment during the 1990s to EITC expansions. 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.

More recently, Hoynes and Patel (2018) estimated the total direct and indirect effects of the EITC on poverty, finding that the 1993 expansion reduced post-tax poverty rates and lifted employment by roughly 6 percentage points among unmarried mothers. 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; Kleven (2024) offered a contrary view. Although most studies focus on unmarried mothers, Eissa and Hoynes (2004) found evidence for moderately reduced labor force participation among married women around EITC expansions.

The present study goes beyond estimating labor supply responses to join a smaller body of research exploring the mechanisms through which EITC impacts occur. Notable examples include Michelmore and Pilkauskas (2021), who demonstrated a strong link between the EITC and child care provision, and Wilson (2020), who examined the role of EITC in helping workers weather potentially job-ending adverse shocks. Barr, Eggleston and Smith (2022) presented the first empirical evidence around the EITC and personal transit, an inquiry I develop fully here. This study also makes methodological contributions around the choice of measures for the EITC in empirical applications. The simulated instrument employed here produces more precise estimates and allows for exploration of geographical heterogeneities in EITC responses.

1 The EITC and Personal Commuting

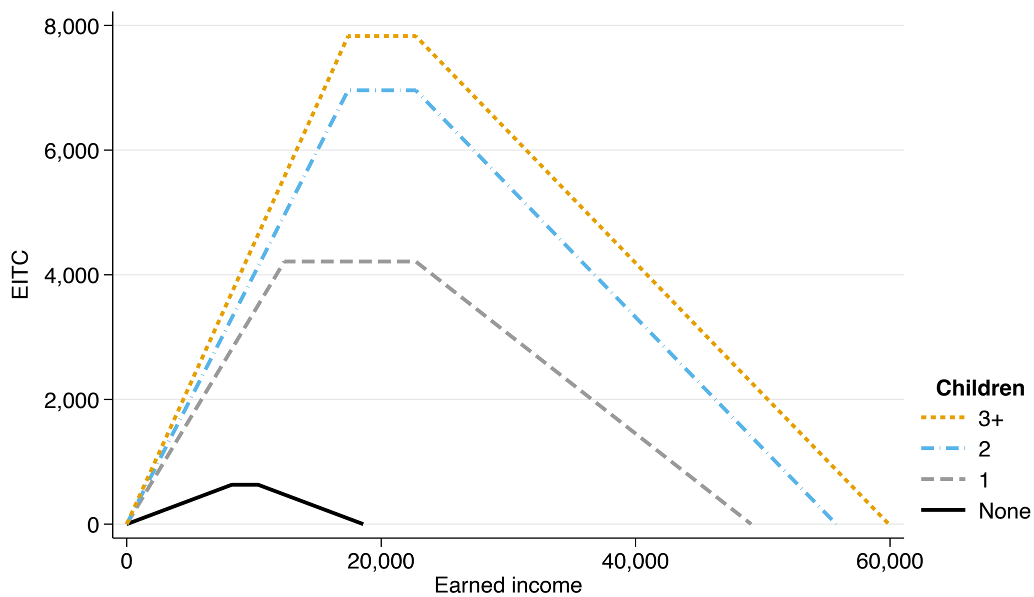
1.1 EITC program history and design

Congress established the Earned Income Tax Credit in 1975 and expanded it in 1986, 1990, 1993, and 2009. The largest and most-studied expansion, passed as part of a larger federal omnibus bill in 1993 and phased in 1994–1996, enlarged refunds to all eligible filers, with an especially large boost to households with two or more children. A 2009 expansion lifted maximum refunds for those with three or more children. 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 fixed percentage to the federal EITC.

Research focused on EITC expansions in the 1990s must take into account the concurrent shift in anti-poverty policy from direct cash assistance to tax credits and in-kind support. The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 implemented work requirements and imposed stricter time limits on cash benefits. In the preceding years, dozens of states received waivers allowing them to restrict welfare eligibility, reforms typically associated with higher employment among unmarried mothers (Ziliak, 2016). Recognizing cars' importance for accessing job opportunities, states also revised welfare asset limits during this time to exempt some or all of a vehicle's value.

The EITC is structured to incentivize work on the extensive margin while minimizing intensive-margin distortions in its phase-out range.[1] The amount of the credit depends on family structure, rising as a percentage of earnings to its first "kink" where it plateaus, then phases out as household earnings or income continue to rise. The result is the trapezoid-shaped schedule depicted in Figure 1. In 2024, a single tax filer with two children was eligible to earn a maximum tax credit of $6,690 if they earned between $17,400 and $22,720. For each dollar of earnings or adjusted gross income (AGI) above $22,720, the tax credit was phased out up to $55,768.[2] The maximum credit for a childless adult between the ages of 25 and 64 was $632, phasing out fully at an AGI of $18,591. Married taxpayers filing jointly face EITC schedules identical to those of single filers except for phase-out thresholds (Tax Policy Center, 2024).

Figure 1: Federal EITC eligibility and benefits schedule, single filers, 2024



Source: Tax Policy Center (2024)

Figure A1 shows total maximum federal and federal-plus-state EITC benefits by number of children, 1989–2019. In 2024, 23 million workers and families received the EITC, with benefits totaling $64 billion (Internal Revenue Service, 2024). The majority of filers are single adults with children, a group that receives about three-quarters of all EITC benefits (Hoynes and Patel, 2018). Because it is a refundable tax credit, the EITC takes the form of cash payments for those

[1] Chetty, Friedman and Saez (2013) found modest intensive-margin responses using a bunching-based research design. Nichols and Rothstein (2016) concluded that intensive-margin elasticities are likely positive though small.

[2] 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 the greater of earned income and AGI, which includes investment income and retirement distributions. Households with investment income above a certain threshold ($11,600 in 2024) are ineligible.

with low or no tax liability. The IRS begins disbursing refunds to filers on February 15 and the bulk of EITC payments arrive in February and March (Wilson, 2020).

1.2 The personal transportation mechanism

Although researchers have studied a multitude of outcomes in connection with the EITC,[3] relatively little work probes the mechanisms by which the EITC increases employment. The question of plausible mechanisms animates some scholarly skepticism around measured EITC impacts. Both Mead (2014) and Kleven (2024) highlighted the informational frictions that prevent workers from internalizing tax incentives (for these authors, estimated EITC impacts in the 1990s are due to the confounding effects of a strong economy and welfare reforms). Up to one-quarter of eligible workers fail to claim EITC refunds (Nichols and Rothstein, 2016; Internal Revenue Service, 2022). Eissa and Liebman (1996, 634) described interviews with potential recipients who displayed "virtually no awareness of the credit." Surveys of adults using nonprofit tax preparation clinics have found low awareness of the EITC and substantial uncertainty over expected refund size (Bhargava and Manoli, 2015; Caldwell, Nelson and Waldinger, 2023).

Yet if the liquidity provided by EITC refunds promotes employment, extensive-margin EITC impacts need not depend on detailed knowledge of tax schedules. As Nichols and Rothstein (2016, 191) write, "it seems plausible given general ignorance about tax 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." For low-earning households, tax rebates can amount to more than one-third of annual income, an amount boosted further by state supplements. Spending out of this windfall, particularly when credit constraints bind, can help maintain the physical and social capital required for parents to retain a foothold in the labor market.[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, he 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). As to whether this finding arose from learning about the EITC or increased liquidity among recipients, the lack of seasonality in the results favored the information channel.

One specific pathway from EITC-driven liquidity to employment is child care. Michelmore and Pilkauskas (2021) used a simulated measure of the EITC in a difference-in-differences frame-

[3]Some outcomes beyond employment 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).

[4]Although EITC recipients face deep uncertainty about future credits, they often treat refunds as a form of forced saving 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).

work to show that employment increases associated with EITC expansions are driven by mothers with the youngest children. Supplemental estimates suggested 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] Past studies have detected increased vehicle investment among low-income workers as a response to other exogenous income increases, including minimum wage hikes (Aaronson, Agarwal and French, 2012) and guaranteed income pilots (Bartik et al., 2024). Yet the transportation channel has been addressed only glancingly in the literature on the EITC.

The most relevant evidence on transportation comes from Barr, Eggleston and Smith (2022), who use birthdays around January 1 as a source of variation in 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 adult earnings and educational attainment. Notably, the study reports 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 strengthen labor market attachment.

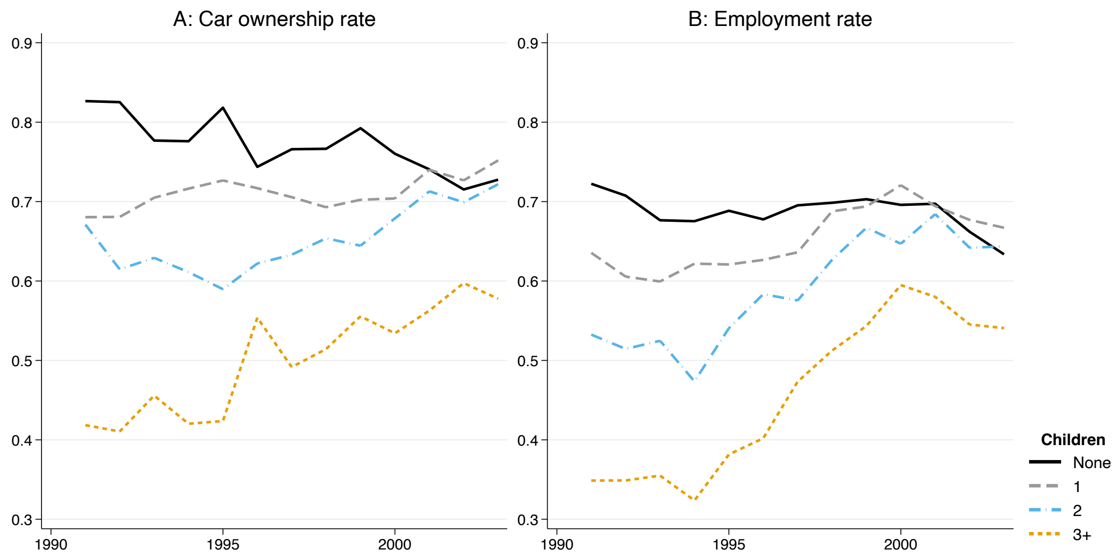
Aggregate trends around the 1990s EITC expansions are consistent with a transportation-EITC pathway. The 1990s witnessed strong increases in both car ownership and employment among unmarried less-educated mothers, as Figure 2 illustrates. These increases were especially brisk among those with two or more children, for whom the 1993 expansion was largest. Although causality between employment and car ownership likely goes both ways, the broad alignment between these two measures over the time period in question justifies further exploration.

Although research exploring the transportation mechanism of the EITC is scarce, a wide range of evidence both qualitative and quantitative emphasizes the importance of automobile spending out of EITC benefits. Drawing on 115 in-depth interviews with EITC recipients, Sykes et al. (2015) noted the frequency of unprompted responses linking EITC refunds to upward

[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.

[6]A low-earning working family with a child born December 31 can claim EITC benefits during tax season two or three 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.

Figure 2: Car ownership and employment of low-education unmarried women, 1991–2003



Source: Panel A from SIPP 1990–2001 panels with asset topical modules (calendar years 1991–2003). Car ownership measured at the household level. Panel B from CPS ASEC 1991–2003. Both samples limited to unmarried women ages 20–50 with educational attainment of a high school degree or less. All dependents in the household ages 18 or younger are considered children.

mobility through the purchase and maintenance of assets, particularly cars. For lower-income households, "building assets—investing in durable goods and building up savings—is usually possible only at tax time" (Sykes et al., 2015, p. 3).

Similarly, in the book-length study of Halpern-Meekin et al. (2015), many interviewees linked refunds to "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. Virtually none of their subjects described altering employment decisions or hours worked in order to maximize tax refunds, a fact attributable both to the complexity of the tax code and to earnings uncertainty (Halpern-Meekin et al., 2015, 85).

Surveys of low-income tax filers often find transportation-related expenses are a large part of planned or realized EITC usage. A pair of surveys conducted in the late 1990s found that roughly one-quarter of respondents planned to use their credit on a vehicle (Romich and Weisner, 2000; Smeeding, Phillips and O'Connor, 2000), a finding reinforced in subsequent research (Linnenbrink et al., 2008; Mammen and Lawrence, 2006). Since transportation shocks are unpredictable, realized automobile spending often exceeds expectations. 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. 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 the EITC-vehicle link. Exploiting seasonal variation in refund timing, Goodman-Bacon and McGranahan (2008) estimated that the EITC drives 35% more monthly spending on vehicles in February for EITC recipients relative to non-recipients; see also Fisher and Rehkopf (2022). Using data from a large used-car dealer, Adams, Einav and Levin (2009) 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) and some dealerships even offer tax preparation services to help with down payments (Tompor, 2019).

An especially apt illustration of the interactions between the EITC and transit can be found in a March 1995 newspaper article on the effects of a strike that shut down Philadelphia's public transit system. The story featured a used-car dealer who sold two inexpensive cars to train commuters soon after the strike began. Quoting the salesman, the reporter noted that 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).

2 Conceptual Framework

The personal transportation mechanism outlined above hypothesizes a channel from EITC refund receipt to vehicle investment to labor supply decisions. To ground ideas, I sketch a stylized fixed-cost model of static labor supply, described fully in Appendix B. In this broad class of models following Cogan (1981), work incurs costs in terms of both money and time. As a result, workers have a reservation number of hours (depending on the wage) which rises with monetary fixed costs but has an ambiguous relationship with time costs.

The model involves two stages that encompass vehicle investment, labor supply, and commuting method. In the first stage workers draw a cost of a vehicle usage from an exogenous distribution representing maintenance needs or the cost of a newly purchased vehicle. In the presence of credit constraints, some workers cannot meet this cost and thus lack personal transportation. The EITC enters here as a source of liquidity that can help workers make down payments, a phenomenon well documented in the literature cited in the previous section. For the sake of simplicity, I present this stage as a simple reduced-form function in which the share of workers owning vehicles, $\theta(\tau)$, varies positively with the generosity of the EITC, parameterized by τ.

The second stage consists of a labor supply model with fixed costs of working. Monetary fixed costs are constant across the population (though could vary without loss of generality). Workers draw two values for time costs, one for public transit and one (if they still have a car) for personal transportation. In a model adapting a typical form in the literature (Keane and Rogerson, 2015), workers choose whether to participate given their vehicle access and time costs.

Workers with access to vehicles choose the optimal commuting method given the two time costs, or else choose not to work. Workers without cars either commute by public transit or do not work. The EITC enters this stage of the model by increasing the post-tax wage and providing a direct inducement to supply labor.

Therefore we have the following: A share $\theta(\tau)$ of car-owning workers choose between driving and public transit. Among these workers, labor supply is determined by the two alternative shocks to the time cost of working associated with either public transit and personal vehicle usage. The remaining $1 - \theta(\tau)$ workers choose whether to participate based only on public-commuting cost shock since driving is not available to them. Intuitively, workers with access to cars are more likely to work since they have a backup option if one of their two cost shocks is too high.

In the absence of any other heterogeneity, it would be challenging to distinguish between the two mechanisms of the EITC's effects on labor supply, the information channel and the liquidity channel. Yet underlying differences between geographical areas in public transportation availability offers a source of identifying heterogeneity. While changes in expected post-tax earnings driven by EITC policy updates should have roughly similar effects everywhere (conditional on wage offers), the personal transportation mechanism will be more pronounced where public transportation options are scarcer. In the model, this heterogeneity is characterized as a greater difference between expected time costs of driving and expected time costs of public transit. Appendix B shows formally how a greater difference between the two commuting cost distributions predicts a larger response to EITC changes in more car-dependent areas.

The model described above provides a simplified picture of what is of course a more complicated set of phenomena. Most importantly, the model abstracts from interdependence between two sets of decisions, asset purchase and employment. Why separate these two decisions into two independent stages? Beyond tractability, there is ample evidence that workers indeed view and use EITC as a savings vehicle for asset purchase as distinct from other consumption spending, as the literature cited in Section 1.2 illustrates.

Behavioral household finance provides a theoretical rationale for treating tax-refund income separately from wage income. An indication of the behavioral nuances shaping EITC usage can be seen in the fact that very few workers took up the advance EITC, an option to receive payments monthly that Congress scrapped in 2010. As Romich and Weisner (2000) argued, this pattern was consistent with three key elements of the behavioral life cycle model: consumers see self-control as a cost (and are willing to pay to avoid exercising it), they engage in mental accounting (treating separate income sources differently), and they are affected by framing (choosing differently depending on context). In this view, the EITC presents an opportunity to build up savings that enter mental accounts as being earmarked for large asset purchases.

A more fully realized behavioral life-cycle model could simultaneously incorporate saving, investment, labor supply, and shocks to the cost of working. Additional desiderata for such a model might include uncertainty around future income, even within the current year, and the behavioral biases cited above. A further consideration might be the timing of decisions; most tax refunds are received in a three-month window around tax filing, yet employment and asset decisions occur throughout the year. Such a model, however, is beyond the scope of this study.

The model used here prompts additional questions. One is whether the personal transportation channel arises only as a reduction in separations to non-employment, given that workers need earnings in the prior tax year to benefit from the EITC. Yet it could be the case that the worker had separated prior to EITC receipt. In practice, low-income workers tend to shift frequently between labor market states. Even those without prior-year earnings, and thus ineligible for the EITC, may benefit when other members of the household share or loan vehicles purchased or maintained with the help of tax refunds.

The model presented here is also silent on the endogeneity of household location decisions to labor market opportunities and transit options. By promoting vehicle ownership and job stability, the EITC might allow households to locate farther from central cities or public transit arteries, decisions that are likely to be affected by neighborhood amenities. The net effect on average transit times may thus be muted, as households that initially saved transit time by switching to car transit opt for more remote (but higher-quality) neighborhoods. A related issue left unaddressed by this model is variation between areas in spatial mismatch between job opportunities and housing options, holding public transit constant. EITC expansions may have greater impacts in cities with more severe spatial mismatch.[7]

3 Empirical strategy

The model outlined above illustrates two mechanisms by which the EITC might affect labor supply: through expectations of larger tax refunds (the information channel) and by expanding access to vehicles for commuting (the liquidity or personal transportation channel). Furthermore, the model predicts that EITC effects are unambiguously larger in areas where public transportation is relatively scarce. The latter prediction provides a practical way to test the personal transportation channel of the EITC in econometric models: interacting EITC measures with indicators for local commuting characteristics.

To proceed, however, it is necessary to address two methodological issues relating to the EITC measure: the timing of policy changes and their magnitude. Empirical studies of the EITC generally take for granted that the EITC schedule of the current tax year—the EITC eligible to *earn*—is the main explanatory variable.[8] Yet taking the liquidity channel seriously introduces additional modeling questions. If households are affected by the receipt of refunds

[7] I thank one of this paper's referees for raising this consideration.

[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).

and not only the expectation of receiving them, then the EITC measure of interest should correspond to the tax year prior to the survey year, or the EITC eligible to *receive*.

A separate issue is the magnitude of the EITC measure. The model assumes that the only important variation between geographies lies in the time cost of public transit relative to time cost of personal transportation. If there is any correlation between transit characteristics and socioeconomic factors related to EITC receipt, this assumption will be violated. As it happens, high-public-transit metro areas such as New York City and San Francisco also have higher housing costs and, consequently, higher wages for low-education workers.[9] As a result, fewer are likely to be eligible for the EITC and those who are eligible may be farther along the phase-out region. This kind of local variation motivates Fitzpatrick and Thompson (2010), who examined heterogeneous impacts of the 1993 EITC expansion according to housing costs, finding higher labor supply responses in the lowest-cost areas.

State-level data provide a clear indication of how underlying differences in relative wages and family structure translate into EITC receipt. In Mississippi, the state with both the lowest median income and highest prevalence of single-parent households, 12% of the population claimed the EITC and the average refund was \$2,962 in tax year 2022. In high-income New Hampshire, 5.1% of the population received EITC refunds, which averaged \$1,966 (Internal Revenue Service, 2022).

In order to capture local variation in EITC exposure, I use a simulated instrument approach similar to that of Michelmore and Pilkauskas (2021) or 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 themselves (Currie and Gruber, 1996). In this case, I use 1990 data on household structure and income to project region-specific EITCs for future years. Construction of this instrument, called $SimEITC$, is detailed fully in the data section below. While past EITC research has utilized simulated instruments (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.

Aside from the novel EITC measures, 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 extend this basic strategy by exploring heterogeneity in employment responses according to regional differences in commuting characteristics.

Equation 1 describes the econometric model for the OLS regressions used to estimate the

[9] 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, less-educated workers must receive higher wages to induce them to live and work in a high-amenity, high-cost city.

effects of the EITC on labor market outcomes Y_{ijst} for individual i in community j, state s, and year t. The first coefficient of interest is β_1, which captures the overall effect of $SimEITC_{ijs,t-1}$, the simulated EITC eligible to receive, calculated with reference to the year $t-1$ tax schedule and specific to family type of person i, economic characteristics of community j, and policy in state s. The second is β_2, the coefficient on the interaction of $SimEITC_{ijs,t-1}$ with $Comm_j$, which is a dichotomous indicator capturing the commuting characteristics of the respondent's community (e.g., whether it has abundant public transportation). It is this relationship that is of primary interest: if the EITC acts in part through the personal transportation channel, β_2 should be negative for high-public communities and positive for high-auto communities.

$$Y_{ijst} = \beta_0 + \beta_1 SimEITC_{ijs,t-1} + \beta_2 SimEITC_{ijs,t-1} \times Comm_j + \beta_3 X_{ist} + \gamma_{js} + \gamma_t + \varepsilon_{ijst} \quad (1)$$

The model uses a wide range of demographic and state controls in X_{ist}. These include 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 seven state welfare-related policies interacted with number of children. All specifications also include state-community and year fixed effects γ_{js} and γ_t.

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 that are correlated with changes in the EITC, for example, unmarried mothers of one child versus unmarried 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.

4 Data

The primary data set is the Annual Social and Economic Supplement of the Current Population Survey (ASEC) via IPUMS (Flood et al., 2022). Also called 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 demographic detail and national coverage. Some supplemental analyses use monthly CPS data.

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. ASEC captures age and relationships for all members of a household, linking parents and children. 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. Table 1 reports summary statistics for the sample while Appendix Table A1 reports selected statistics over time to show how the composition of non-college women 20–50 changes. The most notable trend is a diminishing share of white women within the sample.

Three outcomes of interest are observed weekly (labor force participation, weekly employment, hours worked) and three are observed annually (employment, weeks worked, and pretax earnings). When retrospective annual variables serve as outcomes, I adjust other variables to reflect their values for the prior year, including the age band used to define the sample. Since I explore the use of both current-year and prior-year EITC policies as explanatory variables, it is necessary to adjust the number of children used to calculate the simulated EITC.[10]

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	33.26	10.10	33.20	8.82	32.47	6.79
Black non-Hispanic	0.26	0.44	0.21	0.40	0.28	0.45	0.37	0.48
Hispanic	0.17	0.37	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	25.07	4.81	26.48	4.41	25.50	4.36	21.96	4.45
Maximum EITC, 000s	1.97	2.01	0.32	0.26	3.00	0.70	4.35	1.71
Simulated EITC, 000s	0.97	1.11	0.05	0.05	1.54	0.45	2.27	0.95
In metropolitan area	0.81	0.39	0.82	0.39	0.80	0.40	0.80	0.40
In high-public-transit locale	0.29	0.45	0.30	0.46	0.27	0.44	0.28	0.45
In high-automobile locale	0.21	0.41	0.20	0.40	0.22	0.41	0.21	0.41
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	0.75	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	108,972		54,360		24,890		29,722	

Source: CPS ASEC 1989–2004. Sample consists of unmarried women ages 20–50 with educational attainment of high school or less.
Note: Earnings includes wages and salaries and is zero 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 communities by commute type.

[10] For weekly outcomes, both the current-year and the prior-year (lagged) EITC variables are based on the number of children born by the survey date, since children younger than one year are treated as being born by December 31 of the prior year. But when the outcome is annual 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 in the prior year.

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. I use MSAs rather than commuting zones because the county identifiers necessary to construct the latter are less consistently available between data sets and over time. For those living outside defined metro areas or whose metro areas do not link to the full sample of CPS, I create residual non-metro areas defined at the state level.[11] I use the term "communities" to refer both to these residual regions as well as to officially defined metro areas. MSAs spanning multiple states are defined at the MSA-state level where available. The final data set contains 219 communities.[12]

To test for differential responses to EITC increases by local commuting characteristics, I create indicators capturing the shares of commuters using public transit and personal automobiles. I construct these variables using the 1990 5% Census sample, which asks workers how they commute to work. I restrict the sample to those with a high school education or less and aggregate to communities, as defined above. In the reported models, the variables *high public* and *high auto* indicate whether a respondent's place of residence is in the top quartile of communities by public transportation commuting and commuting by automobile, respectively, similar to Barr, Eggleston and Smith (2022). Appendix Figure A2 depicts the distribution of commuting shares by public transit and automobile in the sample. As is evident, most communities rely heavily on auto transit. Only 11 communities exhibit public commuting shares above 10%.

While it may seem redundant to use both *high public* and *high auto*, given that the two vary inversely, the groupings indeed capture different types of communities. As Appendix Figure A3 shows, not all low-public communities are necessarily high-auto communities; a group of outlier communities records both low public transit shares *and* relatively low auto shares. Representative communities from these three groups—high public, high auto, and the outlier group—are listed in Appendix Table A2.

High-public communities tend to be large cities in the Northeast or West Coast (e.g., New York City, San Francisco, Chicago). Car-heavy communities tend to be small or mid-sized metro areas and rural regions in the South and Midwest (e.g., non-metro Alabama and Flint, Michigan). The outlier group of low-public and relatively low-auto communities, listed in panel C, includes a mix of remote rural areas (e.g., non-metro portions of Alaska and the Dakotas) and those with a large military presence (San Diego). In these places, many residents walk or work from home.

Given the different groupings that arise from splitting the sample based on high-public or high-auto designations, I report results using interactions of the EITC variable with both indi-

[11] 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.

[12] Census redefinitions of MSA boundaries do occur throughout the sample period, the most consequential of which occurs in 2005. Aside from state-level analyses, however, none of the estimation samples used here span the 2005 change.

cators separately. Note that while these groups are roughly stable over time, some communities move into and out of the high-public and high-auto designations between 1990 and 2000, as illustrated in Appendix Figure A4. Roughly 9% of the underlying Census 5% sample used to calculate public and auto shares shifts between categories over the decade. Only one community (Las Vegas, Nevada) shifts from high auto to high public; none moves in the other direction.

The key variable in this study is the simulated instrument reflecting effective EITC receipt defined at the community level. To construct this variable, called $SimEITC$, I begin with the full sample of unmarried female respondents with a high school education or less from the 1990 Census 5 percent Public Use Microdata Sample via IPUMS (Ruggles et al., 2023). I 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).[13]

I use the 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 community $\times$ year $\times$ marital status $\times$ family-size cells. Marital status is either married or unmarried and family size is defined by the number of children (0, 1, 2 or 3+).[14] Finally, these simulated EITC measures are inflation-adjusted to 2019 dollars to produce the SimEITC variable. The result can be thought of as an "effective" EITC in that it estimates the expected EITC benefit that workers in various community-demographic cells would receive. Note that this measure reflects the total credit amount, not the portion that can be returned as a refund. In the simulated population, 57% of EITC recipients owe no federal tax and simulated recipients on average receive 76% of their credit as a refund. Weighting this figure by EITC dollar amount indicates that 86% of EITC dollars are eligible to be refunded in the simulated population (these calculations all use the 1996 tax schedule).

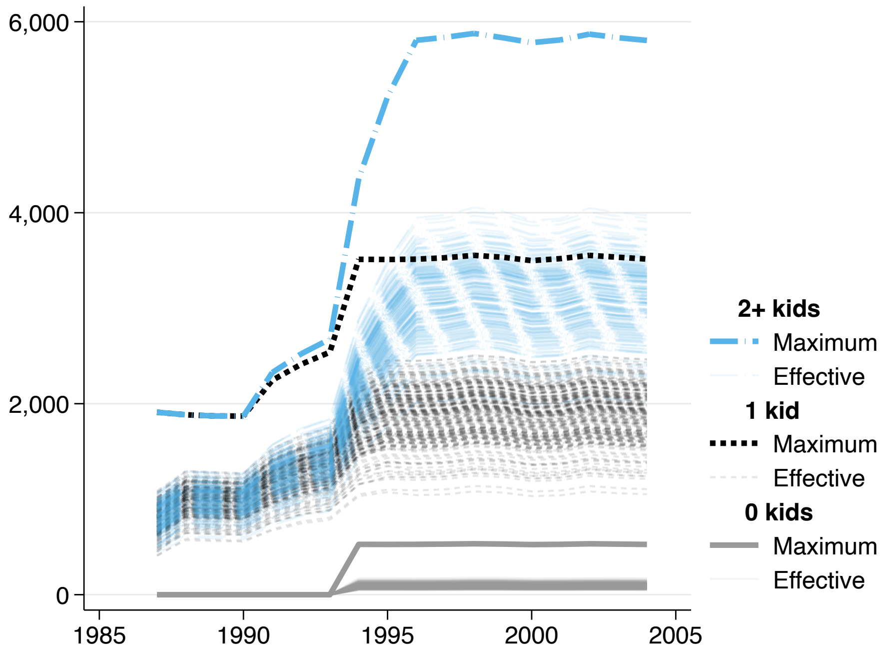
SimEITC sums federal and state EITCs, as in Michelmore and Pilkauskas (2021) and Bastian and Lochner (2022). Including state EITCs may prompt concerns about state-level policy endogeneity (Leigh, 2010; Bastian and Jones, 2021). Yet excluding state EITCs also risks omitted-variable bias. As discussed below, results hold when excluding state EITCs.

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 3 illustrates the high degree of variation between communities. The simulated EITC for unmarried mothers of two in 1996 ranges from $2,007 in the Virginia

[13]For smaller communities, some community-demographic cells have low sample size in the Census 5% sample (for instance, unmarried mothers 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 communities no larger than twice the recipient community's population. I draw enough donors to ensure a sample size of at least 50 for calculation of each community-demographic cell's simulated instrument. Only 0.4% of the eventual sample used to create the simulated instrument consists of these hot-deck imputations.

[14]Collapsing by marital status is unnecessary for most of the analyses reported below, which restrict the sample to unmarried women. Supplemental analyses, however, focus on married women.

Figure 3: 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) by unmarried women 20–50 with at most a high school education. 2019 dollars.

suburbs of Washington, D.C., to $3,932 in Monroe, Louisiana (2019 dollars). The figure also demonstrates that the maximum EITC (heavy dashed lines) significantly overstates the average EITC benefits that members of a family type are likely to receive based on projected earnings, especially for childless single women.

The main regressions also include numerous state-level controls: 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 (2024) and state EITCs are from Komro et al. (2020) cross-checked with Shapiro (2019).

An additional source of policy heterogeneity is state-level variation in welfare policies tied to vehicle values. Prior to PRWORA, welfare eligibility was limited to families owning less than $1,000 in assets, with the first $1,500 of a vehicle's equity value excluded from this limit. Welfare waivers allowed states to relax vehicle exemptions in asset tests. By 2003, 29 states fully exempted the value of one or more vehicles in considering TANF eligibility. Since these policies have been shown likely to affect both employment and car ownership (Rice and Bansak, 2014), I control for state-level variation over time in vehicle-related exemptions.[15]

5 Results

In this section I present results from regressions exploiting the large early-1990s federal EITC expansion and variation from state expansions over the 1990s and early 2000s. First I present preliminary results motivating the choice of the treatment variable and the functional form of the regression model. The next part documents the main results comparing the estimated effects of EITC expansions on communities of differing commuting characteristics. I then describe tests of robustness for the main results before turning attention to supplemental tests of the main hypothesis. Finally I present results using a separate data set to explore vehicle ownership.

5.1 Preliminary results

As a preamble to the main specifications, Table 2 reports results of regressions without EITC-by-commuting interactions building up to the preferred specification, which uses the set of "full controls" indicated in column 6. 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 weekly employment by 5.5 percentage points and annual employment by 6.5 percentage points. The same pattern holds for the other four outcome variables used throughout the study, reported in Appendix Table A4.

The estimates survive a large number of controls, including regressors intended to capture the effects of welfare reforms. The regressions with full controls (column 6) include family and demographic factors, state controls, state linear trends, and reform-kids fixed effects, which interact number of children with six state welfare-reform indicators. The "full" reform-kids specification includes an interacted dummy for whether state welfare policy exempts cars from asset tests. EITC coefficients shrink when reform-kids controls are introduced, including a notable decline in the presence of vehicle-related controls. This is consistent with the idea that the welfare reforms of the 1990s confound estimates of the EITC's effects on labor supply. Robustness exercises discussed below, however, provide assurance that welfare reforms do not drive the results.

To further motivate the preferred treatment variable, lagged SimEITC, Table A3 reports the results of regressions using four separate versions of the explanatory variable: either maximum

[15]The controls are an indicator for full vehicle exemption and a variable reflecting the stringency of asset limits for states not fully exempting vehicle values, defined as the inverse of the exemption threshold value and set to 0 for states with no dollar limit. I do not distinguish between state rules relating to the market value versus equity value of the car. State-level policy variables are drawn from the Urban Institute Welfare Rules Database and Rice and Bansak (2014).

Table 2: The effect of the EITC on labor supply outcomes, various controls

	(1)	(2)	(3)	(4)	(5)	(6)
	A: Weekly Employment					
SimEITC	0.0798***	0.0750***	0.0751***	0.0762***	0.0605***	0.0549***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
R^2	0.0686	0.0829	0.0836	0.0845	0.0853	0.0856
Observations	108,972	108,972	108,972	108,972	108,972	108,972
	B: Annual Employment					
SimEITC	0.0882***	0.0833***	0.0832***	0.0843***	0.0720***	0.0654***
	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
R^2	0.0616	0.0738	0.0744	0.0756	0.0764	0.0768
Observations	105,138	105,138	105,138	105,138	105,138	105,138
Demographics		✓	✓	✓	✓	✓
State controls			✓	✓	✓	✓
State trends				✓	✓	✓
Reform-kids FE					✓	✓
Reform-kids FE (full)						✓

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

Source: CPS ASEC, 1989–2004. Sample is unmarried women ages 20–50 with at most a high school diploma.
Note: Table reports results of regression equation 1, without commuting-type interactions. 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. "Full" includes interactions between number of children and state vehicle exemption policy. All models include state-MSA and year fixed effects. Controls included in column 6 are "full controls." Standard errors are clustered at the community level.

EITC or simulated EITC, each in either its current-year or lagged form (these regressions use the full controls described in Table 2). 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 $t-1$. Across all weekly outcomes, the best-fit models (according to R^2) are those using SimEITC with a lag; for two of the annual outcomes, the contemporary SimEITC has a better fit. It is worth noting that the coefficient estimates on the simulated versions of the EITC variable are larger than corresponding maximum-EITC estimates. This is because, for the average respondent in the sample, the simulated EITC is roughly one-third the magnitude of the maximum EITC.

Although the results presented in Table A3 build confidence in the choice of lagged SimEITC as the explanatory variable, they are not a formal test of the EITC's causal pathways. If EITC effects grow over time, as event-study designs suggest, a lagged EITC variable should be expected to perform well. Still, it is reassuring that the additional variation in the simulated EITC improves model fit, which would not be the case if adjusting the EITC measure for local exposure to policy changes merely added noise.

5.2 Main results

Having established the basic structure of the econometric model, the main results are presented in Table 3. In addition to the full controls from column 6 of Table 2, these models interact commuting characteristics with SimEITC in order to probe for differential responses across communities with different transportation constraints, as formalized in equation 1. Separate columns of the table use separate dependent variables (at both weekly and annual time scales) and the panels denote separate commuting-type indicators applied in separate regressions.

Table 3: Effects of the EITC on labor supply outcomes by local commuting characteristics

	Employed (Weekly) (1)	LFP (Weekly) (2)	Hours (Weekly) (3)	Employed (Annual) (4)	Weeks (Annual) (5)	Earnings (Annual) (6)
	A: High Public Transit					
SimEITC	0.0568***	0.0591***	1.899***	0.0689***	3.197***	1.871***
	(0.006)	(0.006)	(0.248)	(0.007)	(0.294)	(0.275)
SimEITC × high public	-0.0129*	-0.0126*	-0.249	-0.0234***	-0.922**	-0.769***
	(0.005)	(0.006)	(0.199)	(0.006)	(0.283)	(0.222)
R^2	0.0857	0.0742	0.0879	0.0774	0.0999	0.0868
	B: High Auto					
SimEITC	0.0503***	0.0523***	1.712***	0.0600***	2.844***	1.598***
	(0.006)	(0.005)	(0.226)	(0.005)	(0.271)	(0.237)
SimEITC × high auto	0.0122*	0.0132**	0.402	0.0144**	0.576	0.424
	(0.006)	(0.005)	(0.227)	(0.005)	(0.319)	(0.269)
R^2	0.0857	0.0743	0.0880	0.0770	0.0997	0.0866
Observations	108,972	108,972	108,972	105,138	105,138	105,138

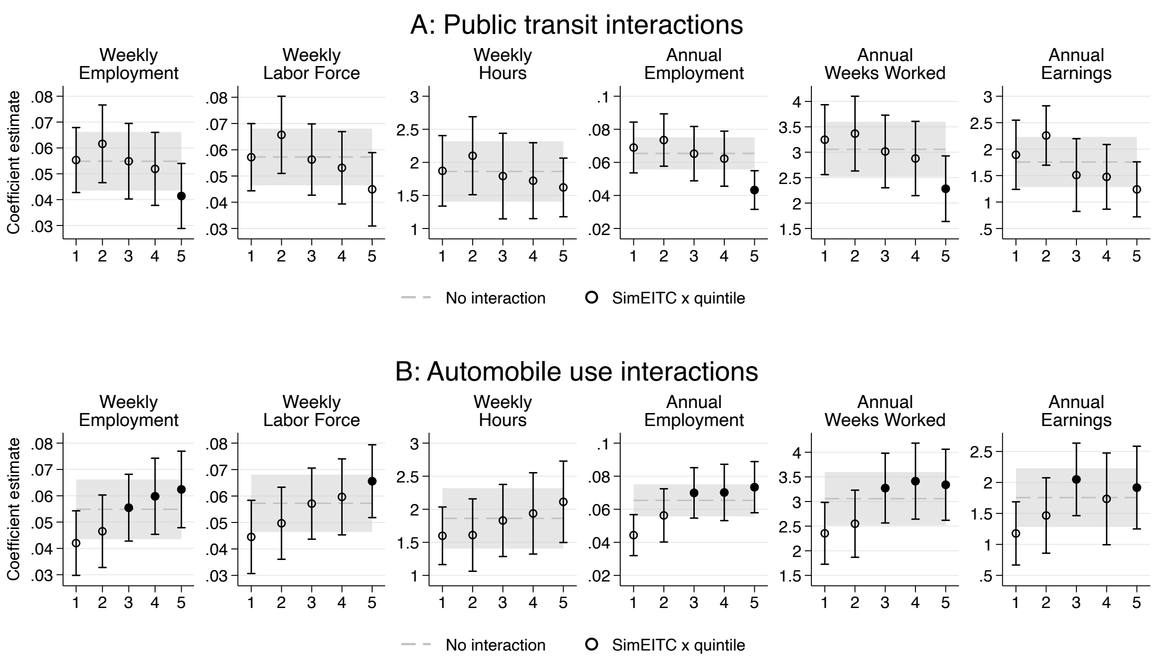
Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS ASEC, 1989–2004. Sample consists of unmarried women ages 20–50 with educational attainment of high school or less.
Note: Table reports results of regression equation 1. All models use lagged SimEITC and full controls as described in Table 2. 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. Earnings and SimEITC are measured in 1,000s of 2019 dollars. Standard errors are clustered at the community level.

The results of Table 3 broadly support the hypothesized transportation mechanism of the EITC's extensive-margin effects. Across dependent variables and model specifications, the relationship of the simulated EITC with labor supply measures remains positive and highly significant, and these estimates vary systematically according to community type. Estimates presented in Panel A indicate that the effect of the EITC on weekly and annual labor supply measures is 20–30% smaller in communities with high public-transit access relative to those without (recall that *high public* indicates the community is in the top quartile by commuting via public transit). A $1,000 increase in the SimEITC boosted weekly employment by 5.7 percentage points in most of the country, but only by 4.4 percentage points in those communities with the most abundant public transportation, a 23% difference.

The results in Panel B yield the same broad takeaways despite using an alternate commuting-type indicator: whether a community is a high-auto area, that is, in the top quartile of communities in commuting by car. The treatment effect on the weekly labor supply measures is significantly greater in highly car-dependent areas, again with differences falling in the 20–25% range. The estimated effect of a $1,000 increase in SimEITC on weekly employment is roughly 6.2 percentage points in high-auto communities versus roughly 5.0 percentage points elsewhere. These patterns hold across all six dependent variables.

Figure 4: Effect of EITC on labor force outcomes by quintiles of commuting characteristics



Note: Point estimates and 95% confidence intervals reflect estimates associated with interactions of SimEITC with quintiles of community commuting characteristics. All models use lagged SimEITC and full controls as described in Table 2. Solid markers indicate $p < 0.05$ that the estimate is equal to that of the first quintile (note that coefficients can be significantly different even when confidence intervals overlap). Dotted line and shaded area depict the point estimate and 95% confidence interval associated with uninteracted SimEITC from baseline regression reported in Table 2, column 6. Earnings and SimEITC are measured in 1,000s of 2019 dollars.

Another way to test whether local commuting patterns moderate the EITC–labor supply relationship is to interact SimEITC with quantiles of commuting characteristics. Figure 4 shows coefficient estimates and confidence bands associated with the interaction of SimEITC with quintiles of public transit and automobile usage. These estimates are overlaid with the baseline (uninteracted) SimEITC estimates, depicted as a dotted horizontal line and shaded region. The estimates for all six outcomes exhibit a mostly monotonic relationship across commuting-type quintiles, falling as public transit increases (panel 1) and rising as auto usage increases (panel 2). The same general pattern holds, though with noisier estimates, when communities are divided into deciles of commuting patterns rather than quintiles, (Appendix Figure A5).

The other major candidate mechanism through which EITC-driven liquidity boosts employment stability is child care. Inspired by Michelmore and Pilkauskas (2021), I interact $SimEITC \times Commute$ with an indicator for the presence of children younger than 5 in order to explore whether the transportation channel operates to a similar degree for mothers with different levels of potential care demands. These results are presented in Table 4. As in Michelmore and Pilkauskas, mothers with young children exhibit a significantly larger labor supply response to the EITC across all of the outcomes, consistent with the EITC helping to relieve care burdens. Heterogeneity in treatment effect by community type does not depend on having young children.

Finally, I run the same models on a sample of married women to examine how different households respond to the EITC across different community types. The results, presented in Table A5, show positive and significant labor supply responses to the EITC among married women with high school education, though the effects are smaller than for unmarried women. This is in line with expectations, both because dual incomes put more households out of the EITC range and because some married women face disincentives to work due to the EITC schedule. Still, the results show substantial heterogeneity between areas with different commuting characteristics, with the effects concentrated in high-auto communities, suggesting that the EITC eases transit-related burdens even among married families.

5.3 Sensitivity

The pattern of results established in Table 3 is robust to various specifications. One concern is that the novel choice of community-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 an indicator $HighExp$ conveying whether a state is in the top half of states by the share of sample population whose income is at or below the plateau region of the 1993 EITC schedule in 1990-1992 (measured using ASEC data). This variable captures some of the variation in EITC exposure contained in SimEITC, though in a coarser fashion. 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.

Table 4: Effects of the EITC on labor supply outcomes by local commuting characteristics—child age interactions

	Employed (Weekly) (1)	LFP (Weekly) (2)	Hours (Weekly) (3)	Employed (Annual) (4)	Weeks (Annual) (5)	Earnings (Annual) (6)
	A: High Public					
SimEITC × (child < 5)	0.0711***	0.0777***	2.381***	0.0751***	3.388***	2.243***
	(0.007)	(0.007)	(0.262)	(0.009)	(0.343)	(0.334)
SimEITC × (child < 5) × high public	-0.0136	-0.0179	-0.275	-0.0323**	-0.859	-1.043***
	(0.008)	(0.010)	(0.266)	(0.010)	(0.436)	(0.288)
SimEITC × (no child < 5)	0.0446***	0.0436***	1.491***	0.0648***	3.059***	1.619***
	(0.006)	(0.006)	(0.287)	(0.007)	(0.299)	(0.271)
SimEITC × (no child < 5) × high public	-0.0125**	-0.00864*	-0.237	-0.0174***	-0.967***	-0.585*
	(0.005)	(0.004)	(0.187)	(0.005)	(0.286)	(0.281)
	B: High Auto					
SimEITC × (child < 5)	0.0640***	0.0676***	2.160***	0.0628***	3.030***	1.795***
	(0.007)	(0.007)	(0.256)	(0.006)	(0.312)	(0.274)
SimEITC × (child < 5) × high auto	0.0137*	0.0211**	0.498	0.0208**	0.640	0.874*
	(0.006)	(0.007)	(0.256)	(0.008)	(0.383)	(0.353)
SimEITC × (no child < 5)	0.0385***	0.0393***	1.329***	0.0581***	2.711***	1.462***
	(0.006)	(0.006)	(0.264)	(0.005)	(0.291)	(0.259)
SimEITC × (no child < 5) × high auto	0.0112	0.00748	0.333	0.0105	0.539	0.147
	(0.006)	(0.006)	(0.246)	(0.006)	(0.337)	(0.278)
N	108,972	108,972	108,972	105,138	105,138	105,138

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

Source: CPS ASEC, 1989–2004. Sample consists of women ages 20–50 with educational attainment of high school or less.
Note: Table reports results of regression equation 1 with triple interaction of SimEITC–child-age–commute-type. All models use lagged SimEITC and full controls as described in Table 2. 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. Earnings and SimEITC are measured in 1,000s of 2019 dollars. Standard errors are clustered at the community level.

Table A6 reports the results of the maximum EITC–exposure–commuting interactions. Panel A shows significantly larger coefficients on $MaxEITC$ in high-exposure states, as expected. The triple interactions with high-public-transit indicators (Panel B) show that among low-exposure states, high-public-transit communities exhibit a smaller combined EITC effect. Heterogeneity by commuting characteristics also holds in high-exposure states, though the differences are less precisely estimated. Similar patterns are evident the results using interactions with low and high auto indicators (Panels C and D). Overall, the results echo the main set of results though with less consistency and precision across models.

Another concern has to do with potential confounding from unobserved local heterogeneity. High-transit areas may differ systematically in public amenities, government programs, or other factors that mediate the EITC–employment relationship. To test for such confounding, I construct five additional measures at the community level: two capturing public housing access, one measuring public kindergarten availability, one reflecting child care costs, and a final variable indicating the share of local employment in manufacturing. Each measure captures a potentially meaningful mediator of EITC impacts: public housing alleviates household budget pressures, schooling and child care free up maternal time, and the manufacturing share may proxy for local economic trends affecting non-college workers. Data for the covariates is selected to pre-date the mid-1990s EITC expansion as much as possible.[16]

To ensure the local confounders described above do not affect the main results, I repeat the baseline SimEITC–commuting regressions of Table 3, adding interactions between SimEITC and the additional covariates (all of which are all standardized continuous variables). The results, plotted in Appendix Figures A6 and A7, show that the coefficient estimates associated SimEITC and its interactions with *high public* and *high auto* remain stable across all covariate specifications. In each case, the coefficient associated with the potentially confounding covariate is small and precisely estimated. The only covariate-SimEITC interaction statistically different from 0 in most cases is that associated with public housing units. The negative (though small) coefficient on this interaction aligns with intuition: access to public housing relieves household budget constraints, dampening the liquidity effect of tax refund receipt.

Next I address potential confounding from welfare reforms in the 1990s. Table A7 shows results of regressions excluding 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 eschewed welfare waivers in the 1990s—the coefficients on SimEITC remain comparable in magnitude and precision while heterogeneities by commuting characteristics follow the same pattern as in the main results. A separate concern is that the 1996 national welfare reform legislation PRWORA drives the results. Results in Table A8 use a shortened sample, 1989–1995, as in Bastian and Jones (2021).[17] The results largely mirror the main results. As a final check for welfare-reform-related effects, Table A9 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.

One might also worry about potential endogeneity in state-level EITC policy (a concern discussed in more detail in Section 6.2). To ensure that such endogeneity does not drive treatment-effect heterogeneity across community types, Table A10 reports results using the federal-only simulated EITC. Other than being slightly larger in magnitude than estimates using the state-plus-federal treatment, the results are virtually equivalent. The difference in magnitudes between federal only and state-plus-federal EITC estimates—a pattern also present in Bastian and Jones (2021) and Schanzenbach and Strain (2021)—likely reflects omitted-variable bias in the estimates obtained from using only the federal EITC. For states offering EITC top-ups, federal expansions

[16]Details on variable construction are provided under Appendix Figures A6 and A7.

[17]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 but includes four years of EITC variation, including the post-1990 increases and the first year of the post-1993 increase.

feed through directly to higher state benefits and, presumably, to labor supply responses. State EITCs are positively correlated both with the outcomes of interest and the explanatory variable, which should bias the federal-only coefficient estimate upwards.

Finally, Table A11 reports results from a quasi-placebo, restricting the sample to women with a college degree. Though the uninteracted coefficient estimates are still positive, they are smaller in magnitude and less precisely estimated. This is in line with what we would expect from a higher-educated group whose earnings more often place them in the phase-out region of the EITC schedule or higher.

5.4 Seasonality

If the EITC acted through increased liquidity and vehicle purchase, we might expect to see a larger EITC effect in the early months of the year, particularly for those communities with higher rates of car usage. The assumption is that the increased labor supply from a newly purchased or repaired car is concentrated in the months around EITC receipt. This may not be the case, for instance, if EITC recipients 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 larger refunds could allow recently unemployed workers to search longer (LaLumia, 2013).

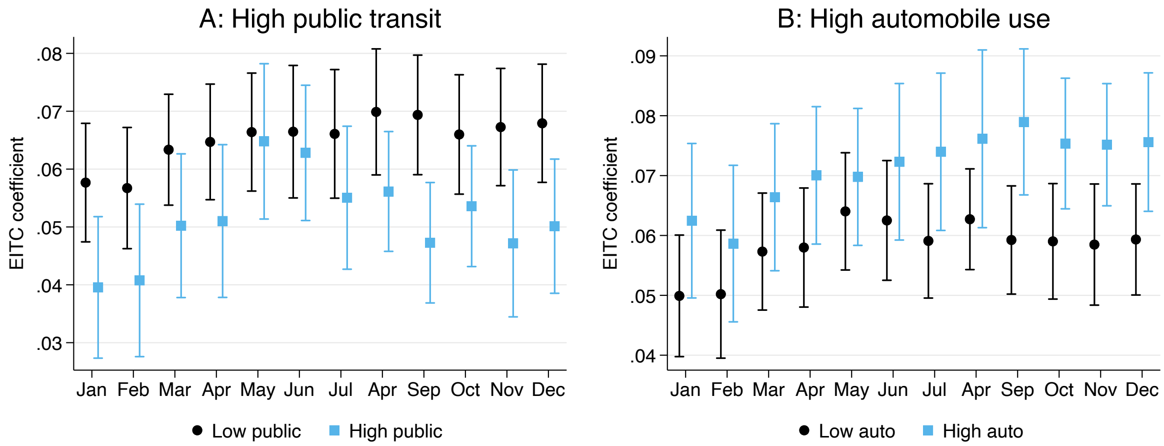
To explore seasonality in EITC responses I turn to the monthly CPS.[18] Figure 5 displays the results of regressions using the triple interaction of SimEITC, commuting characteristics and calendar month. The regressions also include fixed effects for the interaction of calendar month and presence of a school-age child (ages 5–18), capturing influences of the K-12 calendar on female labor force participation (Price and Wasserman, 2023). Recall that *high auto* and *high public* indicate a community is in the top quartile by each transit type; thus *low auto* $\neq$ *high public* and vice versa.

None of the EITC-by-month-by-commuting type estimates exhibit any conspicuous 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). For the high–public transit side, the results show a divergence between EITC responses in different community types in all but the summer months. This possible 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 less during the summer break, when parents everywhere need to find child care arrangements.

There is a slightly different picture when high-auto interactions are used in the second plot of Figure 5. 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 (and statis-

[18] This sample is defined equivalently to the ASEC sample except that three months of observations in 1994 are dropped due to a change in survey design that required geographic identifiers to be dropped.

Figure 5: Effect of EITC on employment by month and local commuting characteristics



Note: 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 2) as well as controls for (any school-age child) × month. Standard errors are clustered at the community level.

tically significant) throughout the rest of the year. This is not the pattern that would be expected if EITC-driven vehicle investments produced immediate labor supply effects, though they could be consistent with vehicle investments made around tax season helping keep cars running throughout the year.

5.5 Car ownership

The results up to this point approach car ownership indirectly, inferring the importance of the personal-transportation mechanism from heterogeneity in coefficient estimates across different community types. In this section I explore the EITC-car link directly using two separate data sources: the Survey of Income and Program Participation (SIPP) and Census 5% samples. Select waves of multi-year SIPP panels between 1990 and 2001 include topical modules asking respondents about asset ownership, including vehicles in the household. I link responses from these modules to core waves in order to create household-level measures of car ownership.[19]

I employ the same models as described in the main empirical section, with minor adjustments, for the SIPP car ownership data. SIPP identifies a different set of metropolitan areas and in earlier years groups together smaller states, making small-state identification impossible. I exclude these nine smaller states[20] and define a set of 128 communities—metropolitan areas and residual non-metro state areas—consistent across the 1990–2001 panels and present in the 1990

[19]The SIPP vehicle data comes from waves 4 and 7 of the 1990, 1991, 1992, and 1993 panels; waves 3, 6, 9 and 12 of the 1996 panel; and waves 6 and 9 of the 2001 panel. Respondents are asked about employment outcomes for the prior four months; I use data pertaining only to the most recent month at interview time. In later panels metropolitan-area identifiers are suppressed. The final sample covers calendar years 1991–2003.

[20]They are Alaska, Idaho, Iowa, Maine, Montana, North Dakota, South Dakota, Vermont and Wyoming.

Census 5% sample. I re-estimate the simulated EITC and transportation measures from Census 5% sample for the SIPP-specific communities.

The dependent variables of interest are whether the household owns a car, the number of cars owned, and the estimated value of the most valuable car in the household (1,000s of 2019 dollars, $0 for households without a car). I also construct three labor supply variables: monthly employment, denoting any paid work in a month; usual weekly hours at the job with the most hours in a month; and total pretax monthly wage and salary earnings. Regressions on these outcomes use the entire available SIPP sample, not just those waves with topical modules.

Table 5 reports the results of regressing SIPP vehicle-related and labor supply outcomes on lagged SimEITC, together with the full set of controls outlined in Table 2. The first three columns of Panel A indicate a strong association between EITC levels and car ownership among the sample of non-college unmarried women. A $1,000 increase in the simulated EITC is associated with a 4.6 percentage point increase in household car ownership, 0.1 additional cars in the household, and a $715 increase in the value of the primary vehicle. Panel B shows that EITC effects for car ownership are significantly attenuated in high-public-transit communities. There is no significant treatment effect heterogeneity for the two other vehicle-related outcomes (number of cars and car value) nor when comparing high-auto communities to others (Panel C).

To further validate the main CPS results, Table 5 also reports regressions using labor supply outcomes: whether employed at all in the prior month, usual weekly hours at the primary job, and total monthly earnings. In all cases the EITC is associated with large and significant increases in the labor supply outcomes. Heterogeneity by community type follows the established pattern and is broadly statistically significant. This consistency between these findings and those using the March CPS lends credence to the main results. Additionally, I use SIPP data to repeat four of the other main specifications from above: interactions by commuting quintiles (Figure A8), results for married women (Table A12), interactions with having young children (Table A13), and results for women with college degrees (Table A14). The patterns in labor supply outcomes broadly replicate in the SIPP data, while the EITC–car ownership relationship is attenuated for married women and those with college degrees.

Additional evidence around car ownership can be gleaned from the Census 5% samples in 1990 and 2000. These surveys asked employed workers about work commutes and time in transit (recall that the public and auto indicators are derived from these survey items). Because these responses are necessarily missing for non-workers, I restrict the sample to those who are working, otherwise defining the sample equivalently to the other analyses. Sample inclusion is therefore affected by the treatment, raising the risk of selection bias. In the absence of a valid instrument for employment, the results presented here should be interpreted as merely a first pass at assessing the role of the EITC in transportation choices.

Table 6 reports estimates from models exploring the association between the simulated EITC and two outcomes: whether the worker reports driving to work and reported transit time in

Table 5: Effects of the EITC on car ownership and labor supply outcomes by local commuting characteristics

	Owns Car (1)	Number of Cars (2)	Primary Car Value (3)	Employed (Monthly) (4)	Hours (Weekly) (5)	Earnings (Monthly) (6)
	A: No Interaction					
SimEITC	0.0463***	0.0987***	0.715***	0.0789***	2.779***	0.112**
	(0.012)	(0.025)	(0.191)	(0.011)	(0.430)	(0.040)
R^2	0.1836	0.1686	0.1326	0.0953	0.0914	0.1023
	B: High Public					
SimEITC	0.0458***	0.0984***	0.715***	0.0788***	2.775***	0.111*
	(0.011)	(0.027)	(0.191)	(0.012)	(0.452)	(0.043)
SimEITC × high public	-0.0285***	-0.0200	-0.00252	-0.0144*	-0.534	-0.0777***
	(0.006)	(0.023)	(0.145)	(0.007)	(0.277)	(0.021)
R^2	0.1843	0.1686	0.1326	0.0955	0.0916	0.1027
	C: High Auto					
SimEITC	0.0460***	0.0880**	0.788***	0.0702***	2.360***	0.0726
	(0.012)	(0.028)	(0.185)	(0.011)	(0.445)	(0.040)
SimEITC × high auto	0.000427	0.0216	-0.147	0.0187*	0.900**	0.0842**
	(0.008)	(0.016)	(0.116)	(0.007)	(0.324)	(0.026)
R^2	0.1836	0.1687	0.1327	0.0957	0.0919	0.1029
Observations	32,685	32,685	32,685	123,675	123,675	123,675

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

Source: SIPP panels 1990–2001 (calendar years 1991–2003). Columns 1–3 restricted to waves with asset topical modules. Sample consists of unmarried women ages 20–50 with educational attainment of high school or less.
Note: Table reports results of regression equation 1. All models use lagged SimEITC and full controls as described in Table 2. 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. Earnings, primary car value, and SimEITC are measured in 1,000s of 2019 dollars. Car ownership variables are defined at the household level. Primary car refers to the most valuable car in the household. Monthly employment denotes any paid work in a month. Hours refers to usual weekly hours at the job with the most hours in a month. Standard errors are clustered at the community level.

minutes. The estimates show a positive and marginally significant relationship between the EITC and driving: $1,000 of simulated EITC generates a roughly 0.4 percentage point increase in commuting by personal vehicle, relative to a baseline of 81% of the sample driving to work. For variation by community type, it is not possible to reject the null.

The association with transit time is less precisely estimated and not significantly different from zero. It is likely that multiple factors influence transit time outcomes. The EITC may help some commuters substitute from public transportation to driving, cutting commute times (in this sample, commutes average 20 minutes for drivers and 40 minutes for public commuters). With the data at hand, however, it is not possible to distinguish between this kind of substitution and

Table 6: Effects of the EITC on commuting method and time by local commuting characteristics

	Drives to work (1)	Drives to work (2)	Drives to work (3)	Transit time (4)	Transit time (5)	Transit time (6)
SimEITC	0.00394	0.00413*	0.00403	-0.0158	-0.0332	-0.0119
	(0.002)	(0.002)	(0.002)	(0.105)	(0.101)	(0.110)
SimEITC × high public		-0.00179			0.164	
		(0.003)			(0.124)	
SimEITC × high auto			-0.000260			-0.0108
			(0.002)			(0.084)
R^2	0.1469	0.1469	0.1469	0.0607	0.0607	0.0607
Observations	692,841	692,841	692,841	692,841	692,841	692,841

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

Source: U.S. Census 5% samples, 1990 and 2000. Sample consists of working unmarried women ages 20–50 with educational attainment of high school or less.
Note: Table reports results of regression equation 1. All models use lagged SimEITC and full controls as described in Table 2 as well as indicators for vehicle asset limits in state welfare policies. 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. SimEITC is measured in 1,000s of 2019 dollars. Transit time measured in minutes. Standard errors are clustered at the community level.

changing composition of workers in the employed sample. For instance, increased EITC refunds could allow marginal workers with particularly long commutes to participate, raising estimated commute times.

6 The 2009 EITC Expansion and State EITC Supplements

Having established the viability of the simulated instrument approach, it is worthwhile to examine further applications of this methodology to the EITC. This section explores 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.

6.1 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; Michelmore and Pilkauskas, 2021). 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). Diverging from those contributions, event-study approaches in both Kleven (2024) 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.

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 community-specific SimEITC measure. I also recalculate the indicators for local commuting characteristics. Using these updated variables I repeat the main regressions for the period 2005–2019. 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 regressions use the full controls outlined in Table 2, but with two exceptions: I substitute monthly date fixed effects for year fixed effects and reduce the number of welfare-related indicators interacted with number-of-kids dummies to one, whether vehicles are exempted from asset tests, since 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, 2024).

The results of these regressions, reported in Table 7, provide qualified support for a labor supply effect in the most car-heavy areas. In the broader sample, including both mothers and non-mothers, estimated labor supply responses are positive and significant, with heterogeneity by commuting types following the familiar pattern. Yet when the sample is restricted to mothers in order to better isolate the policy effects, which are concentrated on those with 3 or more children, the SimEITC coefficient estimates fall and standard errors rise. Heterogeneity by commuting type carries the same signs in the restricted sample, but estimates for $SimEITC \times high\ auto$ are significant only for employment and labor force participation; the sum of coefficients is not statistically different from zero for any of the outcomes. All coefficient estimates are small relative to those estimated for the 1990s expansions.

These results suggest that the labor supply effects of the 2009 EITC expansion were felt, if at all, in those areas where workers depend most on commuting by car. The fact that the broad pattern of estimates matches those of the models focused on the 1990s lends additional, if modest, support to the transportation-mechanism hypothesis.

6.2 State EITCs

States have implemented supplements to the federal EITC since the 1980s, yet there remains uncertainty 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

Table 7: Effects of the 2009 EITC expansion on labor supply outcomes by local commuting characteristics

	Employed (Weekly) (1)	LFP (Weekly) (2)	Hours (Weekly) (3)	Employed (Weekly) (4)	LFP (Weekly) (5)	Hours (Weekly) (6)
	Sample: 0+ children			Sample: 1+ children		
	A: High Public Transit					
SimEITC	0.0171**	0.0176**	0.893***	0.00323	0.000619	0.553
	(0.006)	(0.005)	(0.220)	(0.010)	(0.009)	(0.364)
SimEITC × high public	-0.00381	-0.00261	-0.168	-0.00627	-0.00552	-0.241
	(0.002)	(0.002)	(0.087)	(0.006)	(0.006)	(0.203)
	B: High Auto					
SimEITC	0.0126*	0.0136*	0.758***	-0.000729	-0.00329	0.423
	(0.006)	(0.005)	(0.208)	(0.010)	(0.009)	(0.355)
SimEITC × high auto	0.00806**	0.00701**	0.260*	0.0143*	0.0164**	0.348
	(0.003)	(0.003)	(0.120)	(0.006)	(0.006)	(0.209)
Sum of coefficients						
SimEITC + EITC × high auto	0.0207**	0.0206***	1.017***	0.0136	0.0132	0.771
	(0.006)	(0.006)	(0.240)	(0.010)	(0.010)	(0.396)
Observations	819,708	819,708	819,708	366,859	366,859	366,859

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

Source: Monthly CPS, 2005–2019. Sample consists of unmarried women ages 20–50 with educational attainment of high school or less. In columns 4–6 the sample is restricted to women with at least one child.
Note: Table reports results of regression equation 1. All models use lagged simulated EITC and full set of controls as described in Table 2 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 in 2000. Standard errors are clustered at community and individual.

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 (2024) failed to find any effect of state EITC implementation in a synthetic-control event-study framework. In Wilson (2020), including state EITCs in a total-EITC measure altered the interpretation of key results.

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 helps mitigate concerns over policy endogeneity since it avoids including control states that are not subject to unobserved factors 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. Although estimating state EITC effects independently for different states reduces sample size dramatically, this strategy is made viable by the use of within-state regional variation: not only is there variation between family sizes in state EITC receipt, but also between communities with varying levels of exposure to the statewide expansion. Leveraging this source of variation provides an alternative to event-study strategies that treat state EITCs as dichotomous events.

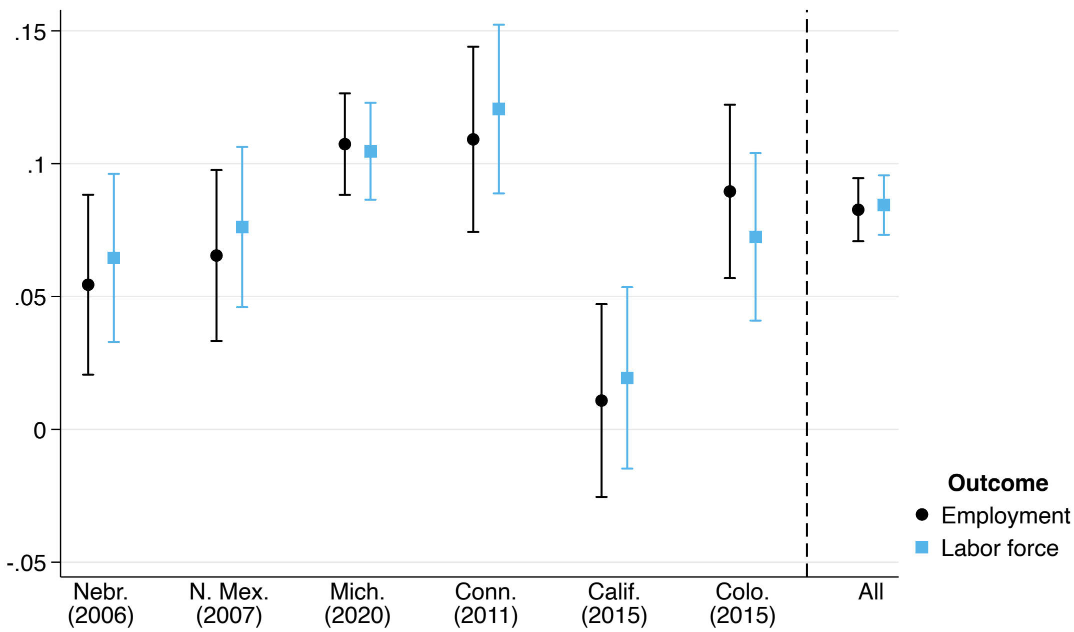
In choosing which state EITC expansions to examine, I focus only on those occurring after 2000 in order to avoid contamination from the major federal EITC expansion of the 1990s. I also include only those implementations that offered refundable tax credits and whose supplements rose to at least 10% of the federal EITC within five years. This leaves six expansions: Nebraska (2006), New Mexico (2007), Michigan (2008), Connecticut (2011), California (2015) and Colorado (2015).[21] For each state expansion the sample includes five years prior to state EITC expansion and seven years after (where available), 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 outlined in Table 2, excluding state-level controls. In line with the approach to the 2009 expansion, the EITC variable is the simulated total (state plus federal) EITC with donors for the simulation drawn from the 2000 5% Census sample.

Figure 6 plots the coefficient estimates on SimEITC for each of the state-level regressions and for both outcomes (weekly employment and labor force participation). For all states but California, the coefficients on the SimEITC are positive and significant, falling broadly within the range of the baseline estimates. The rightmost coefficient estimates aggregate the state-level estimates using inverse-variance weighting. These estimates, both around 0.08, are higher than the corresponding nationwide estimates obtained around the 1990s expansion, which are closer to 0.06. This discrepancy could be at least partly explained by state supplements prompting increased participation in the federal EITC (Neumark and Williams, 2020). The low and imprecisely estimated coefficients for California may relate to the unusual design of the state's EITC supplement, whose generosity peaks and phases out more quickly than the federal EITC, thus reaching only a subset of federal EITC beneficiaries (Neumark and Li, 2024).

A further test of both state EITC implementation and the transportation mechanism is to interact state-level SimEITC with commuting characteristics for the three states home to high-public-transit communities among those with large post-2000 EITC implementations: Connecticut, California and Colorado. In none of theses cases, however, are the interaction terms significantly different from zero (results available on request). Taken together, the state-specific results further confirm the labor supply effects of the EITC but provide no additional evidence for the transportation hypothesis.

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

Figure 6: Coefficient estimates for state-specific EITC expansion regressions



Note: Coefficient estimates and 95% confidence intervals associated with SimEITC from state-specific models with monthly CPS data. Regressions control for family and demographic variables listed in Table 2 as well as monthly date and community fixed effects. Samples span from five years prior to state EITC implementation to seven years after. Standard errors clustered on individuals.

7 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 channel: 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 5.7 percentage points in most communities, but only 4.4 percentage points in those areas with the most abundant public transportation—a statistically significant 23% difference in effect size. Similarly, the weekly employment effect of the EITC is roughly 24% higher in those metropolitan areas and rural regions with the greatest dependence on cars (6.2 percentage points vs 5.0 percentage points). These results carry over across other outcomes, including hours worked and annual weeks worked, and hold up to a range of alternative specifications. The findings replicate in a separate nationally representative data set, the Survey of Income and Program Participation, where I also document a significant relationship between the EITC exposure and car ownership.

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 and family types—have two main benefits. First, they make it possible to estimate heterogeneous effects of the EITC between different types of communities without picking up the confounding effects of underlying income differences between communities (and thus differences in exposure to the EITC). These simulated instruments also allow for more precise estimates of employment responses across the U.S. and within states.

This improved empirical strategy 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 the results for the 2009 expansion 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 transportation-mechanism hypothesis.

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. Additionally, the main results find no support in supplemental analysis into the seasonality of employment responses to increased EITC benefits that would correspond to increased liquidity around tax season helping recipients immediately secure transportation and get to work. Finally, I do not observe the same households over time to fully trace the causal pathway under consideration.

This study informs future research exploring the mechanisms behind the labor supply effects of the EITC observed empirically. 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 entrants 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.

This research also carries potential policy implications. One interpretation for 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 unreliable used cars until they ultimately (and unexpectedly) break down.

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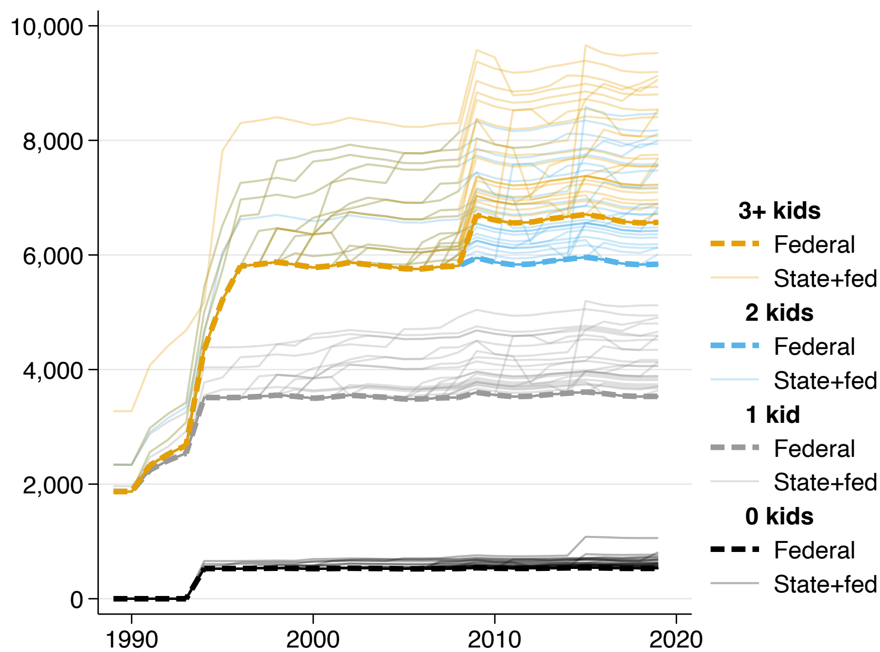
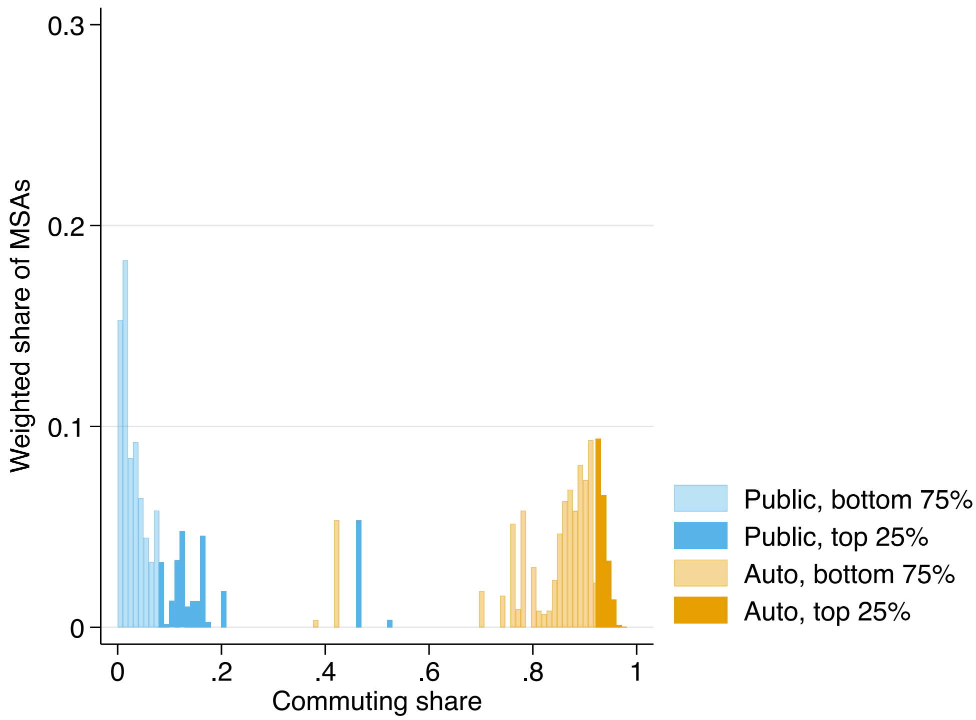
A Additional Figures and Tables

Figure A1: Maximum real federal and state-plus-federal EITC benefits



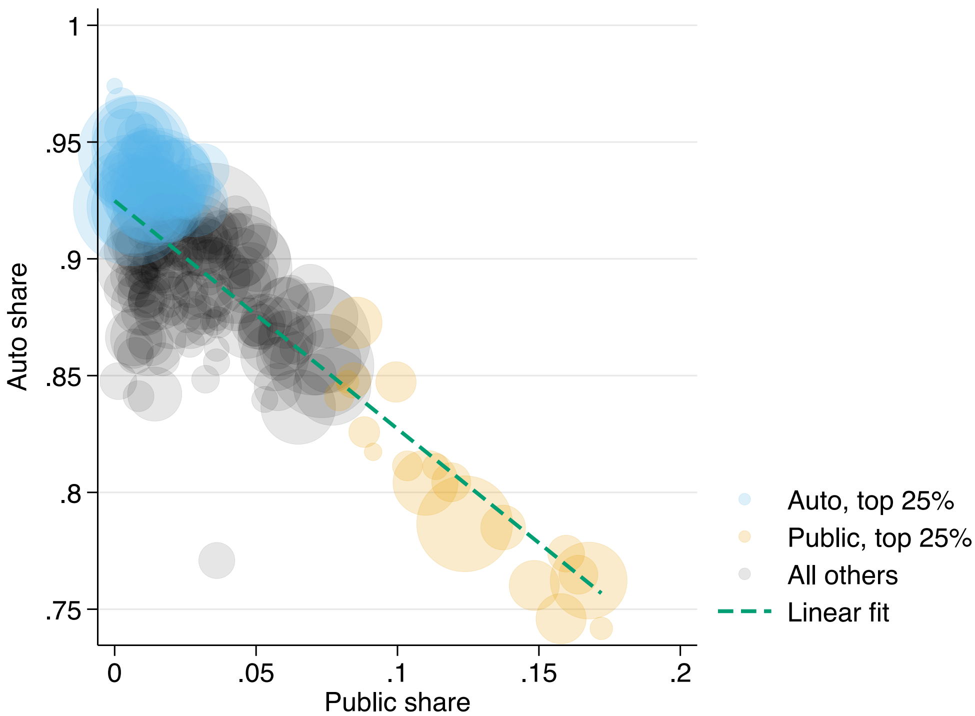
Note: State EITCs include only those state tax credits that are fully refundable. State EITC histories from Komro et al. (2020) and Shapiro (2019). Amounts expressed in 2019 dollars.

Figure A2: Distribution of communities by commuting share



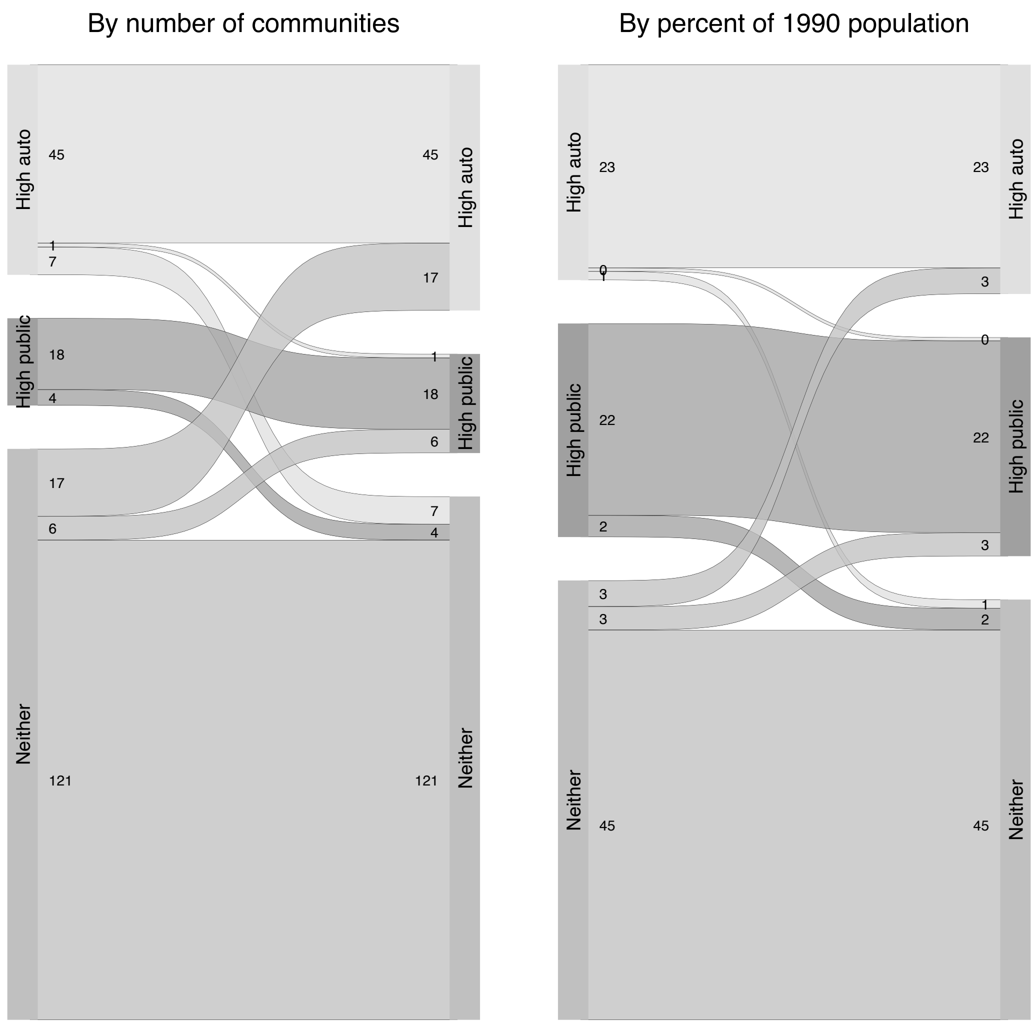
Note: 1990 5% Census sample, IPUMS. Histograms depict the share of communities in each bin of commuting share by type of commute. Commuting shares calculated as the share of workers with at most a high school education who either commute by public transit or by automobile. Communities are weighted by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

Figure A3: Relationship between public and auto commuting shares



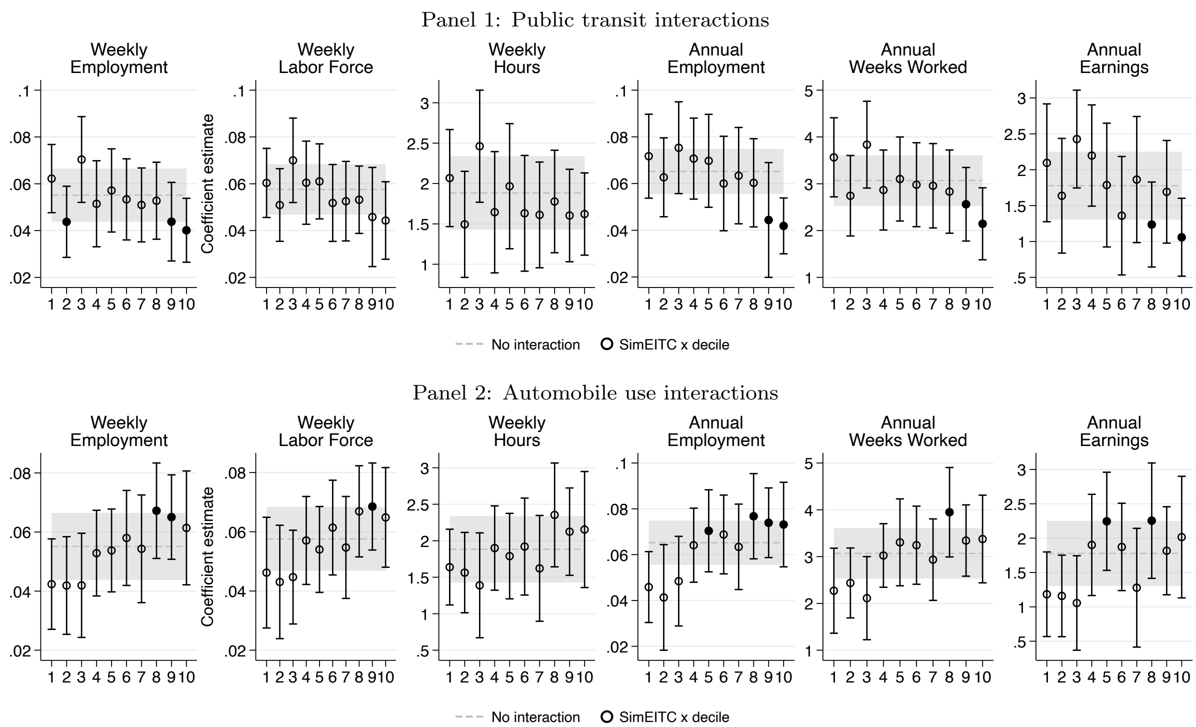
Note: 1990 5% Census sample, IPUMS. Commuting shares calculated as proportions commuting by either public transit or automobile among workers with at most a high school education. Points are weighted by working high-school-educated population. New York City and District of Columbia are excluded for ease of viewing.

Figure A4: Change in commuting characteristics groups, 1990–2000



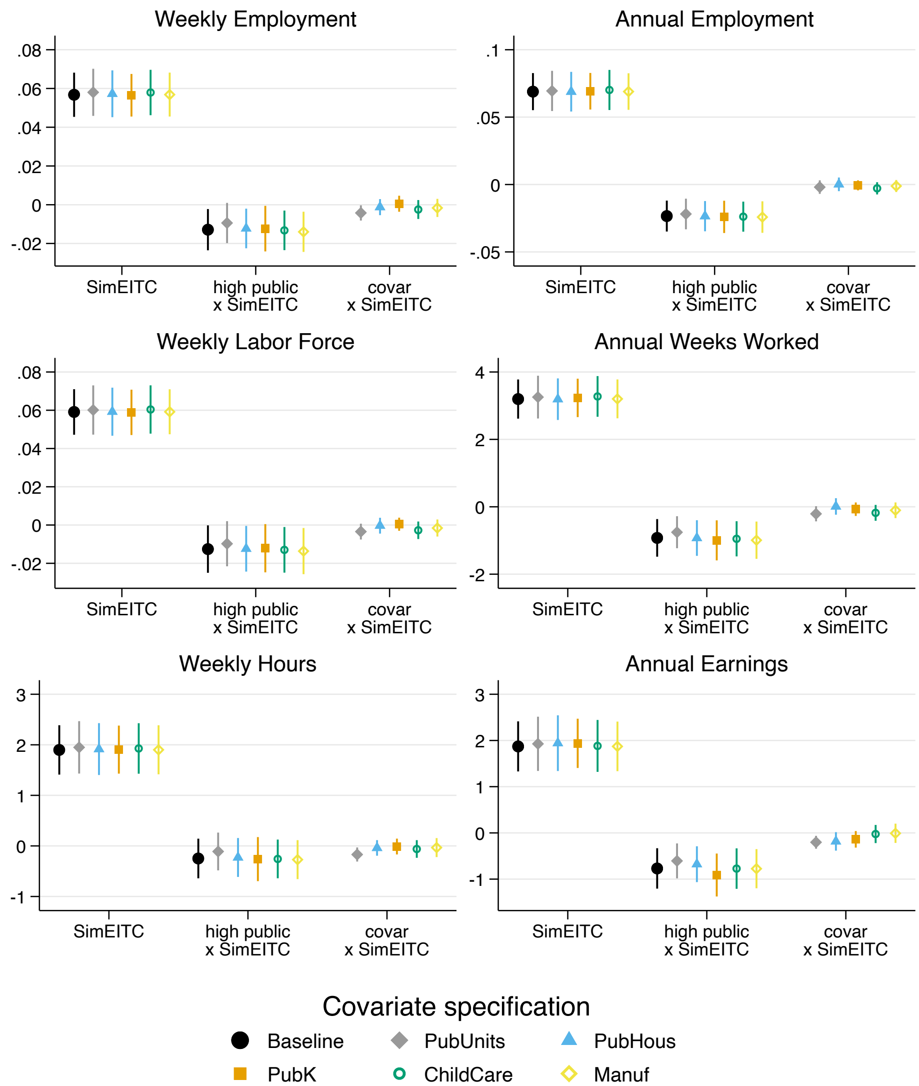
Note: 1990 and 2000 5% Census samples, IPUMS. Plots show shifts of communities between categories of high commuting characteristics between 1990 and 2000. The *high public* and *high auto* indicators reflect whether the community is in the top quartile of commuting by public transport or automobile. Commuting shares calculated as the share of workers with at most a high school education who either commute by public transit or by automobile.

Figure A5: Effect of EITC on labor force outcomes by deciles of commuting characteristics



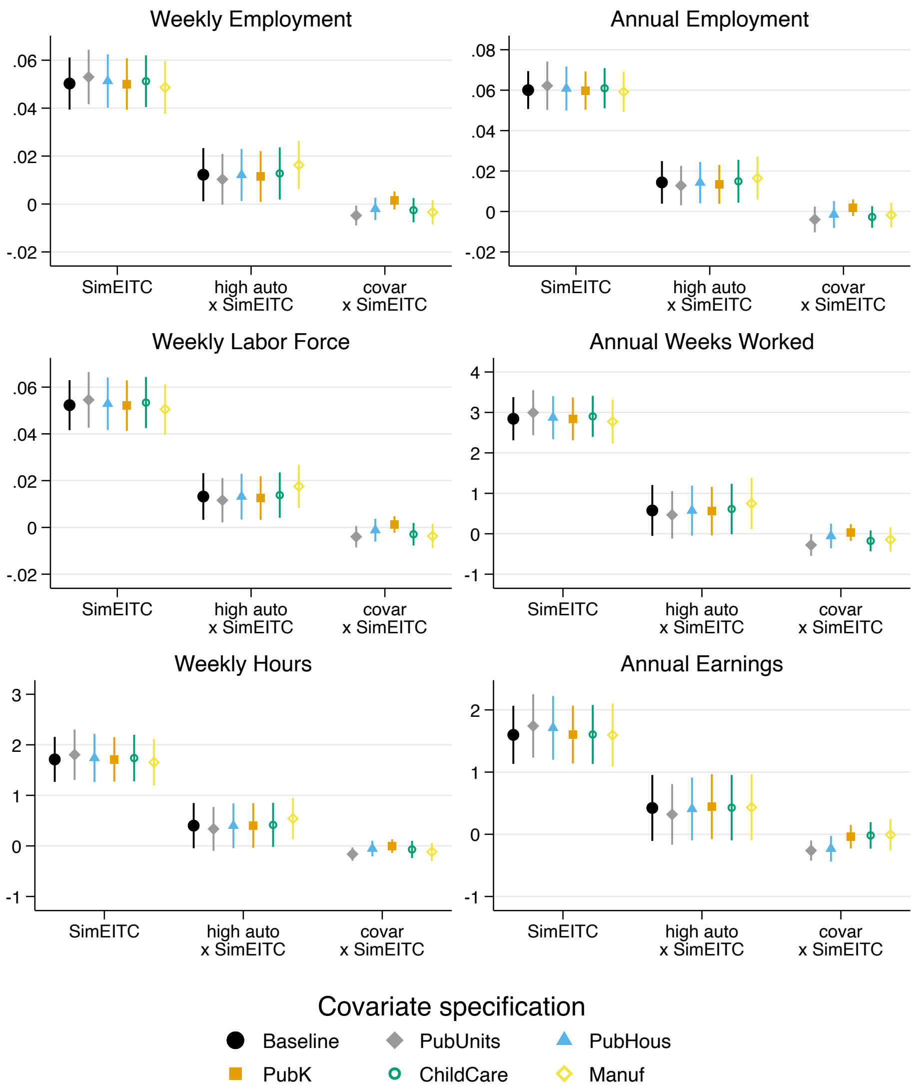
CPS ASEC, 1989–2004. Sample consists of unmarried women ages 20–50 with educational attainment of high school or less. All models use lagged SimEITC and full controls as described in Table 2. Point estimates and 95% confidence intervals reflect estimates associated with interactions of SimEITC with deciles of community commuting characteristics. Solid points indicate $p < 0.05$ that the estimate is equal to that of the first decile (note that coefficients can be significantly different even when confidence intervals overlap). Dotted line and shaded area depict the point estimate and 95% confidence interval associated with uninteracted SimEITC from baseline regression reported in Table 2, column 6. Earnings and SimEITC are measured in 1,000s of 2019 dollars.

Figure A6: Effect of EITC on labor force outcomes with public commuting and additional covariate interactions



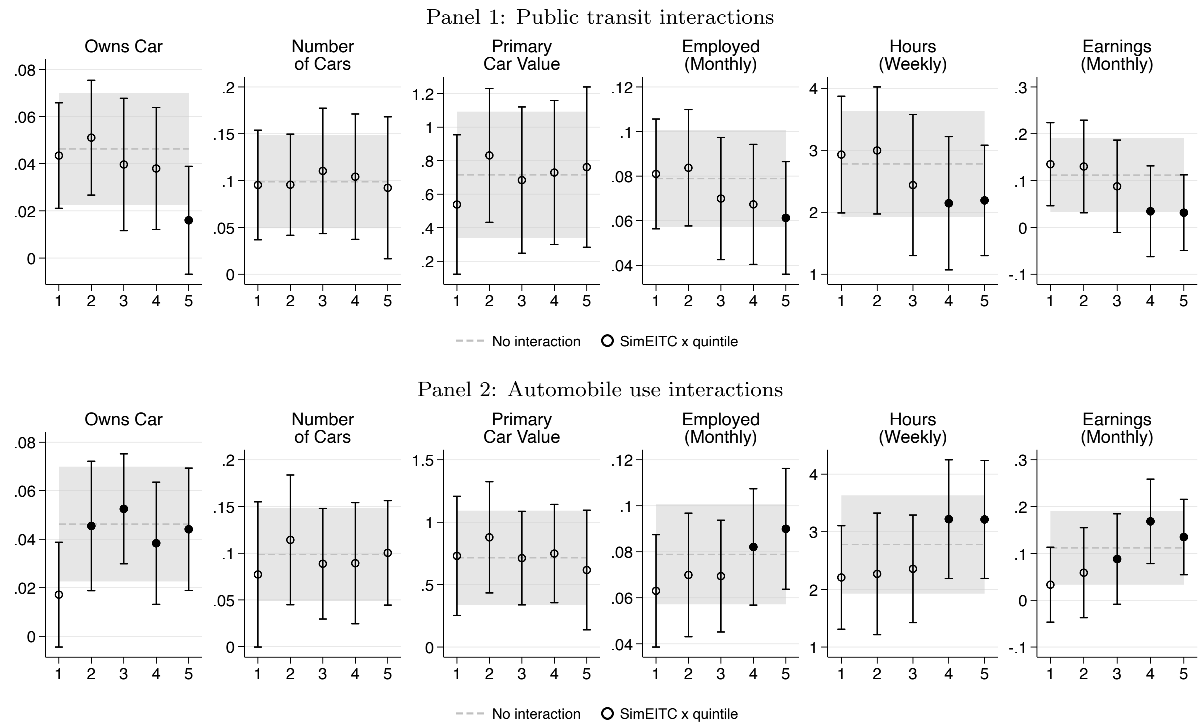
CPS ASEC, 1989–2004. Sample consists of unmarried women ages 20–50 with educational attainment of high school or less. All models use lagged SimEITC and full controls as described in Table 2. Point estimates and 95% confidence intervals reflect estimates associated with SimEITC and listed SimEITC-covariate interactions. Each marker type indicates a separate regression. *PubUnits* measures the number of federally subsidized housing units per capita, with data drawn from the 1996 Picture of Subsidized Households data set from the U.S. Department of Housing and Urban Development. *PubHous* measures the share of respondents reporting living in public housing, pooled CPS-ASEC 1989–1993. *PubK* measures the share of children aged 3–6 attending public kindergarten, from the CPS October Supplement pooled 1986–1993. *ChildCare* measures local average weekly earnings of workers in NAICS industry 6244, Child Day Care Services, as a share of total private weekly earnings in 1990; Quarterly Census of Employment and Wages (QCEW). *Manuf* measures the share of local employment in the manufacturing sector as a share of total local employment in 1990, QCEW. All covariate measures standardized.

Figure A7: Effect of EITC on labor force outcomes with auto commuting and additional covariate interactions



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 2. Point estimates and 95% confidence intervals reflect estimates associated with SimEITC and listed SimEITC-covariate interactions. Each marker type indicates a separate regression. *PubUnits* measures the number of federally subsidized housing units per capita, with data drawn from the 1996 Picture of Subsidized Households data set from the U.S. Department of Housing and Urban Development. *PubHous* measures the share of respondents reporting living in public housing, pooled CPS-ASEC 1989–1993. *PubK* measures the share of children aged 3–6 attending public kindergarten, from the CPS October Supplement pooled 1986–1993. *ChildCare* measures local average weekly earnings of workers in NAICS industry 6244, Child Day Care Services, as a share of total private weekly earnings in 1990; Quarterly Census of Employment and Wages (QCEW). *Manuf* measures the share of local employment in the manufacturing sector as a share of total local employment in 1990, QCEW. All covariate measures standardized.

Figure A8: Effect of EITC on car ownership and labor force outcomes by quintiles of commuting characteristics, SIPP



Note: SIPP panels 1990–2001 (calendar years 1991–2003). Sample is unmarried women 20–50 with high school education or less. All models use lagged SimEITC and full controls as described in Table 2. Point estimates and 95% confidence intervals reflect estimates associated with interactions of SimEITC with quintiles of community commuting characteristics. Solid points indicate $p < 0.05$ that the estimate is equal to that of the first quintile (note that coefficients can be significantly different even when confidence intervals overlap). Dotted line and shaded area depict the point estimate and 95% confidence interval associated with uninteracted SimEITC. Earnings, primary car value, and SimEITC are measured in 1,000s of 2019 dollars.

Table A1: Summary statistic means by year and community type

	Age	White	Less than HS	Has child	Rural	Real earnings, 000s
A: Full sample						
1989	32.0	0.60	0.29	0.46	0.19	25.2
1994	33.0	0.53	0.31	0.49	0.20	25.1
1999	33.5	0.52	0.29	0.48	0.17	25.0
2004	33.4	0.49	0.29	0.50	0.18	25.0
2009	33.5	0.47	0.29	0.48	0.17	24.9
2014	32.7	0.43	0.26	0.46	0.17	24.8
2019	32.6	0.42	0.23	0.41	0.14	24.9
Total	32.9	0.49	0.28	0.47	0.17	25.0
B: High public						
	Age	White	Less than HS	Has child	Rural	Real earnings, 000s
1989	32.1	0.47	0.30	0.44	0.00	29.3
1994	33.3	0.38	0.34	0.48	0.00	29.1
1999	33.7	0.39	0.29	0.44	0.00	29.2
2004	33.8	0.33	0.34	0.47	0.00	29.3
2009	33.9	0.31	0.30	0.46	0.00	29.4
2014	33.1	0.28	0.28	0.45	0.00	29.4
2019	32.7	0.29	0.26	0.37	0.00	29.5
Total	33.2	0.35	0.30	0.45	0.00	29.3
C: High auto						
	Age	White	Less than HS	Has child	Rural	Real earnings, 000s
1989	32.5	0.60	0.33	0.51	0.48	21.4
1994	33.4	0.59	0.31	0.50	0.51	21.5
1999	33.9	0.60	0.32	0.48	0.44	21.6
2004	33.6	0.56	0.29	0.51	0.46	21.6
2009	33.3	0.56	0.28	0.50	0.41	21.5
2014	32.9	0.55	0.27	0.50	0.43	21.5
2019	32.9	0.53	0.20	0.42	0.35	21.7
Total	33.2	0.57	0.28	0.49	0.44	21.5

Note: CPS ASEC 1989–2019. Sample consists of unmarried women ages 20–50 with educational attainment of high school or less. Earnings includes wages and salaries and is zero for those who did not work in the prior year; 2019 dollars.

Table A2: Selected communities by commuting statistics

A: Top 12 by public transit use				
Metro area	Public	Auto	Home	Walk/bike
Washington, DC (DC)	52.0	37.7	1.9	7.4
New York-Northern NJ-Long Island (NY)	46.5	42.0	2.0	8.9
Philadelphia-Camden-Wilmington (PA)	20.4	70.3	1.8	6.9
Honolulu (HI)	17.2	74.2	2.0	5.9
Chicago-Gary (IL)	16.8	76.2	2.2	4.0
New Orleans (LA)	16.4	76.5	1.7	4.4
Washington, DC (MD)	16.0	77.4	3.7	2.3
San Francisco-Oakland-Vallejo (CA)	15.8	74.6	3.6	5.0
Boston (MA)	14.8	76.0	2.5	6.1
Baltimore (MD)	13.7	78.5	2.6	4.4
Los Angeles-Long Beach (CA)	12.4	78.7	3.2	4.6
Pittsburgh (PA)	11.9	80.4	1.9	5.4
B: Top 12 by personal auto use				
Metro area	Public	Auto	Home	Walk/bike
Chattanooga (GA)	0.0	97.4	1.4	0.5
Charlotte-Gastonia-Rock Hill (SC)	0.2	96.7	1.4	0.9
Johnson City-Kingsport-Bristol (TN)	0.9	95.6	1.2	1.9
Flint (MI)	0.4	95.5	2.0	1.3
Augusta-Aiken (SC)	1.1	95.2	1.5	1.5
Non-metro Tennessee	0.6	95.2	2.2	1.3
Youngstown-Warren (OH)	0.9	95.2	2.0	1.3
Saginaw-Bay City-Midland (MI)	0.9	94.8	1.6	2.1
Waco (TX)	0.9	94.7	1.3	2.4
Non-metro Alabama	0.8	94.7	2.1	1.5
Non-metro North Carolina	0.7	94.6	2.0	1.8
Asheville (NC)	1.7	94.6	1.7	1.3
C: Bottom 12 by personal auto use, among those low in public transit				
Metro area	Public	Auto	Home	Walk/bike
Non-metro Alaska	3.6	77.1	5.3	11.0
San Diego (CA)	6.5	83.7	4.6	4.2
Madison (WI)	5.3	84.0	4.0	6.3
Non-metro North Dakota	0.8	84.1	6.4	8.3
Non-metro Minnesota	1.4	84.2	7.9	6.0
El Paso (TX)	5.8	84.5	4.0	4.1
Minneapolis-St. Paul (MN)	7.7	84.5	4.7	2.5
Non-metro South Dakota	0.1	84.8	8.4	6.0
Santa Barbara-Santa Maria-Lompoc (CA)	3.2	84.8	4.3	7.0
Bridgeport (CT)	7.1	85.1	1.3	5.6
Houston-Brazoria (TX)	7.3	85.4	2.8	3.5
San Antonio (TX)	7.6	85.6	2.8	3.2

Note: 1990 U.S. Census 5% sample, IPUMS. Table reports percent of non-college workers in each community who use the specified commuting type ("other" category not shown). Panel C restricted to bottom 75% by public transportation.

Table A3: Effect of the EITC on labor supply outcomes for different EITC variable specifications

	Employed (Weekly) (1)	LFP (Weekly) (2)	Hours (Weekly) (3)	Employed (Annual) (4)	Weeks (Annual) (5)	Earnings (Annual) (6)
MaxEITC	0.0239***	0.0265***	0.997***	0.0349***	1.439***	0.720***
	(0.004)	(0.004)	(0.145)	(0.004)	(0.218)	(0.147)
R^2	0.0849	0.0733	0.0876	0.0766	0.0991	0.0863
MaxEITC, Lag	0.0289***	0.0320***	0.997***	0.0354***	1.633***	0.897***
	(0.005)	(0.005)	(0.145)	(0.004)	(0.244)	(0.173)
R^2	0.0852	0.0737	0.0876	0.0762	0.0990	0.0863
SimEITC	0.0478***	0.0497***	1.640***	0.0653***	2.826***	1.528***
	(0.006)	(0.005)	(0.232)	(0.005)	(0.255)	(0.221)
R^2	0.0853	0.0737	0.0877	0.0773	0.0997	0.0865
SimEITC, Lag	0.0549***	0.0573***	1.862***	0.0654***	3.058***	1.756***
	(0.006)	(0.005)	(0.232)	(0.005)	(0.274)	(0.240)
R^2	0.0856	0.0741	0.0879	0.0768	0.0996	0.0865
Observations	108,972	108,972	108,972	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. Table shows coefficient estimates on different versions of the treatment variable for outcomes listed above. MaxEITC is the maximum federal EITC benefit by year and family size. SimEITC is the simulated EITC described in Section 3. Earnings and EITC are measured in 1,000s of 2019 dollars. All models use full controls described in Table 2. Standard errors are clustered at the community level.

Table A4: The effect of the EITC on labor supply outcomes, various controls—additional dependent variables

	(1)	(2)	(3)	(4)	(5)	(6)
	A: Weekly Labor Force Participation					
SimEITC	0.0820***	0.0776***	0.0777***	0.0788***	0.0646***	0.0573***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
R^2	0.0626	0.0713	0.0718	0.0729	0.0737	0.0741
Observations	108,972	108,972	108,972	108,972	108,972	108,972
	B: Weekly Hours					
SimEITC	2.803***	2.637***	2.635***	2.669***	2.127***	1.862***
	(0.212)	(0.205)	(0.204)	(0.203)	(0.237)	(0.232)
R^2	0.0685	0.0854	0.0859	0.0870	0.0877	0.0879
Observations	108,972	108,972	108,972	108,972	108,972	108,972
	C: Annual Weeks Worked					
SimEITC	4.124***	3.933***	3.925***	3.973***	3.365***	3.058***
	(0.246)	(0.235)	(0.235)	(0.235)	(0.280)	(0.274)
R^2	0.0812	0.0970	0.0976	0.0986	0.0993	0.0996
Observations	105,138	105,138	105,138	105,138	105,138	105,138
	D: Annual Pretax Earnings (1,000s of 2019 dollars)					
SimEITC	2.301***	2.258***	2.245***	2.232***	1.966***	1.756***
	(0.229)	(0.216)	(0.218)	(0.217)	(0.245)	(0.240)
R^2	0.0572	0.0852	0.0855	0.0861	0.0864	0.0865
Observations	105,138	105,138	105,138	105,138	105,138	105,138
Demographics		✓	✓	✓	✓	✓
State controls			✓	✓	✓	✓
State trends				✓	✓	✓
Reform-kids FE					✓	✓
Reform-kids FE (full)						✓

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. "Full" includes interactions between number of children and state vehicle exemption policy. All models include state-community and year fixed effects. Standard errors are clustered at the community level.

Table A5: Effects of the EITC on labor supply outcomes by local commuting characteristics—married women

	Employed (Weekly) (1)	LFP (Weekly) (2)	Hours (Weekly) (3)	Employed (Annual) (4)	Weeks (Annual) (5)	Earnings (Annual) (6)
	A: No Interaction					
SimEITC	0.0238^{*}	0.0329^{**}	1.633^{***}	0.0352^{**}	1.955^{***}	2.826^{***}
	(0.011)	(0.011)	(0.451)	(0.011)	(0.554)	(0.577)
	B: High Public Transit					
SimEITC	0.0235^{*}	0.0326^{**}	1.622^{***}	0.0352^{**}	1.956^{***}	2.828^{***}
	(0.011)	(0.010)	(0.458)	(0.011)	(0.573)	(0.594)
SimEITC × high public	-0.0421^{*}	-0.0529^{**}	-2.020^{*}	-0.0549^{*}	-1.914^{*}	-3.180^{**}
	(0.018)	(0.020)	(0.785)	(0.024)	(0.858)	(1.185)
	C: High Auto					
SimEITC	0.00179	0.0139	0.824	0.0162	1.122^{*}	1.708^{**}
	(0.011)	(0.011)	(0.456)	(0.011)	(0.560)	(0.547)
SimEITC × high auto	0.0461^{***}	0.0399^{**}	1.701^{**}	0.0408^{**}	1.791^{**}	2.405^{***}
	(0.012)	(0.014)	(0.525)	(0.014)	(0.666)	(0.585)
Observations	170,861	170,861	170,861	174,237	174,237	174,237

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

Note: CPS ASEC, 1989–2004. Sample consists of married women ages 20–50 with educational attainment of high school or less. All models use lagged SimEITC and full controls as described in Table 2. 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. Earnings and SimEITC are measured in 1,000s of 2019 dollars. Standard errors are clustered at the community level.

Table A6: Effect of maximum EITC on labor supply by local commuting characteristics and binary exposure, weekly outcomes

	Employed (Weekly) (1)	LFP (Weekly) (2)	Hours (Weekly) (3)	Employed (Annual) (4)	Weeks (Annual) (5)	Earnings (Annual) (6)
	A: No Interaction					
MaxEITC × LowExp	0.0228***	0.0265***	0.793***	0.0292***	1.307***	0.670***
	(0.003)	(0.004)	(0.128)	(0.003)	(0.177)	(0.149)
MaxEITC × HighExp	0.0380***	0.0400***	1.299***	0.0438***	2.073***	1.203***
	(0.004)	(0.004)	(0.172)	(0.004)	(0.187)	(0.175)
Difference						
HighExp – LowExp	0.0152***	0.0135***	0.506***	0.0146***	0.766***	0.532***
	(0.003)	(0.003)	(0.124)	(0.004)	(0.184)	(0.156)
	B: High Public Transit					
MaxEITC × LowExp	0.0265***	0.0302***	0.850***	0.0367***	1.585***	0.880***
	(0.003)	(0.004)	(0.147)	(0.005)	(0.186)	(0.175)
MaxEITC × LowExp × HighPublic	-0.00623**	-0.00608	-0.100	-0.0126***	-0.461**	-0.332*
	(0.002)	(0.003)	(0.093)	(0.003)	(0.146)	(0.128)
MaxEITC × HighExp	0.0389***	0.0414***	1.307***	0.0466***	2.181***	1.317***
	(0.004)	(0.004)	(0.183)	(0.005)	(0.208)	(0.188)
MaxEITC × HighExp × HighPublic	0.00151	-0.00388	0.105	-0.00945**	-0.383*	-0.668***
	(0.003)	(0.003)	(0.124)	(0.003)	(0.181)	(0.145)
	C: High Auto					
MaxEITC × LowExp	0.0224***	0.0261***	0.781***	0.0287***	1.288***	0.660***
	(0.003)	(0.004)	(0.128)	(0.003)	(0.170)	(0.147)
MaxEITC × LowExp × HighAuto	0.00915	0.0104*	0.302	0.0122**	0.452	0.267
	(0.005)	(0.005)	(0.208)	(0.004)	(0.256)	(0.215)
MaxEITC × HighExp	0.0377***	0.0392***	1.290***	0.0426***	2.046***	1.172***
	(0.004)	(0.004)	(0.190)	(0.005)	(0.216)	(0.188)
MaxEITC × HighExp × HighAuto	0.00266	0.00423	0.0883	0.00557	0.159	0.133
	(0.005)	(0.004)	(0.188)	(0.004)	(0.273)	(0.214)
N	108,972	108,972	108,972	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 2). *HighExp* indicates residence in state with high exposure to the 1990s EITC reforms, as described in text. The third row in panel A reports the linear combination of coefficients $MaxEITC \times HighExp - MaxEITC \times LowExp$. The *HighPublic* and *HighAuto* indicators reflect whether the respondent's place of residence is in the top 25%, of commuting by public transport or automobile. Earnings and MaxEITC are measured in 1,000s of 2019 dollars. Standard errors are clustered at the state level.

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

	Employed (Weekly) (1)	LFP (Weekly) (2)	Hours (Weekly) (3)	Employed (Annual) (4)	Weeks (Annual) (5)	Earnings (Annual) (6)
	A: High Public Transit					
SimEITC	0.0603^{***}	0.0611^{***}	1.936^{***}	0.0739^{***}	2.803^{***}	1.756^{***}
	(0.010)	(0.011)	(0.392)	(0.015)	(0.578)	(0.477)
SimEITC × high public	-0.0175^{**}	-0.0107	-0.238	-0.0264^{***}	-1.103^{**}	-1.103^{***}
	(0.006)	(0.008)	(0.238)	(0.007)	(0.347)	(0.247)
	B: High Auto					
SimEITC	0.0491^{***}	0.0527^{***}	1.727^{***}	0.0587^{***}	2.152^{***}	1.166^{**}
	(0.007)	(0.009)	(0.298)	(0.009)	(0.378)	(0.349)
SimEITC × high auto	0.0233^{**}	0.0223^{***}	0.600	0.0275^{***}	1.235^{**}	0.929^{*}
	(0.007)	(0.006)	(0.327)	(0.008)	(0.362)	(0.405)
Observations	32,815	32,815	32,815	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 SimEITC and full controls as described in Table 2. 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. Earnings and SimEITC are measured in 1,000s of 2019 dollars. Standard errors are clustered at the community level.

Table A8: Effect of the EITC on labor supply outcomes by local commuting characteristics—pre-1996 sample

	Employed (Weekly) (1)	LFP (Weekly) (2)	Hours (Weekly) (3)	Employed (Annual) (4)	Weeks (Annual) (5)	Earnings (Annual) (6)
	A: High Public Transit					
SimEITC	0.0522***	0.0624***	1.750*	0.0996***	5.515***	5.540***
	(0.016)	(0.016)	(0.673)	(0.023)	(1.191)	(1.027)
SimEITC × high public	-0.0438***	-0.0376*	-1.306**	-0.0672***	-2.218**	-2.293***
	(0.011)	(0.017)	(0.450)	(0.016)	(0.720)	(0.584)
	B: High Auto					
SimEITC	0.0380*	0.0495**	1.256	0.0963***	5.199***	5.467***
	(0.016)	(0.015)	(0.707)	(0.027)	(1.249)	(1.148)
SimEITC × high auto	0.0321*	0.0290*	1.093	0.0334*	1.418	1.077
	(0.014)	(0.014)	(0.558)	(0.014)	(0.811)	(0.593)
Observations	47,935	47,935	47,935	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 unmarried mothers ages 20–50 with educational attainment of high school or less. All models use lagged SimEITC and full controls as described in Table 2. 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. Earnings and SimEITC are measured in 1,000s of 2019 dollars. Standard errors are clustered at the community level.

Table A9: Effect of the EITC on labor supply outcomes by local commuting characteristics—no-waiver states, pre-1996 sample

	Employed (Weekly) (1)	LFP (Weekly) (2)	Hours (Weekly) (3)	Employed (Annual) (4)	Weeks (Annual) (5)	Earnings (Annual) (6)
	A: High Public Transit					
SimEITC	0.0800**	0.0923**	3.169*	0.178***	8.474***	6.840***
	(0.028)	(0.027)	(1.193)	(0.047)	(2.221)	(1.646)
SimEITC × high public	-0.0464*	-0.0546*	-1.263	-0.0899***	-3.469**	-2.493*
	(0.018)	(0.025)	(0.651)	(0.020)	(1.066)	(0.984)
	B: High Auto					
SimEITC	0.0522	0.0654*	2.069	0.164**	7.456**	6.517***
	(0.029)	(0.028)	(1.048)	(0.055)	(2.256)	(1.602)
SimEITC × high auto	0.0637**	0.0617*	2.529**	0.0806**	3.865**	2.128*
	(0.022)	(0.025)	(0.868)	(0.024)	(1.305)	(0.929)
Observations	13,851	13,851	13,851	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 unmarried mothers 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 SimEITC and full controls as described in Table 2. 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. Earnings and SimEITC are measured in 1,000s of 2019 dollars. Standard errors are clustered at the community level.

Table A10: Effects of federal-only EITC on labor supply outcomes by local commuting characteristics

	Employed (Weekly) (1)	LFP (Weekly) (2)	Hours (Weekly) (3)	Employed (Annual) (4)	Weeks (Annual) (5)	Earnings (Annual) (6)
	A: High Public Transit					
SimEITC	0.0636***	0.0653***	2.161***	0.0752***	3.563***	2.065***
	(0.006)	(0.007)	(0.268)	(0.008)	(0.320)	(0.302)
SimEITC × high public	-0.0113*	-0.0113	-0.188	-0.0226***	-0.848**	-0.724**
	(0.005)	(0.006)	(0.195)	(0.006)	(0.276)	(0.219)
	B: High Auto					
SimEITC	0.0584***	0.0595***	1.999***	0.0685***	3.310***	1.863***
	(0.006)	(0.006)	(0.256)	(0.006)	(0.283)	(0.261)
SimEITC × high auto	0.00992	0.0111*	0.322	0.0119*	0.443	0.349
	(0.006)	(0.005)	(0.225)	(0.005)	(0.310)	(0.264)
Observations	108,972	108,972	108,972	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 2. The SimEITC employed here includes only simulated federal EITCs, excluding state EITCs. 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. Earnings and SimEITC are measured in 1,000s of 2019 dollars. Standard errors are clustered at the community level.

Table A11: Effect of the EITC on labor supply outcomes by local commuting characteristics—college-educated

	Employed (Weekly) (1)	LFP (Weekly) (2)	Hours (Weekly) (3)	Employed (Annual) (4)	Weeks (Annual) (5)	Earnings (Annual) (6)
	A: High Public Transit					
SimEITC	0.00993	0.00660	0.779	0.00992	0.411	0.323
	(0.008)	(0.008)	(0.506)	(0.007)	(0.408)	(0.794)
SimEITC × high public	-0.00268	-0.00223	-0.290	-0.00332	0.0916	0.843
	(0.005)	(0.005)	(0.304)	(0.005)	(0.331)	(0.552)
	B: High Auto					
SimEITC	0.00865	0.00447	0.654	0.00564	0.267	0.506
	(0.009)	(0.008)	(0.518)	(0.007)	(0.424)	(0.860)
SimEITC × high auto	0.00310	0.00556	0.299	0.0115^{**}	0.426	-0.353
	(0.005)	(0.005)	(0.367)	(0.004)	(0.276)	(0.653)
Observations	48,557	48,557	48,557	49,522	49,522	49,522

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 a college degree. All models use lagged simulated EITCs and full set of controls as described in Table 2. 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. Earnings and SimEITC are measured in 1,000s of 2019 dollars. Standard errors are clustered at the community level.

Table A12: Effects of the EITC on car ownership and labor supply outcomes by local commuting characteristics—married women, SIPP

	Owns Car (1)	Number of Cars (2)	Primary Car Value (3)	Employed (Monthly) (4)	Hours (Weekly) (5)	Earnings (Monthly) (6)
	A: High Public					
SimEITC	0.0205	0.0985	0.407	0.0535*	2.906**	0.258**
	(0.011)	(0.058)	(0.396)	(0.023)	(0.974)	(0.080)
SimEITC × high public	-0.00516	-0.144	0.0605	-0.0207	-0.851	-0.115
	(0.031)	(0.106)	(0.856)	(0.025)	(1.022)	(0.107)
R^2	0.0751	0.1193	0.0929	0.0785	0.0744	0.0740
	B: High Auto					
SimEITC	0.0199	0.0912	0.408	0.0467	3.156**	0.270**
	(0.013)	(0.059)	(0.489)	(0.025)	(1.056)	(0.088)
SimEITC × high auto	0.00166	0.0276	-0.00804	0.0152	-0.420	-0.0138
	(0.011)	(0.069)	(0.455)	(0.024)	(0.890)	(0.066)
R^2	0.0751	0.1192	0.0929	0.0785	0.0744	0.0739
Observations	48,357	48,357	48,357	185,368	185,368	185,368

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

Note: SIPP panels 1990–2001 (calendar years 1991–2003). Columns 1–3 restricted to waves with asset topical modules. Sample consists of married women ages 20–50 with educational attainment of high school or less. All models use lagged SimEITC and full controls as described in Table 2. 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. Earnings, primary car value, and SimEITC are measured in 1,000s of 2019 dollars. Car ownership variables are defined at the household level. Primary car refers to the most valuable car in the household. Monthly employment denotes any paid work in a month. Hours refers to usual weekly hours at the job with the most hours in a month. Standard errors are clustered at the community level.

Table A13: Effects of the EITC on car ownership and labor supply outcomes by local commuting characteristics and age of youngest child, SIPP

	Owns Car (1)	Number of Cars (2)	Primary Car Value (3)	Employed (Monthly) (4)	Hours (Weekly) (5)	Earnings (Monthly) (6)
	A: High Public					
SimEITC × (child < 5)	0.0442***	0.0778**	0.779***	0.0906***	3.259***	0.144***
	(0.012)	(0.030)	(0.199)	(0.013)	(0.459)	(0.041)
SimEITC × (child < 5) × high public	-0.0176*	-0.00320	0.0863	-0.0212*	-0.784	-0.118***
	(0.007)	(0.021)	(0.127)	(0.011)	(0.439)	(0.030)
SimEITC × (no child < 5)	0.0471***	0.117***	0.656**	0.0687***	2.361***	0.0837
	(0.012)	(0.028)	(0.206)	(0.012)	(0.505)	(0.048)
SimEITC × (no child < 5) × high public	-0.0359***	-0.0323	-0.0600	-0.00926	-0.344	-0.0490
	(0.007)	(0.028)	(0.180)	(0.009)	(0.351)	(0.025)
	B: High Auto					
SimEITC × (child < 5)	0.0492***	0.0762*	0.906***	0.0790***	2.725***	0.0901*
	(0.014)	(0.032)	(0.203)	(0.012)	(0.465)	(0.038)
SimEITC × (child < 5) × high auto	-0.0102	-0.00000936	-0.277	0.0254**	1.161**	0.117***
	(0.009)	(0.020)	(0.152)	(0.009)	(0.364)	(0.029)
SimEITC × (no child < 5)	0.0430**	0.0979**	0.680**	0.0628***	2.054***	0.0580
	(0.014)	(0.029)	(0.202)	(0.012)	(0.499)	(0.045)
SimEITC × (no child < 5) × high auto	0.00901	0.0395*	-0.0436	0.0127	0.670	0.0564
	(0.010)	(0.018)	(0.117)	(0.008)	(0.354)	(0.029)
N	32,685	32,685	32,685	123,675	123,675	123,675

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

Note: SIPP panels 1990–2001 (calendar years 1991–2003). Columns 1–3 restricted to waves with asset topical modules. 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 2. 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. Earnings, primary car value, and SimEITC are measured in 1,000s of 2019 dollars. Car ownership variables are defined at the household level. Primary car refers to the most valuable car in the household. Monthly employment denotes any paid work in a month. Hours refers to usual weekly hours at the job with the most hours in a month. Standard errors are clustered at the community level.

Table A14: Effects of the EITC on car ownership and labor supply outcomes by local commuting characteristics—college-educated, SIPP

	Owns Car (1)	Number of Cars (2)	Primary Car Value (3)	Employed (Monthly) (4)	Hours (Weekly) (5)	Earnings (Monthly) (6)
	A: No Interaction					
SimEITC	0.0115	0.0513	0.108	0.0265	1.888*	0.207
	(0.021)	(0.054)	(0.475)	(0.016)	(0.797)	(0.150)
R^2	0.1081	0.1444	0.0940	0.0718	0.0934	0.1351
	B: High Public					
SimEITC	0.0125	0.0538	0.121	0.0262	1.864*	0.201
	(0.016)	(0.051)	(0.434)	(0.014)	(0.717)	(0.116)
SimEITC × high public	-0.0292	-0.0777**	-0.423	-0.0148	-1.279*	-0.325**
	(0.016)	(0.026)	(0.371)	(0.009)	(0.507)	(0.109)
R^2	0.1089	0.1449	0.0942	0.0720	0.0939	0.1359
	C: High Auto					
SimEITC	0.0136	0.0366	0.108	0.0230	1.395	0.114
	(0.024)	(0.057)	(0.529)	(0.016)	(0.799)	(0.149)
SimEITC × high auto	-0.00594	0.0414	0.000553	0.00876	1.234*	0.233*
	(0.013)	(0.039)	(0.248)	(0.009)	(0.489)	(0.099)
R^2	0.1081	0.1445	0.0940	0.0719	0.0939	0.1355
Observations	16,419	16,419	16,419	61,810	61,810	61,810

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

Note: SIPP panels 1990–2001 (calendar years 1991–2003). Columns 1–3 restricted to waves with asset topical modules. Sample consists of married women ages 20–50 with a college degree. All models use lagged SimEITC and full controls as described in Table 2. 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. Earnings, primary car value, and SimEITC are measured in 1,000s of 2019 dollars. Car ownership variables are defined at the household level. Primary car refers to the most valuable car in the household. Monthly employment denotes any paid work in a month. Hours refers to usual weekly hours at the job with the most hours in a month. Standard errors are clustered at the community level.

B Labor Supply Model

This section outlines a simple model of individual labor supply featuring fixed costs to working and choice in commuting method. The starting point is a standard fixed-cost labor supply model (Cogan, 1981) in which individuals choose hours h to maximize a quasi-linear utility function $U(h)$. Working incurs a monetary cost m as well as a time cost h_c.

$$U(h) = wh - m + \frac{\alpha}{1-\frac{1}{\gamma}}(H - h_c - h)^{1-\frac{1}{\gamma}} \tag{2}$$

Optimal hours conditional on working can be found by differentiating with respect to h:

$$h^* = H - h_c - \left(\frac{w}{\alpha}\right)^{-\gamma} \tag{3}$$

If U_0 is the reservation utility derived from non-work, the condition for a worker to participate in the labor market is $U(h^*) \geq U_0$. Plugging in the expression for h^* above and rearranging yields the following expression for the time cost of working to ensure nonzero work hours:

$$h_c \leq H - \frac{U_0 + m - Aw^{1-\gamma}}{w} \tag{4}$$

where $A \equiv \frac{\alpha^\gamma}{\gamma-1}$. This expression has intuitive properties: the maximum time cost h_c that a worker will accept rises with the wage. It falls with the monetary cost of working m and the reservation utility U_0.

Now suppose a worker has two commuting options: driving and public transit. Driving incurs a time cost h_c^d and public transit incurs the time cost h_c^p, random variables with cumulative distribution functions F_d and F_p, respectively. I will assume that the public transit cost distribution first-order stochastically dominates the driving cost distribution, or $F_p(x) \leq F_d(x)$ for all x with strict inequality at some x. This captures the notion that it tends to take longer to commute by public transit than by driving.

Workers observe both h_c^d and h_c^p and simultaneously choose whether to work and, if so, how to get there. In a group of workers who are homogeneous in all ways other than their realizations of the time costs of commuting, the participation rate can be expressed as

$$\ell = F_d(B) + \bar{F}_d(B)F_p(B) \tag{5}$$

where B is the right-hand side of Equation 4 and $\bar{F}(\cdot) \equiv 1 - F(\cdot)$.

The EITC is now introduced in two ways. First, workers eligible for the EITC value it for the refund they expect to receive, though this valuation is less than the equivalent utility from consumption today out of the expected refund.[22] If the post-tax wage is $(1+\tau)w$, with $\tau > 0$

[22]This decision is not strictly necessary, as the model would have the same properties if workers valued their expected post-tax wages at their full dollar amount. Yet if this were the case, it would become awkward to

indicating the (negative) effective tax rate for EITC-eligible workers, the value workers place on their post-tax wage is $v(w, \tau)$, where $v_w > 0$, $v_\tau > 0$, and $w \leq v(w, \tau) \leq (1 + \tau)w$. The right-hand side of Equation 4 can now be characterized as $B(\tau) \equiv H - \frac{U_0 + m - Av(w,\tau)^{1-\gamma}}{v(w,\tau)}$ where clearly $B_\tau > 0$.

The most straightforward way to justify the less-than-complete valuation of EITC refunds posited above is workers' well-documented uncertainty as to their future tax refunds (see references in Section 1). Risk-averse workers who face uncertainty over the size of their eventual benefit will undervalue that benefit, even if their beliefs are accurate in expectation.

The second pathway by which the EITC affects labor supply decisions is through the liquidity it provides upon receipt of tax refunds. Suppose that, in a first stage of decision-making prior to labor supply decisions, workers draw a monetary cost-of-driving shock c_d. This cost could represent maintenance needs or the down payment on a newly purchased vehicle. Due to credit constraints, not all workers will be able to meet this cost and will forgo the option of driving, relying instead on public transit. To put this channel in a simple reduced form, assume that a share $\theta(\tau)$ of workers manage to pay the driving cost shock c_d, with $\theta_\tau > 0$ conveying the intuition that a larger tax refund promotes greater vehicle investment (as suggested by the scholarship around this relationship surveyed in Subsection 1.2).

Since not all workers have the choice to drive, the participation rate now has the expression

$$\ell = \theta(\tau)[F_d(B(\tau)) + \bar{F}_d(B(\tau))F_p(B(\tau))] + (1 - \theta(\tau))F_p(B(\tau)) \tag{6}$$

For a change in EITC policy we have[23]

$$\begin{aligned} \frac{d\ell}{d\tau} &= \theta_\tau F_d(\cdot)\bar{F}_p(\cdot) \\ &\quad + \theta(\tau)B_\tau \left[f_d(\cdot)\bar{F}_p(\cdot) - f_p(\cdot)F_d(\cdot)\right] + f_p(\cdot)B_\tau \end{aligned} \tag{7}$$

which is unambiguously positive given $B_\tau > 0$ and, by assumption, $\theta_\tau > 0$. Note that the second line can be rewritten $\theta(\tau)B_\tau f_d(\cdot)\bar{F}_p(\cdot) + f_p(\cdot)B_\tau\left[1 - \theta(\tau)F_d(\cdot)\right] > 0$.

The first line in Equation 7 indicates the change in participation owing to increased car ownership. The second line reflects increasing participation stemming from the rise in the value of post-tax labor.

A useful property of line 1 of Equation 7 is that it is increasing in the distance $F_d(\cdot) - F_p(\cdot)$.[24] In geographic regions where the difference in time costs between public transit and driving tends

separate the decision-making into two separate stages. Behavioral considerations discussed in the main text provide justification for maintaining a distinction between the vehicle investment and labor supply decisions.

[23]For simplicity, and since this is a one-period model, I will treat as equivalent the EITC eligible to receive (based on last year's tax schedule) and the EITC eligible to earn (based on this year's tax schedule). I restore this distinction in the empirical part of the paper.

[24]To see this, consider the value $y = a(1 - b)$ with $a \geq b$ and define the difference $x \equiv a - b$. This yields $y = (1 + x)a - a^2$ which is clearly increasing in x.

to be large, the model predicts a larger participation response for a change in the EITC. By contrast, in areas where the time costs of driving and public transit tend to be similar—i.e., in areas with abundant public transit—a boost to EITC generosity will have smaller transit-driven participation effects.

Less can be said about the relationship between transit cost differences $F_d(\cdot) - F_p(\cdot)$ and the portion of $\frac{d\ell}{d\tau}$ owing to workers' valuation of increased EITC generosity. How the value of line 2 of Equation 7 varies with $F_d(\cdot) - F_p(\cdot)$ depends on the particular shapes of the distributions and where in those distributions $B(\tau)$ falls. Therefore, unlike with commuting-driven changes in participation owing to EITC expansions, the model provides no unambiguous prediction about regional variation in information-driven changes in participation.