THREE ESSAYS IN PUBLIC ECONOMICS

By

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ABSTRACT

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This dissertation is composed of two chapters on the economy-wide effects of the Earned Income Tax Credit and one chapter on the effects of monopolistic market structure in urban rental markets. Each chapter considers unintended consequences of public actions given an interconnected market place. For chapter this is skill substitutability, chapter two spatial connections, and chapter three preferences and market power.

Chapter one studies the general equilibrium incidence of the Earn Income Tax Credi by formalizing the theoretical mechanisms and quantifying its empirical importance. The Earned Income Tax Credit is a $67 billion tax expenditure that subsidizes 20% of all workers. Yet all prior analysis uses partial equilibrium assumptions on gross wages. I derive the general equilibrium incidence of wage subsidies and quantify the importance of EITC spillovers in three ways. I calculate the GE incidence of the 1993 and 2009 EITC expansions using new elasticity estimates. I contrast the incidence of counterfactual EITC and Welfare expansions. I quantify the effect of equalizing the EITC for workers with and without children. In all cases, I find spillovers are economically meaningful relative to the intended direct effects.

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

Chapter three is joint with Oren Ziv. We investigate the sources, scope, and implications of landowner market power in New York City rental markets. We show how zoning regulations generate spillovers through increased markups and derive conditions under which restricting landownership concentration reduces rents. Using new building-level data from New York City, we find that a 10% increase in ownership concentration in a Census tract is correlated with a 1% increase in rent. Market power is substantial: on average, markups account for nearly a third of rents in Manhattan. Furthermore, pecuniary spillovers between zoning constraints and markups at other buildings are appreciable. Up-zoning that results in 417 additional housing units at zoning-constrained buildings reduces markups on policy-unconstrained units and generates between 5 and 19 additional units through increased competition.

To Alyssa, family, friends, mentors, and all lovers of economics.

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KEY TO ABBREVIATIONS

2SLS Two Stage Least Squares

ACS American Community Survey

AFDC Aid for Families with Dependent Children

ARRA American Recovery and Reinvestment Act of 2009

AR Anderson & Rubin test statistic

ASEC Annual Social and Economic Supplement sample

ATR Average Tax Rate

Act Actual or True

BA Bachelor's Degree (4-year College degree)

BBL Borough-Block-Lot building identifier

BK Brooklyn (Kings county, New York)

BLP Berry, Levinsohn, and Pakes (1995)

BX the Bronx (Bronx county, New York)

CBD Central Business District

CES Constant Elasticity of Substitution

CI 95% Confidence Internal

CPI Consumer Price Index

CPS Current Population Survey

CRS Constand Returns to Scale

Cft Counterfactual

DHS Dickert, Houser, and Scholz (1995)

DOF Department of Finance (specifically in New York City)

EITC Earned Income Tax Credit

FAR Floor Area Ratio

FE Fixed Effect

FTC Federal Trade Commision

GDP Gross Domestic Product

GE General Equilibrium

GIM Gross Income Multiplier

GMM General Method of Moments

HHI Herfindahl-Hirschman Index

HS High School degree

IGMM Iterated Generalized Method of Moments

IPUMS Integrated Public Use Microdata Series

IRS Internal Revenue Service

IV Instrumental Variable

KP rk Kleibergen-Paap (2006) rank statistic

LATE Local Average Treatment Effect

LFP Labor Force Participation

LHS Less than High School Degree (Did not complete High School)

MDRC Multiple Dwellings Registration Contacts

MN Manhattan (New York county, New York)

MOP Montiel Olea-Pflueger (2013) effective F statistic

MR Meyer & Rosenbaum (2001)

MV Market Value

NBER National Bureau of Economic Research

NIT Negative Income Tax

NPV Notice of Property Value

NTA Neighborhood Tabulation Areas

NYC New York City

OBRA Omnibus Budget Reconciliation Act of 1994

ORG Outgoing Rotation Group sample

PE Partial Equilibrium

PLUTO Primary Land Use Tax Lot Output

PRWORA Personal Responsibility and Work Opportunity Reconciliation Act of 1996

QN Queens (Queens county, New York)

RCL Random Coefficients Logit

RCNL Random Coefficients Nested Logit

SBFE State Border Fixed Effect

SBRD State Border Regression Discontinuity

SE Standard Errors

SI Staten Island (Richmond county, New York)

SQFT Square-foot

SQUAREM Squared Extrapolation Methods for Accelerating EM-Like Monotone Algorithms

TANF Temporary Assistance for Needy Families

TAXSIM Tax Simulation (specifically the program by the National Bureau of Economic Research)

TRIM3 Urban Institute's Transfer Income Model 3

WIVR SE Weak Instrument Robust Standard Errors

CHAPTER 1

THE GENERAL EQUILIBRIUM INCIDENCE OF THE EARNED INCOME TAX CREDIT

1.1 Introduction

The Earned Income Tax Credit (EITC) is one of the largest anti-poverty programs in the United States. Over 20% of all workers and 40% of single parent workers receive a share of the $67 billion expenditure. At the end of the 'phase-in' portion, the EITC yields a 19%-34% subsidy on gross earnings for workers with children. Lawmakers and policy advocates often propose expansions of EITC benefits and eligibility.

Yet essentially all prior research has assumed away the possibility of gross wage distortions when analyzing policy effects on labor supply. Since the EITC amount is based on gross earnings, if the program feeds-back into market wages – e.g., decreasing wages for low-income workers – then the anti-poverty policy goals will be undermined. With each expansion that increases benefits or expands eligibility, using partial equilibrium assumptions seems less tenable. Given the scope of the EITC, its place in anti-poverty policy discussions, and the importance of labor market earnings on its overall efficacy, this oversight looms large.

I model and evaluate the EITC by deriving a general equilibrium incidence equation that relates changes in average tax rates to changes market wages and labor supply.[1] My approach allows me to decompose wage changes into the direct and indirect effects on both the treated and untreated workers. I parameterize the incidence equation by estimating EITC specific labor supply and substitution elasticities and then perform four quantitative evaluations. I calculate the empirical incidence of the 1993 expansion for different demographic groups. I compare counterfactual marginal expansions of the pre-reform (1992) EITC and social safety-net 'Welfare' programs to

[1] I refer to pre/post-tax wages as gross/net wages. I reference EITC tax rates as subsidies are 'negative taxes.' I define a 'partial equilibrium effect' as the direct effect of a policy change holding all else equal; a 'general equilibrium effect' as the total policy effect allowing all endogenous variables to adjust.

compare how different tax incentives affect incidence and spillovers. Using the estimated elasticities to parameterize a structural labor supply model, I calculate the incidence of the out-of-sample 2009 EITC expansion, and I conduct a counterfactual EITC reform that equalizes the credit schedule for workers with and without children.

To conduct these exercises, I estimate labor supply elasticities for different demographic groups and a labor substitution elasticity that governs the curvature of labor demand. I use EITC policy variation tied to the 1993 Omnibus Budget Reconciliation Act (OBRA) on labor market data from the Current Population Survey. I assign workers to demographic-based labor market cells and estimate the cell-specific expected EITC policy reform exposure via a simulated instrument approach that uses a fixed distribution of worker characteristics from the 1990 Census. This approach uses all possible EITC policy information but purges endogenous behavioral responses from the policy changes. My estimation strategy allows me to avoid the assumption that women with and without children respond the same way to wage changes, as in typical difference-in-differences based analysis of the EITC.[2] Because the incidence depends on the wage responsiveness of different labor markets, capturing granular differences in supply responsiveness is important for accurately measuring incidence effects.[3]

My primary theoretical contribution is to formalize the labor market forces that generate 'spillover effects' from targeted wage taxes between treated and untreated workers and across labor market segments. A policy that increases the absolute quantity of one worker group increases the marginal product of complementary workers and decreases that of substitutable workers. These changes in marginal product cause labor demand shifts that I interpret as spillover effects. I show that these spillover effects have 'first order' importance in market wages changes, and for treated workers positive marginal product spillovers attenuate the negative direct wage effect. The partial equilibrium incidence (or direct effect) is the *upper bound* for treated workers and the *lower*

[2]In Appendix A.3.3, I show how previous estimation approaches confront worker heterogeneity and/or the presence of spillovers for identification.

[3]In Appendix A.5, I show that using a constant labor supply elasticity of 0.75 for all groups implies implies larger (in magnitude) wage declines (up to 33%) yet 10% larger net earnings effects relative to my estimated elasticities results.

bound for untreated workers relative to the general equilibrium gross wage incidence. Because the behavior of all *other* economic agents is held fixed in PE, the marginal product changes are ignored so wage spillover effects are also ignored. Since the spillover effects are 'first order' and opposite relative to the direct effects, the general equilibrium incidence is theoretically ambiguous due to cascading feedback across labor markets.

For example, suppose there are two sets of workers, $\{A, B\}$ that are complementary to each other in the production process,[4] and we treat group A to a work subsidy. The labor supply increase of the *treated* set of workers will increase the marginal product of the *untreated* set; this causes labor demand to increase for the *untreated* workers; the resulting quantity increase in *untreated* workers will then increase the marginal product of the *treated* workers; and so on... Figure 1.1 displays these forces graphically using a two factor model with a targeted labor subsidy.

My primary empirical contribution is to quantify the magnitude of EITC induced spillovers using four policy evaluations. On an individual level, spillovers are small both in magnitude and relative to the direct effects; however, because spillovers affect every worker, spillovers are economically important when aggregated.[5] In the empirical incidence evaluations, I find spillovers increase aggregate net earnings by about 22.2% for the 1993 OBRA EITC expansion and by 17.6% for the 2009 ARRA expansion. When comparing the EITC vs Welfare, the superiority of an EITC expansion relative to a Welfare expansion in terms of net-earnings becomes 21% larger when accounting for spillovers. Equalizing the EITC for workers with and without children would cause a 395% increase in net earnings change of unmarried women without children but at the expense of 88% *decrease* for unmarried mothers. I also calculate wage changes, labor supply changes, and the fiscal externality of EITC reforms across education, marriage, and parental status that highlights the heterogeneous distributional effects of the EITC.

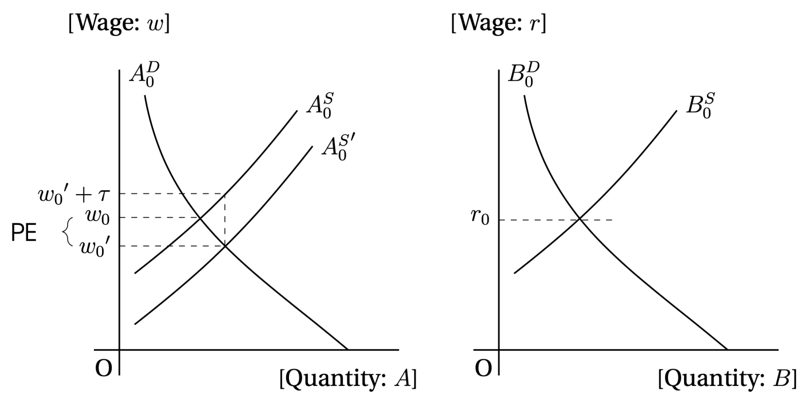
My results highlight important features of the EITC and labor market programs in general. First, inducing labor supply mechanically expands the economy's possibilities frontier, while programs

[4]For example, research assistants and professors in the production of research, where more RAs increase productivity of professors and more *vice versa*.

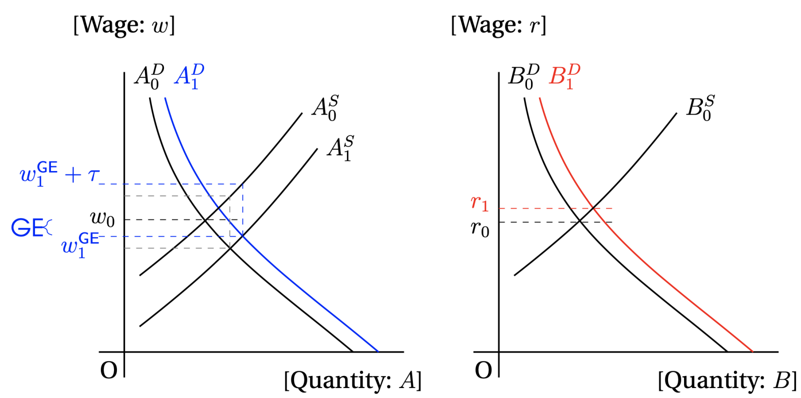
[5]I focus on aggregate effects but Appendix A.5 displays individual effects.

Figure 1.1: Labor Subsidy Incidence in Two Factor Model: $\{A, B\}$

(**a**) Partial Equilibrium



(**b**) General Equilibrium



In (**a**), a supply subsidy shifts A^S to the right. In (**b**), assuming worker complementarity, the resulting marginal product spillovers cause *both* labor demands to shift right, which attenuates the PE gross wage decline for A-market. Labor demands are derived from a supermodular production function.

that incentivize leaving the labor force will contract the frontier. Thus, policies that expand the labor force, such as the EITC, have additional pro-growth benefits, while policies that subsidize leisure have additional costs to the economy. Second, the positive spillovers onto higher-income workers seems like an unintended transfer; however, with progressive taxation, these workers have a positive tax rate and the spillovers are taxed back. Thus, the EITC can help 'pay for itself' by indirectly increasing the tax-base, in addition to the direct effect of moving workers to employment (Bastian and Jones, 2018). These forces are omitted in Rothstein (2010) whose partial equilibrium approach shows the EITC in its worst light.[6] Finally, untreated-substitute workers face downward pressure on wages while untreated-complementary workers, who are already have higher wages, get a wage bump. In the medium to long run, this may incentivize the untreated-substitute workers to either become eligible (have children) or to up-skill out of the low-wage market.

An additional empirical contribution is that by isolating EITC specific policy variation, I allow for a more fine-tuned estimate of the treatment effects of the 1993 EITC expansion. Recently work by Kleven (2018) points out that Welfare reform during the 1990's potentially contaminates estimates of the EITC expansion effects. Partially, this is because prior analysis has used 'difference-in-differences' techniques where treatment is simply group membership interacted with year indicators.[7] My estimates imply that labor supply for unmarried women with children increased 1.27% due to the 1993 EITC expansion, which is lower by a third to a tenth of the estimates summarized by Hotz and Scholz (2003).[8] This supports the claim that prior EITC estimates were contaminated by macroeconomic conditions while also showing that the EITC *did* increase women's labor supply and thereby affected the market wages of the economy.

The overarching message of this paper is that the impact of general equilibrium spillovers of conditional wage subsidies – such as the EITC, social safety-net programs, or proposed Universal Basic Income – on labor market outcomes are of first order importance. Further, because the labor market is central to the distribution of goods and services in the economy, tax policy aimed at ameliorating the financial hardships of the working poor can nevertheless have unintended consequences across all sectors of the economy.

[6]Section 1.8 and Appendix A.5 replicate Rothstein (2010) in alternate ways with each finding the EITC superior to either a parameterized AFDC or NIT expansion in general equilibrium.

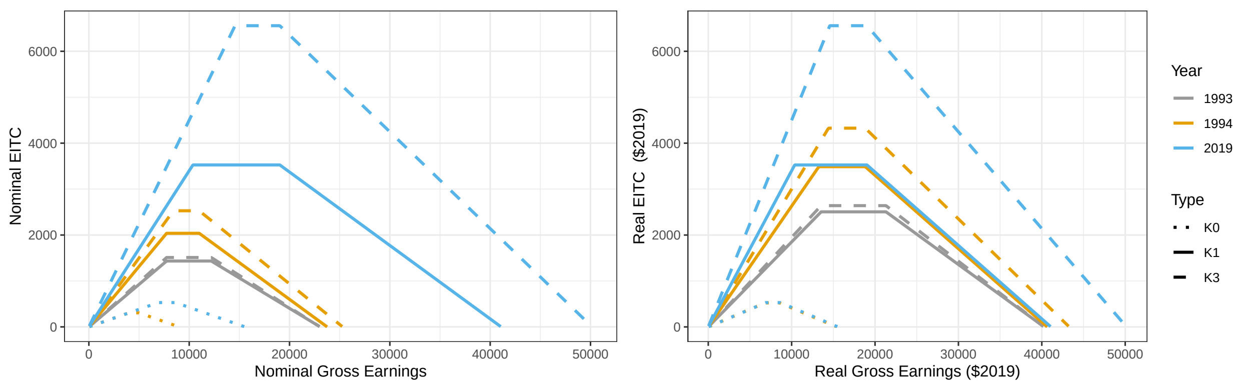
[7]In a standard labor supply model, the DID estimator is equivalent the reduced-form regression where group and time are instrumenting net-wages.

[8]Table 4 in Hotz and Scholz (2003) summarize much of the earlier empirical literature and describe the effects in terms of elasticities.

1.2 Overview of the EITC and Related Literature

This work is part of a long running effort to understand and quantify the economic and social effects of the Earned Income Tax Credit. The EITC is a $67 billion federal tax expenditure program designed to encourage work by subsidizing earned income through a refundable tax credit using a non-linear benefit schedule. Figure 1.2 shows how the program has expanded since the early 1990's to the present.

Figure 1.2: EITC Schedule by Year and Number of Children



EITC schedules for single filing household for years 1993, 1994, and 2019 by zero, one, and three children. Joint filers have a higher maximum credit and extended plateau and phase out regions. Both nominal and real ($2019) values plotted. Parameters from Tax Policy Center (2019).

The defining feature of the EITC is the phase-in region of the schedule, which increases the subsidy as earnings increase, and unambiguously promotes greater labor supply (Hotz and Scholz, 2003; Nichols and Rothstein, 2016). The phase-in differentiates the EITC from a Negative Income Tax and a traditional Welfare program, which start at a high level and tax away the benefit as earnings increase.

Roughly 40% of all single parent families and 25% of married parent families are eligible for the EITC, and 40% of all families where the primary earner has less than a high school degree are EITC eligible (Nichols and Rothstein, 2016). This massive intervention in the labor market should have economically meaningful effects on labor market sorting and equilibrium.

Previous studies have consistently found that the EITC benefit structure successfully encourages labor force participation and increases employment rates for eligible groups – primarily unmarried women workers with children and low levels of education. Two comprehensive survey articles – Hotz and Scholz (2003); Nichols and Rothstein (2016) – or two specific applications of the labor supply effects – Eissa and Liebman (1996); Eissa and Hoynes (2004) – provide a general overview of prior EITC studies.[9] Given the size of the EITC as a labor market intervention, we should expect wage and price distortions. However, most papers in the EITC labor literature assume that the EITC has had no effect on gross wages (Dickert et al., 1995; Eissa and Liebman, 1996; Saez, 2002; Eissa and Hoynes, 2004; Chetty et al., 2013). As noted by Hotz and Scholz (2003), this assumption had never been tested in first decade of EITC research.[10]

Leigh (2010) and Rothstein (2010) study the gross wage incidence of the EITC in partial equilibrium.[11] Leigh (2010), using state and federal variation, finds that a 10% increase in the maximum EITC amount leads to a 5% decrease in the real wages of high school dropouts, and, using predicted labor supply within gender-age-education labor market cells, finds that 10% increase in cell labor supply leads to a 9% decrease in real wages within the labor market cell. As mentioned earlier, Rothstein (2010) simulates a hypothetical EITC expansion change and reports that for every dollar of intended transfer real wages decrease by $0.34 (in partial equilibrium). These results imply that the EITC is not *as effective* a program as policy makers may believe and may be an

[9]More recent papers on labor market effects and net-income distributions include Fitzpatrick and Thompson (2010); Chetty et al. (2013); Jones (2017); Kasy (2017); Hoynes and Patel (2018); Bastian (forthcoming). In addition, there are many papers that assess the social impact of the EITC on various non-labor-market outcomes – health (Dahl and Lochner, 2012; Evans and Garthwaite, 2014; Hoynes et al., 2015); education (Maxfield, 2015; Bastian and Michelmore, 2018); and marriage & fertility (Dickert-Conlin and Houser, 2002; Baughman and Dickert-Conlin, 2003).

[10]Some of these papers are explicit (Eissa and Liebman, 1996; Saez, 2002; Chetty et al., 2013) and others are implicit in by holding wages fixed when simulating labor market effects (Dickert et al., 1995; Eissa and Hoynes, 2004). In Chetty et al. (2013), their model's the production function implies workers are perfect substitutes (thus no spillovers) and their empirical results depend on the stable unit treatment assumption. One potential reason for the absence is a greater initial interest in the individual policy treatment effects of tax reforms rather than policy effect on labor markets.

[11]Azmat (2019) studies the incidence, also in partial equilibrium, of a conceptually similar Working Families Tax Credit program in the UK. She finds that, due to differences in salience unique to the UK program, gross wages fall by 7% for claimants and 1.7% for non-claimants. Also, Hoynes and Patel (2018) look at after-tax income distributional effects of the EITC and show that indirect effects increase net-income of workers near the poverty threshold.

unintended transfer to non-targeted groups, such as business owners and wealthier households. My contribution to these papers is to allow for labor market spillovers that affect both treated and untreated workers, to derive an analytical formula that allows me to estimate the empirical incidence of the EITC rather than its maximum credit or hypothetical expansion, and to create a framework to predict and evaluate out-of-sample expansions.

Agrawal and Hoyt (2018b) study general equilibrium tax incidence in a multi-product consumer goods markets. They find that tax rate overshifting is possible when related goods are substitutes and find that spillovers are empirically important in alcohol markets. My paper considers taxes in multi-factor *input* markets, applies this to empirically to the EITC and Welfare programs, and also finds spillovers are empirically important.

In terms of general equilibrium effects of the EITC, this work is part of a small group. Lee and Saez (2012) allow for endogenous wages and argue that an EITC combined with an optimal minimum wage policy can prevent some of the incidence effect; however, the authors do not actually attempt to calculate the GE incidence. To build on their work, I incorporate spillover effects between labor markets and firm entry decisions allowing for an arbitrary number of factors with heterogeneous supply responses and tax changes. Kasy (2017) develops a novel estimation procedure using maximum EITC amounts to calculate the change in gross wages and labor supply along age, education, gender, and income distribution cells and finds negative earnings effects that dominate the credit, as if labor demand were completely inelastic – similar to Leigh (2010); Rothstein (2010). Because I do not rely on a difference-in-difference strategy between those with and without children, I allow for labor supply heterogeneity along parental status.[12] In addition, because I used empirical tax rates, I can compute both gross and net earnings effects. Finally, Froemel and Gottlieb (2019) develop a macroeconomic model to analyze consumption, savings, and wage determination, and find that both the gross earnings and wealth gap increase but the net earnings gap shrinks due to the EITC. To come to these conclusions, the authors use a two

[12]Additionally, the author omits common-policy-shock effects by using year indicator variables in his empirical specification. This may be one reason that his empirical estimates are similar to partial equilibrium analysis.

skill model, focus solely on married households, use an approximated EITC policy function, and ignore the distinction between workers with and without children. My work is able to account for most of these forces while maintaining a rich degree of individual heterogeneity in skills and wage responsiveness and exactly modeling the EITC.

Finally, my results are able to rationalize a startling null-finding by Kleven (2019). The author uses every state and federal EITC reform since the program's inception and only finds "clear employment increases" from the OBRA expansion, which he notes occurred along with confounding macroeconomic and policy forces. I contribute to his work by estimating labor supply elasticities using purely EITC policy variation and by calculating the incidence by a structural approach that holds these confounding variables constant. Additionally, by separately calculating the labor market effects of the OBRA and ARRA expansion, I show that most EITC expansions likely do not generate economic forces large enough to be observed using difference-in-difference methods.

1.3 Model

In this section, I describe a general equilibrium labor market model to investigate the effect of targeted labor subsidies. The primary assumptions are that worker utility is quasi-linear in a composite consumption good, production technology has constant elasticity of substitution between factors and is constant returns to scale, and worker characteristics are observed by all market participants. To make analysis simpler, I abstract from other taxation issues by assuming the subsidy is financed by lump-sum taxes on workers, except I allow for an unemployment benefit.

For exposition, I present a model with only two labor skill levels. In Appendix A.1, I derive welfare measures for the model, show that the model easily generalizes to arbitrary labor types with type-specific tax changes, and discuss two extensions: allowing labor market 'switching' and two output sectors.

1.3.1 Workers

Let there be a mass N of workers, where each is defined by a skill level, $e \in \{0, 1\}$, a parental status, $c \in \{0, 1\}$, and a continuous and stochastic disutility of labor, $\nu \sim F_{e,c}(\nu)$. Suppose that only skill determines worker productivity, so wages are positively related to skills but unrelated to parental status conditional on skill. Given perfect information and perfect labor competition, all workers with the same skill will earn the same wage.

Each worker has preferences over a homogeneous consumption good, X, and labor, L, representable by a quasi-linear utility function, $U(X, L; \nu) = X - \nu \cdot L$. Workers maximize utility by choosing a feasible labor-consumption bundle given wages (w) and the tax system. That is, each worker solves:

$$\max_{X,L}\{X - \nu \cdot L\} \quad \text{s.t. } X \leq T_c(w_e \cdot L) \quad \& \quad L \in \{0, 1\}, \tag{1.1}$$

where $T_c(w_e)$ is the net earnings after taxation, which depends on gross earnings and parental status.[13]

After substituting the budget constraint, the utility maximization problem becomes a discrete choice problem:

$$\max_{L=\{0,1\}} \left\{ \underbrace{T_c(0)}_{L=0}, \underbrace{T_c(w_e) - \nu}_{L=1} \right\} \tag{1.2}$$

The solution yields worker output demand and labor supply functions, X_i^D and L_i^S. Let $\mathsf{v}_{e,c} = T_c(w_e) - T_c(0)$, then by definition $\Pr(\nu \leq \mathsf{v}_{e,c} \mid e, c) = F_{e,c}(\mathsf{v}_{e,c})$. With specific density functions, $F_{e,c}(\mathsf{v})$, the labor supply probability of each type of worker is known; e.g., with Type-1 Extreme Value draws, labor supply has a logit form: $F_{e,c}(\mathsf{v}) = \mathsf{e}^{\mathsf{v}}/(1 + \mathsf{e}^{\mathsf{v}})$.

Thus, the aggregate labor supply functions are:

$$L_{e,c}^S = F_{e,c}(\mathsf{v}_{e,c}) \cdot N_{e,c} \quad \& \quad L_e^S = \sum_{c \in \mathcal{C}} L_{e,c}^S \quad \& \quad L^S = \sum_{e \in \mathcal{E}} L_e^S. \tag{1.3}$$

[13]In this section I ignore non-labor income as there are no income effects; however, in the empirical sections I incorporate non-labor income when calculating effective tax rates.

The labor supply elasticity for demographic group (e, c) is:

$$\frac{\partial L^S_{e,c}}{\partial w}\frac{w_e}{L_{e,c}} = \left[\frac{\partial T_{e,c}}{\partial w} f_{e,c}(\mathsf{v}_{e,c})\right] \cdot \frac{w_e}{L_{e,c}} := \varepsilon^L_{e,c}. \tag{1.4}$$

Using the logit example, $\varepsilon^L_{e,c} = \frac{\partial T_{e,c}}{\partial w} w_e(1 - F_{e,c}(\mathsf{v}_{e,c}))$. As there are no income effects for labor supply, the Marshallian and Hicksian elasticities are equivalent.

1.3.2 Production

Let there be mass J of potential producers indexed by $j \in \mathcal{J}$, each endowed with one unit of capital (K), that can hire labor to produce a homogeneous consumption good. Firms draw a capital supply cost (or entry cost), ξ_j, from a continuous distribution, $G(\xi)$. Technology is represented by a nested constant elasticity of substitution (CES) production function:

$$\begin{aligned} q^S_j = Q(\{L_{e;j}\}_e, K_j) &= A_j \left[\left(\sum_{e\in\mathcal{E}} \vartheta_e (L^D_{e;j})^{\frac{1+\rho}{\rho}}\right)^{\frac{\rho}{1+\rho}}\right]^\alpha K_j^{(1-\alpha)} && (1.5)\\ &= A_j \cdot \mathbf{L}_j^\alpha K_j^{(1-\alpha)}, && (1.6)\end{aligned}$$

where A_j is a Hick-neutral productivity term, $L^D_{e;j}$ is the firm-j type-e labor demand, and $\mathbf{L}_j$ denotes the aggregate labor index for the firm. The elasticity of substitution between labor skill-groups is parameterized by:

$$\rho = \mathsf{d}\ln[L_{e''} \,/\, L_{e'}]/\mathsf{d}\ln[w_{e''}/w_{e'}] < 0, \text{ for } e', e'' \in \mathcal{E}. \tag{1.7}$$

This technology features constant returns to scale (CRS) and assumes fixed substitution elasticities between factors.[14] Firms maximize profits: $\pi_j = p \cdot Q(\{L_{e;j}\}_{e\in\mathcal{E}}, K_j) - \sum_{e\in\mathcal{E}} w_e L_{e;j} - rK_j$. Aggregate output is defined as $q^S = \int_j q^S_j \mathsf{d}j$. Price taking, zero profits, and identical production functions imply all firms choose the same factor input bundle, so by CRS the aggregate production function is also nested CES. I normalize the output price to one, $p = 1$, so wages and capital rents are in terms of the final good.

[14]Note, when there are more than two skill groups, ρ is the *partial* elasticity of substitution. The primary modeling benefit to this technology is that it allows for tractable analytic solutions with an arbitrary number of labor types, as I use in the generalized model for the empirical applications.

Under these assumptions, the firm capital supply is synonymous with firm entry and is endogenously determined by firm capital supply costs, ξ_j, and the price of capital, r. Firm j will enter if $\xi_j \leq r$. In equilibrium, this determines the aggregate capital supply function, $K^S(r)$, and the aggregate capital supply elasticity, $\varepsilon_K^S = r \cdot \frac{g(r)}{G(r)}$.

1.3.3 Tax and Transfer System

For simplicity, suppose that initially the government raises revenue using lump-sum taxation at the level n, provides an unemployment benefit at level b, and balances its budget. Then, the government reforms the tax system to provide a labor subsidy for low skill workers with children, $\tau_{(0,1)}$ (that is paid for by lump-sum tax changes). This implies the following skill specific aggregate labor supply functions (recalling equation 1.3):

$$L_0^S = L_{0,0}^S(w_0) + L_{0,1}^S(w_0 + \tau_{(0,1)}) \tag{1.8}$$

$$L_1^S = L_{1,0}^S(w_1) + L_{1,1}^S(w_1) \tag{1.9}$$

Equation 1.8 provides intuition for the incidence formula I will demonstrate in the next section. The subsidy directly creates a work-incentive for the subsidized group. However, the equilibrium effect on gross wages distorts labor supply for unsubsidized workers.

1.3.4 Equilibrium

An equilibrium in the economy is a wage and rent schedule such that the factor market clears and firms make zero profits (thus clearing the output market). The economy is in equilibrium when no worker wishes to adjust her labor supply and no firm wishes to adjust its input bundle.

Due to the CRS assumption, the scale of factor demands cannot be determined. Fortunately, the model can be solved in terms of demand ratios. In equilibrium, the labor demand bundle must satisfy:

$$\frac{L_0^D}{L_1^D} = \left(\frac{w_0/\vartheta_0}{w_1/\vartheta_1}\right)^{\rho} \tag{1.10}$$

While the labor-aggregate and capital demand bundle must satisfy:

$$\frac{\mathbf{L}^D}{K^D} = \left(\frac{\bar{w}/\alpha}{r/(1-\alpha)}\right)^{-1}, \tag{1.11}$$

where $\bar{w} = \left(\vartheta_0 \left(\frac{w_0}{\vartheta_0}\right)^{1+\rho} + \vartheta_1 \left(\frac{w_1}{\vartheta_1}\right)^{1+\rho}\right)^{\frac{1}{1+\rho}}$ is a labor cost index. The unit cost function has the following form: $c(w_0, w_1, r) = (1/A)\left(\frac{\bar{w}}{\alpha}\right)^{\alpha}\left(\frac{r}{1-\alpha}\right)^{1-\alpha}$.

I find the the model's equilibrium conditions by equating the factor demand and supply functions and enforcing zero profits using the unit cost function, with output price normalized to one. Thus, the general equilibrium of the economy is any $\{w_0, w_1, r\}$ that solves the following equations:

$$\text{Labor Clearing} \quad \frac{L^S_{e_0}}{L^S_{e_1}} = \left(\frac{w_0/\vartheta_0}{w_1/\vartheta_1}\right)^{\rho} \tag{1.12}$$

$$\text{Factor Clearing} \quad \frac{L^S_{e_0} + L^S_{e_1}}{K^S} = \left(\frac{\bar{w}/\alpha}{r/1-\alpha}\right)^{-1} \tag{1.13}$$

$$\text{Zero Profits} \quad 1 = c(w_0, w_1, r). \tag{1.14}$$

1.4 Incidence

In this section, I present the partial and general equilibrium incidence of targeted labor subsidies for the two skill model which provides all necessary economic intuition. At the end, I present the incidence result for the full model that allows for arbitrary labor types which I use in the empirical applications. The partial equilibrium section essentially replicates Rothstein (2010) using the above model notation.

1.4.1 Partial Equilibrium

The tax reform introduces a labor subsidy for low skill workers with children, $\tau_{0,1}$. Because there is no subsidy for other types of workers, I refer to $\tau_{0,1}$ simply as τ. I find the partial equilibrium incidence by totally differentiating the labor clearing condition (equation 1.12) while holding $\{L_1, K, w_1, r\}$ constant. In the limit when the market size of subsidized group goes to zero, this result is equivalent to the general equilibrium result, discussed next. This yields (when $\hat{\tau} > 0$):

$$\hat{w}_0^{\mathsf{PE}} = \left(\frac{\varepsilon_{0,1}^L}{\varepsilon_0^L - \rho}\right) \cdot \theta_{0,1} \cdot \hat{\tau} := \gamma_0 \cdot \hat{\tau} < 0 \tag{1.15}$$

where $\hat{x}_e = x_e / w_e$ is the percent of wage change for the e-group, $\theta_{e,c} = L_{e,c}/L_e$ is the within skill share of subsidized workers, and ε_e^L and $\varepsilon_{e,c}^L$ are the group and sub-group supply elasticities, respectively, where $\varepsilon_e^L = \theta_{e,1}\varepsilon_{e,1}^L + (1-\theta_{e,1})\varepsilon_{e,0}^L$. Recall equation 1.8 and note that the numerator uses the elasticity of the subsidized group while the denominator uses the aggregate supply elasticity for the low skill market.

Interestingly, the model implies that the partial equilibrium labor demand elasticity for labor is constant, equivalent for all labor types, and equal to the labor elasticity of substitution. To see why this is the case, consider the following:[15]

$$L_0^D(w_0) = L_1^S(w_1(w_0)) \cdot \left(\frac{w_0/\vartheta_0}{w_1(w_0)/\vartheta_1}\right)^{\rho} \implies \eta_0^D = \rho + \frac{\partial w_1}{\partial w_0}\left(\varepsilon_1^L - \rho\right). \tag{1.16}$$

When $\frac{\partial w_1}{\partial w_0} = 0$ by partial equilibrium assumption, the demand elasticity equals the substitution elasticity between factors.[16] Holding w_1 and r fixed is equivalent to holding those factors' marginal product constant, but this is invalid when L_0 increases (except when the low skill group is infinitesimal).

1.4.1.1 Implication and Interpretation for Policy

When there are multiple labor types with heterogeneous subsidy changes, aggregating the PE results yields an 'employment weighted average partial equilibrium effect.' This is not of theoretical or practical interest unless it is *ex-ante* known that spillover effects will be negligible. The PE assumptions require that for any specific labor group no other group adjusts its supply, which creates a set of mutually exclusive assumptions.

[15] In the two factor CRS case, Lee and Saez (2012) show that in equilibrium, the supply responses of the second factor can be used to pin down the first factor's demand and second factor's price as only a function of the first factor's price, despite the unknown scale of production.

[16] Another way to see this is that: $\eta_e^D = \frac{\mathsf{d}\ln[L_e^D]}{\mathsf{d}\ln[w_e]} = \frac{\mathsf{d}\ln[L_e^D/L_{e'}^D]}{\mathsf{d}\ln[w_e/w_{e'}]} = \rho$ if $\mathsf{d}\ln[L_{e'}^D] = \mathsf{d}\ln[w_{e'}] = 0$.

Rothstein (2010) implies that decreases in gross wages are a transfer to *firms* at the expense of workers: "this implies that employers of low-skill labor capture a portion of the intended EITC transfer" and "...targeted work subsidies produce unintended transfers to employers...".[17] While Rothstein's partial equilibrium analysis is technically correct, the interpretation of his result does not necessarily follow for two reasons.

First, with zero profits, there are no explicit profits for firms. With CRS technology, if one factor price goes down, then another must increase, so the *owners* of the other factors benefit if low skill wages fall.[18] Second, if entrepreneurs own some of the other factors (such as capital), then entrepreneurs may 'capture' the wage subsidy because their own factor payments increase. However, the production function in Rothstein (2010) only includes labor factors, so there is no possible factor to be owned by entrepreneurs.[19]

However, the 'all else equal' for the PE incidence requires the prices and quantities of all other factors be held *fixed*, which means that owners of other factors *cannot* actually realize any factor price increases. Thus, a partial equilibrium story is incapable of yielding Rothstein's conclusion about transfers to firms at the expense of workers. In order to render the conclusion about firm owners benefiting from changes in gross wages, one must use a general equilibrium analysis.

1.4.2 General Equilibrium

To calculate the incidence, I totally differentiate equations 1.12, 1.13, and 1.14 with respect to $\{w_0, w_1, r, \tau\}$. Since the two type model system has three equations and three unknowns ($\mathsf{d}w_0, \mathsf{d}w_1, \mathsf{d}r$), I can solve the for a change in low skill wages using iterative substitution. Use the zero profits condition to solve $\mathsf{d}r = f(\mathsf{d}w_0, \mathsf{d}w_1)$, use the labor clearing condition to solve $\mathsf{d}w_1 = g(\mathsf{d}w_0, \mathsf{d}\tau)$, and then substitute into the factor clearing condition for $\mathsf{d}w_0 = h(\mathsf{d}\tau)$. This

[17]Kasy (2017) makes a similar claim based on his results.

[18]Alternatively, holding other wages and rents constant, the output price must decrease which benefits consumers – especially low income – rather than firm owners.

[19]In an earlier working paper, Rothstein's production function did include capital but this was omitted in the published version.

yields:

$$\hat{w}_0^{\mathsf{GE}} = \left(\frac{-\varepsilon_{0,1}^L \theta_{0,1}}{\left(\varepsilon_0^L - \rho\right)} + \frac{s_{L0}\left(\frac{\varepsilon_{0,1}^L \theta_{0,1}}{(\varepsilon_0^L - \rho)}\right)\left(\frac{\varepsilon^K + 1}{s_K} + \frac{1+\rho}{s_L}\right)}{(\varepsilon_0^L - \rho)\left(1 + \left(\frac{\varepsilon^K + 1}{s_K} + \frac{1+\rho}{s_{L0} + s_{L1}}\right)\left(\frac{s_{L0}}{\left(\varepsilon_0^L - \rho\right)} + \frac{s_{L1}}{\left(\varepsilon_1^L - \rho\right)}\right)\right)} \right)\hat{\tau} \tag{1.17}$$

$$:= (\gamma_0 + \Gamma_0) \cdot \hat{\tau},$$

where γ_0 is the PE gross wage effect and Γ_0 is the GE spillover term, and s_h are factor cost shares. Thus, the GE incidence is the direct (PE) effect plus a weighted sum of cross-factor effects.[20] Since $\Gamma_0 \geq 0$, a subsidy increase for low skill labor implies that the spillover effects attenuate the PE wage effects, so workers retain more of the subsidy than is implied by the PE analysis.

Solving for the other price effects (when $\hat{\tau} > 0$): $\hat{w}_1^{\mathsf{GE}} = \left(\frac{\varepsilon_0^L - \rho}{\varepsilon_1^L - \rho}\right)\Gamma_0 \hat{\tau} \geq 0$ and $\hat{r}^{\mathsf{GE}} = -\left(\frac{s_{L0}}{s_K}\hat{w}_0^{\mathsf{GE}} + \frac{s_{L1}}{s_K}\hat{w}_1^{\mathsf{GE}}\right)$.[21] With only a low skill labor subsidy, the PE analysis provides an upper bound for the low skill labor market wage effect, but PE is completely uninformative about the magnitude of the other input price effects since these depend on GE spillover terms.

As alluded to before, $\hat{w}_0^{\mathsf{PE}} = \hat{w}_0^{\mathsf{GE}}$ only if $s_{L0} = 0$, which is a small-market assumption that makes little sense in a two type model.[22] Figure 1.3 provides a visual comparison of PE and GE incidence for a 1% effective subsidy increase for L_0 as implied by different endogenous cost shares.

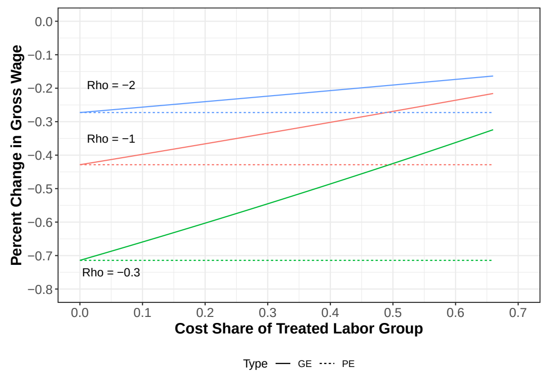
Figure 1.3 also shows the importance of the substitution elasticity, ρ. When inelastic, as in Rothstein (2010), the PE incidence implies large wage effects; however, when more elastic, as in my estimates presented in Section 1.5, the wage effects are smaller. This pattern is because a larger elasticity implies a firm can more easily adjust its factor demand bundle to take advantage of cost savings.

[20]Equation 1.17 resembles the result in Agrawal and Hoyt (2018b) in that the general equilibrium incidence is a linear function of the PE incidence and GE spillover effects.

[21]A sufficient condition for $\hat{r}^{\mathsf{GE}} > 0$ is that $(s_L/s_K)\varepsilon^K + (1/s_K) > -\rho$. If $s_K = 0.33$ and $\varepsilon^K = 1$, then $\hat{r}^{\mathsf{GE}} > 0$ when $\rho > -5$, which other authors and I find empirically (Katz and Murphy, 1992; Goldin and Katz, 2009; Borjas et al., 2012).

[22]As noted earlier, around 20% of tax units receive the EITC and 40% of all workers with children (Nichols and Rothstein, 2016).

Figure 1.3: Incidence Comparison Across Labor Substitutions



This plots the percent change in gross wages for low skill workers from a 1% subsidy increase at different substitution elasticities and cost shares. Other parameters: $\varepsilon_0^L = 0.75$, $\varepsilon_1^L = 0.6$, $\varepsilon^K = 1$. Details in Appendix A.1.

1.4.2.1 General Equilibrium Incidence with Many Labor Markets

Adding additional types of labor in this context is relatively simple given the symmetry of the model.[23] Let skills be indexed by $e \in \{0, 1, 2, \ldots, E\} = \mathcal{E}$. I allow arbitrary skill-specific subsidies ($\hat{\tau}_e$), and then solve the equations in the same manner as before using iterative substitution after totally differentiating. Full details are in Appendix A.1.

The general equilibrium incidence for type e' labor is:

$$\hat{w}_{e'}^{\text{GE}} = \frac{-\varepsilon_{e',1}^L \theta_{e',1} \hat{\tau}_{e'}}{\varepsilon_{e'}^L - \rho} + \frac{\Lambda \left(\sum_e \frac{s_e \varepsilon_{e,1}^L \theta_{e,1} \hat{\tau}_e}{\varepsilon_e^L - \rho} \right)}{(\varepsilon_{e'}^L - \rho) \left(1 + \Lambda \left(\sum_e \frac{s_e}{\varepsilon_e^L - \rho} \right) \right)} \tag{1.18}$$

$$= \left(\gamma_{e'} + \Gamma_{e'} \right) \hat{\tau}_{e'} + \Psi_{e'}(\{\tau_e\}_{e \in \mathcal{E} \setminus e'}) \tag{1.19}$$

$$\text{where } \Lambda = \left(\frac{\varepsilon^K + 1}{s_K} + \frac{1 + \rho}{s_L} \right). \tag{1.20}$$

Generally, one cannot sign equation 1.18 without knowing the magnitude of each $\{\tau_{e'}\}_e$. For example, if the tax change for one group is small *but* all other changes are large and positive, then the GE spillovers may dominate, so the wage change would be positive.

Equation 1.18 shows three first order terms with respect to a tax reform: the direct effect, the

[23] In the empirical applications, $|\mathcal{E}| = 72$ based on age, education, and marital status of women.

own-supply induced marginal product spillovers, and the received marginal product spillovers from other tax changes. Only if both spillover terms are small will $w^{\mathsf{GE}} \approx w^{\mathsf{PE}}$; e.g., if the cost share weighted average tax change is zero: $\mathsf{E}[s_e \tau_e \theta_{e,1}] = 0$.[24]

1.5 Estimating Labor Market Elasticities

In this section, I describe how I estimate labor supply and substitution elasticities: $\left(\{\varepsilon_{e'}\}, \rho\right)$, which are used in the empirical applications in sections 1.7- 1.11. In summary, I combine two data sets to calculate the labor market variables: the 1986-2000 Current Population Surveys (Flood et al., 2018) and the 1990 US Census 5% sample, (Ruggles et al., 2018).[25] Next, I use NBER's TAXSIM (Feenberg and Coutts, 1993) to create EITC induced average tax rate changes as the empirical analogue of $\hat{\tau}$. Finally, I use a two-step efficient GMM to estimate the supply and substitution elasticities. Additional details and results are in Appendices A.2-A.4.

1.5.1 Data

I use the 1986 to 2000 CPS Outgoing Rotation Group (ORG) samples for labor market information by state and year. The sample asks detailed employment, earnings, and household structure information from roughly 100k households per month. I pool the monthly samples for annual level labor market variables.[26]

I assign workers to their labor skill levels based on observable demographic characteristics. Labor skill levels are defined by four education categories, nine age groups, and marriage status – this implies 72 skill levels.[27] I assign workers to a labor markets based on the worker's skill level,

[24]Agrawal and Hoyt (2018b) make this point by supposing that the market share of taxed goods is small relative to a composite consumption good.

[25]I use two subsamples from the CPS: the Outgoing Rotation Groups (ORG) and the Annual Social and Economic (ASEC) samples.

[26]I drop individuals who were not interviewed or in group quarters, variable values that were allocated, married workers without a cohabitating spouse, full time students out of the labor force, and households with greater than 10 members because of the difficulty in assigning children for complex family structures (less than 0.5 percent of the sample).

[27]That is, 72 skill levels for each gender though I focus only on women workers for my empirical analysis.

state, and year. Additionally, I assign workers to demographic groups by dividing the labor market between workers with and without children. This yields $72 \times 51 \times 15$ labor market cells – $e \in \mathcal{E}$– and $2 \times 72 \times 51 \times 15$ demographic cells – $(e, c) = d \in \{\mathcal{E} \times \{0, 1\}\}$.[28]

For labor market quantities, I use total hours worked divided by total potential workers at the labor market level.[29] For labor market prices, I calculate a worker's real effective wage as earnings per week divided usual weekly hours deflated using the the Bureau of Labor Statistics Consumer Price Index (BLS CPI) All Items Research Series (Bureau of Labor Statistics, 2019).[30] Appendix A.2 includes additional details and summary statistics.

I use the 1990 US Census 5% sample to calculate demographic-specific simulated instruments for the EITC policy changes.[31] Specifically, I calculate EITC tax parameters for every tax year using NBER's Internet TAXSIM for the fixed 1990 worker population. The primary EITC tax parameter is the average tax rate associated with the EITC (EITC ATR), defined as $\tau^{\text{EITC ATR}} = \frac{\text{EITC(Actual)}-\text{EITC(No Work)}}{\text{True Earnings}}$. I also calculate an indicator for if a worker is eligible for the EITC and the change in EITC amount from one tax-year to the next holding earnings constant.

I further describe the instrument construction and formalize the exogeneity requirements in Section 1.5.3 and Appendix A.3, but the virtue of using the Census is that by using the fixed population, all variation in the tax parameters is due to policy reforms over time and space and initial exposure levels of the EITC to these reforms.[32] That is, the variation in the simulated tax

[28]This follows the baseline market definition in Rothstein (2010), except I add geographic delineation by state. The benefit to this definition is that I 'observe' the skill level of unemployed workers.

[29]This measure captures both extensive and intensive margin responses that are relevant for labor market equilibrium. In Appendix A.4, I present results using the total number of workers that captures only the extensive margin response.

[30]This variable is the log geometric mean wage, which interpretable as an hours weighted productivity index (Borjas et al., 2012).

[31]Simulating tax parameters to generate instruments is also used in numerous prior studies such as: Dickert-Conlin and Houser (2002); Gruber and Saez (2002); Rothstein (2008); Leigh (2010); Bastian and Michelmore (2018).

[32]In this way, the tax instruments are similar to 'shift-share' instruments. See the following on recent analysis concerning the general identifying assumptions of these instruments: Adao, Kolesár and Morales (2018); Borusyak, Hull and Jaravel (2018); Goldsmith-Pinkham, Sorkin and Swift (2018).

parameters is *not* due to any endogenous behavioral response to the policy reforms – see Figure 1.5 below.

1.5.2 Summary Statistics

Table 1.1 displays the difference in labor market variable means before and after tax year 1993 conditional on marriage and parental status to highlight the identification using EITC policy tax changes. The first two variables are averages of the EITC Average Tax Rates, where the first is the instrument calculated from the 1990 Census and the second use values from the ASEC samples (described later), which incorporate endogenous behavioral responses. Before the reform, the true and simulated tax rates are similar, but post-OBRA the true tax rates are lower (implying a larger credit). This is due to endogenous labor supply increases in the true rates but not the simulated rates, as the instrument calculation holds fixed labor supply decisions.[33]

Additionally, Table 1.1 shows that labor supply increased for unmarried women with children and married women but decreased slightly for unmarried women without children. Despite these supply increases, there are meaningful wage increases for every group in this period. The summary statistics show that the labor demand must dominate the supply increases to result in positive wage growth.[34] For this reason, I use EITC-specific policy variation that is unrelated to demand shocks to untangle these competing forces.

I plot the data from Table 1.1 in Figures 1.4 and 1.5. In Figure 1.4 I plot log total hours per worker and mean log gross wages by demographic groups during the 1990's. These are the primary outcome and and endogenous explanatory variable in the empirical specification, respectively.

In Figure 1.5, I plot the simulated EITC ATRs and EITC take-up shares against the empirical measures from the ASEC. The primary policy change for unmarried mothers occurred over tax

[33]While this could be due to earnings decreases (from lower wages or less supply) that cause workers to qualify for more credits, Table 1.1 shows wage and labor supply increased for unmarried mothers.

[34]The 1990s were a time of technological change and favorable macroeconomic conditions which can exaggerate EITC effects on labor supply and confound the wage effects (Nichols and Rothstein, 2016; Kleven, 2019).

Table 1.1: Summary Statistics for Estimation Sample

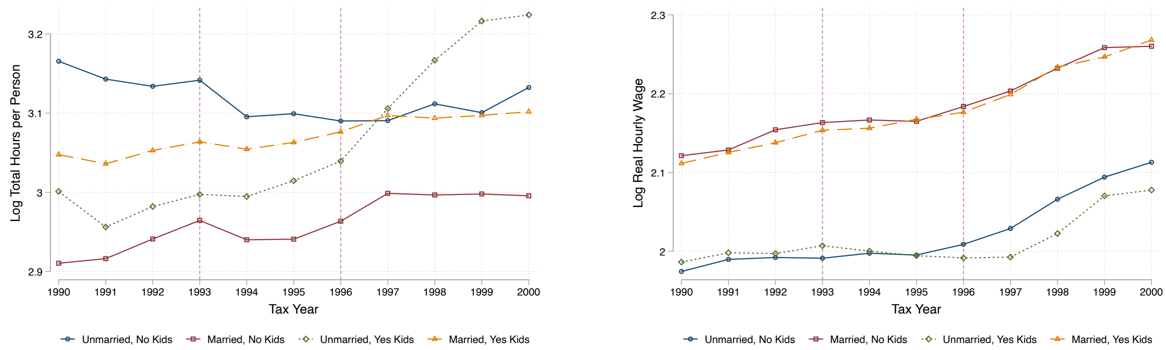
Tax Years	1989 - 1993		1995-1999		Difference	
	Mean	SD	Mean	SD	b	t
	Unmarried Women w/ Children					
EITC ATR - 1990 Census	-0.08	0.04	-0.14	0.08	-0.06***	-40.86
EITC ATR - ASEC	-0.08	0.06	-0.16	0.11	-0.08***	-34.20
Log Hours Per Person - ORG	3.08	0.54	3.19	0.44	0.11***	8.55
Log Real Wage - ORG	2.15	0.31	2.47	0.33	0.32***	39.09
Observations	2560		3854		6414	
	Unmarried Women w/o Children					
EITC ATR - 1990 Census	0.00	0.00	-0.01	0.01	-0.01***	-69.49
EITC ATR - ASEC	0.00	0.00	-0.01	0.01	-0.01***	-32.69
Log Hours Per Person - ORG	3.32	0.37	3.28	0.35	-0.05***	-5.01
Log Real Wage - ORG	2.15	0.31	2.47	0.33	0.32***	39.47
Observations	2589		3864		6453	
	Married Women w/ Children					
EITC ATR - 1990 Census	0.00	0.00	0.00	0.01	0.00***	14.72
EITC ATR - ASEC	0.00	0.01	0.00	0.02	0.00	1.92
Log Hours Per Person - ORG	3.03	0.40	3.10	0.34	0.07***	8.34
Log Real Wage - ORG	2.23	0.30	2.58	0.32	0.35***	54.45
Observations	3809		5349		9158	
	Married Women w/o Children					
EITC ATR - 1990 Census	0.00	0.00	0.00	0.00	-0.00***	-7.65
EITC ATR - ASEC	0.00	0.00	0.00	0.00	-0.00***	-4.53
Log Hours Per Person - ORG	3.27	0.39	3.29	0.34	0.02**	2.68
Log Real Wage - ORG	2.23	0.30	2.58	0.32	0.35***	54.49
Observations	3844		5336		9180	

All data from CPS Samples 1990 to 2000 and 1990 US Census. EITC ATRs calculated using TAXSIM.

years 1993 to 1996, while the only policy change for unmarried women without children was in tax year 1993. For unmarried mothers, the true ATR is less than the simulated ATR that holds labor supply fixed, which is consistent with workers entering the labor force at lower earnings. The simulated share predicts that fewer unmarried mothers would claim the EITC starting in tax year 1996 due to an added income test.

Many empirical EITC studies assume that the EITC policy changes for workers without children is not enough to affect behavior. The figures show this is a reasonable assumption because I can

Figure 1.4: Labor and Wages Across Demographic Groups

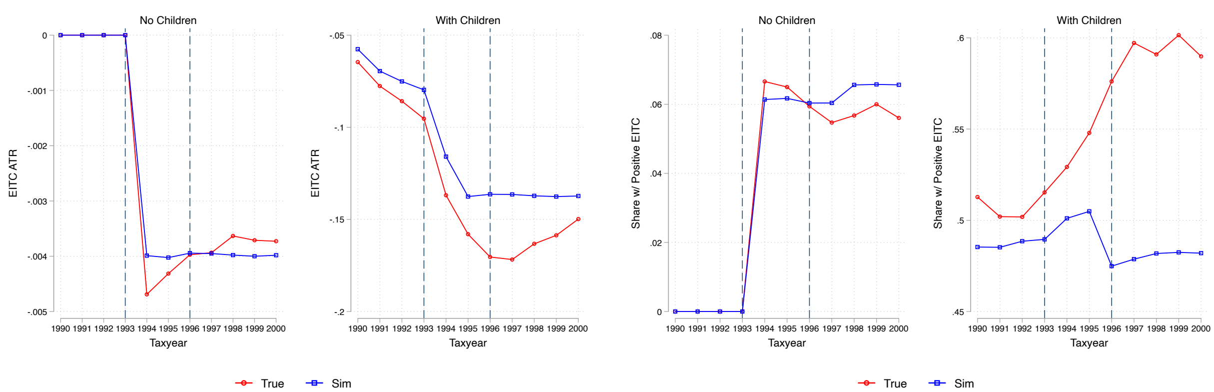


(a) Log Hours per Worker (b) Log Wage

This plots log total hours per worker (**a**) and mean log real wage (**b**) using CPS ORG samples of women (1990-2000) by marriage and parental status. Log total hours per worker is used as the measure of labor quantity and mean log real wage as labor prices.

predict the the EITC ATR and share using only the 1990 distribution of labor supply and inflation.

Figure 1.5: Simulated vs True EITC Parameters



(a) EITC ATR (b) Share w/ EITC

This plots the average EITC ATR (**a**) and share with EITC (**b**) for unmarried women-headed tax units calculated using ASEC ('true') or 1990 Census ('sim') samples and NBER TAXSIM. The 1990 Census values are uses as simulated IVs for labor market outcomes.

1.5.3 Identification

Succinctly, the incidence model – and Figure 1.1 – elucidates that the EITC creates both supply and demand variation in wages that can be used to identify labor supply and labor substitution elasticities:

$$\underbrace{\mathsf{d}w_{est}}_{\substack{\text{Wage Variation}\\ \text{in the Data}}} = \underbrace{\underbrace{\gamma_e \mathsf{d}\tau_{est}}_{\text{Supply Shift}} + \underbrace{\Psi_{est}(\{\tau_{e'st}\}_{e'})}_{\text{Demand Shift}}}_{\text{Incidence Model}} + \underbrace{v^w_{est}}_{\substack{\text{Unobserved}\\ \text{Variation}}} . \tag{1.21}$$

As discussed in Watson (2020), supply elasticities are identified using spillover based demand variation and conditioning on the own tax rate that controls for supply shifts; whereas, demand elasticities are identified using the tax reform supply shock and conditioning on the demand spillovers.

A sufficient set of identifying assumptions for both labor supply and substitution elasticities is that:

$$\mathsf{E}[\tau_{est} \cdot u^D_{e'st} \mid \Psi_{est}, X] = 0, \ \forall\, e, e' \in \mathcal{E} \tag{1.22}$$

$$\mathsf{E}[\Psi_{est} \cdot u^S_{e'cst} \mid \tau_{ecst}, X] = 0, \ \forall\, e, e' \in \mathcal{E}, \tag{1.23}$$

where $\tau_{est} = \theta_{e0st}\tau_{e0st} + \theta_{e1st}\tau_{e1st}$.[35] In words, tax rate variation is uncorrelated to both unobserved non-spillover demand shocks – e.g., skill biased technical change or changes in hiring costs – and to unobserved supply shocks – e.g., employment opportunity costs. See Appendix A.3.1 for more details and derivation.

To empirically implement this, I create two sets of IVs using the 1990 Census sample, which I call the own-market IVs and the substitute-market IVs. The own-market IVs are calculated using a simple average of simulated individual EITC variables within a given demographic-skill state-year group. These variables measure the direct effect of the EITC on a given market group. This is what is plotted in Figure 1.5.

The substitute-market IVs are calculated using two sets of 'leave-out' averages in the same state-year. The first set is based on similar education groups and the second is based on similar age group. For example, consider the group 'young, unmarried women with less than a high school degree,' then the first IV set is based on averaging across all women with less than a high school

[35] These assumptions are slightly stronger than necessary, particularly for the substitution elasticity, but would imply the necessary assumption I show in Appendix A.3.1.

degree but leaving out the young, unmarried group in that average. Further, by conditioning on the own-market EITC parameters, the remaining variation is orthogonal to the direct tax shock to any particular group. These IVs use EITC exposure, but not responsiveness, of close-substitute workers.

1.5.4 Estimating Equations

To estimate the labor supply and substitution elasticities, $(\{\varepsilon_{e'}\}, \rho)$, I use two-step efficient GMM with standard errors clustered at the labor market level (Hansen, 1982).[36] While theoretically possible to estimate the supply and substitution elasticities jointly, I estimate the parameters in two separate steps (Zoutman et al., 2018; Watson, 2020).[37]

I identify the labor supply elasticities, ε_d^L, using variation *within* demographic cells across state-years. That is, identification comes from the differences in EITC induced wage spillovers – i.e., demand shocks – within a demographic group due to differential exposure to EITC reforms in a given state-year. For example, suppose in state A relative to B there are more unmarried mothers, then state A has greater exposure to EITC reforms, so the resulting supply shock will create larger demand spillovers. Additionally, if A and B have similar shares of unmarried mothers but A implements a state EITC, then A will have a larger EITC policy shock.[38] By conditioning on the demographic group's own EITC change, the remaining skill-level variation in the EITC is due to demand shocks. I describe this argument in greater detail in Appendix A.3.1.

To estimate the heterogeneous labor supply elasticities while controlling for market conditions via fixed effects, I specify the coefficient on log market wage as function of marriage, parental, and

[36]Appendix A.4 additional empirical specifications.

[37]The linearized deviations from equilibrium, used to arrive at equation 1.18, form a linear system of equations that could be estimated using GMM, similar to Suárez Serrato and Zidar (2016). At the expense of efficiency, separating the estimation tasks allows for the parameters to be transparently identified and more robust to misspecification.

[38]Fourteen states (and DC) had an state EITC program between 1990 and 2000: CO, IA, IL, KS, MA, MD, ME, MN, NJ, NY, OR, RI, VT, WI.

education status. This leads to the following estimation equations:

$$\ln[W]_{dst} = \pi_0 + Z_{dst}\Pi_1 + [Z_{dst} \cdot \mathsf{g}_d]\,\Pi_d + \pi_2\tau_{dst} + \pi_3 \ln[P_{dst}]$$
$$+ \mathsf{d}_d + \mathsf{d}_{st} + \mathsf{d}_{w_0^{\%},t} + \mathsf{d}_{lst}^{\mathsf{BMW}} + \mathsf{d}_{kst}^{\mathsf{waiver}} + e_{dst}^{w} \tag{1.24}$$

$$\ln[L]_{dst} = \beta_0 + \varepsilon_1^L \ln[W]_{dst} + \varepsilon_g^L\,[\ln[W]_{dst} \cdot \mathsf{g}_d] + \beta_2\tau_{dst} + \beta_3 \ln[P_{dst}]$$
$$+ \mathsf{d}_d + \mathsf{d}_{st} + \mathsf{d}_{w_0^{\%},t} + \mathsf{d}_{lst}^{\mathsf{BMW}} + \mathsf{d}_{kst}^{\mathsf{waiver}} + e_{dst}^{L} \tag{1.25}$$

where Z are market level simulated EITC instruments from the 1990 Census, τ_{dst} is the own EITC ATR simulated from the 1990 Census, $\ln[P_{dst}]$ is log cell population, g_d are indicator variables for marriage, parental, and education status, d_d are demographic group fixed effects (FEs), d_{st} are state-year FEs, $\mathsf{d}_{w_0^{\%},t}$ are FEs for initial (1989) wage percentiles interacted with year indicators, $\mathsf{d}_{lst}^{\mathsf{BMW}}$ are FEs for percent of workers in 1990 that are have wages at or below the prevailing state minimum wage interacted with year indicators, and $\mathsf{d}_{dst}^{\mathsf{waiver}}$ are FEs for state welfare waivers interacted with parental status indicators. The implied elasticity for a given labor market is $\varepsilon_d^L = \varepsilon_1^L + \varepsilon_{g(d)}^L$.

The controls are meant to absorb any demand or supply shocks other than the EITC policy changes that may affect labor supply. The demographic group FEs, d_d, control for any time invariant correlation between wages and labor supply that is specific to a demographic group; e.g., demographic level tastes for working. The state-year FEs, d_{st}, control for any state-year level correlations across demographic groups; e.g., a state policy change that affect the cost of working for all workers. The initial wage percentile FEs, $\mathsf{d}_{w_0^{\%},t}$, control for any correlations at specific to a market's wage segment before the EITC expansions; e.g., mean-reversion in wages or skill biased technological change. The binding-minimum-wage FEs, $\mathsf{d}_{lst}^{\mathsf{BMW}}$, control for the degree to which supply responses are limited by binding minimum wages[39], Finally, the waiver FEs, $\mathsf{d}_{dst}^{\mathsf{waiver}}$, control for correlations that are due to state welfare changes prior to the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA), provided by Kleven (2019).

I identify the substitution elasticity by using using variation *between* skill levels across state-years. I use relative EITC supply shocks across skills as the identifying variation, and condition on

[39]As discussed earlier, a binding-minimum-wage limits the degree of price responsiveness which in turn limits the changes in market quantities underlying the general equilibrium forces.

market spillovers. I estimate a single substitution elasticity for all skill groups using the following equation:

$$\begin{aligned}\widetilde{\ln[W]}_{est} &= \gamma_0 + \gamma_1 \tilde{\tau}_{est} + \gamma_2 \tilde{Z}_{est} + \gamma_3 \widetilde{\ln[P]}_{est} \\ &\quad + \mathsf{d}_{et} + \mathsf{d}_{st} + \mathsf{d}_{w_0^{\%},t} + u^{w}_{est} \qquad (1.26)\\ \widetilde{\ln[L]}_{est} &= \alpha_0 + \rho \widetilde{\ln[W]}_{est} + \alpha_2 \tilde{Z}_{est} + \alpha_3 \widetilde{\ln[P]}_{est} \\ &\quad + \mathsf{d}_{et} + \mathsf{d}_{st} + \mathsf{d}_{w_0^{\%},t} + u^{L}_{est}, \qquad (1.27)\end{aligned}$$

where $\tilde{x}_{est} = x_{est} - x_{0st}$, the log difference. I use controls analogous to the supply model but with interpretation based on relative quantities and wages.[40] I make one important change in FEs: the market level FE d_{et} pools married and unmarried markets (i.e., only interacts age and education) and is additionally interacted with year to absorb skill-specific shocks to labor demand.[41]

1.5.5 Elasticity Estimates

Table 1.2 displays the estimated elasticities.[42] The results show that labor supply responsiveness decreases with education, that having children makes one less responsive to wages, and that married women are more responsive than unmarried women.

My estimate for the labor supply elasticity for unmarried mothers with low education attainment is quite similar to other estimates. I estimate the value 0.82 while Rothstein (2008) estimates a value of 0.75 and Meyer and Rosenbaum (2001) estimate 0.83 for participation for work in an

[40]I do not use state Welfare Waivers in this specification because at the market level they are perfectly colinear with the state-year FEs. I do not use binding-minimum-wage FEs, but unreported robustness tests show no meaningful change in elasticity estimates.

[41]Each change is of first order importance for the estimated elasticity. Interacting skill with year is justified by the theoretical relationship: $\ln[L^A_t/L^B_t] = \rho\left(\ln[w^A_t/w^B_t] - \ln[\theta^A_t/\theta^B_t]\right)$. The decision not to include marriage status in the FE is of necessity as its inclusion absorbs too much variation in the instruments and causes the covariance matrix to be nearly singular. See Appendix A.4 for additional empirical specifications that display the issue.

[42]In Appendix A.4, I present additional specification results, including alternative dependent variables.

average week.[43] I find that unmarried women without children and less than a high school degree have an elasticity of 1.16, and I can reject that the labor supply elasticities for unmarried women with and without children are equal. This can imply a violation of "parallel trends" when using difference-in-difference methods because workers will respond differently to labor market effects on gross wages.

My estimates for married women with low education are higher than previous estimates. I estimate the value 0.89 while Eissa and Hoynes (2004) estimate 0.27 for similarly educated married women.[44] Bargain and Peichl (2016) survey labor supply elasticities across countries and show estimates for married women range from almost perfectly inelastic to 1.50 for the United States.

Table 1.2: Labor Supply Elasticity Estimates by Labor Groups: ε_d^L

	Hours per Worker			
	w/o Children		w/ Children	
	Unmarried	Married	Unmarried	Married
Less HS	1.16	1.36	0.82	0.89
	(0.07)	(0.07)	(0.08)	(0.08)
HS	0.85	1.05	0.51	0.58
	(0.06)	(0.05)	(0.06)	(0.06)
Some College	0.82	1.02	0.48	0.55
	(0.05)	(0.05)	(0.05)	(0.05)
BA Plus	0.53	0.73	0.19	0.26
	(0.05)	(0.04)	(0.6)	(0.05)
	Obs	AR F	KP rk Wald F	MOP Effective-F
	47,339	39.84	39.76	16.68

All data from ORG 86-00, 1990 Census; EITC ATRs calculated using TAXSIM. Standard Errors clustered by (144) demographic groupings. Weighted by number of observations in each labor market. Model controls: log cell population, FEs for demographics, State-Year, Initial-Wage-Pct-Year, and Welfare Reforms. KP rk Wald F is cluster robust Cragg-Donald stat; AR is cluster robust F stat of IVs on structural equation residuals. MOP Effective-F is an alternative weak-IV F-statistic, calculated using a linear function of wages (Olea and Pflueger, 2013; Pflueger, 2015)

[43] Additionally, Dickert et al. (1995) calibrate a labor supply estimate of 0.85 and the difference-in-differences result from Eissa and Liebman (1996) implies an elasticity of 1.16, which coincidentally is my estimate for unmarried women with low education but no children.

[44] One reason for the difference could be that Eissa and Hoynes (2004) estimate a joint labor supply decision at the individual level while I hold constant the married partner's labor supply and treat this an non-labor income for the wife. Another reason could be that Eissa and Hoynes (2004) use a longer time series of policy variation, while my variation linked to the 1993 OBRA expansion only.

Table 1.3 presents estimates of the labor substitution elasticity between labor markets for the two relative labor supply measures. Column (1) is just identified using the 'relative' EITC ATR and column (2) is overidentified using the 'relative' EITC ATR, change in EITC amount, and share in with EITC. For each estimate I report the cluster robust standard error in parentheses. Additionally, I report the Weak IV Robust confidence interval based on Andrews (2018). For both specifications, I can reject that the substitution elasticity is inelastic, which is in line with the immigration literature estimates around -1.4 (Katz and Murphy, 1992; Goldin and Katz, 2009; Borjas et al., 2012). A more inelastic estimate of ρ will tend to imply larger magnitude incidence effects since ρ is in the denominator of equations 1.15 and 1.18.

Table 1.3: Labor Substitution Elasticity Estimates Across Labor Markets

	Hours per Worker	
	(1)	(2)
ρ	-1.81	-1.57
Wald SE	(0.30)	(0.45)
WIVR CI	[-2.43,-1.29]	[-3.11,-1.38]
KP rk Wald F	67.28	13.77
Anderson-Rubin F	39.47	5.68
MOP Effective-F	110.08	15.74
# IVs	1	3
Obs	19,501	19,501

All data from ORG 86-00, 1990 Census; EITC ATRs calculated using TAXSIM. Column (1) is just identified using relative EITC ATRs; columns (2) uses additional IVs. Weighted by geometric mean of labor market observation pairs. Standard Errors clustered by (63) labor market groupings. Weak IV Robust CIs based using AR (1) or LC test (2,3) (Andrews, 2018; Sun, 2018). Model controls: log relative cell population, FEs for Edu-Age-Year, State-Year, and Initial-Wage-Quintile-Year. KP rk Wald F is cluster robust Cragg-Donald stat; AR is cluster robust F stat of IVs on structural equation residuals. MOP Effective-F is an alternative weak-IV F-statistic (Olea and Pflueger, 2013; Pflueger, 2015).

1.6 Empirical Policy Evaluation Methodology

In this section, I outline how I combine the incidence model, estimated elasticities, and data to derive the policy evaluation results. I present three types of results: model implied gross wage changes, labor changes, and per dollar effects (multipliers). The wage and labor changes are based on estimates elasticities and tax/subsidy changes. The per dollar effects closely follow Rothstein (2010) but incorporate spillovers and update formulas to allow for changes in welfare program usage and tax payments given earnings changes.

1.6.1 Data

I use the Annual Social and Economic sample from the March CPS as this sample contains employment and income information in the previous calendar year that is necessary to calculate Federal average tax rates and EITC specific ATRs (Flood et al., 2018). Specifically, I use the 1994 ASEC for the 1993 OBRA expansion and the 2009 ASEC for the 2009 ARRA expansion. This sample delivers baseline labor and wage levels, unearned income levels, cost shares (labor share by demographic group), and average tax rates (Federal and EITC). I use the same definition of skills and demographics as in the empirical section. However, for the policy evaluations, I no longer distinguish between states and only use Federal EITC variation due to the ASEC being about 1/10 the sample size as the ORG samples in the empirical section. While the ASEC sample asks about welfare program usage, I combine this sample with the output of the Urban Institute's Transfer Income Model 3 (Urban Institute, 2020) to complement the reported amount.[45] The TRIM3 simulates household and family level transfer program amounts that is analogous to the NBER's TAXSIM model for tax rates and credits. For more details about the sample, see Appendix A.2.2.

1.6.2 Model Wage and Labor Changes

To calculate model implied wage and labor changes, I combine the data described above and the elasticities from the Section 1.5 results. I calculate and report the model implied wages percent

[45] At every point in the earnings distribution, I find self reported amounts are less than from the TRIM model (Meyer et al., 2015; Meyer and Mittag, 2019). For the Empirical 1993 Incidence results, I take the simple average of the two measures for welfare usage; using the self reported amount is more conservative while the TRIM implies larger effects. For the EITC vs Welfare Reform counterfactuals I use the TRIM3 model exclusively since I am altering the program's parameters directly.

changes, $\hat{w}_e$, using the general incidence formula in equation 1.18. I calculate the model implied labor percent changes as: $\hat{L}_{e,c} = \varepsilon_{e,c}\left(\hat{w}_e - \hat{\tau}_{e,c}\right)$. I then report the percentage point changes in labor force participation as $\mathrm{d}L_{e,c} = \hat{L}_{e,c} \cdot L_{e,c}$.

1.6.3 Per Dollar Effects

I calculate per dollar effects by summing the changes in total income for the economy divided by the change in EITC expenditure. By defining gross earnings as $Z^G = w \cdot L$ and net earnings as $Z^N = (1-\tau) \cdot Z^G$, I can look at sources of change in total income from the EITC reforms by totally differentiating the income measures. The total change in gross earnings is $\mathrm{d}Z^G = w\mathrm{d}L + \mathrm{d}wL + \mathrm{d}w\mathrm{d}L$ and the total change in net earnings is $\mathrm{d}Z^N = (1-\tau)\mathrm{d}Z^G - \mathrm{d}\tau(Z^G + \mathrm{d}Z^G)$.

I report the change in gross earnings due to labor changes ($w\mathrm{d}L$), the change due to wage changes ($\mathrm{d}wL$), the total gross earnings change ($\mathrm{d}Z^G$), and the total net earnings change ($\mathrm{d}Z^N$). I additionally include what Rothstein (2010) refers to as the change in net-transfers ($\mathrm{d}Z^G + \mathrm{d}\tau Z^G$) and the net-earnings ($\mathrm{d}Z^G + \mathrm{d}\tau Z^G$), which hold all other taxes and transfers constant rather than allowing them to adjust given the gross earnings changes. Finally, the table reports the *ex post* 'fiscal externality' that measures the policy reform's effect on the government budget constraint incorporating extensive labor supply effects, $\mathrm{dFE} = \tau w\mathrm{d}L$ (Hendren, 2016a; Kleven, 2018).[46,47] To put these in per dollar terms, I divide the measures by the total new EITC expenditure.

1.6.4 Caveats

There are two caveats to the results I wish to make salient. First, I hold workers' market designation fixed, which could be interpreted as a short-run assumption. That is, while I allow for wages ('skill

[46]I calculate the extensive margin change in welfare usage, B, as $\mathrm{d}B = \left(B\mid_{L=0}\right) \cdot \frac{\mathrm{d}\Pr(L=1)}{1-\Pr(L=1)} + \left(B\mid_{L=1,\text{Phase-In}}\right) \cdot \frac{\mathrm{d}\Pr(L=1)}{\Pr(L=1)}$, which assumes that labor force entrants, originally receiving maximal demographic average welfare benefit, enter into the EITC Phase-In earnings region and use Welfare programs at the pre-reform demographic average level for the Phase-In region earnings.

[47]Assuming a utilitarian social welfare function with a unit marginal value of cost of government revenue, one can interpret this as Consumer Welfare measure. See Section A.1.2.2 for a derivation of this result

prices') to adjust, I do not allow workers to respond to the price adjustment other than through staying or leaving the labor market. This ignores human capital investment responses, such as through education (Maxfield, 2015; Bastian, forthcoming), health (Dahl and Lochner, 2012), and marriage and fertility (Dickert-Conlin and Houser, 2002). However, incorporating these responses is outside the scope of this paper.[48]

Second, my model ignores potential frictions in the wage-labor adjustment process. The clearest example of a friction is the minimum wage. Recall the two type model as presented earlier, where group A is subsidized. The incidence model supposes that as the labor supply for A increases, the gross wage for A falls that then shifts labor demand for the B market outward. Suppose that A is the low-wage group with and that there is a binding minimum wage. If firms cannot absorb additional workers at the binding wage, then unemployment rises rather than employment and so there is no increase in labor demand for the B market.

While the model is silent about this, I make two points about how the results incorporate this potential friction. First, the elasticity estimates are ultimately local average treatment effects (LATEs) for the effect of the 1993 EITC expansion on wages. Thus, any market frictions that existed with the EITC should be captured in the elasticity estimates. For example, if a binding minimum wage prevents workers from responding, then I would estimate perfectly inelastic labor supple responses—as shown above this is not the case. Because I am ultimately interested in the effects of this program, the LATEs exactly provide the variation I wish to use in estimating program effects. Next, unlike in the elasticity estimation, the incidence results pool workers nationally rather than use state specific market definitions. Thus, while imperfect, if nationally market frictions 'wash-out', then the results can be trusted. Exactly dealing with this issue is beyond the scope of the paper, and I am currently unaware of any study empirically dealing with this issue.[49]

[48]In Appendix A.1, I present a two-skill model that allows for high-skill workers to switch to the lower-skill market, similar to Saez (2002), and show how this augments to the incidence equation.

[49]Lee and Saez (2012) theoretically consider an optimal EITC with a minimum wage, but do not empirically test any results.

1.7 Incidence of 1993 EITC Expansion

In this section, I use the estimated elasticities and the empirical average tax changes to calculate the general equilibrium incidence of the 1993 EITC expansion. I use data from the 1994 Annual Social and Economic Supplement (ASEC) of the CPS that includes labor market information for tax year 1993 (Flood et al., 2018). The ASEC includes labor and non-labor income information that allows me to calculate tax parameters necessary for estimating the effect of the 1993 expansion. In Appendix A.2, I describe the variable construction and present summary statistics for the empirical incidence sample. Here, I focus on aggregate effects, but in Appendix A.5 I display individual level effects along with alternative elasticity specifications.

1.7.1 1993 Incidence Results

In Table 1.4, I present my estimates of the gross wage incidence effects of the 1993 OBRA EITC expansion. The table displays own EITC ATR change, PE Incidence (direct effect), GE Incidence (direct + spillover), and the relative magnitude ('Size') of the spillover and direct effects. Note, the incidence effects are *not* normalized by a 1% tax change since the incidence effects depend on multiple tax changes across skill groups. Unmarried women without a high school degree, which had the largest tax decrease, see the largest gross wage changes. In aggregate, spillovers represent between 11-18% of the total gross wage effects for unmarried women and 56-60% for married women.

Table 1.5 translates the net wage changes into labor supply effects using the estimated labor supply elasticities. As expected, unmarried women with children and low levels of education increase their labor supply, but other groups have marginal labor supply changes. Figure 1.6 visually shows the model implied GE change in labor force participation by demographic group and compares it to three alternative empirical strategies, Dickert et al. (1995); Meyer and Rosenbaum (2001), and a simple difference in difference model, described in Appendix A.4.1.[50] This figure supports the

[50]These estimates are selected from Hotz and Scholz (2003) who document several empirical estimates of EITC expansions from 1986 to 2002.

Table 1.4: Empirical Incidence of the 1993 EITC Expansion on 1993 Gross Wages

	Unmarried No Children				Unmarried w/ Children			
(%)	$\mathsf{d}\tau$	PE	GE	Size	$\mathsf{d}\tau$	PE	GE	Size
Less HS	-1.47	-0.41	-0.39	7.20	-2.98	-0.95	-0.93	4.30
HS	-1.16	-0.28	-0.25	10.20	-1.73	-0.41	-0.38	7.20
Some College	-0.71	-0.15	-0.12	19.30	-1.11	-0.24	-0.21	12.30
BA +	-0.25	-0.04	-0.01	47.30	-0.29	-0.04	-0.01	44.00
Total	-0.94	-0.23	-0.21	18.70	-1.70	-0.45	-0.43	11.70
	Married No Children				**Married w/ Children**			
(%)	$\mathsf{d}\tau$	PE	GE	Size	$\mathsf{d}\tau$	PE	GE	Size
Less HS	-0.42	-0.16	-0.13	19.40	-0.04	-0.02	0.01	34.10
HS	-0.05	-0.02	0.00	53.10	0.05	0.01	0.04	66.50
Some College	0.05	0.01	0.04	63.50	0.12	0.03	0.06	50.20
BA +	0.06	0.01	0.04	79.80	0.08	0.01	0.04	74.30
Total	-0.06	-0.03	0.00	56.50	0.06	0.01	0.04	59.60

All data from 1994 March CPS, Women from Tax Units, and TRIM3 model. Note: GE = PE + Spillover; Size = abs(Spillover) / (abs(PE) + abs(Spillover)). Values are average percent changes. Labor supply elasticities from Table 1.2 and column 1 in Table 1.3.

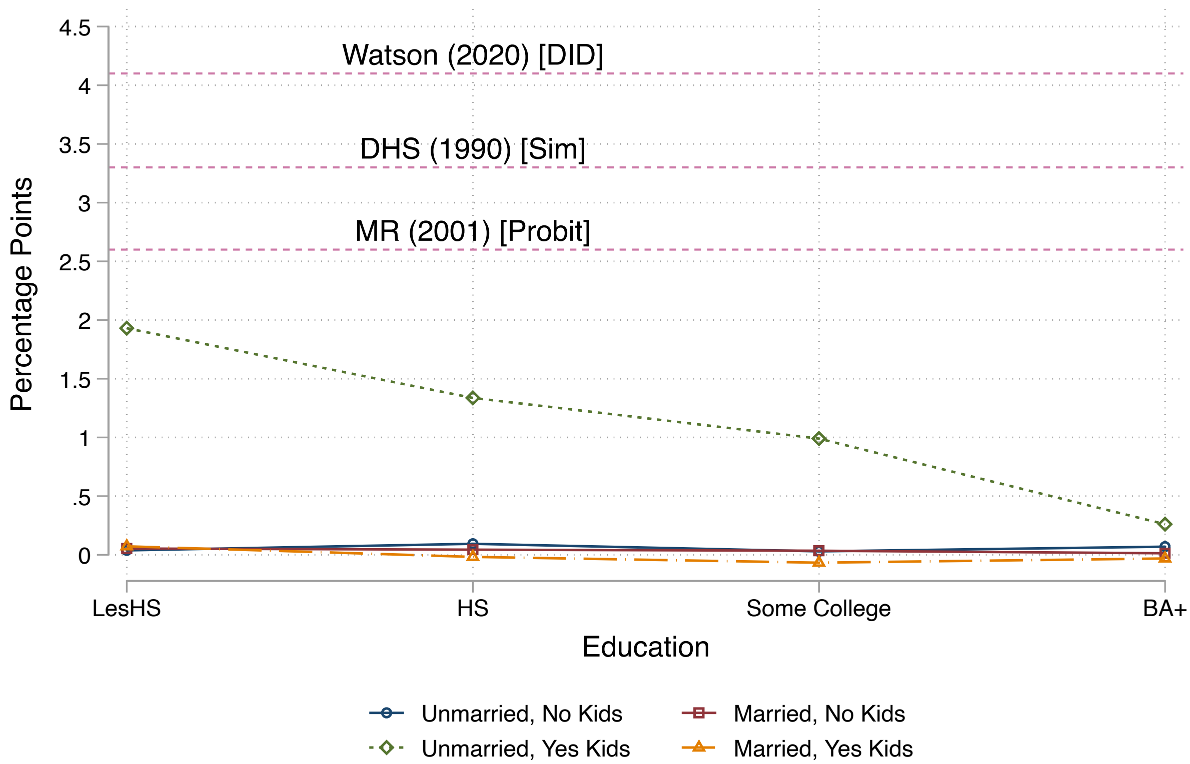
claim by Kleven (2018) that prior EITC elasticity estimates may have been contaminated by concurrent factors and biased up. Using my simulated IV and model based estimate, I find attenuated (but clearly positive, non-zero) labor supply effects that are below all other estimates.

Table 1.5: Empirical Incidence of the 1994 EITC Expansion on Labor Supply

	Total		Unmarried No Children		Unmarried w/ Children		Married No Children		Married w/ Children	
$\mathsf{d}L$	PE	GE	PE	GE	PE	GE	PE	GE	PE	GE
Less HS	0.31	0.33	-0.01	0.01	2.11	2.12	0.04	0.06	0.06	0.07
HS	0.17	0.18	0.06	0.07	1.37	1.38	0.03	0.05	-0.02	-0.01
Some College	0.10	0.12	-0.01	0.01	1.11	1.12	0.02	0.04	-0.06	-0.05
BA+	0.01	0.02	0.04	0.06	0.16	0.17	-0.01	0.01	-0.02	-0.01
Total	0.15	0.16	0.02	0.04	1.35	1.36	0.02	0.04	-0.02	-0.01

Note: $\%\Delta L_{e,k} = \varepsilon_e^L \left(\%\Delta w_e - \mathsf{d}\tau_{e,k}\right)$. All data from 1994 March CPS, Women from Tax Units, and TRIM3 model. Values are average percentage point changes. Labor supply elasticities from Table 1.2 and column 1 in Table 1.3.

Figure 1.6: Model Implied Change in LFP by Demographic Group



This plots the GE change in LFP by marriage, parental, and education group from the incidence model as well as the estimated change from alternative empirical strategies, Dickert et al. (1995); Meyer and Rosenbaum (2001), and a simple difference in difference model, described in Appendix A.4.1.

Table 1.6 displays the incidence effects in terms of aggregate earnings changes per dollar of new EITC expenditure to make the effects.[51] The 1993 EITC expansion effect on earnings is dominated by the labor supply effect. The aggregate change in gross earnings increases by $0.14 in partial equilibrium and $0.24 accounting for spillover effects, which is a 71% increase. The aggregate GE effect on net earning holding taxes constant is $1.24 but is $0.55 after accounting for changes in taxes and transfers due to earnings changes. Note, this difference is almost entirely due to lower net earnings for married mothers, who are more likely to be higher income workers with positive tax rates, rather than unmarried women who are lower income workers.

The fiscal externality is a $0.09 increase per dollar of new EITC spending, implying a small net

[51]In Appendix A.5.1, I present individual level effects of the 1993 expansion.

Table 1.6: Empirical Incidence Results: Change Per Dollar of New Expenditure

	Total		Unmarried No Children		Unmarried w/ Children		Married No Children		Married w/ Children	
Dollars	PE	GE	PE	GE	PE	GE	PE	GE	PE	GE
Labor	0.32	0.36	0.05	0.06	0.29	0.29	0.01	0.03	-0.03	-0.02
Wages	-0.18	-0.12	-0.12	-0.10	-0.07	-0.07	-0.00	0.01	0.01	0.03
Gross Earnings	0.14	0.24	-0.07	-0.04	0.22	0.23	0.01	0.04	-0.02	0.02
Net Transfer, Fixed Taxes	0.82	0.88	-0.06	-0.05	0.32	0.33	0.02	0.03	0.54	0.56
Net Earn, Fixed Taxes	1.14	1.24	-0.02	0.1	0.61	0.62	0.03	0.06	0.51	0.55
Net Earnings	0.45	0.55	-0.01	0.02	0.58	0.59	0.01	0.03	-0.12	-0.09
Fiscal Externality	0.09	0.09	0.01	0.01	0.08	0.08	0.00	0.00	-0.00	-0.00

Units in table are changes in dollars of earnings summed across demographic groups. Note: $Z^G = w \cdot L,\ Z^N = (1-\tau) \cdot w \cdot L$. All data from 1994 March CPS, Women from Tax Units, and TRIM3 model. Labor elasticities from Table 1.2 and column 1 in Table 1.3.

increase in government spending despite the large EITC expansion! This result complements the empirical finding by Bastian and Michelmore (2018) that the EITC 'pays for itself' as unmarried mothers who do not work tend to receive the maximal welfare benefits which is larger than the maximal EITC credit amount. Thus, moving an unmarried mother from non-work to the phase-in region of the EITC schedule results in a net positive position for the government budget.[52]

Across demographic groups there is considerable heterogeneity. Gross earnings decline for unmarried women without children but rise for other groups of women because the former group faces gross wage losses with essentially no increase in transfers. Net earnings decrease only for married women with children for three reasons. First and foremost, the OBRA reform implemented an asset test that *decreased* EITC amounts for higher income tax units, which tend to be married workers. Additionally, a large portion of married workers with positive EITC also face positive tax rates due to spousal earnings, so the EITC is 'taxed back.' Finally, since many married tax filers are in the phase-out region, increased gross earnings due to spillovers decreases the EITC amounts even more.

[52]Hendren (2016a) uses labor supply elasticities from the EITC literature to calculate a fiscal externality of $-\$0.09$ potentially due to holding constant welfare expenditure changes. If I hold welfare program expenditure constant, then I find a fiscal externality of -0.03 that is now negative but still smaller, which likely due to the smaller labor supply elasticities that I estimate.

Interestingly, although wages fall for unmarried women without children, I find that *in GE* net earnings actually rise for this group. While the change is quite small, given that the PE net earnings effect is negative, the positive GE forces counteract the incidence effects which was one of the principal concerns of EITC expansions.

In Table 1.7, I show the incidence affects by real wage quintiles pooling across demographic groups. As expected the labor supply effects, wage declines, and transfers are concentrated in low-wage groups. For the highest wage group, GE spillovers causes wages to be net-positive. One interesting result of the EITC reform is that wage inequality increases, which implies a greater 'skill premium,' but income inequality goes down due to the transfer. As discussed earlier, while main model ignores 'market switching'—see Appendix A.1 for a model that allows for such changes—the change in 'skill prices' may change human capital investment decisions in the medium/long run.

Table 1.7: Empirical Incidence Results: Change Per Dollar of New Expenditure

	Quintile 1		Quintile 2		Quintile 3		Quintile 4		Quintile 5	
Dollars	PE	GE	PE	GE	PE	GE	PE	GE	PE	GE
Labor	0.11	0.11	0.09	0.10	0.07	0.07	0.05	0.06	0.00	0.01
Wages	-0.05	-0.05	-0.06	-0.05	-0.03	-0.02	-0.04	-0.03	-0.00	0.02
Gross Earnings	0.06	0.07	0.03	0.06	0.04	0.05	0.01	0.03	-0.00	0.03
Net Transfer, Fixed Taxes	0.34	0.35	0.28	0.30	0.09	0.09	0.07	0.08	0.04	0.06
Net Earn, Fixed Taxes	0.45	0.46	0.37	0.40	0.16	0.17	0.11	0.14	0.04	0.07
Net Earnings	0.20	0.21	0.11	0.14	0.11	0.12	0.05	0.07	-0.02	0.01
Fiscal Externality	0.03	0.03	0.02	0.02	0.02	0.02	0.02	0.02	0.00	0.00
Mean Wage	$ 5.63		$ 8.06		$ 9.72		$ 11.21		$ 15.68	

Units in table are changes in dollars of earnings summed across demographic groups. Note: $Z^G = w \cdot L$, $Z^N = (1 - \tau) \cdot w \cdot L$. All data from 1994 March CPS, Women from Tax Units, and TRIM3 model. Labor elasticities from Table 1.2 and column 1 in Table 1.3.

1.8 Comparing EITC and Welfare Reforms

In this section, I use my estimated labor market elasticities to compare three hypothetical policy reforms based on the OBRA and PRWORA reforms in the mid-1990's. The first is an exogenously funded \$100 million dollar expansions of the 1992 EITC. The second is equal sized expansion of the combined 1992 ADFC and Food Stamps programs (which I refer to as simply 'Welfare').[53] The third experiment, which I call the Net EITC reform, simultaneously expands the EITC and contracts Welfare benefits to create an *ex ante* revenue neutral EITC expansion with no distortions on higher wage markets.[54] This allows me to ignore the distortionary effects of financing the expansion as well as mirroring the tax and transfer system policy reforms of the 1990s.

1.8.1 Simulating the Tax Reforms

To implement the simulation, I characterize the tax system with transfer inclusive average tax rates, calculated using the reported income data, NBER TAXSIM, and the Urban Institute's TRIM3 welfare simulator (Feenberg and Coutts, 1993; Urban Institute, 2020).[55] For each reform, I suppose that the government wishes to increase the generosity of its tax and transfer system for low income tax units by \$100 million through either an EITC expansion or Welfare expansion, but does not consider behavioral changes in response to the reforms. To implement the EITC expansion, I solve for the new maximum credit amount holding fixed the existing 'kink points' such that the total expenditure equals the targeted amount. To implement the welfare expansion, I approximate the existing welfare system as a fixed benefit and a rate at the benefit is taxed away, and then solve for the change in the benefit such that total new expenditure equals the targeted amount while keeping the same rate. The Net EITC reform implements the EITC expansion above and the *negative* of the welfare expansion to make the reform *ex-ante* revenue neutral.[56] Figure 1.7 visually shows the reform transfer programs.

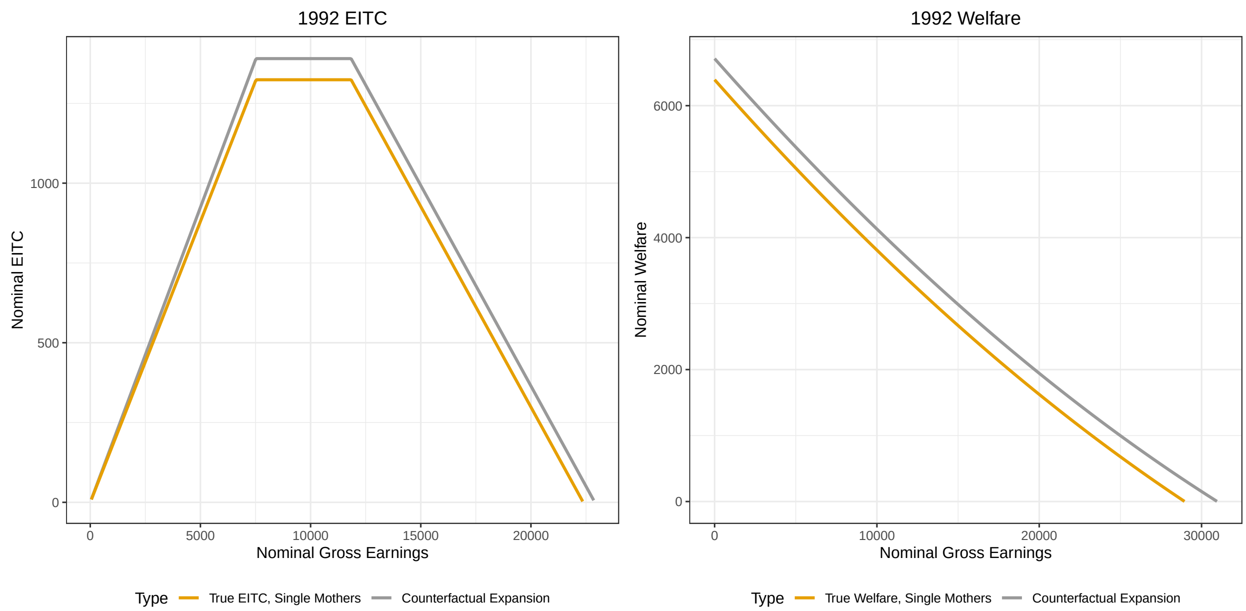
[53] This reform is roughly the same as the hypothetical Negative Income Tax reform Rothstein (2010) considers. In Appendix A.5, I replicate his experiments and find qualitatively similar results.

[54] Because the experiment is a 'marginal reform,' taking the negative of the values reported for the Net EITC reform would be the same as conducting a Net Welfare expansion.

[55] I do not consider an intensive hours margin, so I do not consider marginal tax rates. This accords with the preferred specification in Rothstein (2010), and most empirical literature on the EITC.

[56] These reforms roughly mirror the actual reforms in the 1990's but at a smaller scale.

Figure 1.7: True and Counterfactual 1992 Transfer Programs



Plots EITC and Composite Welfare for single women with one child in 1992 using data from CPS ASEC 1993, NBER TAXSIM, and Urban Institute's TRIM3. The counterfactual EITC expansion raises the max credit holding the first two kink points fixed; the counterfactual Welfare expansion increases the base transfer amount holding the effective marginal tax rate constant.

1.8.2 Simulation Results

Table 1.8 and 1.9 display the incidence results for the EITC, Welfare, and Net EITC simulated tax reforms at the aggregate and demographic level, respectively, and are interpreted the same as Table 1.6. For both tables, columns (1-3) show the partial equilibrium results and columns (4-6) incorporate spillovers. The main takeaway is that the 'bad' aspects of the EITC expansions (gross wage decreases) and the 'good' aspects of Welfare expansions (gross wage increases and positive welfare) are attenuated by the GE forces.

For the EITC, the dollar change due to wages is $-\$0.12$ in PE but only $-\$0.04$ in GE, but for the Welfare reform the \$0.06 wage growth in PE becomes \$0.02 in GE. For the Net EITC reform, the wage decline goes from $-\$0.18$ to $-\$0.06$, a two-thirds decrease due to spillover effects. Aggregate gross earnings increase for the EITC and Net EITC programs but decrease for the Welfare expansion. This is because the Welfare expansion incentivizes workers to exit the labor force, and this source of earnings loss dominates the scarcity induced wage increases.

The difference between Net Earnings with Fixed Taxes, which Rothstein (2010) reports, and Net Earnings is that the latter measure accounts for the fact that the increase in gross wages will be taxed. If one holds taxes fixed, then the whole intended transfer is added to gross earnings, which overestimates the net earnings gain. The net earnings measure reported allows for additional earnings to be taxed (holding the ATR constant), so the some of the intended transfer goes to taxes as well as incidence effects. For the Welfare expansion, net earnings with fixed taxes is \$0.89 in GE but allowing for tax changes net earnings actually decrease by −\$0.41! For the EITC reforms, both measures of net earnings are positive.

As noted earlier, the welfare measure is the *ex post* fiscal externality of the reform. The EITC and net-EITC reforms have decrease of \$0.08 and \$0.09, respectively, but the Welfare expansion essentially has no externality. This means the EITC expansions impose an additional cost to the government to balance the budget but the Welfare reform does not. However, this should be considered along-side the gross and net earnings effects. The EITC expansion increases aggregate gross earnings (analogous to GDP) and net earnings by shifting some economic resources to lower income workers.

The implication of zero fiscal externality of the Welfare reform is worth delving into. Let there be three groups: high income (`H`) who always work, stable-labor low income (`S`) who always work, and marginal-labor low income (`M`) who would work less if able, where `{M,S}` are in same labor market. Conceptually, the government is transferring income from `H` to `M`, which allows `M` to exit market. Equilibrium forces increase the wages of `S` (due to induced scarcity) and lower wages of `H` (due to negative spillovers). This implies that `H` pay less taxes and `S` pay more taxes, and on balance these cancel out the payment to `M`. The payment of the Welfare reform comes from other low income workers rather than high income workers!

Table 1.9 decomposes the aggregate effects by demographic groups for each reform. The EITC reform GE net earnings change for unmarried women with children is \$0.79 and 0.04 for married women with children, while net earnings fall for married and unmarried women without children by −\$0.10, since the latter groups receive almost no subsidy but are exposed to wage decreases.

Table 1.8: Incidence Results:
Aggregate Effects: All Women

Dollars	"PE" EITC (1)	Welfare (2)	Net EITC (3)	GE EITC (4)	Welfare (5)	Net EITC (6)
Intended	1.00	0.65	0.35	1.00	0.65	0.35
Labor	0.22	-0.10	0.32	0.27	-0.13	0.40
Wages	-0.12	0.06	-0.18	-0.04	0.02	-0.06
Gross Earnings	0.10	-0.05	0.14	0.23	-0.11	0.34
Net Transfer, Fixed Taxes	0.88	1.06	-0.18	0.96	1.02	-0.6
Net Earn, Fixed Taxes	1.10	0.95	0.14	1.23	0.89	0.34
Net Earnings	0.50	-0.36	0.21	0.63	-0.41	0.39
Fiscal Externality	-0.09	0.01	-0.09	-0.08	0.00	-0.09

Units in table are changes in dollars of earnings summed across demographic groups. Note: $Z^G = w \cdot L$, $Z^N = (1 - \tau) \cdot w \cdot L$. All data from 1993 March CPS, Women from Tax Units. Labor supply elasticities from Model 1 in Table 1.2 and column 1 in Table 1.3.

The Welfare reform GE net earnings change is negative for women with children and effectively zero for women with children.

The aggregate fiscal externality changes are almost entirely due to changes from unmarried women with children. Because the EITC and Welfare reforms primarily affect unmarried mothers' labor supply, this group drives the fiscal externality.

Finally, Table 1.10 decomposes the incidence effects by wage quintile, where the mean wage for each is $\{5.85, \$7.74, \$9.29, \$10.91, \$15.04\}$, respectively. The wage groups pool the different demographic groups to show the reform effects on different 'skill groups' in aggregate, which may affect human capital decisions. As discussed earlier, the EITC reforms decrease wages for low-wage groups but increase every group's labor supply and net earnings. The Welfare reforms on the other hand decrease net earnings for all wage groups, even for those who have small wage increases. The Welfare reforms raise wages through creating artificial scarcity (through reduced labor supply incentives) but the loss resources to the economy lower the total possible income to be distributed.

Table 1.9: Incidence Results:
Aggregate Effects: Subgroups of Women

Dollars	"PE" EITC (1)	Welfare (2)	Net EITC (3)	GE EITC (4)	Welfare (5)	Net EITC (6)
			Unmarried Mothers			
Net Earn, Fixed Taxes	0.72	0.66	0.07	0.74	0.65	0.09
Net Earnings	0.78	-0.26	0.38	0.79	-0.27	0.40
Fiscal Externality	-0.07	0.01	-0.08	-0.08	0.01	-0.08
			Unmarried Women			
Net Earn, Fixed Taxes	-0.15	0.03	-0.19	-0.11	0.01	-0.13
Net Earnings	-0.14	0.03	-0.17	-0.10	0.01	-0.12
Fiscal Externality	-0.01	0.00	-0.01	-0.01	0.00	-0.01
			Married Mothers			
Net Earn, Fixed Taxes	0.52	0.25	0.27	0.56	0.23	0.33
Net Earnings	-0.14	-0.14	0.01	-0.10	-0.15	0.07
Fiscal Externality	0.00	0.00	0.00	0.00	0.00	0.00
			Married Women			
Net Earn, Fixed Taxes	0.01	0.01	-0.01	0.05	0.00	0.05
Net Earnings	0.01	0.01	-0.01	0.04	0.00	0.05
Fiscal Externality	0.00	0.00	0.00	0.00	0.00	0.00

Units in table are changes in dollars of earnings summed across demographic groups. Note: $Z^G = w \cdot L$, $Z^N = (1-\tau) \cdot w \cdot L$. All data from 1993 March CPS, Women from Tax Units. Labor supply elasticities from Model 1 in Table 1.2 and column 1 in Table 1.3.

1.9 Structural Model Parameterization

The previous results were all derived using only the assumption of quasi-linearity of the utility function. In this section, I add a distributional assumption about the worker specific disutility of labor that allows me to parameterize demographic specific labor supply functions to calculate general equilibrium results for non-marginal and out-of-sample reforms. Specifically, I use labor participation probabilities and my elasticity estimates to parameterize a standard 'logit' binary choice model.

Table 1.10: Incidence Results:
Aggregate Effects: Wage Quintiles

Dollars	"PE" EITC (1)	"PE" Welfare (2)	"PE" Net EITC (3)	GE EITC (4)	GE Welfare (5)	GE Net EITC (6)
			Quintile 1			
Wages	-0.02	0.00	-0.03	-0.02	0.00	-0.02
Gross Earnings	0.03	-0.01	0.03	0.04	-0.01	0.05
Net Earnings	0.14	-0.03	0.07	0.15	-0.03	0.09
Fiscal Externality	-0.04	0.00	-0.05	-0.04	0.00	-0.05
			Quintile 2			
Wages	-0.04	0.01	-0.05	-0.03	0.01	0.02
Gross Earnings	0.04	-0.01	0.05	0.06	-0.02	0.05
Net Earnings	0.19	-0.08	0.08	0.21	-0.08	0.04
Fiscal Externality	-0.03	0.00	-0.03	-0.03	0.00	0.00
			Quintile 3			
Wages	-0.03	0.01	-0.04	-0.02	0.01	0.02
Gross Earnings	0.01	-0.01	0.02	0.06	-0.02	0.05
Net Earnings	0.10	-0.07	0.03	0.12	-0.08	0.04
Fiscal Externality	-0.01	0.00	-0.01	-0.01	0.00	0.00
			Quintile 4			
Wages	-0.02	0.02	-0.03	0.00	0.01	0.02
Gross Earnings	0.03	-0.02	0.05	0.07	-0.03	0.05
Net Earnings	0.10	-0.11	0.05	0.13	-0.12	0.04
Fiscal Externality	-0.00	0.00	-0.00	-0.00	-0.00	0.00
			Quintile 5			
Wages	-0.01	0.01	-0.02	0.02	-0.00	0.02
Gross Earnings	-0.02	0.00	-0.02	0.03	-0.02	0.05
Net Earnings	-0.03	-0.08	-0.02	0.02	-0.10	0.04
Fiscal Externality	-0.00	-0.00	-0.00	0.00	-0.00	0.00

Units in table are changes in dollars of earnings summed across demographic groups. Note: $Z^G = w \cdot L$, $Z^N = (1-\tau) \cdot w \cdot L$. All data from 1993 March CPS, Women from Tax Units. Labor supply elasticities from Model 1 in Table 1.2 and column 1 in Table 1.3. Mean wage for Q1 is \$5.85, Q2 is \$7.74, Q3 is \$9.29, Q4 \$10.91, and Q5 is \$15.04.

1.9.1 Structural Model

The utility problem for workers is the following discrete choice:

$$\max_{L=\{0,1\}} \{\underbrace{u_i\left(T_c(0,m_i)\right) - \nu_i(0)}_{L=0},\ \underbrace{u_i\left(T_c(w_i,m_i)\right) - \nu_i(1)}_{L=1}\}, \tag{1.28}$$

where ν_i is the idiosyncratic disutility of labor drawn from some distribution, $F_{e,c}(\nu)$. Initially, I assumed that $u_i(x) = x$, but now suppose that $u_i(x) = \beta_{e,c} \cdot x$, where $\beta_{e,c}$ can be interpreted as typespecifc marginal utility of consumption (or income). Additionally, suppose $\nu_i(0) - \nu_i(1) = \delta_{e,c} + \epsilon_i$, where ϵ_i distributed independent Type 1 Extreme Value ($F(\epsilon) = \mathrm{e}^{-\mathrm{e}^{-\epsilon}}$) and $\delta_{e,c}$ is interpreted as an unobserved utility cost of labor (a supply 'shifter'). Then, demographic-specific (expected) labor supply function is:

$$\Pr(L^i = 1 \mid w_e, m_{e,c}, T_c) = \frac{\mathrm{e}^{\beta_{e,c} T_c(w_e, m_{e,c}) + \delta_{e,c}}}{\mathrm{e}^{\beta_{e,c} T_c(0, m_{e,c})} + \mathrm{e}^{\beta_{e,c} T_c(w_e, m_{e,c}) + \delta_{e,c}}} := \pi_{e,c}. \tag{1.29}$$

1.9.2 Recovering Structural Parameters

Defining $\mathsf{v}_{e,c} := T_{e,c}(w_e, m_{e,c}) - T_{e,c}(0, m_{e,c})$ as the net wage, the model implies that:

$$\text{Gross Wage Elasticity:} \quad \varepsilon^L_{e,c} = \frac{\partial \pi_{e,c}}{\partial w_e} \frac{w_e}{\pi_{e,c}} = \beta_{e,c} \frac{\partial \mathsf{v}_{e,c}}{\partial w} w_e (1 - \pi_{e,c}) \tag{1.30}$$

$$\text{Net Wage Elasticity:} \quad \eta^L_{e,c} = \frac{\partial \pi_{e,c}}{\partial \mathsf{v}_{e,c}} \frac{\mathsf{v}_{e,c}}{\pi_{e,c}} = \beta_{e,c} \mathsf{v}_{e,c} (1 - \pi_{e,c}). \tag{1.31}$$

If the transfer function is $T_{e,c}(w_e, m_{e,c}) = (1 - \tau_{e,c})(w_e L) + b_{e,c}(1 - L) + t(m)$, so that the net wage is $(1 - \tau_{e,c})(w_e)$, then $\frac{\partial \mathsf{v}_{e,c}}{\partial w} w_e = \mathsf{v}_{e,c}$ so that $\varepsilon^L_{e,c} = \eta^L_{e,c}$. Thus, I can recover the marginal utility of consumption parameters using the following:

$$\frac{\varepsilon^L_{e,c}}{\left(\mathsf{v}_{e,c}(1 - \pi_{e,c})\right)} = \beta_{e,c}. \tag{1.32}$$

With an estimate of $\beta_{e,c}$, I can then recover the unobservable net supply shifters using a Berry (1994) style inversion technique:

$$\ln\left[\pi_{e,c}\right] - \ln\left[(1 - \pi_{e,c})\right] - \left(\beta_{e,c} \mathsf{v}_{e,c}\right) = \delta_{e,c}. \tag{1.33}$$

With the estimated structural utility parameters $\{(\widehat{\beta}_{e,c}, \widehat{\delta}_{e,c})\}_{(e,c) \in \mathcal{D}}$, I can simulate non-differential EITC reforms. Note, I estimate these parameters based on the elasticities estimates from the 1990's, so the underlying assumption of these parameters is that β is a fixed utility parameter and any changes over time (conditional on the net wage) occur through the shifter, δ.

1.10 Childless Worker Reform

Advocacy groups encourage policymakers to reform the EITC schedule such that workers without children are treated the same as workers with children.[57] Advocates cite issues related to horizontal equity on the basis of skill as well as lifting more workers out of poverty. Another reason is, given that there are negative earnings effects for childless workers who are close substitutes, expanding the EITC for these workers can offset the incidence effects just like for unmarried women with children.

To quantify the effects of this reform, I equalize the 1994 EITC schedule for workers without children and workers with one qualifying dependent.[58] That is, I create a counterfactual OBRA expansion where the credit for workers without children was equalized rather than set with a max of $306. My model based approach can describe the labor supply and earnings effects of this and predict any additional take-up that may occur.

Note, the structural model results below and the incidence model results above do not yield the same quantitative values for two reasons. First, the incidence results use analytic results for changes in ATRs, while the structural results numerically solve for market clearing prices. Second, the incidence results, based on marginal changes in ATRs, hold constant other features of the tax and transfer system, while the structural results incorporate tax liability changes when calculating labor supply. Thus, the incidence model describes how the EITC expansions are shared between workers and the structural model shows the total effect of equalizing the EITC schedules on market equilibrium incorporating spillovers.

[57]See discussions in Nichols and Rothstein (2016); Marr et al. (2016); Maag et al. (2019). Nichols and Rothstein (2016) note that both former President Obama and then former House Ways and Means committee chairman Ryan both advocated for increasing the generosity for childless workers.

[58]My reform is larger than many existing proposals. Maag et al. (2019) use the 2016 American Community Survey to parameterize an equalization reform that triples the childless worker maximum credit and doubles the kink-point thresholds, but hold gross wages and labor supply constant, which ignores behavioral responses or incidence effects. President Obama's proposal doubled the maximum credit and extended the second kink-point by half Executive Office of the President and US Department of Treasury (2014).

1.10.1 Childless Worker Reform Results

In Tables 1.11 and 1.12, I display the results of the policy reform. To make comparisons as close as possible, I solve the model using the actual EITC schedule in tax year 1993 as a baseline, next solve the model using the actual 1994 schedule, and then solve the model using the counterfactual 1994 schedule. This holds all non-labor-market variables constant, such as labor supply shifters, aggregate productivity or demand shifts, and capital supply shifts. I then calculate the changes for each expansion from the baseline.

There are two striking elements from the results. First, equalizing the credit schedules would substantially increase labor supply for unmarried workers without children – an 4.8 percentage point (ppt) increase in aggregate. This is because these workers have a greater labor supply elasticity than workers with children and the expanded credit substantially increases their net income. Second, equalization creates a countervailing effect on unmarried mothers' the labor supply – from 1.5 to 1.0 ppts in aggregate and 2.5 to 1.4 ppts for those without a high school degree. This is due the same gross wage incidence effects from the much larger labor supply shock that advocates cite when promoting a childless worker expansion. Gross wages for unmarried workers initially decrease by about 0.6 – 0.7% under the actual expansion but decrease between 2.4 – 3.6% under the expansion regime.[59]

Table 1.12 puts the effects in terms of dollars of planned new expenditure and shows three important facts. First, neither the actual or counterfactual reform has much effect on married women mostly because these workers have household earnings that are too high to be affected by the policy. Second, the reforms have similar aggregate effects in terms of earnings and welfare measures. Third, the reforms have similar aggregate effects because the labor supply effects of the policy are almost exactly reversed for the unmarried women. Those without children supply more labor but those with children become much less likely to join the labor force.

While equalizing the EITC schedule may be more 'fair' and certainly will help many low

[59]The wage changes in Table 1.11 are slightly different between unmarried workers with and children because workers do not perfectly overlap in demographic-skill based markets.

Table 1.11: Empirical Incidence Results:
1994 EITC Expansion + Equalization of Credit Schedule

Percent Change in Wages												
	Unmarried No Children			Unmarried w/ Children			Married No Children			Married w/ Children		
$\%\Delta w$	Act	Cft	Diff	Act	Cft	Diff	Act	Cft	Diff	Act	Cft	Diff
LessHS	-2.33	-7.63	-5.44	-2.10	-6.21	-4.21	-0.10	-1.87	-1.78	-0.30	-0.42	-0.13
HS	-0.17	-2.35	-2.19	-0.25	-1.79	-1.54	0.05	0.31	0.26	0.05	0.33	0.28
Some College	-0.36	-3.03	-2.69	-0.19	-0.98	-0.90	0.05	0.31	0.26	0.05	0.33	0.28
BA+	0.05	-0.09	-0.15	0.06	0.27	0.21	0.06	0.36	0.30	0.06	0.38	0.32
Total	-0.76	-3.55	-2.84	-0.65	-2.41	-1.79	0.03	0.01	-0.02	0.01	0.25	0.24

Percentage Point Change in Labor Supply												
	Unmarried No Children			Unmarried w/ Children			Married No Children			Married w/ Children		
dL	Act	Cft	Diff	Act	Cft	Diff	Act	Cft	Diff	Act	Cft	Diff
LessHS	1.54	7.94	6.17	2.52	1.38	-1.07	-0.06	1.45	1.52	0.49	0.51	0.02
HS	-0.12	4.98	5.11	1.46	1.02	-0.42	0.03	0.19	0.16	0.02	0.14	0.12
Some College	0.40	5.93	5.47	1.05	0.82	-0.22	0.04	0.25	0.21	0.02	0.14	0.12
BA+	0.02	0.94	0.92	0.07	0.13	0.06	0.03	0.20	0.17	0.01	0.08	0.06
Total	0.50	5.33	4.76	1.45	0.96	-0.47	0.02	0.39	0.37	0.08	0.17	0.10

'Act' : Actual EITC schedules; 'Cft' : Counterfactual EITC schedule where workers with no children get same credit as workers with one child; 'Diff' : Equalization specific effects All data from 1994 March CPS, Women from Tax Units. Values are average percent changes, weighted population.

income workers, these results imply that such a reform does not come without a cost. Policymakers wishing to reform the EITC face a dilemma: the current structure disadvantages workers without children but reforming the EITC may harm workers with children (and through secondary effects their children). Just as policymakers should consider the spillover effects from the current EITC structure, they should be sure to understand the trade-offs in terms of families from a structural reform of the EITC.

1.11 Incidence of the 2009 EITC Expansion

In this section, I consider the labor market effects of the 2009 EITC expansion that was part of the American Recovery and Reinvestment Act of 2009. The reform made the credit schedule more generous for workers with three or more qualifying children as well as for married workers

Table 1.12: Empirical Incidence Results:
1994 EITC Expansion + Equalization of Credit Schedule
Change Per Dollar of New Planned Expenditure

	Total		Unmarried No Children		Unmarried w/ Children		Married No Children		Married w/ Children	
Dollars	Act	Cft	Act	Cft	Act	Cft	Act	Cft	Act	Cft
Labor	0.62	0.65	0.11	0.51	0.42	0.05	0.03	0.05	0.05	0.03
Wages	-0.12	-0.11	-0.14	-0.16	-0.07	-0.04	0.04	0.04	0.05	0.06
Gross Earnings	0.50	0.51	-0.03	0.33	0.35	0.01	0.07	0.08	0.11	0.09
Net Transfer, Fixed Taxes	0.88	0.89	0.12	0.70	0.64	0.07	0.04	0.05	0.08	0.06
Net Earn, Fixed Taxes	1.50	1.51	0.23	1.19	1.06	0.12	0.07	0.10	0.14	0.09
Net Earnings	1.37	1.41	0.23	1.14	0.97	0.12	0.05	0.08	0.11	0.07
Welfare	-0.10	-0.09	-0.01	-0.06	-0.07	-0.01	-0.01	-0.01	-0.01	-0.01

'Act' : Actual EITC schedules; 'Cft' : Counterfactual EITC schedule where workers with no children get same credit as workers with one child. Units in table are changes in dollars of earnings summed across demographic groups. Note: $Z^G = w \cdot L,\ Z^N = (1 - \tau) \cdot w \cdot L$. All data from 1994 March CPS, Women from Tax Units.

by extending the 'max credit' portion of the EITC to reduce 'marriage penalties' (Nichols and Rothstein, 2016).[60] The reform was intended to provide counter-cyclical income support for low wage workers rather than strengthening labor force attachment.[61] Nevertheless, because the expansion is the second largest EITC reform after the 1993 expansion, the reform gave economists an opportunity to revisit the EITC's labor market effects. In short, Iribarren (2016) and Kleven (2019) find no statistically significant effect from this reform.

There are three potential explanations for this. First, there was no effect, which is a conjecture recently advanced by Kleven (2019). Second, there were prevailing forces that dominated any EITC effect and a clean experiment is not possible. The existing papers rely on treatment and control group based estimates that should purge the overall economic forces during the recession period, so the results depend on appropriateness of these grouping decisions. Third, the reform was too small to see a large labor supply effect, even holding economic conditions constant. The

[60] The expansions were set to expire in 2017 but have since been made permanent.

[61] It is theoretically ambiguous how the EITC fares in a recession since the laid-off worker will likely lose eligibility whereas workers with reduced hours may become eligible. Jones (2017) uses linked CPS-IRS data to show that unmarried mothers with low education had a higher likelihood of losing eligibility and lower likelihood of gaining eligibility through lost earnings.

expansion increased the maximum credit by $600, which may not be enough to create large labor supply changes, and the targeted groups – workers with 3+ children, married workers – are a small proportion of the EITC claimers.

My incidence analysis allows me to provide a benchmark estimate of the 2009 expansion effects. If the labor market effects are small even when I am able to hold all other economic conditions constant, then this implies that standard difference-in-difference evidence may simply be under-powered to detect an effect. However, if the effects of the expansion are comparable to the larger 1993 expansion, then the a change in labor market fundamentals is necessary to explain the empirical null findings. Additionally, the results provide insight into *why* EITC expansion may have different effects over time. If labor supply elasticities are falling or costs increasing, then larger and larger EITC expansions are necessary to achieve the same labor supply effects.

1.11.1 2009 Incidence Results

Compared to Table 1.4, Table 1.13 shows that the tax rate change for unmarried women was less than a third of the 1993 EITC expansion but the expansions were similar for married women. As such, the 2009 direct and spillover effects are much smaller than the 1993 case; in fact, the spillover effects are effectively zero.

Table 1.14 shows that unmarried mothers' aggregate labor supply should have increased by 0.6% while other groups show essentially no change, compared with 1.4% for the 1993 expansion. Despite the fact that the 2009 expansion reduced the two earners 'marriage penalty,' there is essentially no effect for married workers. The aggregate general equilibrium labor supply change effect is only 0.05%.

Finally, in Table 1.15, I show the per dollar effect of the 2009 expansion. Again, the direct and spillover effects are much smaller than the 1993 expansion, with essentially no scope for spillovers. The aggregate gross and net earnings changes are both less than half of the 1993 per dollar effects. This implies near zero fiscal externality because there was little behavioral change.

Table 1.13: Empirical Incidence of the 1993 EITC Expansion on 1993 Gross Wages

	Unmarried No Children				Unmarried w/ Children			
%	dτ	PE	GE	Size	dτ	PE	GE	Size
LessHS	-0.40	-0.12	-0.12	5.00	-0.85	-0.31	-0.31	2.90
HS	-0.38	-0.09	-0.09	8.60	-0.66	-0.15	-0.15	4.80
Some Col.	-0.23	-0.06	-0.05	13.60	-0.45	-0.11	-0.11	7.10
BA+	-0.08	-0.02	-0.01	34.40	-0.12	-0.02	-0.02	24.20
Total	-0.27	-0.07	-0.06	15.30	-0.53	-0.14	-0.14	8.40
	Married No Children				**Married w/ Children**			
%	dτ	PE	GE	Size	dτ	PE	GE	Size
LessHS	-0.19	-0.07	-0.07	15.80	-0.21	-0.08	-0.07	23.00
HS	-0.02	-0.01	0.00	35.80	0.04	0.01	0.02	33.20
Some Col.	0.04	0.01	0.02	49.70	0.13	0.03	0.04	20.30
BA+	0.05	0.01	0.01	49.40	0.10	0.01	0.02	33.20
Total	0.01	0.00	0.00	42.20	0.06	0.01	0.02	28.60

All data from 2009 March CPS, Women from Tax Units. Note: GE = PE + Spillover; Size = abs(Spillover) / (abs(PE) + abs(Spillover)). Values are average percent changes, weighed by population. Labor supply elasticities from structural model implied by equation 1.32.

Table 1.14: Empirical Incidence of the 2009 EITC Expansion on Labor Supply

	Total		Unmarried No Children		Unmarried w/ Children		Married No Children		Married w/ Children	
dL	PE	GE	PE	GE	PE	GE	PE	GE	PE	GE
Less HS	0.11	0.11	-0.05	-0.05	1.00	1.00	0.00	0.01	0.10	0.10
HS	0.06	0.07	-0.06	-0.06	0.65	0.65	0.01	0.01	-0.02	-0.02
Some College	0.03	0.04	-0.04	-0.04	0.56	0.56	0.01	0.02	-0.07	-0.07
BA+	-0.00	0.00	-0.00	-0.00	0.18	0.18	0.01	0.01	-0.03	-0.03
Total	0.04	0.05	-0.04	-0.04	0.60	0.60	0.01	0.01	-0.03	-0.03

Note: $\%\Delta L_{e,k} = \varepsilon_e^L \left(\%\Delta w_e - \mathrm{d}\tau_{e,k}\right)$. All data from 2009 March CPS, Women from Tax Units. Values are average percent point changes, weighed by population. Labor supply elasticities from structural model implied by equation 1.32.

1.12 Conclusion

I evaluate the Earned Income Tax Credit allowing for general equilibrium interactions in the labor market and heterogeneous wage responsiveness. My approach allows one to evaluate any

Table 1.15: Empirical Incidence of the 2009 EITC Expansion:
Change Per Dollar of New Expenditure

	Total		Unmarried No Children		Unmarried w/ Children		Married No Children		Married w/ Children	
Dollars	PE	GE	PE	GE.	PE	GE	PE	GE	PE	GE
Labor	0.12	0.14	-0.04	-0.04	0.21	0.21	0.01	0.02	-0.05	-0.05
Wages	-0.07	-0.05	-0.05	-0.05	-0.04	-0.04	0.00	0.01	0.02	0.03
Gross Earnings	0.05	0.09	-0.10	-0.09	0.17	0.17	0.02	0.03	-0.03	-0.02
Net Transfer, Fixed Taxes	0.93	0.95	-0.05	-0.04	0.21	0.21	0.02	0.03	0.75	0.75
Net Earn, Fixed Taxes	1.05	1.09	-0.09	-0.08	0.42	0.42	0.03	0.04	0.69	0.70
Net Earnings	0.17	0.20	-0.08	-0.07	0.43	0.43	0.01	0.02	-0.200	-0.19
Welfare	0.00	0.00	-0.00	-0.00	0.01	0.01	0.00	0.00	-0.01	-0.01

Units in table are changes in dollars of earnings summed across demographic groups. Note: $Z^G = w \cdot L,\ Z^N = (1-\tau) \cdot w \cdot L$. All data from 2009 March CPS, Women from Tax Units. Labor supply elasticities from structural model implied by equation 1.32.

large scale program that affects average tax rates by mapping those changes to gross wages and labor supply as long as one has information on initial wages, quantities, and elasticities. When labor markets are imperfect substitutes, a tax induced supply change in one market will affect the marginal product of workers in other markets, creating cascading marginal product and wage spillovers across labor markets. Because the general equilibrium wage changes are theoretically ambiguous, I quantified the importance of general equilibrium effects in three ways.

First, I calculated the empirical incidence of the 1993 OBRA and 2009 ARRA EITC expansion. I find that spillovers represent about 15-30% of aggregate wage and net earnings effects in the direction of increasing dollars to workers. Second, to compare how different labor market policies affect spillovers, I simulated a \$100 million expansion of the EITC, of the AFDC and Food Stamps programs, and a reform that pays for the EITC expansion by reducing Welfare benefits. For all three policy reforms experiments, the GE incidence is less than one-third the PE incidence – for the EITC reforms this implies more dollars go to workers while for the Welfare reform workers receive fewer dollars. Third, I used my elasticities to parameterize a structural labor supply model to consider the effect of equalizing the the EITC schedule for workers with and without children. I find that equalizing the EITC would have the opposite issue of current EITC expansions: gross wage decreases would causes marginal workers with children *not* to enter the labor market.

Overall, these results show that the EITC is a cost effective program in transferring income to low wage workers. In all cases, the fiscal externality of the EITC expansions is always quite small relative to the increases in net earnings. The 1993 expansion created large labor market direct and indirect effects; however, the 2009 expansion appears not to have caused labor market disruptions. The best explanation for this seems to be simply that the 2009 expansion was smaller, focused on a smaller group, and in an environment where many people were already working. When labor market disruptions are small, the program is primarily functioning as an immediate anti-poverty tool in that dollars go to low income workers without distorting untreated workers' behaviors. When they are large, the program is acting as a immediate and long-run anti-poverty tool by increasing the earnings potential of workers and the economy as a whole.

The above assessment of the EITC's cost effectiveness is not without some caveats. First, the EITC has a positive fiscal externality only because net transfers from non-employment to employment are positive rather than due to spillovers. Thus, while positive marginal product spillovers expand the economic capacity of the economy and tax base, ignoring this interaction with other transfers, the EITC would not 'pay for itself.' Second, the EITC has heterogeneous effects that may not yield horizontal equity. Similar skilled workers without children will be subject to gross earnings effects but will not receive the subsidy. I find that the welfare effects are ultimately small for these workers; nevertheless, proponents of expanding the EITC must accept that some workers will be financially hurt. As indicated, this also holds for those who want to expand the EITC for worker without children. Third, choosing an EITC expansion over a Welfare expansion – or any other policy that links benefit levels with non-employment – implies a judgement about the marginal value of leisure for workers on the margin of the labor supply threshold.

Finally, my approach still makes a number of simplifications worth pointing out. First, the production technology assumes a constant elasticity of substitution, so all factors are (imperfectly) substitutable in the same way. Second, the incidence is derived assuming frictionless labor market assumptions; e.g., perfect competition, price taking. Third, the model has abstracted from fully modeling the tax system or incorporating different industries or trade patterns. Incorporating and resolving these issues would be an interesting, informative, and potentially important contribution to understanding the incidence effects of government programs.

CHAPTER 2

THE LOCAL LABOR MARKET EFFECTS OF STATE EARNED INCOME TAX CREDIT SUPPLEMENTS

2.1 Introduction

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

While previous analyses have used state EITC policy variation for identification, there has been no systematic evaluation of these supplements on local labor markets absent the federal portion of the program.[2] Kleven (2019) uses stacked event study designs to investigate individual level effects of the programs and finds a precise zero.[3] Neumark and Williams (2016) find using state level tax return data that state expansions do increase federal EITC take-up. Additionally, Neumark and Shirley (2017) consider long run effects of anti-poverty policies for urban census tracts and find mixed evidence of long-run employment responses. I complement these efforts by focusing on local aggregate outcomes using different data, methods, and variation.[4]

I evaluate these questions at the county level using two empirical designs that exploit policy

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

[2]For example, consider Leigh (2010); Neumark and Williams (2016); Kasy (2017); Bastian (forthcoming) use the maximum state EITC credit as a continuous difference in difference style design.

[3]Specifically, he uses two different methods for this. In the first he creates a synthetic control state for each expansion state for unmarried women with children (and a check using a triple difference including unmarried women without children) for an aggregate state level regression, and in the second it is a more conventional event study design using individual level data.

[4]Buhlmann et al. (2018) use an event study and border pair design to look at tax bunching at EITC kink points, but do not look at other outcomes.

variation across state borders. First, I use a state border pair fixed effect design (SBFE) that generalizes a case-study approach while controlling for local economic conditions, similar to Holmes (1998); Huang (2008); Dube, Lester and Reich (2010). Second, I use a state border distance regression discontinuity design (SBRD) that accounts for the degree a county's economic activity occurs near a state border, similar to Dieterle, Bartalotti and Brummet (2020). These designs allow me to control for local macroeconomic shocks that previous EITC studies have not controlled, which would bias results if present.[5]

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

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

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

This could imply that state supplement variation is subject to underlying trends in near-by states that are also correlated with labor market variables in the expansion state. This threat to

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

identification of causal EITC effects has not been explored previously, as far as I am aware.

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

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

In aggregate, my results suggest a modest increase in federal EITC take-up, no effect on migration or commuting, and an inconclusive effect on employment and earnings. State supplements increase benefits to low income workers but do not necessarily increase local employment to offset state expenditures. This implies that state EITC supplements function as a conditional cash transfer, where the condition is having low gross earnings and qualifying children, rather than as an economic development tool, which is the explicit rational for several of the state programs.

Thus, while state EITC programs may be a worthwhile anti-poverty program, it is not obvious that the programs pay for themselves in terms of labor market effects. This result implies that state EITC supplements do not fulfill the economic development justification of some states for their implementation. However, it may be possible that state programs generate demand effects, which could indirectly increase tax income tax revenue. This remains to be explored.

2.2 State EITC Supplements

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

Two justifications for EITC programs are that they provide economic stimulus benefits and/or provide economic relief to low income workers. Michigan justifies its program using the former: " *The earned income tax credit, at both the federal and state levels, is intended to increase work effort and attachment to the labor force and is a good example of a tax expenditure designed to influence taxpayer behavior* (Executive Budget Appendix on Tax Credits, Deductions, and Exemptions)." While California includes the latter justification in the text of the law itself: " *...The purpose of the California Earned Income Tax Credit is to reduce poverty among California's poorest working families and individuals* (CA Rev & Tax Code §17052.12, 2018)."[7]

Table 2.1 reports several policy features of state supplements and usage. Columns (b)-(e) report state supplement rates, the tax year 2020 average[8] maximum credit in the state (equal to the supplement rate times the average federal max credit), whether the state supplement is refundable, and how the state supplement treats non-resident workers. States that make the EITC refundable

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

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

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

effectively can make average state tax rates negative, while non-refundability reduces the salience and effect of a state supplement.[9] Most states make nonresidents ineligible for state credits; however, seven make them available at a prorated rate equal to the portion of 'state AGI' to 'total AGI' and four place no limit on the credit (though none of these are refundable).

Columns (f)-(i) report total state EITC claims, state expenditures, and these values as a fraction of federal EITC usage in the states. The table shows that number of state EITC claim roughly matches federal claims. New York and the District of Columbia have claims above the federal amount while Hawaii, Virginia, Wisconsin, and South Carolina have much fewer claims than the federal program.[10] However, the tax expenditure of each state is typically much lower given that state supplement rates are bounded between 0% and 40% across states. The average of column (i) is 15.7% that is slightly lower the average state supplement rate in column (b), 17.1%, because (i) incorporates differential take-up of state EITCs.

Next, Figure 2.1 shows the variation in State EITC supplement rates over time. The states in pink do not supplement the Federal EITC, while darker shades of blue correspond to larger state supplement rates.[11] Interestingly, there seems to be some spatial correlation in State EITC spread, where most states with a program border another state with a program.

Figure 2.1 also shows for 2017 the state variation in State EITC program supplement rates and maximum credits and county level distribution of Federal EITC returns and average EITC amount deciles. There appears to be a negative correlation between State EITC programs and Federal EITC usage. Federal EITC usage appears to be concentrated in the South and Sunbelt while State EITC programs are mostly in the Plains and and Midwest. The figure also shows the 2000 distribution of unmarried mothers and of all mothers (married and unmarried) in the labor force at

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

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

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

Table 2.1: State EITC Returns and Amounts
Tax Years: 2017-2020 Most Recent Value

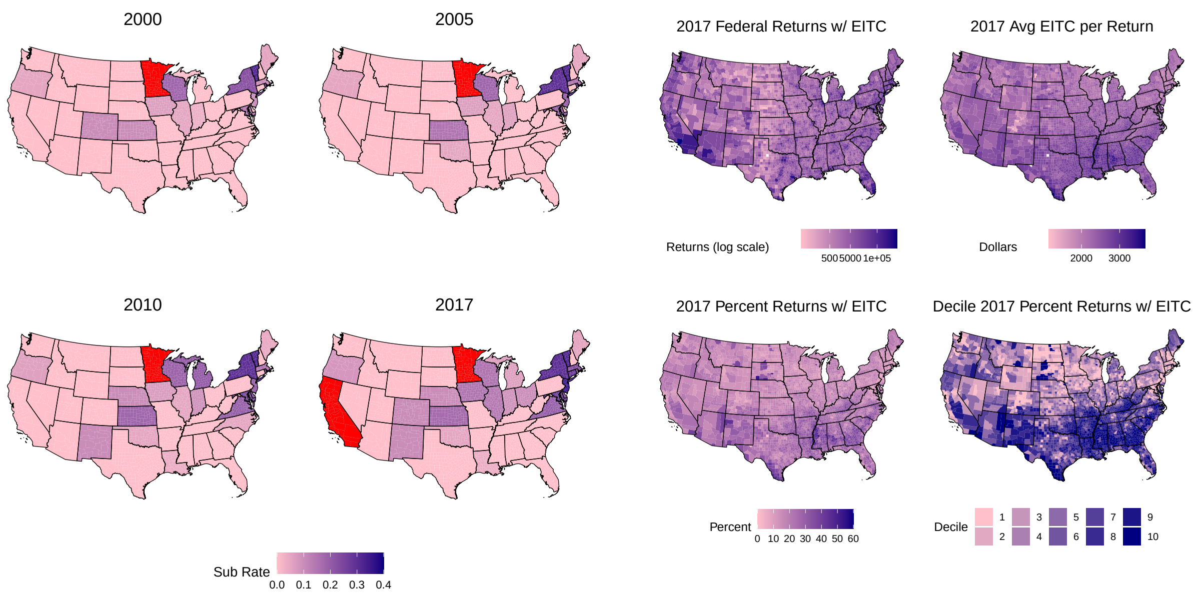
State	Subsidy Rate (%)	State Max ($)	Refundable (Y/N)	Non-Resident Treatment	State Claims (1000s)	State Amount ($millions)	State % of Fed Claims	State % of Fed Amount
(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)	(i)
CA	–	–	Y	Ineligible	2,046	388	72.5	5.9
CO	10	513	Y	Ineligible	343	74	103.3	10.2
CT	27.5	1412	Y	Ineligible	193	95	89.5	19.7
DE	20	1027	N	Ineligible	–	14	–	7.8
DC	40	2053	Y	Ineligible	63	79	127.6	68.5
HI	20	1027	N	Fraction	56	15	59.2	7.5
IL	18	924	Y	Fraction	914	316	99.1	13.7
IN	9	462	Y	Fraction	–	104	–	8.7
IA	15	770	Y	Fraction	208	69	107.5	15.2
KS	17	873	Y	Ineligible	197	79	100.9	16.9
LA	3.5	180	Y	Ineligible	–	49	–	3.5
ME	5	257	Y	No Refund	100	10	105.3	5.1
MD	28	1437	Y	Ineligible	–	166	–	18
MA	23	1181	Y	Ineligible	–	205	–	25.2
MI	6	308	Y	No Refund	–	118	–	6.2
MN	–	–	Y	Ineligible	315	244	99.8	34.9
MT	3	154	Y	Ineligible	–	–	–	–
NE	10	513	Y	Ineligible	120	29	95	9.5
NJ	37	1899	Y	Ineligible	–	440	–	31.5
NM	10	513	Y	Ineligible	198	50	99.4	10.1
NY	30	1540	Y	No Refund	2,332	1,082	143.9	28.5
OH	10	513	N	No Limit	783	179	88.3	8.2
OK	5	257	N	Ineligible	300	16	93.3	1.9
OR	8	411	Y	Fraction	247	49	96	9
RI	15	770	Y	Fraction	93	28	116	15.5
SC	20.1	1032	N	Ineligible	60	21	12.7	1.8
VT	36	1848	Y	Ineligible	40	27	96.6	34.2
VA	20	1027	N	No Limit	347	136	59.5	9.7
WI	15	770	Y	Ineligible	239	93	68	11.8

State EITC returns and amounts data accessed from individual state websites; typically state tax expenditure reports. Most recent value is reported. Federal EITC returns and amounts from tax year 2018 (IRS SOI). The New York number of returns uses both NY state and NY city EITC programs and likely double counts the number claims. CA and MN have non-standard programs that do not map into a single subsidy rate. MT implemented its program for tax year 2019 and has not released expenditure reports. Sources for table in Appendix B.1.

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

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

Figure 2.1: EITC Policy and Use Variation



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

2.2.1 Across State EITC Policy Coordination

Figure 2.2 plots policy variation at state borders due to state supplements across several dimensions. All three plots are plotted in 'event-time' of a state EITC implementation that has occurred after 2000. Figure (2.2.a) shows the average change in max state credit (federal plus state EITC) when a state implements a supplement across all state borders. On average, this change is \$466 or a 9.5% increase in generosity, which is roughly a 1-2% increase in annual gross earnings for a single tax-filer with one qualifying dependent in the max-credit region.

As Figure 2.1 shows, some state borders have only one state supplement (e.g., Virginia and Kentucky) while others have supplements on both sides (e.g., Virginia and Maryland). I call borders with only one supplement 'one-sided' and borders with two supplements 'two-sided.' In the case of two-sided borders, the older program is the 'incumbent' and the newer program is the 'implementing' program. For this paper, I focus on state supplements introduced after 2000, so all incumbents have programs initiated before 2000 and implementing programs after 2000.

Figure (2.2.b) compares the state supplement rates between state borders that are one-sided versus two-sided borders. The plot shows that states implement more generous subsidies when their neighbor already has a state program. On average, implementing states make their supplement 7 percentage points more generous than implementing states without a neighboring incumbent program.

Figure (2.2.c) plots the incumbent state's policy reaction to their neighbor's new supplement. Specifically, the plot shows whether the incumbent's subsidy rate in each period is statistically different from the rate the year before the new program is implemented. The result suggests that incumbents make their programs on average 2 percentage points more generous in the five years after their neighbor's implementation.

2.2.1.1 Implications of Coordination

Overall, Figures (2.2.b-c) imply that state borders where both sides have state supplements may not be setting their state supplement rate completely exogenously, which may limit what can be learned from expansions along these borders.

For two states, $s \in \{1, 2\}$ along a given border segment, b, let r_{sbt} be the state EITC supplement rate. The results in Figure 2.2 tell us that $\mathsf{Cov}(r_{1bt}, r_{2bt}) \neq 0$. This is not obviously a concern.

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

$$y_{sbt} = \alpha_s + \lambda_{bt} + \gamma r_{sbt} + u_{sbt}. \tag{2.1}$$

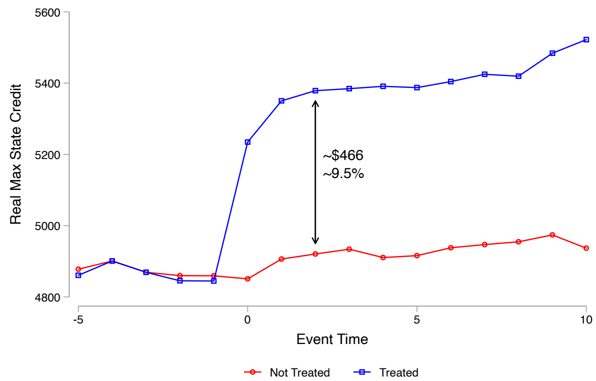
If a neighbor's policy is uncorrelated with unobservable trends in the outcome variable, then the OLS regression estimate of γ is unbiased despite the policy coordination:

$$\mathsf{Cov}(r_{1bt}, u_{2bt}) = 0 \implies \mathsf{E}[\hat{\gamma}^{\mathsf{OLS}}] = \gamma. \tag{2.2}$$

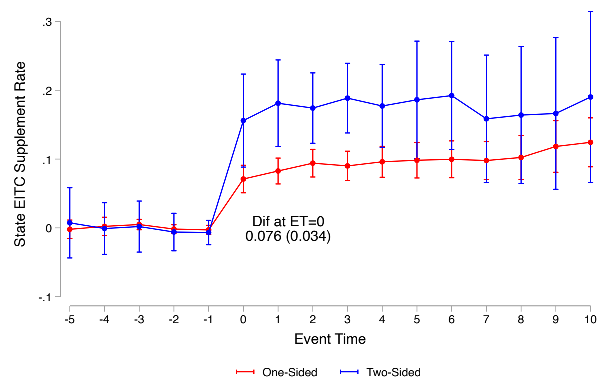
However, if the variables are correlated, then the policy coordination will bias the estimate: $\mathsf{Cov}(r_{1bt}, r_{2bt}) \neq 0 \land \mathsf{Cov}(r_{1bt}, u_{2bt}) \neq 0 \implies \mathsf{E}[\hat{\gamma}^{\mathsf{OLS}}] \neq \gamma$. Examining this theoretical relationship is beyond the scope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

Figure 2.2: Effect of State Supplement Implementation

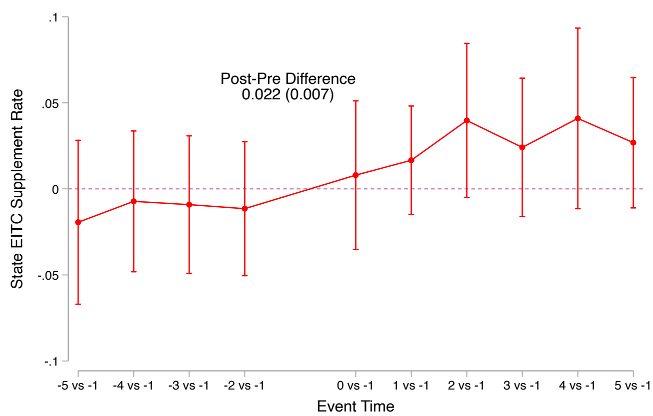
(a) Average Effect on Real Max Credit



(b) Supplement Rates by Neighbor's EITC Status



(c) Neighbor's Reaction to Supplement Introduction



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

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

2.3 Evaluating State EITC Supplements

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

Let $\mathcal{S}$ be the set possible locations – 'counties' – in the economy, and the counties in $\mathcal{S}$ can be partitioned into M 'states,' $\mathcal{S} = \{S_1, S_2, \ldots, S_M\}$. Let there be a unit mass of individuals indexed by $i \in N$ making a residence and work location choice with the option of unemployment. Suppose that individuals have preferences such that the probability that an individual chooses a work and residence pair (o, d) as:

$$\underbrace{\Pr((\mathsf{o}^i, \mathsf{d}^i) = (o, d))}_{:=\pi_{o,d}} = \underbrace{\Pr(\mathsf{d}^i = d \mid \mathsf{o}^i = o) \cdot \Pr(\mathsf{o}^i = o)}_{:=\pi_{d|o} \cdot \pi_o}. \tag{2.3}$$

That is, individuals have a two-stage decision process such that they first choose a residence location, $o \in \mathcal{S}$, and then a work choice $d \in \{\mathcal{S} \cup \{\text{Unemployment}\}\}$ based on some (potentially endogenous) indirect utility value; e.g., a residence-location specific amenity plus post-tax earnings. I denote agent i's choice bundle as $(\mathsf{o}^i, \mathsf{d}^i)$.[14]

The fiscal externality of a marginal tax reform is equivalent to the behavioral effect on tax revenues (Hendren, 2016b; Finkelstein and Hendren, 2020; Kleven, 2020). If state government s uses residence based income taxation[15] with origin-destination specific tax rates, $R^s = \sum_{o \in s} R^o = \sum_{o \in s} \left(\sum_{d \in \mathcal{S}} t^o_d w^o_d \pi_{d|o} \pi_o \right)$, then the first-order[16] fiscal externality as a proportion of initial revenue

[13] The model is similar to Monte et al. (2018), who document variation in local labor supply elasticities, and conceptually similar to Agrawal and Hoyt (2018a) who document the effect of tax differentials on commuting patterns.

[14] Such preferences can be microfounded based a stochastic taste shifter drawn from a Generalized Extreme Value distribution, one example of which leads to the Nested Logit model.

[15] If residents spend a fixed portion of net income across goods (via homothetic preferences), then the income tax is isomorphic to a composite tax on labor income and purchases.

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

is (where $\hat{x} = \mathsf{d}x/x$):

$$\frac{\mathsf{FE}^s}{R^s} = \sum_{o \in s} \frac{R^o}{R^s} \left(\underbrace{\hat{\pi}_o}_{\text{Migration}} + \underbrace{\frac{R^o_o}{R^o}(\hat{w}^o_o + \hat{\pi}_{o|o})}_{\text{Own Employment}} + \underbrace{\sum_{d \in \mathcal{S} \setminus o} \frac{R^o_d}{R^o} \cdot (\hat{w}^o_d + \hat{\pi}_{d|o})}_{\text{Commuting}} \right). \tag{2.4}$$

It can be shown that the fiscal externality is a sufficient measure of the change in aggregate welfare divided by the marginal cost of public funds (μ) when evaluated at utilitarian social welfare weights ($g^i = 1$): $\frac{\mathsf{d}W/\mathsf{d}\theta}{\mu}|_{g^i=1} = \mathsf{FE}$ (Hendren, 2016b; Kleven, 2020). Kleven (2020) notes if it is possible to directly estimate the behavioral effect on tax liabilities, then this quantity can theoretically be estimated without estimating specific response elasticities. However, given the possibility of migration and commuting, it is not obvious what the appropriate control group would be for such an empirical exercise.

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

2.4 Empirical Designs

I use two empirical designs on the set of counties that are at state borders with a policy difference. The first is a state-border fixed effect (SBFE) design that removes common time-varying shocks between each border county pair. The second is a state-border regression discontinuity (SBRD) design that parametrically controls for distance to the policy border.

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

For all the designs below, let y be the log of some outcome variable, let X be controls, let r be the state supplement rate, and let T be an indicator equal to one if the state's program is in-effect. For all the regressions, I control for log population (or log tax returns), log real state GDP, county fixed effects, and either county-pair-year fixed effects or state-border-year fixed effects interacted with distance to the state border. In addition, I weight all regressions by county population in 2000. I explain each design in more detail separately.

2.4.1 Max State Credit Variation

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

Previous studies[17] have used the '(log) state maximum credit' where the state maximum credit is constructed as a dependent-size weighted maximum credit, where the weights represent the number of families with 1, 2, or 3+ dependents. Literally, these studies calculate this as $C_{st} = 0.4C_{st}^{1} + 0.4C_{st}^{2} + 0.2C_{st}^{3+}$, where C_{st}^{i} is the max state credit for i dependents. As most states programs use a 'top up' formula, each C_{st}^{i} term is calculated as $(1 + r_{st}) \cdot C_{t}^{i}$, where C_{t}^{i} is the federal max credit for i dependents.[18] Combining these two facts, the real max state credit is one plus the state supplement rate times a weighted average of the federal max credits for dependents: $C_{st} = (1 + r_{st}) \cdot (0.4C_{t}^{1} + 0.4C_{t}^{2} + 0.2C_{t}^{3+})$.

A regression of C_{st} or $\ln[C_{st}]$ on the supplement rate and year indicators, $\{r_{st}, D_t\}$, will absorb all the variation in the state max credit variable and yield an R^2 value of 1.[19] Thus the state max credit variable is econometrically equivalent to using the state subsidy rate and year indicators. One cannot separately identify the effect of the level of EITC on an outcome variable from common year

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

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

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

effects captured by year indicator variables; rather, one can only identify the relative differences between states within a given year. Since nearly all work on the EITC includes year indicators as control variables, any prior work that claimed to identify the effect of 'dollars of additional EITC' misstated their actual finding.

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

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

2.4.2 State Border Fixed Effect

The SBFE design uses every county pair with a policy difference to generalize the case study approach (Dube et al., 2010). The design residualizes by a pair-year fixed effect that is assumed to capture common unobservable trends. Under that assumption and uncorrelatedness with the error term, differences correlated to state EITC policies are interpreted causally.

The regression equation is:

$$y_{cpst} = X_{cpst}\beta + \gamma^r \ln\left[C_{st}\right] + \lambda_c + \lambda_{pt} + u_{cpst}, \tag{2.5}$$

where c indexes counties, p for county pairs, s for states, and t for years. For inference, I cluster standard errors at the state-border level.

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

2.4.3 State Border Regression Discontinuity

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

An ideal study would use as fine a local geography as possible, such as census blocks, to take full advantage of using distance to the border as an identification strategy. Dieterle et al. (2020) note that counties are not ideal for this analysis since counties are a political jurisdiction rather than an economic market area and county land area varies greatly by state.[21] I use a global polynomial method because the implied measurement error from using counties forces the use of a parametric method rather than a non-parametric local method (Dieterle et al., 2020).

The regression equation is:

$$\begin{aligned} y_{csbt} &= X_{csbt}\beta + \gamma^{C} \ln\left[C_{st}\right] + \lambda_c + \lambda_{bt} \\ &\quad + D_{bt} \cdot \left[1 + \sum_k \theta_{bt}^{0;k}(1 - T_{st}) \cdot m_{csb}^{k} + \sum_k \theta_{bt}^{1;k} T_{st} \cdot m_{csb}^{k}\right] + u_{cst}, \end{aligned} \tag{2.6}$$

where c indexes counties, s for states, b for state borders, t for years, and k for the order of the global polynomial. I consider only linear ($k = 1$) and quadratic ($k = 2$) terms but allow the distance regressions to vary depending on being on the treated or untreated side.[22] For inference, I cluster standard errors at the state-border level.

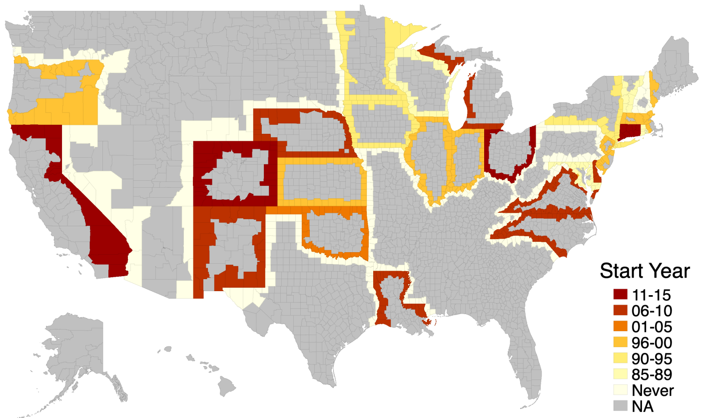
2.5 Data

The data used in the analysis are based on the contiguous border counties in the United States. There are 3,144 county equivalents (including DC) in the US of which 1,184 share a border segment with another county, but only 905 have a policy discontinuity due to a state EITC program at some point in time. The median border county has two contiguous neighbors, but there are 30 counties

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

[22] This is similar to Dieterle et al. (2020) except they implement a more data-driven approach by allowing the number of polynomials to vary for each state border.

Figure 2.3: Border Counties by Treatment Status



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

with 5 or more neighbors. I observe these county-pairs from 2000 to 2018.[23] I focus on the period starting in 2000 to avoid using variation from the 1994 OBRA expansion and welfare reform in the late 1990s. Figure 2.3 shows the specific counties used in the paper by state supplement start.[24]

The tax return data is from the IRS Statistics of Income (IRS SOI).[25] The migration data is also based on the IRS Statistics of Income County to County Flows.[26] The commuting data is from the Census Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics Data, where I aggregate to the county level. Finally, the employment and earnings data are from the Census Quarterly Workforce Indicators.

For the migration and commuting data, I calculate the net migration / commuting percent as the difference of entrants minus exiters over an initial local value. Specifically, the net migrants percent is entrants minus exiters divided by the start of year county residents, while the net commuting percent is the difference between in-commuters and out-commuters divided by employed county

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

[24]CO had a short-lived program from 1999-2001 that I have omitted; SC's program started in 2018.

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

[26]The years 1990 to 2000 are adapted from pre-formated files from Hauer (2019).

residents (equal to the out-commuters plus non-commuting workers).

I collect state EITC parameters from the supplementary information for NBER's TAXSIM (Feenberg and Coutts, 1993).[27] County population is from the Census Population and Housing Unit Estimates, which estimates county level population between census years. State Gross Domestic Product (GDP) data is from the Bureau of Economic Analysis's Gross Domestic Product by State series.

In Table 2.2 I present summary statistics for the data used. Column (a) includes all counties in the continental US while columns (b-d) only use the 905 contiguous border counties that I use in the estimation. Counties that are never-treated (c) appear to differ relatively more from all counties in column (a) than the ever-treated counties (d).

Table 2.2: Summary Statistics

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

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

[27] I have also manually checked the values by going to the various state websites.

2.6 Results

Given the patterns shown in Figure 2.2, I present three sets of results. First, I present results that use all possible state borders with at least one year of a policy discontinuity. Second, I focus on the one-sided state borders where only one state has a EITC supplement for the whole sample period. Each of these use variation in the maximum credit available in the state based on the state's subsidy rate.

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

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

2.6.1 All Borders

Table 2.3 displays the results using all borders. The table presents either semi-elasticities (returns, employment, earnings) or level changes (net migration percent, net commuting percent). Recall, on average a state supplement increases EITC generosity by about 10 percentage points from a federal max credit of $4,870 in 2017, the final year in the sample.[28]

Panel A shows that a one percentage point increase in EITC generosity induces between $[0.16, 0.44]$ percent additional Federal EITC returns for the county relative to counties across the state border. Each semi-elasticity is statistically significantly different from zero. This result implies that state supplements induce greater take-up of the federal EITC either due to greater

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

awareness or increasing earnings to require filing a tax return.

Panels B and C display estimates of a one percentage point increase in the state supplement implies a γ-percentage point change in the net migration / commuting. Neither set of estimates is statistically different from zero. The migration coefficient estimates are between $[0.003, 0.017]$ from a mean of 0.004. The commuting coefficient estimates are between $[-0.54; -0.23]$ from a mean of 0.18. While the migration change estimates seem plausible, taken literally the commuting changes imply huge economic effects given that supplements increased by 10 percentage points.

Panels D and E display employment and earnings semi-elasticities, similar to Panel A. The estimated employment semi-elasticity is between $[-0.26, -0.07]$, which would imply that state supplements decrease the number of workers in a county relative to counties across the border. None of the estimates is statistically different from zero. The estimated earnings elasticity is between $[-0.12, -0.09]$, which would imply that state supplements decrease the workers' earnings in a county relative to counties across the border. These estimates are each statistically different from zero. Assuming the employment effect is weakly negative, the negative earnings effect could be the result of workers reducing their hours (an income effect) or potential subsidy capture by employers.[29]

2.6.2 One-Sided Borders

Table 2.4 displays the results using only one-sided borders with state implementations after 2000. This subsample mirrors the event study analysis presented in the next subsection but uses maximum credit variation as in Table 2.3.

On balance, these results fail to provide evidence of recent state EITC supplements affecting labor market outcomes. In Panel A, instead of being positive and statistically different from zero, the new returns semi-elasticity is near-zero for the SBFE and negative for the SBRD. In Panel B, the migration changes are similar in magnitude as before and are still statistically indistinguishable

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

Table 2.3: Effect of State EITC Programs: All Borders

Model:	SBPFE (a)	SBRDD:L (b)	SBRDD:Q (c)
Panel A: (Annual)	ln [Fed EITC Returns]		
State Supp. Rate	0.15	0.21	0.37
	(0.05)	(0.17)	(0.14)
Observations	34,790	18,606	18,606
Panel B: (Annual)	Net Migration Percent		
State Supp. Rate	0.003	0.006	0.017
	(0.005)	(0.010)	(0.012)
Observations	34,812	18,612	18,612
Panel C: (Annual)	Net Low+Mid Wage Commuting Percent		
State Supp. Rate	-0.28	-0.54	-0.23
	(0.38)	(0.39)	(0.35)
Observations	32,878	17,578	17,578
Panel D: (Quarterly)	ln [Total Employment: Women, Less HS]		
State Supp. Rate	-0.08	-0.20	-0.31
	(0.08)	(0.16)	(0.17)
Observations	141,129	75,234	75,234
Panel E: (Quarterly)	ln [Avg Earnings: Women, Less HS]		
State Supp. Rate	-0.11	-0.12	-0.10
	(0.04)	(0.03)	(0.04)
Observations	139,518	65,394	65,394
Cluster	State Border	State Border	State Border

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

from zero. In Panel C, the commuting changes magnitudes vary by three orders of magnitude depending on the design. In Panel D, the employment semi-elasticities are now all positive rather than negative. In Panel E, two of the earnings semi-elasticities are now also positive.

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

Table 2.4: Effect of State EITC Programs : One-Sided State Borders

Model:	SBPFE (a)	SBRDD:L (b)	SBRDD:Q (c)
Panel A: (Annual)	ln [Fed EITC Returns]		
State Supp. Rate	0.07	-0.07	-0.30
	(0.12)	(0.25)	(0.43)
Observations	11,974	6,366	6,366
Panel B: (Annual)	Net Migration Percent		
State Supp. Rate	-0.006	-0.012	0.005
	(0.012)	(0.015)	(0.054)
Observations	11,998	6,372	6,372
Panel C: (Annual)	Net Low+Mid Wage Commuting Percent		
State Supp. Rate	0.03	0.15	1.73
	(0.13)	(0.26)	(0.93)
Observations	11,196	5,949	5,949
Panel D: (Quarterly)	ln [Total Employment: Women, Less HS]		
State Supp. Rate	0.23	0.54	0.13
	(0.15)	(0.55)	(1.37)
Observations	47,672	25,298	25,298
Panel E: (Quarterly)	ln [Avg Earnings: Women, Less HS]		
State Supp. Rate	-0.04	0.21	0.56
	(0.10)	(0.26)	(0.61)
Observations	47,075	24,941	24,941
Cluster	State Border	State Border	State Border

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

2.6.3 Stacked Event Studies

To probe the differences between Table 2.3 and 2.4, I perform event study analyses that use variation in state program implementation timing.

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

The specifications I estimate is the following:

$$y_{cpst} = X_{cpst}\beta + \sum_{v \in \mathsf{V}} \gamma_v \cdot 1[t - T_{ps} = v] \cdot 1[D_s = 1] + \lambda_c + \lambda_{pt} + u_{cpst} \tag{2.7}$$

$$\begin{aligned} y_{csbt} = X_{csbt}\beta + \sum_{v \in \mathsf{V}} \gamma_v \cdot 1[t - T_{ps} = v] \cdot 1[D_s = 1] + \lambda_c + \lambda_{bt} \\ + D_{bt} \cdot \left[1 + \theta^0_{bt}(1 - T_{st}) \cdot m_{csb} + \theta^1_{bt} T_{st} \cdot m_{csb}\right] + u_{cst}, \end{aligned} \tag{2.8}$$

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

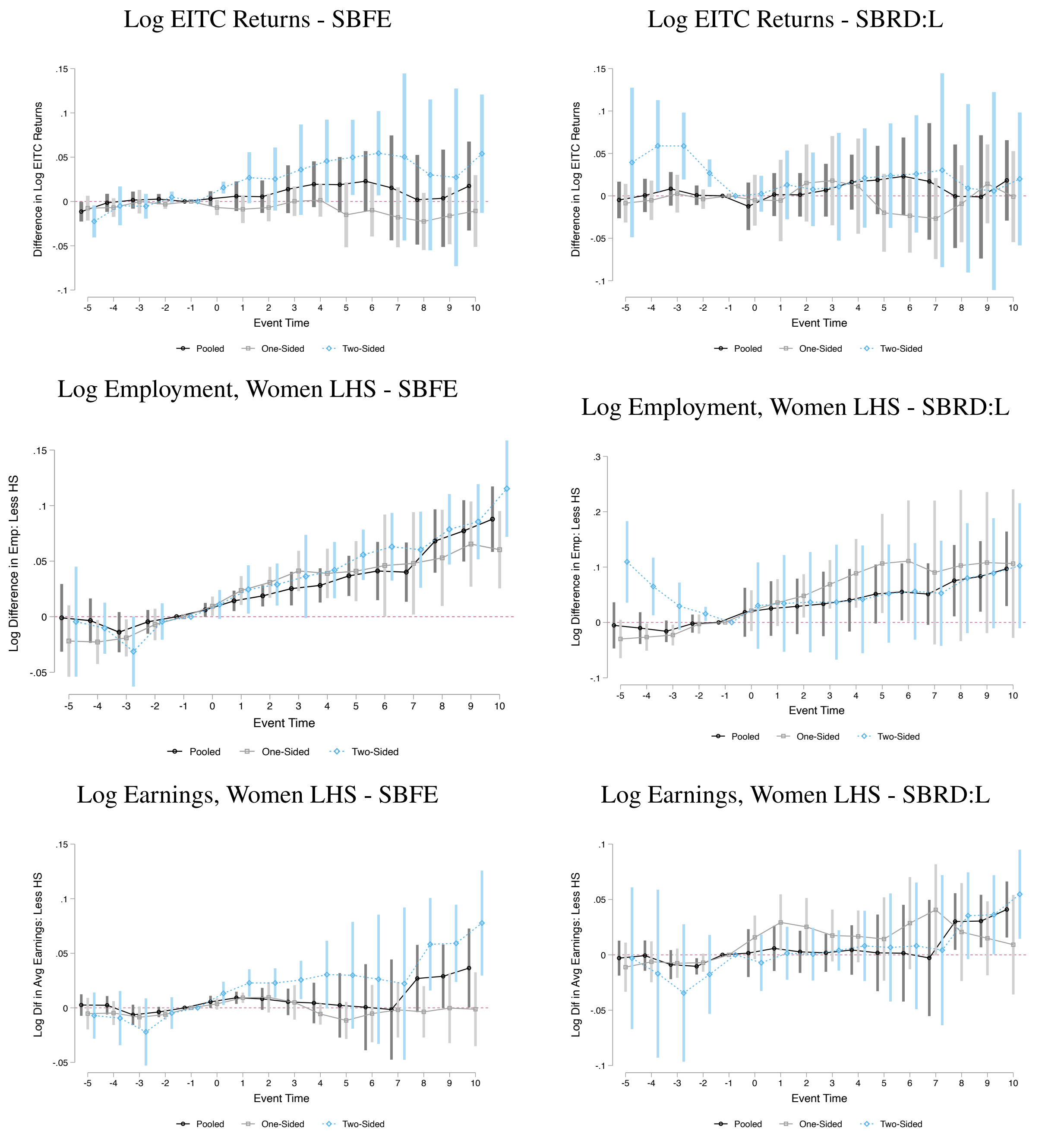
I am able to split the analysis by one- and two-sided borders and to inspect pre-trends and anticipation effects. The pooled results correspond to the results in Table 2.3 and the one-sided results correspond to Table 2.4. Almost all the estimates are not statistically different from zero, which again fails to provide evidence that state supplements affect labor market outcomes. Generally, the pooled and one-sided sample pre-treatment periods are centered around zero implying no pre-trends; however, the two-sided sample results appear to have pre-trends that raise concerns about the treatment effects.

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

2.7 Conclusion

The Earned Income Tax Credit is one of the largest anti-poverty programs in the United States and is increasingly supplemented by the states. Several states explicitly justify their programs as an economic development tax expenditure meant to increase labor force participation. I documented variation in state EITC policies and test this claim using two empirical designs that use variation at

Figure 2.4: Stacked Event Study Plots



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

state borders. I test for effects in federal EITC take up, county migration, county commuting, and employment and earnings for women with less than a high school degree.

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

Overall, my results imply that state EITC expansions do not function as economic development tools. Thus, state EITC function as an anti-poverty program but with little (or no) labor market distortions. My evaluation centered on the labor market effects, so it is possible that expansion increase local demand. This channel remains to be explored.

CHAPTER 3

IS THE RENT TOO HIGH: LAND OWNERSHIP AND MONOPOLY POWER (WITH OREN ZIV)

3.1 Introduction

Property rights grant landowners exclusive use over parcels of land. Since Chamberlin (1933), and as far back as Adam Smith, economists have considered whether this arrangement endows landowners with monopoly pricing powers.[1] *A priori*, property rights need not generate monopoly power, and it is standard for models of real estate markets to assume competition is perfect.[2] Moreover, the empirical relevance of any potential landowner market power and, as a result, its policy implications are poorly understood.

This paper investigates the economic impact of market power due to land rights. We answer two questions: is this power economically meaningful, and how should this alter our understanding of urban land use policies? Using data on multi-unit residential rental buildings in New York City (NYC), we find that monopoly markups are on average about a third of rental prices. We show how monopoly markups interact with zoning regulations, and examine the possibility that restrictions on land ownership concentration can reduce rents.

Using a model that nests two monopoly power generating mechanisms—vertical and horizontal differentiation—we first explore the theoretical implications of monopoly markups for urban policy. Previous work has focused almost exclusively on a justification of rent control based on landowner monopoly power (Arnott, 1989; Arnott and Igarashi, 2000; Basu and Emerson, 2003). Our framework allows us to explore how monopoly pricing and a larger set of urban policies interact

[1]For Smith, that the landowners could rent unimproved land lead him to believe that rent was a "monopoly price" (Smith, 1776). Ricardo (1817) considered land a differentiated factor of production, so that rents reflected differentials in marginal product. Marx argued that monopoly land rents came from three sources: quality differences, markups designed to limit access to land, and extraction of rents from producers selling at markups (Evans, 1991).

[2]See Brueckner (1987) for a unified, formal Alnso-Muth-Mills (AMM) model and Glaeser (2007) for standard modelling of competitive developers.

in general equilibrium.[3]

For instance, on the one hand, monopoly power attenuates the impact of up-zoning at up-zoned parcels themselves, as rent and quantity changes revert to monopolistic rather than efficient levels. On the other hand, zoning regulations have an additional impact on rents at other locations through markups, and we show that when the cost function for developing and renting units is nondecreasing, heavier zoning constraints in one parcel always raise rents at other, unzoned parcels by raising markups.

We also explore the potential for municipalities to reduce rents by limiting the concentration of land ownership. Restrictions on concentration have been recently proposed by Berlin housing activists (Stone, 2019). We apply the results of Nocke and Schutz (2018a), part of a growing literature on multi-product oligopoly (Affeldt et al., 2013; Jaffe and Weyl, 2013; Nocke and Schutz, 2018a), to the impact of zoning on monopoly markups, and show that with non-decreasing marginal cost, landowners with higher concentration always raise markups. Intuitively, landowners with multiple lots can potentially internalize the impact of one parcel's pricing decision on that of their other parcels. When cost-related substitution effects between parcels are sufficiently small, this can lead to higher rents and markups. Furthermore, we extend the results of Nocke and Schutz (2018b), finding conditions under which increased concentration also generates increases in prices for all other products, or, in our case, parcels.

While these theoretical channels may exist, a separate question, over which the literature is silent, is whether they are empirically relevant. The extent to which landowners' market power affect rents will depend on the strength of complementarities between renter and building types, as well as the degree to which consumers see housing at similar buildings as substitutes. To answer this question, we construct a new building-level dataset for multi-unit residential buildings in NYC. We obtain building rental income from a combination of scraped public owner communications and deconstructing formulas used by the NYC Department of Finance (DOF) for calculating tax

[3]Diamond et al. (2019) consider the equilibrium effects of rent controls on landowner entry and exit. Urban policies could also interact with monopoly profits through equilibrium entry and exit. We do not know of any paper that explores this interaction.

assessment. Our main results focus on Manhattan buildings, although we probe robustness and derive additional power where necessary from buildings in the Bronx, Brooklyn, and Queens.

First, we find that patterns in the data are consistent with the predictions of our model. In particular, we find that over a seven year period, a 10% increase in Census tract concentration is correlated with a 1-1.6% increase in average building rents. The relationship holds even when fully accounting for time-invariant building characteristics. These correlations are not causal, but they are consistent with the existence of meaningful monopoly power.

Next, we estimate our model in order to ascertain the quantitative scope of markups.[4] The first step in our markup estimation is the estimation of building-level own-price elasticity of demand, accounting for sorting and unobserved building quality. Previous housing demand elasticity estimates focus on the housing-consumption trade-off and, as a result, tend to find inelastic results (Albouy and Ehrlich, 2018), which, if taken literally, would be inconsistent with monopoly pricing.[5] However, the relevant elasticity for a landowner's pricing decision is the own-price elasticity that accounts for substitution *between* rival buildings. We estimate this elasticity and find median building own-price demand elasticity of -3.3 in our preferred specification.[6]

An important aspect of our empirical environment is the ubiquity of constraints on prices and quantities in the form of rent restrictions and zoning regulations.[7,8] In order to use our estimated

[4]Our estimation method is based on differentiated product demand estimation developed by Berry, Levinsohn and Pakes (1995) and Grigolon and Verboven (2014). Within urban housing demand literature, our work is most closely related to Bayer, McMillan and Rueben (2004), who estimate housing demand and resident sorting within San Francisco. See Kuminoff, Smith and Timmins (2013) for a literature overview.

[5]Using hedonic approaches with building-level data, Gyourko and Voith (2000) and Chen et al. (2011) find elasticities compatible with monopoly pricing, but only the latter notes the connection with monopolistic landowners.

[6]When we estimate the change in aggregate rental demand if *all* building rents increased by 1%, which is closer in spirit to previous estimates, we then find an inelastic result of -0.14.

[7]NYC has two forms or rent regulation, rent control and rent stabilization; we use the term rent stabilization for all rent regulation. Control is now rare as it applies only to buildings built before 1947 for tenants in place before 1971. Stabilization by far more common based on a building having 6+ units and built before 1974 and and may pass between different tenants; stabilized units' rent annual growth set by NYC Rent Guidelines Board.

[8]For zoning constraints, we ask whether a building could add one additional minimum sized residential unit based on floor-area-ratios and density limits.

parameters to further estimate markups, we use detailed building characteristics to isolate the set of buildings in our sample which are neither rent stabilized nor constrained by zoning.[9] We call this sample policy-unconstrained. We find that in the policy-unconstrained sample, rents include an average markup over marginal costs of $705 per month, with the mean and median markup being 30% of the rent in our preferred specification. These markups are over "shaddow" marginal costs including amortized purchase and maintenance costs and outside options.

In addition, our model assumes quantity can be set optimally for current-period demand, a condition unlikely to be met in our setting where fixed costs of construction and durable housing stocks make quantity adjustments lumpy. While we show that our model is isomorphic to one with separated developers and landowners with rational expectations, much of the housing stock in our sample was likely constructed (and quantity set) at a time when 21st century demand was unforeseeable. Accordingly, we isolate the subset of the policy-unconstrained sample which were constructed in the last decade of our data, and separately calculate markups for these. We find markups are similarly on average 31-32% of rent for these buildings. In an additional specification, we estimate elasticities and markups using data from the Bronx, Brooklyn, and Queens in addition to Manhattan. We find average markups range by borough between 21-30% for new, policy-unconstrained buildings.

Finally, we use our results to assess the quantitative impact of up-zoning on markups, using the cross-price elasticities generated by our estimates in order to quantify the impact of a marginal relaxation of zoning constraints on rents at policy-unconstrained parcels. As noted by our model's predicted interaction between zoning and markups, the large markups we find may in part reflect the pecuniary spillovers of the (many) zoning-constrained lots on the policy-unconstrained sample. Indeed, we find the ubiquity of zoning constraints appears to have an appreciable impact on rents at policy-unconstrained lots. On average, a policy change resulting in the construction of roughly 417 additional units at zoning-constrained parcels reduces markups by between $6.72 and $7.41 per unit at policy-unconstrained buildings, which implies an additional 5-19 units through increased

[9] We calculate that 92% of Manhattan rental buildings with four or more units are either zoning constrained or rent stabilized.

price competition. For context, the magnitude of this spillover on rents at the significantly smaller unconstrained sample is over 10% of what the magnitude of the (first-order) average rent effect on the up-zoned lots themselves would be.

3.2 Model

We first set up the optimization routines for each agent in our model: landowners endowed with locations and choosing rental rates, and renters endowed with income and choosing residences. We then define and solve the equilibrium in two cases: first, without vertical differentiation in location quality, and, second, without horizontal differentiation. We review how, in each case, the model delivers landowner pricing power.

3.2.1 Setup

Parcels and Landowners The space, a city, is comprised of a set $\mathcal{A} = \{a_0, a_1, a_2, \ldots, a_J\}$ discrete parcels, which differ according to their underlying quality a, drawn without replacement from a distribution $G_1(a)$. Higher values of a have higher amenity value to renters. We refer to a as “location quality" and differences in a as vertical differentiation in parcels. A location's realized quality a will also be used henceforth to index each location in the set $\mathcal{A}$. We make the simplifying assumption that a is exogenous, while noting that in the data building and parcel characteristics are a mix of endogenously chosen and exogenously given. Additionally, we set a_0 as living out of the city; i.e, an outside-option.

Each parcel has a unique landowner $f \in \mathsf{F}$ who maximizes profits by choosing the rent level at her location. Here, we also assume landowners each own a unique parcel, although we relax this later on.

Landowners provide a mass of renters housing at a positive, differentiable marginal cost $c_a(q)$, where q is the mass of renters the landowner accommodates in equilibrium. Total revenue is rent r collected times q. A given landowner f's profits from parcel a are $\pi_a = r \cdot q - c_a(q)$. Landowners determine the constructed quantity and rental price of units, and subtracting markups from rent backs out a "shadow" marginal costs combining both of these activities. Our estimation will not rely on observing these costs. Appendix B shows that equilibrium prices and quantities are unchanged when we separate the development and rental price problems and the markup is capitalized into the price of the building. In Section 3.6.3, we discuss how we navigate this assumption in our empirical setting, where landowners are constrained by policy and supply is set in advance.

Renters A mass M of heterogeneous renters, indexed by $i \in N$, draw income-types y from distribution $G_2(y)$. Renter utility is derived from consumption and location amenities. Renters also draw idiosyncratic tastes for each location, $\epsilon_{i,a}$, from a type-one extreme value distribution $G_3(\epsilon)$ with scale parameter σ_ϵ. Utility may vary independently by type as well:

$$U_i(a; y_i) = F(a, y_i - r(a), y_i) + \epsilon_{i,a}, \tag{3.1}$$

where consumption is equivalent to income minus rent. Renters choose among all locations a to maximize utility taking amenities, rents, and personal income as given.

3.2.2 Equilibrium

An equilibrium will be defined by a schedule of rents and quantities $\{(r_a, q_a)\}_{a\in\mathcal{A}}$ that maximize landowner profits, assign renters to locations a such that no renter can increase utility by choosing to pay rents at any other parcel, and clear the real estate market. Thus, for each type y, the original density of types y is accounted for across all their chosen locations a and the outside option, $g(y) = \sum_{\mathcal{A}} q_a(y) + q_0(y)$.[10]

We make additional assumptions on the renter's payoff function F and the distributions of types to briefly review each source of landowner monopoly in equilibrium.

[10]We do not consider combinations of $G_2(y)$, cost functions, and $G_1(a)$ which result in the full mass of renters choosing the outside option.

3.2.2.1 Equilibrium Under Horizontal Differentiation

For the horizontal differentiation case, we set the quality and income distributions as degenerate; i.e., $a_j = a$ and $y_i = y$. This construction delivers standard multinomial logit choice probabilities for market demand:

$$D_a = \frac{\mathrm{e}^{F(a,y-r(a),y)/\sigma_\epsilon}}{\sum_{a' \in \mathcal{A}} \{\mathrm{e}^{F(a',y-r(a'),y)/\sigma_\epsilon}\}} \cdot M \tag{3.2}$$

We solve the symmetric pricing equilibrium assuming landowners compete in rents and noting that all amenities are equivalent, which yields an inverse elasticity markup rule:[11]

$$r^\star(a) = mc(D_a) - \frac{D_a}{\partial D_a / \partial r} \implies \frac{r^\star(a) - mc(D_a)}{r^\star(a)} = \frac{-1}{\varepsilon_a}, \tag{3.3}$$

where ε_a is the own-price elasticity.

The equilibrium rent at each building equals marginal cost plus a markup related to the curvature of demand, which is a function of the marginal utility of consumption, the scale of the idiosyncratic tastes, and substitution behavior of renters.[12] The solution implies strictly positive markups in rents.[13] To close the model, we apply a market clearing condition that the total number of renters housed in and out of the city equals the total number of renters.

3.2.2.2 Equilibrium Under Vertical Differentiation

For the vertical differentiation case, we assume that the renters' utility function displays increasing complements between renter income y and location quality a and that idiosyncratic draws, $\epsilon_{i,a}$, are

[11] Given the degenerate distribution of amenities, a symmetric solution to the landowner's problem can be reasoned verbally. Suppose all landowners with amenity value a set rent at some equilibrium $r^\star(a)$. Any individual deviation to a higher rent leads to less demand since amenities are equivalent, but any deviation to a lower rent would lead to greater demand.

[12] Caplin and Nalebuff (1991) and Perloff and Salop (1985) show that such an always equilibrium exists.

[13] An economic consequence of the markup is that some renters do not enter though they would if parcels were priced at marginal cost; i.e., $D_a(r^M) < D_a(mc(D_a))$. Thus, there exist renters with a willingness to pay for space greater than their impact on marginal cost, but are nevertheless priced out of the market. See Bajari and Benkard (2003) for more implications from the horizontal discrete choice model.

all zero. We now suppress individual subscripts i as all differences are based on a and y. The model yields vertical oligopoly as in Shaked and Sutton (1983). We assume utility is log-supermodular in renter and parcel type:

$$F(a, y - r, y) = F_1(a, y) \cdot F_2(y - r), \tag{3.4}$$

where function F_1 is log-supermodular in a and y, and F_2 is an increasing function of consumption (equivalent to income minus rent). Landowners set rents according to individual willingness to pay (WTP). Because $(\mathrm{d}F_1/\mathrm{d}a) > 0$, it's clear that all else equal, all types prefer higher a locations, and therefore that $(\mathrm{d}r_a/\mathrm{d}a) > 0$. Moreover, conditional on rents at other locations, different types y will have different WTP for a given parcel of type a. WTP of type y for location a is

$$WTP(y, a) = \min_{\forall b \in \mathcal{A} \setminus a} F_1(a, y) \cdot F_2(y - r_a) - F_1(b, y) \cdot F_2(y - r_b). \tag{3.5}$$

The equilibrium is given by a set of rents r_a and cutoffs $y_1, ..., y_{N-1}$. Between any cutoff y_{a-1} and y_a, the willingness to pay of individuals assigned to location a is heterogeneous and single-peaked in type y at some $y_{a,peak} \in [y_{a-1}, y_a]$. In other words, increasing complementarity acts within assignments of continuous types to the discrete number of parcels to create variation in WTP.

The landowner pricing rule chooses q, and effectively y_{a-1} and y_a such that

$$r_a - mc_a(q) = -\frac{G_2(y_a) - G_2(y_{a-1})}{g_2(y_a)\frac{dy_a}{dr_a} - g_2(y_{a-1})\frac{dy_{a-1}}{dr_a}}. \tag{3.6}$$

Note that $\frac{dy_a}{dr_a} < 0$, $\frac{dy_{a-1}}{dr_a} > 0$, and therefore markups are positive. As landowners adjust rent r_a upwards, they lose renters on two margins, the lowest-type assigned to their parcel, y_a, who flee to the cheaper next-best option $a - 1$, and those near the top of the distribution at their location that spend more for the option $a + 1$.

To close the model, the housing market must clear. Note that cutoffs are continuous, and for any y_1, if y_1 chooses a parcel in the city all $y > y_1$ do as well. If WTP is negative for the lowest type $\underline{y}$ at the lowest location $\underline{a}$, some mass of types will choose the outside option. A parcel is unoccupied if $y_a = y_{a-1}$.

3.3 Policy Implications: Theory

In this section, we assess the effects of several policies in the context of monopoly markups. First, we discuss the impact of zoning. We show that in the horizontal case, zoning raises rents of parcels that are *not* constrained by zoning, even when marginal costs are constant. Second, we discuss how, under non-decreasing marginal costs, concentration of land ownership raises markups and rents at all parcels. We conclude by discussing the scope for analysis of monopoly power in several other urban policies. Appendix C.1 presents proofs of our propositions.

3.3.1 Old Policies, New Implications

An immediate implication of the above model is that, even in the absence of spillovers, a policy of no zoning is not first-best. Because a monopolist landowner restricts quantity, the quantity difference between zoning-restricted and an identical, unrestricted parcel with a monopolist landowner is less than the difference between zoning-restricted parcels and a competitively priced parcel. Height minimums could reduce rents.

What happens when zoning constraints are not binding everywhere? To the extent that zoning constrains bind at a particular parcel, the quantity must be restricted beyond the monopoly-optimal quantity, and rents as a result must be higher. However, in a city where only some parcels are constrained by zoning rules, those constraints also impact rents at unzoned parcels by affecting equilibrium monopoly power at unconstrained parcels. In both the vertical and horizontal case, the rent at a given parcel is inversely proportional to rents at other parcels. When we restrict ourselves to the horizontal case, we can state the following:

Proposition 1. *With logit demand and non-decreasing marginal cost, all else equal, the imposition of binding zoning constraints on a given parcel increases the rent at all other parcels, including unzoned parcels and parcels where zoning constraints do not bind. When marginal cost is constant, markups at those parcels go up as well.*

Appendix C.1 presents a proof. By raising rents at competing locations, binding zoning constrains have spillover effects on rents at policy-unconstrained locations through monopoly pricing. Likewise, relaxing zoning constraints at one parcel brings down rents everywhere. Of course, even when units are priced competitively, if marginal costs are increasing, by limiting supply at one location, zoning can impact rents and quantities at other locations. But Proposition 1 points out that monopoly power exacerbates the price effects by changing optimal markups. In other words, even in a world of constant marginal costs, zoning constraints at one parcel would raise rents at all other parcels in the city by increasing monopoly markups. This effect operates through the cross-price elasticities, which, in the multinomial logit case can be signed and compared across any equilibria. In Section 3.8, we assess the empirical magnitude of this force by considering a marginal, across the board loosening of zoning constraints in Manhattan.

3.3.2 New Policies, New Implications

Under monopoly pricing, higher rents can generate a positive pecuniary externality on other landowners, and, by increasing demand and affecting elasticity, monopoly markups at one parcel may positively impact markups, rents, and profits at other locations. When landowners own multiple parcels, they internalize these pecuniary externalities, which may result in higher markups and rents overall. Intuitively, monopolists with greater market share may reduce quantity to a greater extent in order to maximize total profits.

In general, however, the impact of changes in land ownership concentration is analogous to mergers in the multi-product oligopoly setting. As in that setting, we cannot make statements on the effects of concentration on the equilibrium without additional assumptions. We extend Nocke and Schutz (2018b) to generate the following proposition:

Proposition 2. *With logit demand and non-decreasing marginal cost, all else equal, landowners with higher market share have higher markups and rents; an increase in the ownership share of one landowner will generate increases in markups and rents at all the landowner's parcels, and increases in rents at all other parcels.*

Because we cannot assume marginal cost is constant, we introduce an even more flexible cost function than those found in Nocke and Schutz (2018b,a). That, in turn, requires an extension to the result on the relationship between own share and others' share on markup and rent. Appendix C.1 provides a proof.

Note that Proposition 2 is only guaranteed to hold when we can exclude the possibilities of scale economies and when there are no systematic variations in individual valuations by individual characteristics; i.e., no sorting. Intuitively, if landowners can raise profits by forcing more individuals into one parcel, generating scale, or if they can affect the sorting equilibrium through manipulations to the rents of multiple parcels, they may find it optimal to reduce, rather than increase rents and markups.

An important implication of this result is that manipulating the ownership structure of parcels affects rents through monopoly pricing. In particular, under specific conditions, reducing ownership concentration will reduce rents. In Section 3.5, we look for evidence of scope for such policies in our New York City dataset.

3.3.3 Additional Policies

We close our policy discussion by briefly and informally discussing the potential interactions of monopoly pricing with three other urban policies: rent regulation, inclusionary zoning, and use laws.

Where previously introduced into the housing literature, the concept of monopoly power among landowners has been used to advocate for rent regulation. The intuition is that reducing rents in the presence of monopoly markups can achieve the efficient equilibrium. By contrast, Diamond et al. (2019), who do not explore monopoly markups, show that rent controls generate an extensive margin impact. While it is beyond the scope of this paper to discuss exit and entry, Appendix B shows how monopoly markups are capitalized into land rents and could impact such decisions.

In this context, inclusionary zoning policies, which mandate affordable housing be included in new developments, can be considered as a policy which moves monopoly quantities to efficient levels similarly to rent controls, but without reducing monopoly profit and therefore without affecting entry decisions.

Finally, we point out that zoning use laws may also operate on monopoly margins. While we only consider markups in a residential market, if demand elasticities vary between residential and commercial markets, use laws may reduce markups by constraining landowners to build in less profitable markets with more elastic demand.

3.4 Data

Sources Our main data are derived from public administrative building-level records, as well as scraped data, from several New York City departments, including the Departments of City Planning, Finance, and Housing Preservation & Development. Our primary dataset combines the Primary Land Use Tax Lot Output (PLUTO) and the Final Assessment Roll (FAR) for all buildings in NYC, as well as current and historic Multiple Dwellings Registration and Contacts (MDRC) datasets (with prior years graciously provided to us by the NYU Furman Center).[14] The PLUTO provides location, zoning, and building characteristics while the FAR provides market values, land values, and building ownership information.

We merge these with data derived from communications between the DOF and landowners, scraped off the Property Tax Public Access web portal, which we call the Notice of Property Value (NPV) dataset. It includes information mailed to building owners including gross revenue and cost estimates and the number of rent stabilized units.[15]

We use the 2010 Decennial Census to allocate rental households to buildings to estimate building vacancies.[16] To determine the size of the rental market, we use the total number of NYC renter households that are in buildings with four or more units.[17]

[14]The MDRC links building owners to shareholders revealing common ownership across buildings.

[15]The NPV dataset was originally web-scraped by a third-party from the DOF's Property Tax Public Access web portal. Full details about this process are available at `http://blog.johnkrauss.com/where-is-decontrol/`.

[16]To allocate rental households, we multiply building residential units by the block level rental occupancy rate. This method assumes that vacancy rates are uniform within Census blocks.

[17]The 2010 Census reports the number of renter households but not stratified by building units, so we scale the 2010 Census value by the ratio of renters in 4+ unit buildings to all renters from the 2010 ACS.

Sample Our data spans from 2008 to 2015. We use all years when analyzing ownership concentration but focus on 2010 for demand estimation.

For demand estimation, we use all private buildings classified as multi-family rental buildings in Manhattan with four or more units, where all units are residential units and there is no missing data. When we construct the instruments based on rival building characteristics, detailed in Section 3.6.2, we expand the sample to include mixed-use, residential rental buildings. We exclude mixed-use buildings in the estimation because we cannot separate building income due to residential versus commercial tenant sources.[18]

For analyzing rents and ownership concentration, we use a subset of our estimation sample that excludes buildings that are zoning constrained or rent stabilized, which we call the unconstrained sample.[19] For the ownership concentration results, we additionally drop buildings where the listed building owner in the FAR data did not match the MDRC data and buildings with less than six units.[20] For more details, see Appendix C.3.

For computation and expositional purposes, our main analysis focuses on buildings in Manhattan. For additional power and robustness, we expand our sample to include buildings from Brooklyn, the Bronx, and Queens; we exclude Staten Island due to relatively small number of multi-unit rental buildings.

Geographic Units We use Census tracts as a unit of observation for ownership concentration as well as for nests in one specification of our elasticity estimation. The large number of tracts provides us greater variation in the data. In addition, as discussed in Appendix C.4, ownership concentration is more easily calculated at the tract level, a feature which will help us in Section 3.5. An obvious downside to this choice is that markets are likely geographically continuous. Individuals at the

[18]In the context of our demand model discussed later, we do *not* push mixed-use buildings to the 'outside' good; instead, we simply do not include them in the estimation.

[19]Specifically, a building is zoning constrained if the building would not be allowed to create an additional unit based on building floor-area-ratios and minimum unit area requirements, and is rent stabilized if more than 10% of units are rent stabilized.

[20]We are able to match over 80% of all building owners across years. We drop buildings with $\{4, 5\}$ units due to NYC assessment methodology changes for these buildings.

borders of tracts are more likely to search at adjacent tracts than in other neighborhoods. The nested logit structure we adopt will not fully capture this, nor will our concentration measures, which will likely attenuate results. We also use an NYC specific geography, Neighborhood Tabulation Areas (NTAs), that is a sub-county collection of Census tracts.

Building Rental Income For 80% of our multi-year sample, we use scraped data from communications between the city and landowners about building income. For the rest of our sample, we rely on public data on assessments records from the DOF, which include methodologies for generating assessments from building income, that allows us to back out income from the assessment data.[21]

In NYC, rental buildings are assessed based on their income generation. The DOF collects annual revenue and cost information for all rental buildings and then applies a statistical formula to translate annual revenue into 'market value' (MV) of the building if it were sold, which is the basis of a building's tax assessment. Importantly, MV is determined by a simple Gross Income Multiplier (GIM) formula:

$$\frac{\text{Market Value}_j}{\text{SQFT}_j} = \text{GIM}_j \cdot \frac{\text{Annual Revenue}_j}{\text{SQFT}_j}, \tag{3.7}$$

where the GIM is determined by the DOF based on actual sales in a given income decile range and location.[22] The DOF reports MV and SQFT for all buildings in the FAR dataset, and so for 80% of the sample we observe both income and MV. We non-parametrically estimate the GIM term as a function of MV/SQFT, borough, and year based on DOF guidance documents.[23] We assess our procedure by using the estimated GIM and reported MV to calculate a fitted income value for the matched sample, and find a correlation of 0.99 and coefficient of determination of 0.98. For more details, see Appendix C.5.

Once we have building income for all buildings, we must subset the data to single-use residential buildings due to our inability to distinguish between residential and commercial income. We divide

[21]See – `nyc.gov/site/finance/taxes/property-assessments.page`.

[22]Effectively, if a building's MV/SQFT is in the q^{th} quantile, then its Annual Rent by SQFT is also in that quantile, and all buildings in a given quantile and location will have the same GIM.

[23]For each borogh-year, we estimate the empirical GIM within 50 quantile bins of MV/SQFT (which we observe for all buildings) and then apply this to the unmatched buildings.

building income by the number of units for average annual unit rent in a building, and again by twelve for average monthly rent. A limitation of this approach is that we rely on building averages as we do not see individual unit income.

Other Variables We link buildings based on their "borough-block-lot" (BBL) identification that is uniquely assigned to real estate parcels, with additional verification based on lot characteristics.[24]

The building-level characteristics that we include are building age, log miles to the central business district (CBD, which we define as City Hall), log miles to nearest subway station, years since the last major building renovation, average unit square-feet, and whether the building has an elevator. We also measure the number of residential buildings, office buildings, retail buildings, and open parks in the Census block group. For location controls we include polynomials of building latitude and longitude coordinates and include location fixed effects.[25] We also use reported land value of parcels, which is constructed by the NYC DOF using a database of building and vacant parcel transactions.

An important limitation of our data is the inability to control for unit-level characteristics. We approach this issue as an omitted variables issue. In our analysis of concentration changes, building fixed effects will be an important control that, together with information on renovations, help us control for these unobservables. In our elasticity estimation, unobsevable unit characteristics will show up as building unobservables and will be an important motivation for our instrumental variable approach.

Summary Statistics Table 3.1 presents summary statistics for 2010 Manhattan rental buildings. Each column represents a cut of the data that we use. As explained above, the first is used for calculating our instruments, the second is used in our estimation, the third is the set of policy-unconstrained buildings—for which we can calculate markups, and the fourth is a subset of the policy-unconstrained buildings that are 10 years old or less in 2010. Figure 3.1 plots the total

[24]Most parcels contain a single building, but large parcels can contain multiple buildings with open space between them. We refer to buildings and BBLs interchangeably throughout.

[25]We use Census tract FEs for the RCL and Neighborhood Tabulation Area FEs for the RCNL.

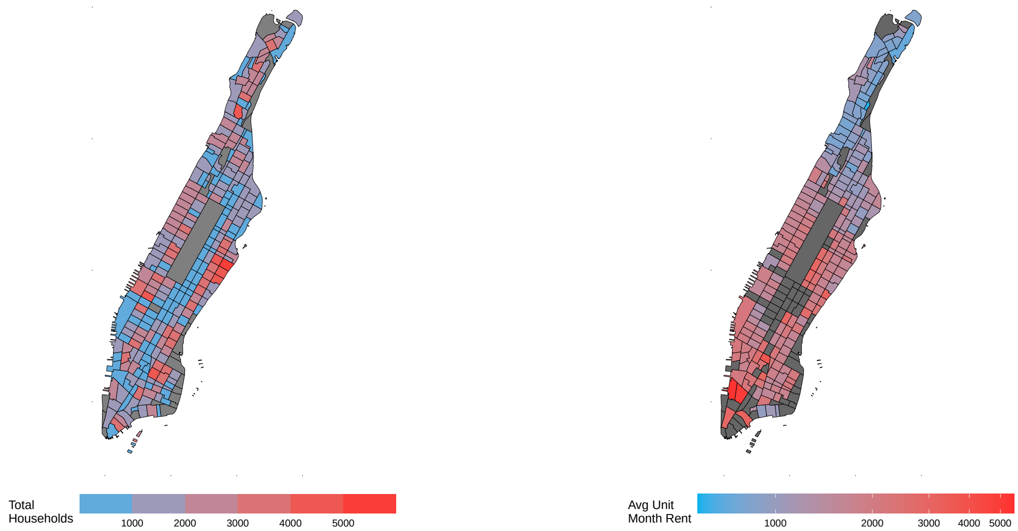
number of households and mean unit rents by Census tract. In Appendix C.3 we plot additional spatial distributions, such as zoning constraints and rent stabilization.

Table 3.1: Summary Stats:
2010 Manhattan Rental Buildings

	IV Sample	Estimation Sample	Unconstrained Sample	New, Unc. Sample
Total Market Share	26.5%	11.7%	0.7%	0.1%
Res.Units per Building	25.3	21.1	20.5	46.3
Households per Building	24.9	20.0	19.4	43.4
Vacancy Rate	5.4%	5.5%	5.7%	5.8%
Percent Mixed-Use	47%	0%	0%	0%
Percent Rent Stabilized	63%	60%	0%	0%
Percent Zoning Constrained	77%	80%	0%	0%
Median Monthly Rent*	–	$1,309	$2,071	$2,247
Median Rent by Median Income*	–	30%	48%	52%
Median Monthly Land Value per Unit	$2,989	$2,520	$5,314	$2,381
Years Since Construction	94	95	87	4
Years Since Renovation	48	48	35	4
log(Distance CBD)	1.34	1.58	1.45	1.32
log(Distance Subway)	-1.94	-1.89	-1.96	-1.72
Avg Unit Sqft	769	752	1,135.11	1,339
Buildings	17,828	9,484	566	53

Note: The table reports summary statistics for our main samples using Manhattan buildings with four or more residential units. The first column, IV Sample, includes mixed-use buildings. The second column, Estimation Sample, includes buildings with only residential units. The third column, Unconstrained Sample, includes buildings with no rent-stabilized units and which are able to add an additional unit according to zoning regulations. The New, Unconstrained Sample (last column) is hte subset of the Unconstrained Sample which were constructed between 2001-2010. Building data from PLUTO, NPV, and FAR files. Market share is the sum of total households in all buildings by large building total renter population in NYC. Households are allocated to buildings based on building units and 2010 Decennial Census and American Community Survey. The vacancy rate is one minus the total households in building divided by total building units. A building is mixed-use if the building has positive commercial area. A building is considered rent stabilized if more than 10% of units are rent stabilized. A building is zoning constrained if the building would not be allowed to create an additional unit based on building floor-area-ratios and minimum unit area requirements. Monthly rental income is building income divided by total units divided by 12. Median income in 2010 for NYC is $ 50,711. Monthly land value per unit is [Land Value / (12 x Residential Units)]. Years since construction and renovation equal 2010 minus the construction year and most recent major renovation year. Geodesic distances are in log miles based on building (lat,lon) coordinates. Avg Unit Sqft is total building area divided by total units. (*) – Rent data is only available for single use buildings

Figure 3.1: Distribution of 2010 Manhattan Renters & Rents



Note: The figure displays the geographic distribution of households and rent in the Manhattan data. The map on the left plots total renter households by Census tract in 2010. The map on the right displays the mean monthly unit rent by Census tract in 2010. Missing values are Census tracts where we have insufficient data, in part due to the exclusion of mixed-use buildings. Red tracts indicate higher households and rents respectively, using a log scale. Data from PLUTO, FAR, NPV, and 2010 Census.

3.5 Concentration and Rents in New York City

We now examine the correlation in the data between ownership concentration and rents. We note that results in this section are not causally identified. Nonetheless, we find, reassuringly and in line with predictions of Proposition 2, that increases in concentration are correlated with increases in rents.

To examine whether the data are consistent with the predictions of Proposition 2, we first construct ownership shares at the Census tract level from 2008 to 2015. Section 3.4 summarizes the trade offs of tract-level analysis, as well as our construction of tract-level ownership data, in tandem with Appendix C.4. As noted in Section 3.4, we calculate concentration, which will be a Herfindahl-Hirschman Index (HHI), off of the full sample of buildings in each year but for rents, our outcome variable, we restrict our sample here to unconstrained buildings with matched ownership information. Note that our sample differs from our estimation sample in Table 3.1 because we pool eight years of data and only use buildings with six or more units in each year.[26] Summary statistics for this sample are available in Table C.1 in Appendix C.3. We begin with our main geography, Manhattan, and then extend the sample to equivalent buildings in the whole of New York City to improve power.

Using our constructions of ownership, we calculate tract-level concentration. Let $\mathcal{A}_{f,g,t}$ be the set of buildings owned by landowner f in tract g in time period t, and let $\mathsf{F}_{g,t}$ be the set of landowners in that tract and time. We thus calculate landowner market shares as:

$$s^{f}_{g,t} := \frac{\left(\sum_{j\in\mathcal{A}_{f,g,t}} D_{j,t}\right)}{\sum_{f'\in\mathsf{F}_{g,t}}\left(\sum_{j\in\mathcal{A}_{f',g,t}} D_{j,t}\right)}. \tag{3.8}$$

Figure 3.2, plots tract-level HHI measures for Manhattan, where HHI is the sum of squared owners' shares, $\mathsf{HHI}_{g,t} := \sum_{f'\in\mathsf{F}_{g,t}}\left(s^{f'}_{g,t}\right)^2$.

To more closely match the predictions of Proposition 2, which links ownership concentration elsewhere to rents, we construct a modified "leave-out" HHI index. For each landowner f, we recalculate the market share of a rival landowner, h, as:

$$\tilde{s}^{h}_{f,g,t} := \frac{\left(\sum_{j\in\mathcal{A}_{h,g,t}} D_{j}\right)}{\sum_{f'\in\mathsf{F}^{\neg f}_{g,t}}\left(\sum_{j\in\mathcal{A}_{f',g,t}} D_{j}\right)}, \tag{3.9}$$

where $\mathsf{F}^{\neg f}_{g,t}$ is the set of rivals to landowner f, and then calculate the leave-out HHI for landowner f as the sum of these rival landowners' squared shares: $\mathsf{HHI}_{f(j),g,t} := \sum_{h\in\mathsf{F}^{\neg f}_{g,t}}\left(\tilde{s}^{h}_{f,g,t}\right)^2$.[27]

We then test the basic prediction that rent increases in concentration. Our main specification estimates

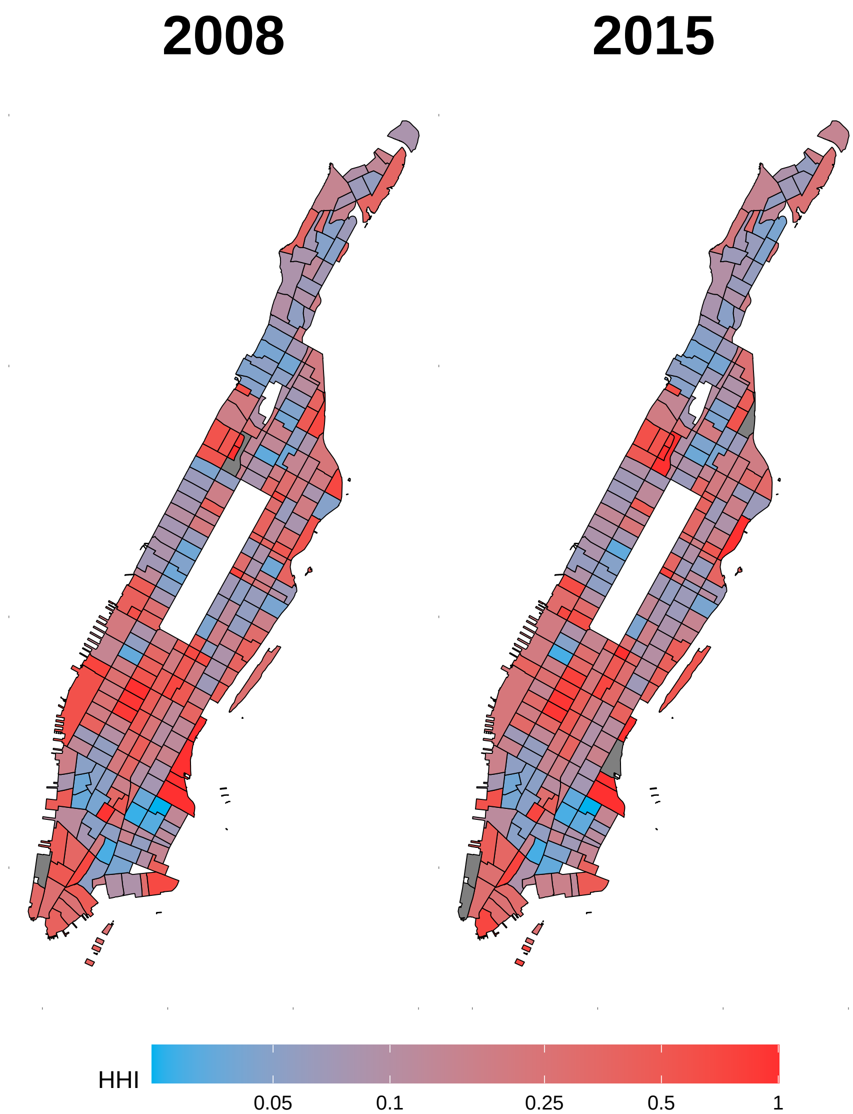
$$\ln[r_{j,g,t}] = \alpha_0 + \alpha_1 \cdot \ln[\mathsf{HHI}_{f(j),g,t}] + \alpha_2 \cdot X_{j,g,t} + \epsilon_{j,g,t}, \tag{3.10}$$

where $r_{j,g,t}$ is the average unit rent of building j in tract g at time t, $\mathsf{HHI}_{f(j),g,t}$ is described above, and α_2 is a vector of coefficients on controls $X_{j,g,t}$. We also include $\ln[s^{f(j)}_{g,t}]$ in some specifications

[26] In Section 3.7 our results use rental income for buildings with four or five units. These are obtained using DOF assessment procedures linking reported market values to rental income. We cannot use these here because of assessment procedures changes over the course of this panel for this group.

[27] In Appendix C.4, we probe robustness using the more standard construction of HHI and shares in Equation (3.8).

Figure 3.2: Distribution of Ownership Concentration in Manhattan



Note: The figure plots the tract-level ownership concentration index $\mathsf{HHI}_{g,t}$ in 2008 (left map) and 2015 (right map) on log scales. Reds indicate more concentration. FTC Horizontal Merger Guidelines consider values above 0.25 to be highly concentrated. Sample is all residential buildings with 4+ units in Manhattan. Data from PLUTO, MDRC.

to separately test for the impact of owners' shares on rents at their own buildings. Note that while we use general subscripts $\{j, g, t\}$ for $X_{j,g,t}$, in specific specifications some controls will be time variant, e.g., when using building fixed effects.

Column (1) of Table 3.2 Panel (A) estimates the specification in Equation (3.9) for Manhattan buildings using year fixed effects, building age, square of building age, the log of distance to nearest subway and the log of distance to the CBD, average square feet of living space per unit, and years since last renovation. The inclusion of year fixed effects treats the data as a repeated cross section, and suffers from clear unobserved variable bias. We refrain from interpreting the small and insignificant resulting coefficient on $\mathsf{HHI}_{f,g,t}$.

In Column (2) of Panel (A), we add tract fixed effects. Here, identifying variation is changes over the course of the panel at the tract level, removing unobserved time-invariant tract-level variation. A 10% increase in tract concentration index is associated with a 1.6% increase in rents. The significant coefficient is consistent with Proposition 2: buildings in tracts where ownership elsewhere in the tract is concentrating experience larger increases in rents.

Column (3) of Panel (A), our most stringent specification, further imposes building fixed effects. Here, building time-consistent controls drop, though years since renovation is an important control that remains. Because of the difficulty in observing key building characteristics, this specification ensures that of Column (2) is not identified off of unobserved differences in building quality. The coefficient is positive but insignificant – a motivation for our inclusion of more data in Panel (B) below.

Finally, Columns (4)-(6) introduce controls for building owners' own share of the tract as a control. According to Proposition 2, we expect owners with growing shares and thus market power to increase rents. An important condition in the Proposition is that costs be non-decreasing, which would be violated if there were scale economies in ownership. Across specifications, the coefficient is small but noisy and inconclusive.

Because our most stringent specifications appear to lack power in Columns (3) and (6), we expand our sample to include three more boroughs: the Bronx, Brooklyn, and Queens (with too few observations per tract in Staten Island, we do not include it in our sample). Here, coefficients are generally in the same direction, and in particular, the coefficients on tract HHI in Columns (3) and (6) are now positive, significant, and economically meaningful, with a 10% increase in concentration again associated with a roughly 1% increase in rents.

An important caveat in this analysis is the inability to observe changing tract conditions that are correlated with both rents and ownership concentration. Tracts with improving overall neighborhood qualities may experience rising rents and rising ownership concentration in tandem. We therefore caution against interpreting these coefficients causally, but instead take reassurance from the stylized fact that increases in concentration are correlated with increases in rents. We use this stylized fact as motivation for our identified estimation results.

3.6 Estimating Elasticities and Markups

To empirically assess the monopoly forces described above, we estimate the building-level demand elasticity for Manhattan rental buildings in 2010. We follow the literature empirically estimating differentiated product models with consumer heterogeneity based Berry et al. (1995) (BLP) and the citing literature.[28] Below, we describe our empirical model and identification strategy.

3.6.1 Renter Demand Econometric Model

As in our theoretical model, the urban rental market is made up of all individuals who will choose to live in a rental property.[29] In our main specification, we differentiate the choice set geographically, such that we consider all rental properties in Manhattan as 'inside' goods and all rental properties in the other boroughs as part of an 'outside' good.[30] We then probe robustness using NYC data

[28]In particular, we follow the methodological advice in Dubé et al. (2012); Knittel and Metaxoglou (2014); Gandhi and Houde (2018); Conlon and Gortmaker (2020).

[29]Our market definition may be better stated as *large* rental properties as we only consider rental properties with four or more units.

[30]This is analogous of comparing utility from a Manhattan property to the average non-Manhattan property for each individual renter.

from four boroughs as separate markets, where the outside goods are smaller buildings in the same borough.

We estimate two versions of our model. Closest to our exposition in Section 3.2, we estimate a random coefficients logit (RCL) model. Second, we estimate a random coefficient *nested* logit (RCNL) model where nests are Census tracts, which by necessity remove our most stringent location controls—tract-level fixed effects—due to collinearity with our definition of building nests. The RCL model is simpler to estimate and allows greater location controls; however, the RCNL model allows for within-nest preference correlation with nearby buildings at the expense of less robust location controls.

We assume that renter i's utility from choosing unit j is composed of a common vertical differentiation component, μ, and idiosyncratic horizontal components, $\{\psi, \epsilon\}$:

$$U_{ij} = \mu_j + \psi_{ij} + \epsilon_{ij} := \underbrace{\delta_j + X_j\beta}_{\mu_j} + \underbrace{\frac{\alpha}{y_i} r_j + \sum_{h \in H_2} \{\gamma_h v_{ih} x_{jh}\}}_{\psi_{ij}} + \epsilon_{ij}. \tag{3.11}$$

Equation (3.11) parameterizes utility as a function of renter income, y, observed covariates and rent, $\{X, r\}$, a scalar unobservable amenity, δ, and covariate-specific taste shifters, v_h. For ease of notation, we express the joint distribution of renter incomes and tastes, $\theta = (y, \{v_h\})$, conditional on observed variables,(X, r), as $F(\theta)$, which we will define empirically when we describe our estimation routine.

For our empirical specifications, building covariates in X include a constant, age, years since last renovation, log distance to CBD, log distance to nearest subway, avgerage unit square feet, and the location controls mentioned in Section 3.4, including Census tract FEs for the RCL and NTA FEs for the RCNL models.[31]

We calculate a building's market demand, D_j, as the aggregation of individual renter demands, d_{ij}. Under the assumption that ϵ_{ij} is distributed Type 1 Extreme Value, the RCL model implies an

[31]For the H_2 subset of covariates with random coefficients, we use a constant, age, years since renovation, log distance to CBD, log distance to nearest subway, and avg. unit square feet.

individual renter's building demand is calculated as:

$$d_{ij} = \frac{\mathsf{e}^{(\mu_j + \psi_{ij})}}{\sum\limits_{k \in \mathcal{A}} \mathsf{e}^{(\mu_k + \psi_{ik})}}. \tag{3.12}$$

Similarly, under the assumption that $\epsilon_{ij} = \left(\tilde{\epsilon}_{i,h(j)} + (1-\rho)\tilde{\epsilon}_{ijh}\right)$, where $\tilde{\epsilon}_{ijh}$ is distributed Type 1 Extreme Value, the RCNL model implies:

$$d_{ij} = d_{ij|h(j)} \cdot d_{i,h(j)} = \frac{\mathsf{e}^{((\mu_j + \psi_{ij})/(1-\rho))}}{\sum\limits_{k \in h(j)} \mathsf{e}^{((\mu_k + \psi_{ik})/(1-\rho))}} \cdot \frac{\sum\limits_{k \in h(j)} \mathsf{e}^{(\mu_k + \psi_{ik})}}{\sum\limits_{h \in \mathcal{H}} \sum\limits_{k \in h} \mathsf{e}^{(\mu_k + \psi_{ik})}}, \tag{3.13}$$

where $d_{ij|h}$ is the within-nest building demand and $d_{i,h}$ is the nest demand. The random variable $\tilde{\epsilon}_{i,h}$ introduces taste variation across nests and ρ governs preference correlations within nests.[32]

3.6.2 Identification and Instruments

There are two endogenous variables for every observation: market share and rent.[33] Our estimation strategy allows us to identify demand parameters while being agnostic to the supply side of the market. While we observe some building amenities directly, rents are likely correlated with unobserved amenities, δ_j. Broadly, these unobervables may either be about buildings' amenities or area amenities not in our data. To identify α, we require an instrument $Z^{(r)}$ that shifts rent but is unrelated to these amenities. To identify the γ coefficients, we require instruments that shift the substitution patterns between products, $Z^{(x)}$. With instruments, $Z = (X, Z^{(x)}, Z^{(r)})$, the identifying moment condition is

$$\mathsf{E}[\delta(X, r, s; \theta) \mid Z] = 0, \tag{3.14}$$

which leads to our use of $\mathsf{E}[Z'\delta]$ as the empirical moment we wish to minimize.

[32]The parameter is defined over the interval $\rho \in [0, 1)$, where $\rho = 0$ collapses to the RCL model and $\rho = 1$ is inconsistent with utility maximization. The r.v. $\tilde{\epsilon}_{i,h}$ is integrated out, but could be included at the expense of increasing the number of non-linear parameters.

[33]We assume that the building-level characteristics are exogenous and can additionally serve as instruments. For a rigorous discussion of identification, see Berry and Haile (2014, 2016).

We construct $Z^{(x)}$ using functions of rival building characteristics. When creating the rival set $K(j)$, we exclude rivals within a 1km radius of a given building, based on Bayer et al. (2004) and Bayer et al. (2007)[34] For the RCNL model, we also create 'local rivals' who are in the same tract (i.e., nest) but not in the same block group. We use "Quadratic Differentiation Instruments" based on Gandhi and Houde (2018). These are a finite order basis function approximation of the optimal instruments in the sense of Amemiya (1977) and Chamberlain (1987). For a given covariate h for building j with rivals $K(j)$, each instrument is defined as:

$$Z_{hj}^{\text{DQ}} = \sum_{k \in \{K(j)\}} (x_{hk} - x_{hj})^2. \tag{3.15}$$

For $Z^{(r)}$, we use the land value of the building parcel; i.e., the market value of vacant land where the building is located, which captures the opportunity cost of the landowner for renting the space out. The exclusion restriction is violated if constructed land value from sales around the city are correlated with unobservable amenities at the building-level, conditional on building observables and location controls. While actual land value in general may be correlated with nearby building characteristics, our measure is constructed by NYC DOF using sales of similar parcels which are not necessarily close. Furthermore, we control directly for location observables (which include tract fixed effects in our RC Logit specification), and as such the residual measure should not be systematically correlated with local building-level unobservable residential amenities. Appendix C.7 describes further details on instrument construction and other aspects of estimation.

Armstrong (2016) discusses the asymptotics of differentiated product estimation when there are few markets and many products and provides sufficient conditions such that markup converges to a constant. If markups converge to a constant 'faster' than the instrumental variables estimator, then the latter is inconsistent because there is not variation in markups to use.

We address with this issue in three ways. First, our RCNL specification follows Armstrong (2016) by splitting products into nests. Armstrong (2016) shows in this setting that nests effectively bound the number of rival products within in a nest, so neither within-nest shares, $D_{j|h(j)}$, nor

[34]The authors use rings of five and three miles, respectively for their instrument construction using homes in the San Francisco bay area.

markups converge to a constant even if the total number of products in the full market goes to infinity.[35] Second, we use a marginal cost shifter in $Z^{(r)}$ that is valid even if the conditions of Armstrong (2016) hold, as that variation is not due to markups. Third, we perform statistical tests for under-identification of the "BLP instruments" on the model implied markups (Armstrong, 2014), and we also calculate a robust first stage F statistic from a linear regression of the endogenous rents on the instrument vector $(Z^{(x)}, Z^{(r)})$, advocated in Armstrong (2016).

3.6.3 Estimating Markups in the Presence of Supply-Side Restrictions

While our elasticity estimation is agnostic to the supply side of the market, to derive markups from estimated demand elasticities, we must account for how landowners set rents and quantities in our setting. In particular, our model assumed landowners are (policy-)unconstrained in their ability to set rents by adjusting supply. Two features of our setting are particularly problematic for this assumption: rent and quantity constraints (through rent stabilization and zoning), and constraints on quantity adjustments due to fixed redevelopment costs and the durability of the housing stock.

In particular, constraints on rent in the form of rent control and rent stabilization, and constraints on supply in the form of zoning restrictions mean that the observed pricing and quantity behavior of a constrained landowner will not be reflective of optimally chosen prices and quantities. In addition, the markups in our model do not account for lumpy redevelopment or the durability of the housing stock. Appendix C.2 shows how our model can be extended to a model with separated developer and landowner quantity and price decisions, but clarifies that monopolist quantities, and thus the ability to derive markups from the price elasticity, are only achieved when developers correctly anticipate the demand faced by landowners. In reality, fixed costs may delay redevelopment and the durability of the housing stock means that current quantities may not reflect current demand.

We approach these limitations by subsetting our data twice. First, our main results derive markups only for policy-unconstrained parcels, which could raise rents and adjust quantities unencumbered by zoning constraints or rent regulation. Second, we separately examine the 53

[35] The median number of buildings per nest is 32, the average is 43, and the maximum is 195.

policy-unconstrained buildings that were built in the last 10 years of our 2010 data, where developers will have been more likely to have correctly anticipated contemporary demand and set monopolist-optimal quantities, according to Appendix C.2.

With those restrictions in mind, we turn to our markup calculation.

3.6.4 Elasticities and Markup Calculations

Using estimated parameters, θ, we can calculate building-level elasticities and markups that will inform our understanding of monopoly power in the Manhattan market. We calculate the building-level demand elasticities using the analytical derivatives of the demand functions, and we calculate the percent markup assuming landowners solve a Bertrand price competition game:

$$\varepsilon_j = \frac{\partial D_j}{\partial r_j}\frac{r_j}{D_j} = \begin{cases} \left[\int_i \left(\frac{\alpha}{y_i}\right) d_{ij}(1-d_{ij})\mathrm{d}F(\theta)\right]\frac{r_j}{D_j} & \text{if RCL} \\ \left[\int_i \left(\frac{\alpha/y_i}{1-\rho}\right) d_{ij}\left(1-\rho d_{ij|h(j)}-(1-\rho)d_{ij}\right)\mathrm{d}F(\theta)\right]\frac{r_j}{D_j} & \text{if RCNL} \end{cases} \tag{3.16}$$

$$\mathsf{Lerner}_j = \frac{r_j - mc_j}{r_j} = \left(\frac{-1}{\varepsilon_j}\right) \tag{3.17}$$

Again, we use Bertrand pricing only for interpretation but not estimation.

Most housing demand literature estimates inelastic demand seemingly incompatible with monopoly pricing (Chen et al., 2011; Albouy et al., 2016). We reconcile this by the fact that the relevant elasticity for landowners is the own-price elasticity, ε_j, rather than the "aggregate elasticity," the change in total housing consumed with a change in (aggregate) rents. To connect our setting to previous housing demand estimates, we calculate the aggregate elasticity which provides the responsiveness of renters to a 1% increase in rent for all 'inside' buildings (Berry and Jia, 2010; Conlon and Gortmaker, 2019):

$$\varepsilon^{\text{Agg}} = \sum_{k\in\mathcal{A}} \left.\frac{D_j(\{r_k+\Delta r_k\}_{k\in\mathcal{J}}) - D_j}{\Delta}\right|_{\Delta=1\%}. \tag{3.18}$$

Foreshadowing results, we will find both monopoly-consistent elasticities ε_j as well as literature-consistent inealistic aggregate elasticity ε^{Agg}.

3.6.5 Estimation Routine

Here we briefly describe our estimation algorithm. We are guided by methodological reviews (Nevo, 2000; Knittel and Metaxoglou, 2014; Conlon and Gortmaker, 2020) and point interested readers to Appendices C.6, C.7, and C.8 for additional details.

We estimate the econometric model using market-level variables on building choice shares, rents, and characteristics, $\{D_j, r_j, X_j\}$. We simulate R renters by drawing $(y_i, \vec{v}_i)$ to calculate the individual demands, and then use pseudo Monte Carlo integration to calculate market demand.[36]

Estimation has four steps, which are iterated until parameters converge.[37] `First`, a non-linear inversion step finds mean product utility, μ, given an initial set of non-linear parameters, $\varphi = (\alpha, \gamma, \rho)$.[38] `Second`, we use linear GMM to estimate mean utility parameters, β, which identify the unobserved mean utility characteristic, δ. `Third`, we use a non-linear minimization routine to estimate the non-linear parameters using the moment condition $\mathsf{E}[Z' \cdot \delta]$. `Fourth`, we update the weight matrix using the residuals from Step 3, and repeat until the parameter vector converges, $\|\varphi^{s+1} - \varphi^s\| \approx 0$.

3.7 Estimation Results

In this section, we report our main results for Manhattan and as a robustness check a similar model using Manhattan, the Bronx, Brooklyn, and Queens as four separate markets.

3.7.1 Results using Manhattan

Table 3.3 presents our main empirical results for Manhattan. We estimate utility parameters based on our empirical model, then calculate building-level elasticities. For the unconstrained subset

[36]We use Halton sequences to approximate uniform random draws. Income is simulated by using a log normal distribution with mean and variance based on the ACS 2010 file.

[37]In finite samples the 2-Step parameters depend on the initial weight matrix and can be subject to greater misspecification errors, leading us to use an Iterated GMM approach (Hansen and Lee, 2019).

[38]For the inversion, we use a tolerance of $\|\mu_j^{r+1} - \mu_j^r\|_\infty < 10^{-12}$. See Appendix C.6 for more details.

of our sample as well as the "new" subsample, we then calculate the markup share of rent. We present both the Logit and Nested Logit models, both estimated via IGMM and using "Quadratic Differentiation Instruments," as described in Section 3.6.2. Of our estimated parameters, we only present our estimates of $\{\alpha, \rho\}$ and their heteroskedasticy robust standard errors. Using Equation (3.16) we calculate the own-price elasticity, Equation (3.17) the markup share or "Lerner index," and Equation (3.18) the aggregate elasticity.

The first four rows of Table 3.3 report our estimates of model parameters α and ρ, with standard errors in parentheses. Our estimates of the rent coefficient, $\hat{\alpha}$, are similar in magnitude between the models with roughly equal standard errors. Our estimate $\hat{\rho}$ is close but statistically different from zero implying only slightly greater within-nest correlation relative to the RCL model. For the full sample, we estimate median own-price elasticities of -2.99 and -3.16 for Logit and Nested Logit specifications, respectively. We calculate but do not interpret the Lerner index for this sample. The model implied building-level own-price elasticities are all elastic, which is consistent with monopoly pricing.

For the unconstrained buildings, the first subset for which we will find meaningful markup results, we find elasticities of -3.40 and -3.30, respectively. We expect these unconstrained landowners have the most control over their rents compared to landowners with rent-stabilized units or pressed against zoning constraints. For the second subset, "new" unconstrained buildings built between 2000-2010, we find elasticities of -3.48 and -3.31.

We find that the median markup share of total rent, the Lerner index, is between 32-33% of total rent for the full sample, with a slightly greater mean (33-35%). For the unconstrained samples the median and mean markup shares are between 29% and 31%. Among the new constructions subset of unconstrained, means and medians range from 29-32%. Overall, were units priced at the marginal cost reflective of the production and maintenance of buildings, we would expect rents to be about 70% of their current levels. Figure 3.3 plots the full distribution of the own-price elasticities and Lerner Index by building for all three samples and both the RC and RCNL models. All three samples of the nested logit model, drawn in thinner lines, are less dispersed. Figure 3.4

plots the mean own-price elasticity and the dollar value of markups in monthly rent by Census tract for the full sample only.

Again, we note that our results differ from the literature on the elasticity of housing demand. Our elasticity of interest is conceptually different than that targeted by that literature, which seeks to measure the substitution between quantity of housing and consumption. In that literature, housing demand is typically found to be inelastic. When we estimate the aggregate elasticity in our data, which is more akin to the parameter estimated in the prior housing demand literature, we find similarly inelastic demand with an elasticity is between $(-0.14, -0.16)$. This estimate is slightly lower than the consensus range in the prior literature: $(-0.64, -0.3)$ (Albouy et al., 2016). This may be due to a differences in setting (Manhattan rental markets) or in methodology as our outside good includes other housing choices in NYC rather than pure consumption.

3.7.2 Results for Manhattan, the Bronx, Brooklyn, and Queens

In this subsection, we report results using all four NYC boroughs for which we have adequate data, using each as a separate market. Our estimation broadly follows that for Manhattan with some necessary changes. First, for computational reasons, we run 2-step rather than iterated GMM. Second, with four markets, we define the outside option as smaller 1-3 unit NYC buildings. As with Manhattan, we run both RC and RCNL models. Appendix C.9 provides more details on this robustness check and reports summary statistics by borough.

Table 3.4 reports the models' parameter estimates and Table 3.5 reports borough-level elasticities and markups. Again nearly all building elasticities are estimated as being consistent with monopoly pricing. Average elasticities and markups for Manhattan are in line with those reported in Section 3.7.1. Markups in other boroughs vary between 20-30%.

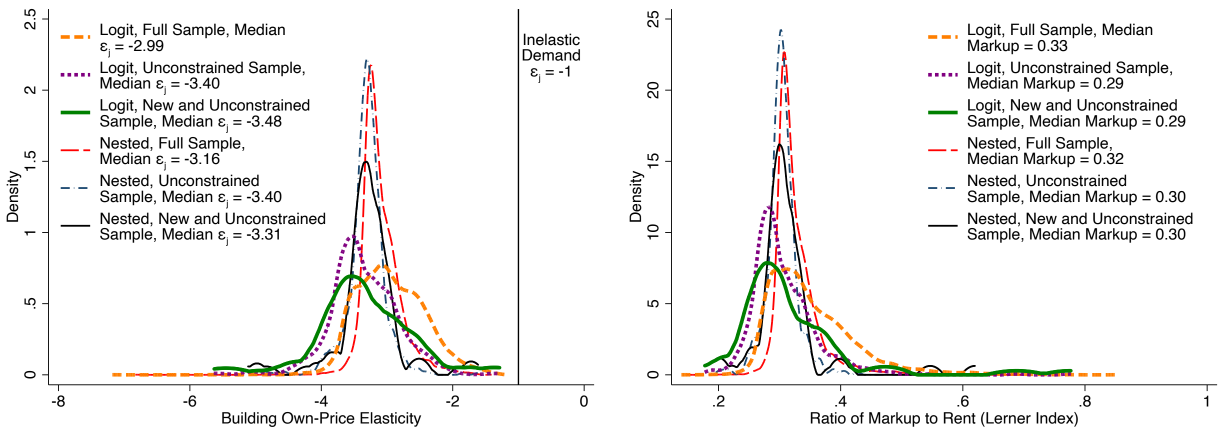
3.8 Up-Zoning's Spillover Effects Through Monopoly Power

In this section, we use our data and the results of our model to quantify the potential effects loosening zoning restriction. In our setting, additional competition from up-zoning puts downward

Figure 3.3: Distribution of Results

(a) Own-Price Elasticity

(b) Markup as a Percent of Rent



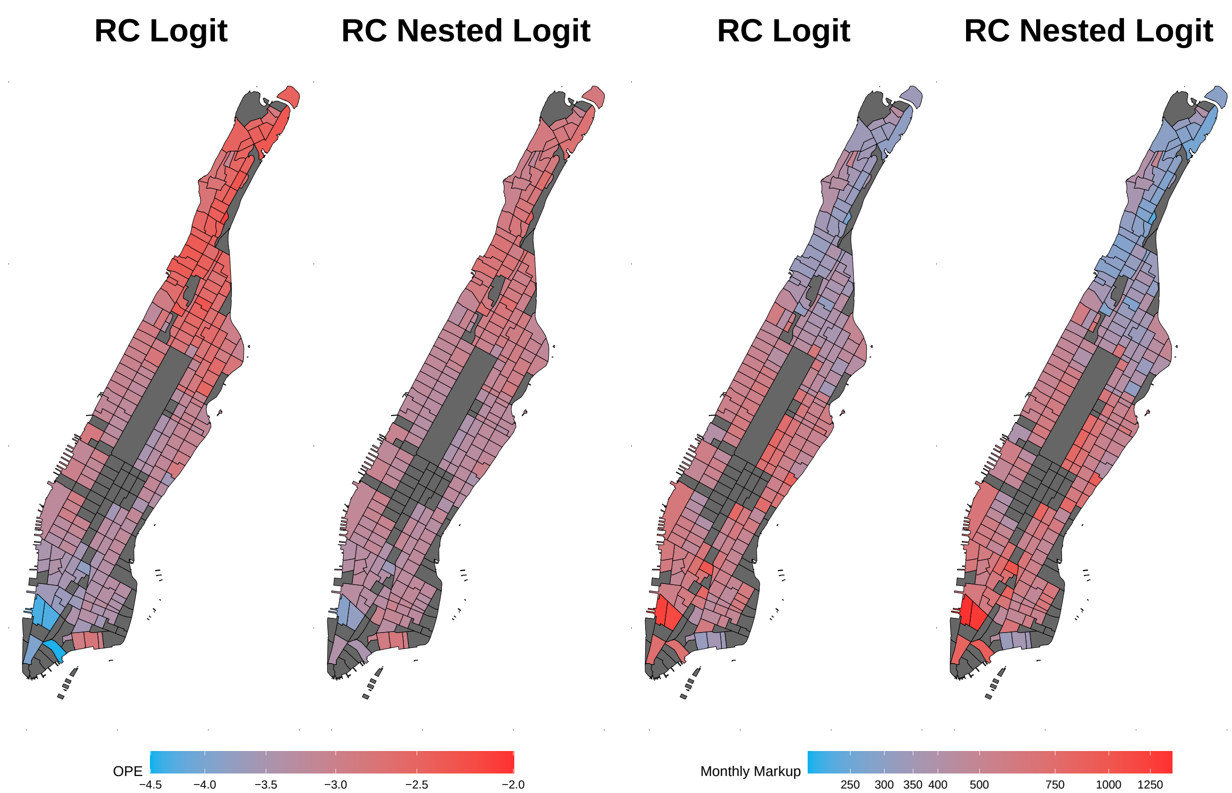
Note: The figure plots the kernel density plot of own-price elasticities (Panel (a)) and markups (Panel (b), Lerner Index), for main results using Manhattan buildings. Thin lines plot results from Random Coefficient Nested Logit model. Thicker lines plot results from Random Coefficient model. Orange dashed and red long-dashed lines plot elasticities and markups for the full sample. Purple short-dashed and navy dot-dashed lines plot results for the unconstrained sample. Green and black solid lines plot results for the new and unconstrained sample. Results based on Table 3.3. The full sample is comprised of all Manhattan single-use residential buildings with four or more units. The unconstrained sample is comprised of all buildings in the full sample that are not zoning constrained and where units are not rent stabilized. The new and unconstrained sample is the subset of the unconstrained sample for which buildings are 10 years old or less. The vertical line in Panel (a) indicates elasticities greater than -1, which would be incompatible with monopolistic pricing. RCL and RCNL models and estimation are described in the text.

pressure on the rents of policy-unconstrained buildings. By contrast, were policy-unconstrained buildings to be priced at marginal cost (e.g., if there was no markup in rent), then we would not expect a loosening of zoning constraints in *other* buildings to affect rents of already unconstrained buildings, excepting changes in marginal cost.

To illustrate and quantify the rent effect of up-zoning constrained buildings on policy-unconstrained buildings, we use the model-estimated elasticities to examine the effect of a marginal change in zoning in the form of a 1% across-the-board reduction in zoning quantity constraints. The price effect that we estimate is the change in monopoly markups for the 566 unconstrained buildings given a marginal reduction in zoning constraints for the set of 3,226 zoning-constrained, non-rent

Figure 3.4: Results for Manhattan

(a) Own-Price Elasticity (b) Monthly Markup in Rent



Note: The figure plots Census tract level average own-price elasticities in Panel (a) and monthly markups (Lerner Index) in Panel (b) for the RCL model (left) and the RCNL model (right). Reds indicate higher own-price elasticities and markups on a log scale. Results based are based on the Full Sample estimation presented in Table 3.3, which use all 2010 Manhattan single-use residential buildings with four or more units. Missing values are Census tracts where we have insufficient data, in part due to the exclusion of mixed-use buildings. RCL and RCNL models and estimation are described in the text.

regulated residential buildings.[39]

We consider a marginal change in constraints rather than a full counterfactual with changes in actual numbers of whole units for specific buildings. For example, a one-unit change for five-unit buildings is a 20% change in demand, and such non-marginal changes would require re-solving the monopolist problem. We also assume marginal cost is constant at unconstrained buildings. Increases in marginal costs would dampen the positive quantity and negative price effects we find.

[39]We exclude rent stabilized buildings where estimated own-price elasticities may not reflect rents of additional units on the margin.

In light of these constraints on our exercise, we view this exercise as an illustration of the interactions between zoning constraints and monopoly rents rather than a policy evaluation.

We implement the exercise as follows. `First`, we use the estimated own-price elasticities to calculate the percent change in rents required to increase the market share of all zoning-constrained buildings by 1%, $\{\%\Delta r_k^{\mathsf{cf}}\}_{k\in\mathcal{Z}}$. `Second`, we totally differentiate the monopoly pricing rule with respect to all rents and solve for a given unconstrained building's rent change, $\{\%\Delta r_j^{\mathsf{cf}}\}_{j\in\mathcal{U}}$. `Third`, we manipulate the solution for an elasticity representation that yields:

$$\%\Delta r_j^{\mathsf{cf}} = \sum_{k\in\{\mathcal{Z}\}} \left\{ \frac{\vartheta_k^j \cdot \%\Delta r_k^{\mathsf{cf}}}{\left(\varepsilon_j - \vartheta_j^j\right)} \right\}, \tag{3.19}$$

where ε is the own-price elasticity and $\vartheta_k^j = \frac{\partial \varepsilon_j}{\partial r_k}\frac{r_k}{\varepsilon_j}$. See Appendix C.10 for a complete derivation.

We also calculate the change in demand for unconstrained buildings from the price and quantity change at constrained buildings: $\%\Delta D_j^{\mathsf{cf}} = \varepsilon_j \%\Delta r_j^{\mathsf{cf}}$. This tells us the first order effects of the increased competition for residences on the overall quantity of space provided. Note that we exclude cross-price elasticities between policy-unconstrained buildings, as well as higher-order effects on all constrained plots. To the extent that these are negative, our estimates are a lower bound on the result.

Table 3.6 presents our results. We find that the RC Logit and RC Nested Logit yield roughly similar results in aggregate. A 1% loosening of zoning constraints for rival buildings leads to a mean markup *decrease* of \$7.41 and \$6.72 per unit for the RCL and RCNL models, respectively, on unconstrained buildings. These are over 10% of the first-order price effects on the directly-impacted units. We find small mean elasticities of −0.017 and −0.012, respectively. Loosening the zoning constraints by 1% would yield a direct increase of about 417 households and the spillover effects from increased competition would add 19 and 5 *additional* households through lower rents for the the RCL and RCNL models, respectively—a 0.16% and 0.04% increase at the unconstrained plots.

Altogether, we interpret these results as additional rationales for easing residential zoning restrictions. Without monopoly power, only changes in marginal cost would affect rent. The price effect we calculate represents *additional* downward pressure on rents that arises purely through the monopoly forces in the model. In addition, these results imply that at least part of the large equilibrium markups on unconstrained parcels we find in our estimation may be a result of spillovers from (the numerous) zoning-constrained parcels.

3.9 Conclusion

While previous housing and urban literatures have considered the scope for monopoly power, we believe we are the first to quantify its importance in urban rental markets, finding that its scope appears economically significant and policy relevant. We find that a 10% increase in Census tract level ownership concentration correlates to roughly a 1% increase in building rents, and that in Manhattan markups account for 30% of rents.

Second, we explore the link between monopoly pricing and urban policies, specifically zoning constraints. We show the theoretical link between zoning constraints and monopoly markups and quantify the relationship in our estimation, finding modest but appreciable spillover effects.

Lastly, we caution that an important aspect of the residential real estate market beyond the scope of this paper is the decision of landowners to enter and exit the market. We have highlighted the existence of monopoly pricing power and the complex interaction between that and urban policies. However, monopoly profits from renting, and thus urban policies affecting those profits, impact entry and exit decisions. Policies which impact those markups will likely impact the size of the rental market.

Table 3.2: The Relationship Between Ownership Concentration and Rent

	(1)	(2)	(3)	(4)	(5)	(6)
			$\ln[\text{Average r}_{j,g,t}]$			
			Panel (A): Manhattan			
$\ln[\text{HHI}_{f(j),g,t}]$	-0.012	0.161	0.075	0.009	0.162	0.075
	(0.032)	(0.080)	(0.076)	(0.038)	(0.076)	(0.076)
$\ln[s_{g,t}^{f(j)}]$				-0.028	0.002	-0.013
				(0.026)	(0.025)	(0.027)
Year FEs	Y	Y	Y	Y	Y	Y
Tract FEs	N	Y	N	N	Y	N
Building FEs	N	N	Y	N	N	Y
Observations	2,519	2,504	2,393	2,519	2,504	2,393
R^2	0.29	0.63	0.75	0.29	0.63	0.75
			Panel (B): Bronx, Brooklyn, Manhattan, Queens			
$\ln[\text{HHI}_{f(j),g,t}]$	0.047	0.122	0.102	0.043	0.128	0.095
	(0.016)	(0.056)	(0.037)	(0.018)	(0.053)	(0.037)
$\ln[s_{g,t}^{f(j)}]$				0.006	0.006	-0.027
				(0.013)	(0.012)	(0.014)
Borough-Year FEs	Y	N	N	Y	N	N
Tract and Year FEs	N	Y	N	N	Y	N
Building and Year FEs	N	N	Y	N	N	Y
Observations	13,651	13,576	12,743	13,651	13,576	12,743
R^2	0.40	0.64	0.77	0.40	0.64	0.77

Note: The table reports the results from regressions of log of building average unit monthly rent on the log of the 'leave-out' HHI index, calculated at the building level by leaving out the building owner's units. Regressions are at the building-year level and are weighted by building units. Columns (4)-(6) add log of building owner's market share as a control. The sample in Panel (A) are all matched, unconstrained buildings in Manhattan; Panel (B) expands the sample to all matched, unconstrained buildings in NYC. Columns (1) and (3) in Panel (A) use year / Panel (B) borough-year fixed effects, running a repeated cross-section. Columns (2) and (4) include tract and year fixed effects, running a panel at the tract level. Columns (3) and (6), our most stringent specifications, include building and year fixed effects, exploring variation in tract-level concentration while controlling for building-level, time-invariant differences. Building controls for all columns include building age, age squared, years since renovation, indicator if building has an elevator; for columns (1,2,4,5) log distance to CBD and log distance to closest subway (omitted in columns (3,6) due to building FEs. Standard errors in parentheses are clustered two ways by tract and year.

Table 3.3: Main Estimation Results: Manhattan

	RC Logit	RC Nested Logit
α	-43.79	-34.80
	(11.66)	(11.96)
ρ		0.065
		(0.037)
	Full Sample	
Mean ε_j	-2.95	-3.09
Median ε_j	-2.99	-3.16
Mean Lerner$_j$	35%	33%
Median Lerner$_j$	33%	32%
Percent $\varepsilon_j < -1$	100%	100%
ε^{Agg}	-0.16	-0.14
N	9,484	9,484
	Policy-Unconstrained Sample	
Mean ε_j	-3.36	-3.31
Median ε_j	-3.40	-3.30
Mean Lerner$_j$	31%	30%
Median Lerner$_j$	29%	30%
N	566	566
	New, Policy-Unconstrained Sample	
Mean ε_j	-3.35	-3.29
Median ε_j	-3.48	-3.31
Mean Lerner$_j$	32%	31%
Median Lerner$_j$	29%	30%
N	53	53
BLP F Stat	42.7	24.9
Linear F Stat	94.2	49.9
GMM Obj	10.3	36.3

Note: The table displays results from the Random Coefficient Logit (RCL) and Random Coefficient Nested Logit (RCNL) models using data on Manhattan multi-unit (four or more) residential buildings. Nests for RCNL are Census tracts. The coefficient α corresponds to the marginal utility of consumption and ρ governs within-nest preference correlations. Both models include random coefficients are on a constant, age, log distance to CBD, log distance to nearest subway, avg unit sqft. RCL uses Census tract fixed effects (FEs), and RCNL uses NYC NTA FEs plus additional location controls: measures residential buildings, commercial buildings, and parks in Census block-group and polynomials of latitude and longitude coordinates. Both models estimated using GMM and use "Quadratic Differentiation Instruments" based on Gandhi and Houde (2018), as described in Section 3.6.2. The own-price elasticity is ε_j, the Lerner index is $-1/\varepsilon_j$, and the aggregate price elasticity, ε^{Agg}, is based on Berry and Jia (2010). Buildings are 'unconstrained' if *not* rent stabilized and *not* zoning-constrained; new buildings were built after 2000. The Robust F statistics are from on regressions of building rent on building characteristics, location controls, and instruments. The BLP-F statistic tests identification of differentiation IVs for the RC model and is based on Armstrong (2014). Standard errors in parentheses are robust to heteroskedasticity.

Table 3.4: Model Parameter Estimates for Four NYC Boroughs

	RC Logit	RC Nested Logit
α	-27.80	-23.74
	(13.97)	(4.23)
ρ		0.069
		(0.043)
BLP F Stat	88.0	32.4
Linear F Stat	111.6	121.9

Note: The table presents results for the Random Coefficient Logit (RC Logit, RCL) and Random Coefficient Nested Logit (RC Nested Logit, RCNL) estimations using Manhattan, the Bronx, Queens, and Brooklyn as four separate markets. The coefficient α corresponds to the marginal utility of consumption and ρ governs within-nest preference correlations. Both models include random coefficients are on a constant, age, log distance to CBD, log distance to nearest subway, average unit square feet, and building controls described in the text. The RCL model uses Census Tract fixed effects (FEs) and the RCNL uses NYC NTA FEs and additional location controls described in the text. Both models use "Quadratic Differentiation Instruments" based on Gandhi and Houde (2018), as described in Section 3.6.2. Both models are estimated using Two-Step Efficient GMM due to computation constraints. The Robust F statistics are from on regressions of building rent on building characteristics, location controls, and instruments. The BLP-F statistic tests identification of differentiation IVs for the RC model and is based on Armstrong (2014). Standard errors robust to heteroskedasticity are in parentheses.

Table 3.5: Estimation Results: Four NYC Boroughs

	Manhattan		The Bronx		Brooklyn		Queens	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	RCL	RCNL	RCL	RCNL	RCL	RCNL	RCL	RCNL
				Full Sample				
Mean ε_j	-3.67	-3.41	-5.10	-4.67	-4.40	-4.08	-3.49	-3.28
Median ε_j	-3.76	-3.54	-5.17	-4.75	-4.50	-4.17	-3.54	-3.33
Mean Lerner$_j$	28%	30%	20%	21%	23%	25%	29%	31%
Median Lerner$_j$	27%	28%	19%	21%	22%	24%	29%	30%
Percent $\varepsilon_j < -1$	99.9%	99.9%	99.9%	99.9%	99.9%	99.9%	99.9%	99.9%
N	9,484	9,484	7,128	7,128	26,136	26,136	10,573	10,573
				Policy-Unconstrained Sample				
Mean ε_j	-3.75	-3.32	-4.94	-4.60	-4.27	-3.98	-3.51	-3.32
Median ε_j	-3.77	-3.36	-4.99	-4.67	-4.39	-4.07	-3.55	-3.36
Mean Lerner$_j$	27%	30%	20%	22%	24%	25%	29%	26%
Median Lerner$_j$	27%	30%	20%	21%	23%	25%	28%	25%
N	566	566	408	408	3,457	3,457	784	784
				New, Policy-Unconstrained Sample				
Mean ε_j	-3.54	-3.35	-4.80	-4.44	-4.01	-3.78	-3.54	-3.35
Median ε_j	-3.58	-3.38	-4.92	-4.59	-4.05	-3.78	-3.58	-3.38
Mean Lerner$_j$	28%	30%	21%	23%	26%	27%	28%	30%
Median Lerner$_j$	28%	30%	20%	22%	25%	26%	28%	30%
N	53	53	32	32	261	261	159	159

Note: The table presents results for the Random Coefficient Logit (RC Logit, RCL) and Random Coefficient Nested Logit (RC Nested Logit, RCNL) estimations using Manhattan, the Bronx, Queens, and Brooklyn as four separate markets. Both models include random coefficients are on a constant, age, log distance to CBD, log distance to nearest subway, avgerage unit square feet, and building controls described in the text. The RCL model uses Census Tract fixed effects (FEs) and the RCNL uses NYC NTA FEs and additional location controls described in the text. Both models use "Quadratic Differentiation Instruments" based on Gandhi and Houde (2018), as described in Section 3.6.2. Both models are estimated using Two-Step Efficient GMM due to computation constraints. ε_j is the own-price elasticity and the Lerner index is $-1/\varepsilon_j$. Sample definitions follow, by borough, those in Table 3.3; buildings are 'unconstrained' if *not* rent stabilized and *not* zoning-constrained; new buildings were built after 2000.

Table 3.6: Spillover Effects from Up-Zoning Manhattan Buildings

	RCL	RCNL
Direct Price Effect of Looser Zoning: $\mathsf{E}\left[\mathsf{d}r_k^{\mathsf{cf}}\right]$	-\$59.64	-\$58.55
Spillover Markup Effect of Looser Zoning: $\mathsf{E}\left[\mathsf{d}r_j^{\mathsf{cf}}\vert_{\mathsf{d}mc_j=0}\right]$	-\$7.41	-\$6.72
Implied Spillover Zoning Elasticity: $\mathsf{E}\left[\frac{\mathsf{d}r_j^{\mathsf{cf}}}{r_j} / \frac{\mathsf{d}D_k^{\mathsf{cf}}}{D_k}\right]$	-0.017	-0.012
Net Increase in Households		
Direct and Spillover	436	421
Spillover Only	19	5

Note: The table reports the effects of up-zoning zoning constrained buildings that are not rent stabilized by a marginal amount; i.e., a 1% increase in allowable quantity, which corresponds to a total addition of 417 whole units. Results are presented separately for the Random Coefficient Logit (RCL) and Random Coefficient Nested Logit (RCNL) models described in the text. $\mathsf{E}\left[\mathsf{d}r_k^{\mathsf{cf}}\right]$ is the first-order average annual price effect on buildings k in the set $\mathcal{Z}$ of 3,226 directly impacted buildings. $\mathsf{E}\left[\mathsf{d}r_j^{\mathsf{cf}}\vert_{\mathsf{d}mc_j=0}\right]$ is the average effect on annual rents on the zoning unconstrained buildings j in the set $\mathcal{U}$ of 566 non-zoning constrained, non-rent regulated buildings, assuming constant marginal costs. This number does not include cross-price effects between buildings $j \in \mathcal{U}$ or other higher order effects. The implied spillover elasticity is the average percent change in annual rents at buildings $j \in \mathcal{U}$ given a 1% increase in maximum quantity allowed at buildings $k \in \mathcal{Z}$. For more details, see Appendix C.10.

APPENDICES

APPENDIX A

APPENDIX TO CHAPTER ONE

A.1 Theory Appendix

In this appendix, I describe additional theoretical details of the model in the main text as well as consider two theoretical extensions. First, I present the parameters for the numerical comparative statics from Figure 1.3 and describe how welfare is calculated within the model. Second, I present the equilibrium conditions that lead to the the many type model that is used in the empirical exercises. Adding additional types of labor in this context is relatively simple due to the symmetry of the modeling assumptions. Next, I return to the two skill model but now the high skill worker is able to switch between sectors. This extension is essentially a simplified version of Saez (2002) with endogenous wages. Finally, in the two skill model, I allow for two consumption goods producing industries that employ both high and low skill workers. This extension essentially 'stacks' the equilibrium conditions used in the single industry model in the main text.

A.1.1 Incidence Value Comparison

Here, I compare the gross wage incidence from a one percent tax change[1] between PE and GE and across labor market elasticities. I use equation 1.15 for the PE incidence and I use equation 1.17 for the GE incidence. The main takeaway is that the incidence effect magnitude depends primarily on the labor substitution elasticity, ρ, and the cost share of the subsidized market, s_{L0}.

In Table A.1, I present incidence values for various parameter pairings. I use the following baseline parameters: $\varepsilon^L_{0,0} = \varepsilon^L_{0,1} = 0.75$, $\varepsilon^L_{1,0} = \varepsilon^L_{1,1} = 0.6$, and $\varepsilon_K = 1$, based on Rothstein (2010), Eissa and Hoynes (2004), and Goolsbee (1998), respectively. For the elasticity of substitution I use $\rho \in \{-0.3, -1, -2\}$, based on Rothstein (2010), my empirical analysis presented later ($\rho = -2$), and

[1]That is I plot $\hat{w}_0/(\theta_{0,1}\hat{\tau})$, so that these results are not affected by the share of eligible workers within a skill level.

an intermediate value. I set $s_L = 0.66$ based on the approximate 1990s labor share of input costs. I set $s_{L0} = 0.125$ and $s_{L1} = 0.66 - s_{L0}$, based on the 1992 March CPS and my own calculations. For the first two panels I assume that only the low wage market is subsidized $\left(\hat{\tau}_{1,1}/\hat{\tau}_{0,1} = 0\right)$, but in the third panel I allow for a smaller subsidy on the high wage workers, $\left(\hat{\tau}_{1,1}/\hat{\tau}_{0,1}\right) > 0$.

Table A.1: Summary:
Percent Change in Gross Wage for Low Wage Market
from 1% Subsidy Increase

	Partial Equilibrium	General Equilibrium
	Using Baseline Supply Elasticities	
$\rho = -0.3$	-0.714	-0.645
$\rho = -1$	-0.429	-0.390
$\rho = -2$	-0.273	-0.252
	Other Elasticities with $\rho = -2$	
$\varepsilon_0^L = 1.0$	-0.333	-0.269
$\varepsilon_1^L = 0.3$	-0.273	-0.254
$\varepsilon_1^L = 0.9$	-0.273	-0.251
$\varepsilon^K = 2$	-0.273	-0.249
	Allowing $\hat{\tau}_{1,1} > 0$ with $\rho = -2$	
$\frac{\hat{\tau}_{1,1}}{\hat{\tau}_{0,1}} = 0.1$	-0.273	-0.240
$\frac{\hat{\tau}_{1,1}}{\hat{\tau}_{0,1}} = 0.2$	-0.273	-0.228

Baseline: $\varepsilon_0^L = 0.75, \varepsilon_1^L = 0.6, \varepsilon_K = 1, \frac{\hat{\tau}_{1,1}}{\hat{\tau}_{0,1}} = 0$. Incidence results computed at $s_{L0} = 0.125, s_L = 0.66$.

Table A.1 shows that the general equilibrium incidence always attenuates the PE incidence, especially as market size grows. The results highlight that the labor substitution elasticity appears to dictate the magnitude of the incidence effect. Using the value $\rho = -0.3$ from Rothstein (2010) implies a PE incidence of -0.71% while a $\rho = -2$ implies only a -0.25% change in gross wages.

Figure 1.3 is a graphical representation of Table A.1. I plot the partial and general equilibrium incidence of the gross wage at different labor cost shares ($s_{L0} \in [0, 1]$) and different substitution elasticities. The flat lines are the PE incidence and the upward sloping lines are the GE incidence. The graphical representation shows that as more workers are subsidized the GE incidence effects can quickly diverge from the PE effects.

A.1.2 Welfare

Here, I describe the measure of welfare in the model and changes due to tax policy.

For this section, I adjust the notation. Let $i \in \mathcal{N}$ index each specific worker: $i = (e_i, c_i, \nu_i)$. Let each worker have some non-labor income, m_i. Let each worker own some share of the firms in the economy,$\varsigma_i \in [0, 1]$, such that $\sum_{i \in \mathcal{N}} \varsigma_i = 1$.

A.1.2.1 Welfare

Total welfare in the economy is the sum of utility given the optimal decisions by workers and firms. In terms of Chetty (2009), with an added capital revenue equation,[2] the model is the following:

$$\text{Utility}: \quad U(X, L; \nu) = X + \nu \cdot L \tag{A.1}$$

$$\text{Tax Function}: \quad T_i(wL, m) = (w + \tau_i)L - b_i(1 - L) - n_i \tag{A.2}$$

$$\text{Capital Revenue}: \quad R = \int_j \left((r - \xi_j) \cdot k_j\right) \mathsf{d}_j \tag{A.3}$$

$$\text{Budget Set}: \quad X + T_i(wL, m) - wL - m \leq 0 \tag{A.4}$$

Thus, aggregate welfare with a Utilitarian SWF is aggregate consumption plus the utility cost of labor for those that work:

$$W = \int_i \nu_i \mathsf{d}_i + \int_i (T_i)\, \mathsf{d}_i \tag{A.5}$$

$$= \int_i ((w_i L_i - T_i) + \nu_i (L_i) + \varsigma_i R)\, \mathsf{d}_i + \int_i (T_i)\, \mathsf{d}_i \tag{A.6}$$

$$= \int_i ((w_i L_i) + (\nu_i \cdot L_i) + \varsigma_i R)\, \mathsf{d}_i. \tag{A.7}$$

A.1.2.2 Welfare Changes

The change in welfare for the economy is determined by totally differentiating the aggregate welfare measure. I follow the methods specified in Chetty (2009) and Kleven (2018). That is, I totally

[2]Recall that each worker has some $\varsigma_\nu \in (0, 1)$ share of capital revenue as part of unearned income that is taken as given in the labor supply choice.

differentiate equation A.6 holding unemployment benefits constant but adjusting the lump sum tax to finance the subsidy increase (and recall that $\tau_i = \mathsf{d}\tau_i = 0$ if $(e_i, c_i) \neq (0, 1)$):

$$\begin{aligned}
\mathsf{d}W^{\mathsf{GE}} &= \int_i \left((\mathsf{d}w_i + \mathsf{d}\tau_i)L_i + (w_i + \tau_i - b_i)\mathsf{d}L_i + \frac{\partial v_i}{\partial L_i}\mathsf{d}L_i + \varsigma_i \mathsf{d}R - \mathsf{d}n_i \right) \mathsf{d}_i \\
&\quad + \int_i (-\mathsf{d}\tau_i L_i - (\tau_i - b_i)\mathsf{d}L_i + \mathsf{d}n_i)\, \mathsf{d}_i && \text{(A.8)} \\
&= \int_i ((\mathsf{d}w_i)L_i + \varsigma_i \mathsf{d}R)\, \mathsf{d}_i + \int_i (-(\tau_i - b_i)\mathsf{d}L_i)\, \mathsf{d}_i && \text{(A.9)} \\
&= \int_i (-(\tau_i - b_i)\mathsf{d}L_i)\, \mathsf{d}_i = -\int_i \left((\tau_i - b_i)\varepsilon_i^L (\mathsf{d}w_i + \mathsf{d}\tau_i) \right) \mathsf{d}_i && \text{(A.10)} \\
&= -\int_i \left((\tau_i - b_i)\varepsilon_i^L ((1 + \gamma_i)\mathsf{d}\tau_i + \Gamma_i) \right) \mathsf{d}_i. && \text{(A.11)}
\end{aligned}$$

From equation A.8 to A.9, I use the envelope condition to remove $\frac{\partial v_i}{\partial L_i}$; from A.9 to A.10, I use the zero profit condition to show that $\mathsf{d}R = \int_i ((\mathsf{d}w_i)L_i)\, \mathsf{d}_i$; and from A.10 to A.11, I use the incidence result to characterize the “fiscal externality” in terms of elasticities (Hendren, 2016a; Kleven, 2018). The welfare measure’s negative sign because the behavioral fiscal externality implies that the government is paying more subsidies due to the extensive margin response. However, if $\mathsf{d}L_i > 0$, then the government is also paying less in unemployment benefits, as empirically shown in Bastian and Michelmore (2018).

The above supposes that lump sum taxation is used, so the fact that wages rise for other workers is not part of the fiscal externality; i.e., the fact that greater earnings lessen the need to change the lump sum tax. If instead an income tax was used (with individual rate t_i), then the change in welfare is the following:

$$\mathsf{d}W^{\mathsf{GE}} = \int_i (t_i w_i \mathsf{d}L_i)\, \mathsf{d}_i = \int_i \left(t_i w_i \varepsilon_i^L ((1 + \gamma_i)\mathsf{d}\tau_i + \Gamma_i) \right) \mathsf{d}_i. \tag{A.12}$$

See that high wage workers now contribute the following term to the welfare change: $t^H w^H \Gamma^H > 0$. Because tax revenues increase for the high wage group, the government’s budget constraint is further loosened which lessens the negative fiscal externality. The welfare change in this case cannot be theoretically signed, so the welfare impact becomes an empirical to question.

A.1.3 Model with Many Worker Types

Here, I allow for each labor type to have a heterogeneous tax change, and then I solve the equations in the same manner as before using substitution after totally differentiating. Let worker types be indexed by $e \in \{0, 1, 2, \ldots, E\} = \mathcal{E}$.[3]

I use the following equilibrium system (suppressing labor supply arguments):

$$\text{Labor Clearing} \quad \frac{L_{\mathring{e},0} + L_{\mathring{e},1}}{L_{\tilde{e},0} + L_{\tilde{e},1}} = \left(\frac{w_{\mathring{e}}/\theta_{\mathring{e}}}{w_{\tilde{e}}/\theta_{\tilde{e}}}\right)^{\rho} \qquad \forall \tilde{e} \in \mathcal{E} \setminus \mathring{e} \tag{A.13}$$

$$\text{Factor Clearing} \quad \frac{\sum_e L_e}{K^S(r)} = \left(\frac{\bar{w}/\alpha}{r/1-\alpha}\right)^{-1} \tag{A.14}$$

$$\text{Zero Profits} \quad P = c\left(\{w_e\}_{e \in \mathcal{E}}, r\right) := 1 \tag{A.15}$$

The incidence is solved using by taking the total derivative to linearize the system and then either iterative substitution or Cramer's rule to solve for the factor price changes as a function of the tax change. By adjusting the labor clearing condition (equation A.13), I can solve for any specific market's incidence.

The general equilibrium incidence for type 0 labor is:

$$\hat{w}_0^{\mathsf{GE}} = \frac{-\varepsilon^L_{(0,1)}\theta_{0,1}\hat{\tau}_0}{\varepsilon^L_0 - \rho} + \frac{\Lambda\left(\sum_e \frac{s_e \varepsilon^L_{(e,1)}\theta_{e,1}\hat{\tau}_e}{\varepsilon^L_e - \rho}\right)}{(\varepsilon^L_0 - \rho)\left(1 + \Lambda\left(\sum_e \frac{s_e}{\varepsilon^L_e - \rho}\right)\right)} \tag{A.16}$$

$$= (\gamma_0 + \Gamma_0)\hat{\tau}_0 + \Psi_0(\{\tau_e\}_{e \in \mathcal{E} \setminus e=0}) \tag{A.17}$$

$$\text{where } \Lambda = \left(\frac{\varepsilon_K + 1}{s_K} + \frac{1+\rho}{s_L}\right). \tag{A.18}$$

Generally, one cannot sign the expression without knowing the direction of each $\{\tau_d\}_d$. This is similar to Agrawal and Hoyt (2018b) in the context of taxing multiple consumer goods. For example, if the own tax change is large but all other tax changes are small, then very likely the partial equilibrium term will dominate, so the expression is negative. However, if the own tax change is small but all other are large and positive, then the general equilibrium spillovers will dominate, so the expression is positive.

[3] In the calibrated model, $|\mathcal{E}| = 72$ based on age, education, and marital status of women.

Again, this shows that generally there will be **two** first order terms with respect to the tax change. Only if the general equilibrium spillover term is small will $w^{\mathsf{GE}} \approx w^{\mathsf{PE}}$. Note, with multiple tax changes, it is no longer sufficient to suppose that $s_0 \approx 0$ for the GE terms to disappear. Rather, one needs to assume that the average cost share weighted tax change is equal to zero: $\mathsf{E}[s_e \theta_{e,1} \hat{\tau}_e] \approx 0$.

A.1.4 Model with Market Switching

Here, I return three factor model but I allow the high wage workers, $e = 1$, to switch between markets. Additionally, I allow for a differential tax change in both labor markets.

This set up is similar to the model used in Saez (2002), only simplified to fewer employment groups. This allows $e = 1$ workers to substitute between unemployment, low wage work, and high wage work. Workers with $e = 0$ are only able to adjust between unemployment and low wage work.

For example, in the EITC context, suppose that high wage mothers see the net low-wage sector wage increase relative to high-wage work, and if this worker is marginally attached to high wage work, then there she will switch to low wage work. Alternatively, if a $e = 1$ worker without children originally chose low-wage work, then the potential real wage decrease relative to the high-wage sector will cause this worker to choose high wage work.

In this framework notation can get messy because workers of the same (e, c) can earn different wages, so I need to track both worker type and worker labor choice for four different types of workers and three sectors. This is not conceptually difficult, but messy. I assume that $e = 1$ workers are paid equal to $e = 0$ if they participate in the low-wage sector. One foundation for this is that low-wage work involves some set tasks that cannot benefit from high-wage worker's skills, so workers of both e types will have the same marginal product.[4]

Let the labor supply of a type (e, c) worker be denoted as $L^e_{g,c}$, where $g \in \{0, 1\}$ designates low or high wage labor group. Let $\varepsilon^L_{e,g,c}$ be the extensive labor supply elasticity, and for type $e = 1$ workers let $\chi_c^{g \to g'}$ be the cross wage elasticity with respect to sector choice for workers. The latter

[4]Note, this rules out pricing power by firms to create a separating equilibrium among worker types.

elasticity is only concerned with incumbent workers who potentially switch sectors. I suppress the group conditional demographic shares, $\theta^e_{g,c}$, to ease notation.

This implies the following equilibrium system (suppressing labor supply arguments):

$$\text{Labor Clearing} \quad \frac{L^0_{0,0}+L^1_{0,0}+L^0_{0,1}+L^1_{0,1}}{L^1_{1,0}+L^1_{1,1}} = \left(\frac{w_0/\theta_{0,1}}{w_1/\theta_{1,1}}\right)^{\rho} \tag{A.19}$$

$$\text{Factor Clearing} \quad \frac{L^0_{0,0}+L^1_{0,0}+L^0_{0,1}+L^1_{0,1}}{K^S(r)} = \left(\frac{\bar{w}/\alpha}{r/1-\alpha}\right)^{-1} \tag{A.20}$$

$$\text{Zero Profits} \quad P = c(w_0, w_1, r) := 1 \tag{A.21}$$

The general equilibrium incidence for this model is:

$$\hat{w}_0^{\mathsf{GE}} = \frac{-(\varepsilon^L_{0,1}-\tilde{\chi}^{1,0}_1)\hat{\tau}_0}{(\varepsilon^L_0-\tilde{\chi}^{1,0}-\rho)} + \frac{\Lambda\left(\sum_d\left(\frac{s_d\hat{\tau}_d(\varepsilon^L_{d,1}-\tilde{\chi}^{\neg d,d}_1)}{(\varepsilon^L_d-\tilde{\chi}^{\neg d,d}-\rho)}\right)\right)}{1+\sum_d\left(\frac{s_d\Lambda+\tilde{\chi}^{\neg d,d}_1}{(\varepsilon^L_d-\tilde{\chi}^{\neg d,d}-\rho)}\right)} \tag{A.22}$$

$$= (\mathsf{p}_0+\Gamma_0+\mathcal{X}_0)\hat{\tau}_0+\Psi_0(\hat{\tau}_2)+\mathfrak{X}_0(\hat{\tau}_2) \tag{A.23}$$

where $\varepsilon^L_{d,1}$ and $\tilde{\chi}^{g,g'}_c$ incorporate the relevant share of workers based on $\theta^e_{g,c}$. As before, $\Lambda = \left(\frac{\varepsilon_K+1}{s_K}+\frac{1+\rho}{s_L}\right)$.

The main difference is that the supply elasticities are more complicated, intuitively, because workers can make more choices and supply is not inelastic between markets. There are now **five** first order terms in the incidence analysis, each capturing a different supply responses to wages.

This shows an additional consequence of partial equilibrium analysis. If worker have the ability to switch between sectors, then a partial equilibrium analysis will hold the supply of the other markets fixed. This omits important equilibrium responses to subsidies even for the market being studied.

A.1.5 Two Sector Model

A.1.5.1 Model

Let there be two final goods, $\{X, Y\}$, for sale at market prices, $\{p_x, p_y\}$, produced using three factors, $\{L, H, K\}$, that are each elastically supplied given factor prices, $\{w_x, w_y, v_x, v_y, r_x, r_y\}$.

I refer to L as low-skill labor, H as high-skill labor, and K as capital (or any other factor which is elastically supplied), w as low-skill wages, v as high-skill wages, and r as capital rents. Let all agents that can supply L or H service (labor) be called 'workers' regardless of their labor force participation; e.g., a low-skill worker either participates in the labor force or does not participate.

Production + Capital

Let $X = F^{(X)}(g_x(L_x, H_x), K_x)$ and $Y = F^{(Y)}(g_y(L_y, H_y), K_y)$, where $F^{\cdot}(\cdot)$ are both CRS production functions with a CES subfunction that aggregates the two labor types. For production I use

$$F = \left((L^{\frac{1+\rho}{\rho}} + H^{\frac{1+\rho}{\rho}})^{\alpha \frac{\rho}{1+\rho}} \cdot K^{(1-\alpha)} \right), \tag{A.24}$$

which is a nested CES production function that satisfies the assumption. Profit for an industry j is defined as $\pi_j = p_j X_j - w_j L_j - v_j H_j - r_j K_j$, and in equilibrium $\pi_j = 0$.

Let K be supplied according to the function $K^S(r_x, r_y)$, where the suppliers of capital consider the two sectors perfect substitutes. For example, if $r_x > r_y$, then $K_x = K^S(r)$ and $K_y = 0$. Thus, in any equilibrium where both goods are produced, $r_x = r_y$, and we may only refer to r.

Utility

Let type s worker utility be $u^s = U^s(X, Y, L_x, L_y, L_o)$, where $L_o = \mathcal{L} - L_x - L_y$ is leisure time. Let utility be separable so that $u^s = C^s(X, Y) + n(L_x, L_y, L_o)$. Further, let $C^s(X, Y) = c(X/Y) \cdot Y$, so that utility is homothetic for goods. Since utility is quasi-linear with respect to aggregate consumption, the labor supply will not depend on relative output prices – this can be relaxed.

Importantly, the disutility of labor depends on the type of labor. Depending on the function form (and stochastic assumptions), this implies that two types of workers may make heterogeneous labor supply decisions given the same market prices. This can be micro-founded by assuming that workers draw a triple ($\{\epsilon_x, \epsilon_y, \epsilon_o\}$) from some distribution, then solve the following problem:

$$\max_{x,y,o}\{V^\star(x) + \epsilon_x, V^\star(y) + \epsilon_y, V^\star(o) + \epsilon_o\}, \tag{A.25}$$

where $V^\star(\cdot)$ is the optimal consumption choice given a labor supply decision and prices. This yields the probability that a worker will work in the respective sectors: p_j^s. This approach is very common in the labor supply literature as well as in Saez (2002).

For an individual, this can be interpreted as the amount of labor supply devoted to each sector, where $\sum_j \mathsf{p}_j^s = 1$. Or, one can assume that each worker truly chooses only one sector but that the aggregate employment is matched exactly: $L = N \cdot \mathsf{p}$.

Budget Constraint + Subsidy

The worker budget constraint is $p_x X + p_y Y \leq \mathcal{T}^s(w_x L_x, w_y L_y, L_o)$. Let $\mathcal{T}^s(\cdot) = (w_x + \tau_s)L_x^s + w_y^s L_y^s + b_s L_o^s - T^s$, where τ_s is a labor subsidy for sector X, b_s in an unemployment benefit, and T^s is a lump sum tax on all workers regardless of labor supply. Given that utility only depends on leisure, the net return to supplying labor in the two sectors implies that in any equilibrium with both goods being produced, $(w_x^s + \tau_s) = w_y^s$.

To pay for the subsidy to sector X and unemployment, the government must set the lump-sum taxes to cover this cost in equilibrium. Let the government budget constraint be $T^L + T^H = \tau_L L_x + b_L L_o + \tau_H H_x b_H H_o$.

A.1.5.2 Equilibrium

The following are the equilibrium conditions:

$$\text{X Labor Market Clearing:} \quad \frac{L_x^S(w_x+\tau_L, w_y, b_L)}{H_x^S(v_x+\tau_H, v_y, b_H)} - \psi_x(w_x/v_x) = 0 \tag{A.26}$$

$$\text{X Factor Market Clearing:} \quad \frac{L_x^S(w_x+\tau_L, w_y, b_L)}{K_x^S(r)} - \psi_x(w_x/v_x)\Psi_x(w_x/r) = 0 \tag{A.27}$$

$$\text{X Zero Profits:} \quad p_x - c_x(w_x, v_x, r) = 0 \tag{A.28}$$

$$\text{Y Labor Market Clearing:} \quad \frac{L_y^S(w_y, w_x+\tau_L, b_L)}{H_y^S(v_y, v_x+\tau_H, b_H)} - \psi_y(w_y/v_y) = 0 \tag{A.29}$$

$$\text{Y Factor Market Clearing:} \quad \frac{L_y^S(w_y, w_x+\tau_L, b_L)}{K_y^S(r)} - \psi_y(w_y/v_y)\Psi_y(w_y/r) = 0 \tag{A.30}$$

$$\text{Y Zero Profits:} \quad p_y - c_y(w_x, v_x, r) = 0 \tag{A.31}$$

The model has seven endogenous prices $\{w_x, w_y, v_x, v_y, p_x, p_y, r\}$ and there are six equations, so I normalize $p_y = 1$.[5] This system is essentially the same as in the main text, but with an extra output sector and additional prices.

A.1.5.3 Solving for Wage Incidence

In this section, I will solve the model for incidence terms by linearizing the system in terms of differential changes in the subsidy.

Let $\tau_H = 0$ and $\mathsf{d}b_s = 0$.

In matrix form, the equilibrium system $A\hat{z} = \nu \cdot \hat{\tau}$ is:

$$\begin{bmatrix} \varepsilon_x^L - \rho_x & -(\varepsilon_x^H - \rho_x) & \chi_x^L & -\chi_x^H & 0 & 0 \\ \varepsilon_x^L + 1 - (1+\rho_x)\frac{s_x^H}{1-s_x^K} & -(1+\rho_x)\frac{s_x^H}{1-s_x^K} & \chi_x^L & 0 & 0 & -(\varepsilon_x^K+1) \\ \chi_y^L & -\chi_y^H & \varepsilon_y^L - \rho_y & -(\varepsilon_y^H - \rho_y) & 0 & 0 \\ \chi_y^L & \varepsilon_y^L + 1 - (1+\rho_y)\frac{s_y^H}{1-s_y^K} & -(1+\rho_y)\frac{s_y^H}{1-s_y^K} & & 0 & -(\varepsilon_y^K+1) \\ s_x^L & s_x^H & 0 & 0 & 1 & s_x^K \\ 0 & 0 & s_y^L & s_y^H & 0 & s_y^K \end{bmatrix} \begin{bmatrix} \hat{w}_x \\ \hat{v}_x \\ \hat{w}_y \\ \hat{v}_y \\ \hat{p} \\ \hat{r} \end{bmatrix} = \begin{bmatrix} -\varepsilon_x^L\hat{\tau} \\ -\varepsilon_x^L\hat{\tau} \\ -\chi_x^L\hat{\tau} \\ -\chi_x^L\hat{\tau} \\ 0 \\ 0 \end{bmatrix}$$

[5] The endogenous quantities, $\{L_j, H_j, K_j, X, Y\}$, all depend on the endogenous prices.

A.1.5.4 Two 'Tricks' for Solving

If $Az = b$, then by Cramer's Rule:

$$\begin{aligned}
\text{Cramer's Rule:} \quad z_i &= \frac{\det(A \mid b)}{\det(A)} && \text{(A.32)}\\
\text{Laplace Expansion:} \quad &= \frac{\sum_j b_{i,j}\det(A^{(j)})}{\det(A)} && \text{(A.33)}\\
&= \frac{\sum_j \frac{b_{i,j}}{a_{i,j}} a_{i,j}\det(A^{(j)})}{\det(A)} && \text{(A.34)}\\
\text{Matrix Derivative:} \quad &= \frac{\sum_j \frac{b_{i,j}}{a_{i,j}} a_{i,j}\left(\frac{\partial \det(A)}{\partial a_{i,j}}\right)}{\det(A)} && \text{(A.35)}\\
&:= \sum_j \left(\left(\frac{b_{i,j}}{a_{i,j}}\right)\left(\gamma_{a_{i,j}}\right)\right), && \text{(A.36)}
\end{aligned}$$

where $\gamma_{a_{i,j}} = \left(\frac{\partial \det(A)}{\partial a_{i,j}} \frac{a_{i,j}}{\det(A)}\right)$ is the elasticity of the determinant with respect to the matrix element.

This parameter is geometrically interpretable as the percent change in the area of the n-dimensional parallelogram formed by the system of equations from a 1% elemental change. Economically, the closest interpretation is that γ summarizes the effect of the exogenous variation (b) through the system of equations (A) from each equilibrium channel (the other elements of z).

Additionally, using some algebra:

$$z_i = \frac{\sum_j \frac{b_{i,j}}{a_{i,j}} a_{i,j} \mathrm{det}(A^{(j)})}{\mathrm{det}(A)} \tag{A.37}$$

$$= \frac{\sum_j \frac{b_{i,j}}{a_{i,j}} a_{i,j} \mathrm{det}(A^{(j)})}{\sum_j a_{i,j} \mathrm{det}(A^{(j)})} \tag{A.38}$$

$$= \sum_j \frac{b_{i,j}}{a_{i,j}} \frac{a_{i,j} \mathrm{det}(A^{(j)})}{\sum_j a_{i,j} \mathrm{det}(A^{(j)})} \tag{A.39}$$

$$= \frac{b_{i,i}}{a_{i,i}} + \left[\sum_{j \setminus i} \left(\frac{b_{i,j}}{a_{i,j}} - \frac{b_{i,i}}{a_{i,i}} \right) \frac{a_{i,j} \mathrm{det}(A^{(j)})}{\sum_j a_{i,j} \mathrm{det}(A^{(j)})} \right] \tag{A.40}$$

$$= \frac{b_{i,i}}{a_{i,i}} + \left[\sum_{j \setminus i} \left(\frac{b_{i,j}}{a_{i,j}} - \frac{b_{i,i}}{a_{i,i}} \right) \gamma_{a_{i,j}} \right] \tag{A.41}$$

A.1.5.5 Low Wage X Sector Incidence

It can be show using Cramer's Rule, Laplace Cofactor Expansion, and some algebra that

$$\frac{\hat{w}_x^L}{\hat{\tau}} = \underbrace{\frac{-\varepsilon_x^L}{\varepsilon_x^L - \rho_x}}_{\text{Partial Equilibrium}} \underbrace{+ \gamma_{a_{2,1}} \left(\frac{(1+\rho_x)(1 - \frac{s_x^H}{1-s_x^K})}{\varepsilon_x^L + 1 - (1+\rho_x)\frac{s_x^H}{1-s_x^K}} \right) + \left(\gamma_{a_{3,1}} + \gamma_{a_{4,1}} \right) \left(\frac{\rho_x}{\varepsilon_x^L - \rho_x} \right)}_{\text{Spillover Terms}} \tag{A.42}$$

A.2 Data Description and Summary Statistics

In this appendix, I provide additional descriptions and summary statistic information for the data used in the empirical sections. Broadly, I use the Current Population Survey from 1986 to 2010 (Flood et al., 2018) and the 1990 US Census 5% sample, (Ruggles et al., 2018). I additionally use the Urban Institute's Transfer and Income Model, which requires the following disclosure:

> Information presented here is derived in part from the Transfer Income Model, Version 3 (TRIM3) and associated databases. TRIM3 requires users to input assumptions and/or interpretations about economic behavior and the rules governing federal programs.

Therefore, the conclusions presented here are attributable only to the authors of this report.

A.2.1 Outgoing Rotation Group Samples

The ORG samples come from the Current Population Survey. A CPS respondent household is surveyed in two waves for four months each with an eight month break. On months four and eight, the surveyors ask the respondent additional labor market questions, such as usual hours and weekly earnings. The month-in-sample is staggered across respondents, so about one-fourth of any monthly sample is in an ORG.

I use the ORG samples for labor market quantities: wages and labor supply.[6] In table A.2, I provide the underlying sample of women in the CPS ORG that are aggregated for the main analysis. As described in the main text, I calculate hourly wages by dividing `usual weekly earnings` by `usual hours worked at main job`. I discard calculated wages from workers with imputed earnings and/or hours. I discard observations where the respondent says their usual hours vary, workers reporting less than one hour per week, workers workers with implied real $1990 wages less than $0.50 or greater than $150.00, and finally if the worker is out of the labor force *and* reports being in school full time over two-thirds of their CPS observations.[7]

In table A.3, I display the number of demographic cells by marriage and education group that are used in the incidence calculations. I only include market-state-year cells that have a minimum of five workers with children *and* five workers without children. This causes me to have an unbalanced panel of cells, but ensures that the market averages are calculated using a reasonable number of workers. The table itself also highlights demographic changes overtime. As can be seen, with population growth, the total number of cells goes from 14.2 thousand to 20.3 thousand. We can also see education attainment increasing, as there is a decrease in workers without a high school

[6]The major issue in using the ORG sample is that cannot it does not have enough information to predict EITC usage, which is based on previous year income and living arrangements.

[7]Additionally, I drop workers who are in group housing, who have no identified head of house, who are in households with greater than ten members (as it is too hard to form tax units), who are in the armed forces, and who are married with absent or separated spouses.

Table A.2: Market State Year Observations for Estimation Sample

	1989-1994		1995-2000		Difference	
	Mean	SD	Mean	SD	Dif	*t*
Age	38.98	12.24	39.91	11.99	0.93***	(39.21)
Married	0.63	0.48	0.62	0.49	-0.01***	(-14.19)
White	0.83	0.37	0.82	0.38	-0.01***	(-19.13)
Black	0.12	0.32	0.13	0.34	0.01***	(9.69)
Less HS	0.15	0.36	0.13	0.33	-0.02***	(-35.49)
High School	0.39	0.49	0.34	0.47	-0.06***	(-59.15)
Some College	0.32	0.47	0.30	0.46	-0.02***	(-22.04)
BA+	0.14	0.35	0.24	0.43	0.10***	(130.04)
Qualifying Child	0.48	0.50	0.47	0.50	-0.01***	(-14.21)
Age of Youngest	7.74	6.07	7.93	5.95	0.19***	(11.18)
LFP	0.68	0.46	0.71	0.46	0.02***	(23.43)
EPOP	0.64	0.48	0.68	7.00	0.03***	(32.98)
Usual Hours Total	37.60	10.48	38.00	10.23	0.39***	(8.11)
Usual Hours Main	36.68	9.90	37.28	9.70	0.61***	(25.53)
Real H.Wage	8.84	4.83	12.47	6.64	3.62***	(188.06)
Real Wage	10.73	6.03	15.71	9.05	5.98***	(234.76)
Real Weekly Earnings	431.63	276.53	629.24	420.29	197.61***	(207.12)
Observations	706,747		612,463		1,319,210	

All data from 1989-2000 CPS MORG samples, only women ages 20-65, accessed from IPUMS. All demographic, employment variables weighted by CPS Basic Weight, real wage and earnings by Earnings Weight × Hours. Real wages and earings inflated to 2018 dollars by BLS CPI Research Series. Real wage based on weekly earnings divided by usual hours for main job. Qualifying child based on child age, school status, and family structure.

degree to those with a college degree. Interestingly, there is an increase in unmarried women with some college but a decrease for married women, as this latter group shifts towards attaining their college degree.

A.2.1.1 Assignment of Children in ORG

We do not observe who claims EITC qualifying children is the CPS, so children must be assigned by the researcher according to some (*ad hoc*) rules. I assign children based on who seems the most likely primary care-giver in the social role of a parent. While not perfect, I heavily use the fact that children typically follow their primary care-giver in the record layout, in addition to family unit and relationship pointer variables. For most cases, this is simple and there is no ambiguity; however,

Table A.3: Market State Year Observations for Estimation Sample

	Less HS		HS		Some College		BA Plus		Total	
Year	Unmarried	Married	Unmarried	Married	Unmarried	Married	Unmarried	Married	Unmarried	Married
1990	246	282	572	714	386	660	46	172	1,250	1,828
1991	258	252	536	738	428	658	46	176	1,268	1,824
1992	268	240	496	680	378	572	166	500	1,308	1,992
1993	210	216	512	684	418	584	158	510	1,298	1,994
1994	186	182	506	634	430	572	142	494	1,264	1,882
1995	182	180	494	602	444	590	176	522	1,296	1,894
1996	158	162	496	580	454	542	152	514	1,260	1,798
1997	156	140	494	550	454	536	160	532	1,264	1,758
1998	144	138	490	544	458	556	190	530	1,282	1,768
1999	154	116	506	546	484	562	218	556	1,362	1,780
2000	156	126	520	532	470	566	204	550	1,350	1,774
Total	2,118	2,034	5,622	6,804	4,804	6,398	1,658	5,056	14,202	20,292

All data from 1993 March CPS, Women from Tax Units, Wage in \$1993. All variables weighted by CPS March Supplement Wt × Hours.

household living arrangements can be complex. The main consequence of my allocation rules can be stated in two examples.

First, consider a household with a 40 year old head of house (HoH), a 16 year old child of HoH, and a 1 year old grandchild of HoH who is directly related to the child. I assign the grandchild to the child rather than to the HoH. Another researcher may assign both to the HoH. Second, consider a household with a 40 year old HoH and a 20 year old non-relative "roommate" (so not a foster or adoptive child) who is unmarried and in school. I do not assign the non-relative to the HoH; although, another researcher may.

IPUMS constructs family relationship information, such as number of own children (`nchild`), based on an their definition of a family. Their goal is a combination of accuracy *and* scalability for many millions of observations. However, I find that this definition is does not suit my purpose of matching children to their most likely care-giver. When Census family identifying variables are available (primarily in the ASEC samples, discussed below), I am able to find many examples of child assignment that are not intuitive. Nevertheless, using the IPUMS family definitions result is the same qualitative results with minimal quantitative differences.

A.2.2 Annual Social and Economic Samples

I use the ASEC samples from the Current Population Survey to perform the simulation exercises: 1993-1995 for the OBRA expansion, 2008-2010 for the ARRA expansion. The ASEC samples is based on the March CPS and an oversampling from other months to increase data quality. March is chosen to coincide with tax-filing season, the surveyors ask additional questions about income, insurance, and other issues from the previous year. To reduce sampling errors, the surveyors include additional households for the ASEC from February and April (starting in 2002) and oversample Hispanic households (starting in 1976) (Flood et al., 2018).

I use the ASEC samples for incidence calculations because the possibility of calculating EITC usage given the income and family variables. However, the wage information is not as good as the ORG sample, since wages must be imputed using previous year annual earnings and work information rather than weekly earnings.

I present summary statistics on the incidence samples of women for tax year 1992 in Table A.6 and for 2008 in Table A.5.[8] As described in the main text, I calculate hourly wages by dividing `annual earnings last year` (all types) by the product `usual hours worked at main job last year` times `weeks worked last year`. The incidence sample is restricted to women ages 16 to 65. I drop women who are full or part time students *and* have not participated in the labor force for over one year and women who have negative tax unit self-employment earnings.[9]

Because the labor market variables are based on annual information, I classify an individual as a 'worker' if she satisfies the following: at least 40hrs of work last year, an average of at least 8hrs per week, must earn at least $100 per year (in $1990 dollars), and must have an implied wage of at least $0.50 (in $1990 dollars). This essentially relabels extreme part-time workers as 'non-workers.'

The most notable feature of the data is that the EITC is heavily concentrated in the unmarried

[8]Note, for the empirical exercise in Section 1.8, I also use the 1993 ASEC, but the sample is marginally different due to simulating the Welfare program measures. There is effectively no impact on the summary statistics in Table A.6.

[9]Additionally, I drop workers who are in group housing, who have no identified head of house, who are in households with greater than ten members (as it is too hard to form tax units), who are in the armed forces, and who are married with absent but non-separated spouses.

women with children segment, but this segment is also the smallest in labor cost terms and labor supply term. This implies that since their market share is reasonably small, that the GE effects are likely to be closer to the PE incidence, all else equal.

Table A.4: Summary Statistics for Simulation Incidence Sample
Tax Year 1992

	Age	Anykids	Married	Get Eic
Unmarried Women	33.00	0.00	0.00	0.00
Married Women	47.62	0.00	1.00	0.00
Unmarried Mothers	34.29	1.00	0.00	0.50
Married Mothers	36.90	1.00	1.00	0.18
	Less HS	HS Only	Less BA	BA+
Unmarried Women	0.26	0.26	0.30	0.18
Married Women	0.15	0.41	0.23	0.21
Unmarried Mothers	0.23	0.39	0.27	0.10
Married Mothers	0.12	0.38	0.28	0.22
	Worker	Wage	Share of Workers	Cost Share
Unmarried Women	0.72	10.14	0.32	0.20
Married Women	0.67	11.18	0.24	0.18
Unmarried Mothers	0.68	9.79	0.10	0.07
Married Mothers	0.70	10.86	0.35	0.23

All data from 1993 March CPS, Women from Tax Units, Wage in $1992. Demographic variables weighted by CPS March Supplement Wt, Wage by Supplement Wt × Usual Hours Last Year.

A.2.2.1 Assignment of Children in ASEC

As discussed above, the assignment of EITC qualifying children is up to the researcher. I use Census coded family unit ID, household record numbers, and relationship pointers to link EITC eligible children to (most likely) parents. Again, for creating tax units, the Census definition is closer in spirit to what researchers are aiming to capture rather than IPUMS definitions.

A.2.2.2 Sample Differences between Rothstein (2010)

There is primary difference between my ASEC sample and that of Rothstein (2010), who uses nearly the same criteria labor market criteria. Rothstein drops from the initial sample any person who is not labeled as the head of a family unit. This is roughly 36% of the initial sample, 13%

Table A.5: Summary Statistics for Simulation Incidence Sample
Tax Year 2009

	Age	Anykids	Married	Get Eic
Unmarried Women	34.16	0.00	0.00	0.05
Married Women	50.20	0.00	1.00	0.04
Unmarried Mothers	35.98	1.00	0.00	0.55
Married Mothers	39.54	1.00	1.00	0.20
	Less HS	HS Only	Less BA	BA+
Unmarried Women	0.23	0.23	0.31	0.23
Married Women	0.08	0.33	0.28	0.31
Unmarried Mothers	0.17	0.32	0.35	0.16
Married Mothers	0.10	0.25	0.28	0.37
	Worker	Wage	Share of Workers	Cost Share
Unmarried Women	0.65	18.13	0.33	0.19
Married Women	0.69	20.19	0.25	0.17
Unmarried Mothers	0.76	16.75	0.12	0.07
Married Mothers	0.71	21.49	0.31	0.21

All data from 2009 March CPS, Women from Tax Units, Wage in \$2008. Demographic variables weighted by CPS March Supplement Wt, Wage by Supplement Wt × Usual Hours Last Year.

of the initial 18 or older sample, and 6% of the initial 25 or older sample, who would not be dependents (sample proportions are unweighted). These individuals have roughly \$4000 less in wage and salary income (conditional on age, education, race, marital status, and gender) meaning they are more likely to qualify for the EITC based on income.[10]

The effect of this is that in Rothstein's analysis there are only *three* women under the age of 24 without children. Such a sample makes sense in the empirical literature in order to perform difference-in-difference estimation (this is because the need for parallel trends pushes one to remove these young workers). However, it is not obvious that it should be done in the incidence calculation, which is mostly theoretical simulation exercise. Because I believe many of these workers are within-market rivals of unmarried women with children, I include them in my simulations. This increases the women in the sample by roughly six thousand individuals and changes the average age of unmarried women without children from 41 to 33.

Additionally,Rothstein essentially assigns all individuals who potentially qualify as EITC de-

[10]They are also younger, more likely to have a high school degree or less, less likely to be white, more likely to be men, and much less likely to be or have been married.

pendents (based on age and education enrollment) to the head of household. In the end, Rothstein assigns about two thousand more workers at least one EITC dependents than my procedure (that is his procedure yields more workers with a qualifying dependent than my sample procedure).

The two changes I make – more workers in the sample and fewer EITC claimants – should *mitigate* the incidence effects.

A.2.3 1990 US Census 5% Sample

I use the 1990 US Census 5% Sample (Ruggles et al., 2018) to create the simulated tax instruments.

Table A.6: Summary Statistics for Simulation Incidence Sample
1990 Census

	Age	Anykids	Married	Get Eic
Unmarried Women	32.68	0.00	0.00	0.00
Married Women	47.29	0.00	1.00	0.00
Unmarried Mothers	35.15	1.00	0.00	0.49
Married Mothers	36.43	1.00	1.00	0.15
	Less HS	HS Only	Less BA	BA+
Unmarried Women	0.30	0.24	0.28	0.12
Married Women	0.20	0.36	0.25	0.13
Unmarried Mothers	0.26	0.34	0.31	0.07
Married Mothers	0.16	0.34	0.30	0.14
	Worker	Wage	Share of Workers	Cost Share
Unmarried Women	0.75	9.29	0.33	0.21
Married Women	0.66	10.26	0.23	0.18
Unmarried Mothers	0.73	9.10	0.09	0.06
Married Mothers	0.70	9.70	0.34	0.22

All data from 1990 US Census, 5% Sample March CPS, Women from Tax Units, Wage in $1989. Demographic variables weighted by Census sample weight, Wage by sample weight × Usual Hours Last Year.

A.3 Empirical Tax Instruments

A.3.1 Identification of Elasticities

To identify the labor supply and labor substitution elasticities, there are two sets of exclusion restrictions. The first set are used for the supply elasticities and the second for the substitution elasticity. The incidence model results imply an identification strategy. Direct changes in the own EITC ATR, τ, shift supply that allows me to identify the labor substitution elasticity that governs labor demand. GE spillover effects shift demand curves that allows me to identify the labor supply elasticities. Below, I formalize this using arguments from Watson (2020).

Consider the following simultaneous equations model [SEM]:

$$l_{it}^{D} = \alpha_0 + \alpha_1 w_{it} + u_{it}^{D} \qquad l_{it}^{S} = \beta_0 + \beta_1 w_{it} + \beta_1 \tau_{it} + u_{it}^{S} \qquad l_{it}^{S} = l_{it}^{D}. \tag{A.43}$$

This implies the following first stage and reduce form equations:

$$w_{it} = \frac{\alpha_0 - \beta_0}{\beta_1 - \alpha_1} + \frac{-\beta_1}{\beta_1 - \alpha_1}\tau_{it} + \frac{u_{it}^{D} - u_{it}^{S}}{\beta_1 - \alpha_1} := \pi_0 + \pi_1 \tau_{it} + v_{it}^{w}, \tag{A.44}$$

$$l_{it} = \frac{\alpha_0\beta_1 - \alpha_1\beta_0}{\beta_1 - \alpha_1} + \frac{-\alpha_1\beta_1}{\beta_1 - \alpha_1}\tau_{it} + \frac{\beta_1 u_{it}^{D} - \alpha_1 u_{it}^{S}}{\beta_1 - \alpha_1} := \mu_0 + \mu_1 \tau_{it} + v_{it}^{L}, \tag{A.45}$$

where all variables are in logs and $\ln[(1+\tau)] \approx \tau$. I assume that labor demand depends on the gross-wage while labor supply depends on the net-wage, and I suppress any dependence on covariates, X.

Now, I use the theoretical results from the main text imply the following wage incidence equation:

$$\underbrace{\mathrm{d}w_{it}}_{\text{Wage Change in Data}} = \underbrace{\gamma_1 \mathrm{d}\tau_{it} + \Psi_{it}}_{\text{Incidence Induced Change}} + \underbrace{\gamma_0 + \upsilon_{it}}_{\text{Unobs Wage Change}}, \tag{A.46}$$

where Ψ_{est} is a theoretical measurement of the GE spillover effect.

Combining the SEM with the incidence equation, the following equivalence must hold in the post period:

$$\underbrace{\gamma_0 + \upsilon_{it} + \gamma_i \mathrm{d}\tau_{it} + \Psi_{it}}_{\text{Incidence + Unobs}} = \underbrace{\mathrm{d}w}_{\text{Data}} = \underbrace{\frac{\alpha_0 - \beta_0}{\beta_1 - \alpha_1} + \frac{-\beta_1}{\beta_1 - \alpha_1}\mathrm{d}\tau_{it} + \frac{\mathrm{d}u_{it}^{D} - \mathrm{d}u_{it}^{S}}{\beta_1 - \alpha_1}}_{\text{SEM}}. \tag{A.47}$$

One obvious way to reconcile the two equations is the following:

$$\upsilon_{it} = \frac{-1}{\beta_1 - \alpha 1}\mathrm{d}u_{it}^{S} \qquad \Psi_{it} = \frac{1}{\beta_1 - \alpha 1}\mathrm{d}u_{it}^{D} \qquad \gamma_0 = \frac{\alpha_0 - \beta_0}{\beta_1 - \alpha_1} \qquad \gamma_1 = \frac{-\beta_1}{\beta_1 - \alpha_1}. \tag{A.48}$$

The above implies that if $\mathsf{Cov}(\tau, Z) \neq 0$, then $\mathsf{Cov}(\tau, u^D) \neq 0$, so τ is technically an invalid instrument in the SEM above. However, using the RF equation, the own tax change and spillovers can be used in tandem to estimate the elasticities:

$$\frac{\partial l}{\partial \Psi} = \frac{\beta_1}{\beta_1 - \alpha_1} \frac{\partial u^D}{\partial Z} \quad \& \quad \frac{\partial w}{\partial \Psi} = \frac{1}{\beta_1 - \alpha_1} \frac{\partial u^D}{\partial \Psi} \quad \Longrightarrow \quad \frac{\partial l / \partial \Psi}{\partial w / \partial \Psi} = \beta_1. \tag{A.49}$$

It is straight-forward to show: $\frac{\partial w}{\partial \Psi} = \frac{\partial [w+\tau]}{\partial \Psi}$ and $\frac{\partial l / \partial u^S}{\partial w / \partial u^S} = \alpha_1$. Additionally, I can allow for orthogonal demand unobservable changes: $\upsilon_{it} = u^S_{it} + u^{D,2}_{it}$, where $\mathsf{Cov}(\tau_{it}, u^{D,2}_{it}) = 0$ and $\mathsf{Cov}(\Psi_{it}, u^{D,2}_{it}) = 0$.

The main conclusion of Watson (2020) is that "in the context of the labor market SEM, we can use the tax reform treatment as a supply shifter and a measure of spillovers as a demand shifter." Let $\dot{y}_x$ be the residual from from a regression of y on x.

Proposition 3.

If τ is exogenous with the above SEM, then $\frac{\widehat{\mathsf{Cov}}(\dot{l}_\tau, \dot{Z}_\tau)}{\widehat{\mathsf{Cov}}(\dot{w}_\tau, \dot{Z}_\tau)} \rightarrow_p \beta_1$ and $\frac{\widehat{\mathsf{Cov}}(\dot{l}_Z, \dot{\tau}_Z)}{\widehat{\mathsf{Cov}}(\dot{w}_Z, \dot{\tau}_Z)} \rightarrow_p \alpha_1$, where 'exogenous' means that $\mathsf{Cov}(\tau, u^S) = 0$.

Thus, to identify β_1, I need a measure of the demand spillovers, which proxy for demand shifters, and to condition on the own tax rate as a proxy for supply shifters. The exclusion restriction is that the EITC tax reform and its spillovers are uncorrelated to unobservable differences in labor supply (conditional on the model controls):

$$\mathsf{E}\left[\tau_{ecst} \cdot u^S_{e'cst} \mid X\right] = 0, \ \forall e, e' \in \mathcal{E}. \tag{A.50}$$

This assumption would be violated if the EITC policy changes across demographic groups and state-years were chosen because the policymakers knew certain groups were more likely to systemically change their labor supply. Because the OBRA expansion was done at the national level (federal EITC rules are uniform across states), this would require that policymakers were able to precisely design the national change to take advantage of sub-state trends. More plausible is that state policy makers strategically implemented state-EITC reforms.[11] However, prior studies find

[11]Nine states had a state program by 1995 and eighteen by 2000.

that state EITC introductions and policy changes appear plausibly exogenous to local economic conditions (Leigh, 2010; Buhlmann et al., 2018).

Alternatively, if there are social program reforms that are correlated with EITC reforms, then I will misattribute to the EITC wage effects that are actual to due other program changes. The most obvious example is PRWORA that replaced Aid to Families with Dependent Children (AFDC) with Temporary Assistance for Needy Families (TANF) in 1996. This reform "was the culmination of state-led welfare reform efforts starting in the late 1980s ... implemented under the heading of welfare waivers, permissions from the federal government allowing states to experiment with their welfare programs Kleven (2019)." To account for this possibility, I interact an indicator for having children with indicators for implementation of state 'welfare waivers'.[12] Given that I include state-year FEs, these variables will control for any variation in EITC ATRs, wages, and supply that are due to differential effects of welfare reforms by parental status.

To identify the substitution elasticity, I rely on a similar argument as for α_1 in the above SEM. I now need to condition on the spillovers and use the direct EITC change as a supply instrument:

$$\mathsf{E}\left[\frac{\tau_{est}}{\tau_{0st}} \cdot u^D_{(e,0),st} \mid X, \Psi_{est}\right] = 0. \tag{A.51}$$

That is, the relative tax change between skills is uncorrelated with the relative demand unobservables conditional on covariates and spillovers.

This assumption would be violated if the EITC was implemented in a way that was complementary to underlying skill biased technical change where firms were demanding more low skill labor just as the EITC was expanding labor supply. To the extent that this occurred, I interact 1990 wage deciles with year indicators to capture any wage trends across states and skills.

A.3.2 Construction

There are two ways of using EITC policy variation as an instrument for market variables. First, one can use the EITC policy parameters directly, such as maximum EITC benefit given number of

[12] These are provided by Kleven (2019) in online replication material accessed on the author's personal website.

children which varies at the state-year level (Leigh, 2010; Kasy, 2017; Bastian and Michelmore, 2018). This variable is very simple to implement but is constant across all labor markets in a state.

The second method is using a simulated tax instrument, similar to Gruber and Saez (2002); Rothstein (2008), for each demographic group across states.[13] Here I describe the construction of the EITC average tax rate in detail. I additionally calculate IVs using the share of a market with positive EITC and the change in EITC based on tax code changes in an analogous way.

Using a fixed distribution of worker characteristics from the 1990 Census, I calculate average tax rates due to the EITC over multiple years of policy changes. By fixing the distribution of workers, endogenous changes in ATRs due to changes in labor market variables are purged. This construction allows the instrument to vary at the labor market-state-year level.

To calculate this, I need to estimate the true EITC benefits and the counterfactual EITC benefits if the worker did not work. I calculate the true EITC benefits, E_i^{act}, by using TAXSIM on the actual data, where E is the federal and state EITC benefit. To calculate the counterfactual benefits, E_i^{cf}, I set the worker's labor earnings equal to zero but leaving all else equal and rerun TAXSIM.[14] Finally, I calculate the EITC Average Tax Rate as the difference in the actual minus the counterfactual benefits over earned income:

$$\tau_i^{\mathsf{EITC}} = \frac{E_i(L = L_i) - E_i(L = 0)}{w_i \cdot h_i}. \tag{A.52}$$

I use the market level sample weighted mean to calculate τ_{ecst}.

As stated above, I use the 1990 Census to calculate the tax instrument. I replicate the data for each tax year and send the data to Internet TAXSIM. To avoid issues of 'bracket-creep', I inflate monetary values by the BLS CPI All Items Research Series but do not change any other quantity.

The above only calculated the EITC ATR for a specific labor market, τ_{ecst}. However, the total incidence also depends on a weighted sum of tax changes in *other* labor markets within a state-year,

[13] Leigh (2010) and Bastian and Michelmore (2018) both also use this type of approach secondary analysis.

[14] In married couple tax units, the counterfactual is with respect to the wife's labor supply decision. I assume the husband's earned income remains unchanged.

$\Psi_{ec}(\{\tau_{e,c'}\}_{e,c'\in\mathcal{D}})$. Thus, I need an empirical counterpart for the Ψ_{ecst} terms, but this depends on the parameters that I wish to estimate – see equation A.16.

I approximate the function by creating two different 'leave-out' averages of the tax change across labor markets matched to a given market. Under the assumption that:

$$\Psi_{ecst} = H\left(\{\tau_{e'cst}\}_{ec'\in\mathcal{D}}\right) \approx \mathsf{a}_1\bar{\tau}_{g_1(e),cst} + \mathsf{a}_2\bar{\tau}_{g_2(e),cst} + \nu_{ecst}, \tag{A.53}$$

for observed $(\bar{\tau}_{g_1(e),cst}, \bar{\tau}_{g_2(e),cst})$, then I can use these observed variables as approximations to the true spillover.

The first match-group is based on age groups and the second match group is based on education groups. I create the leave-out averages by excluding the specific market-segment when creating the averages. For example, if $(\tilde{e}, c)$ is married women with some college between ages of 25 and 30, then $\bar{\tau}_{g_1(\tilde{e}),cst}$ equals the average EITC ATR for women with some college pooled across age groups excluding the specific group, $\bar{\tau}_{g_2(\tilde{e}),cst}$ equals the average EITC ATR for women between ages of 25 and 30 pooled across education groups also excluding the specific group.

Recall, because I include the own EITC ATR as a control variable in both the first stage and structural equation, the variation in these leave-averages is by construction orthogonal to direct EITC variation.

As stated above, I use the EITC ATR and two other simulated EITC statistics as instruments: the share of workers receiving EITC benefits and the mean change in expected real EITC amounts. Below I specify the IVs used in the main results. In Appendix A.4, I show that the elasticity estimates are robust to various combinations of the instruments.

A.3.2.1 Labor Supply Instruments

For every group $\tilde{d} = (e, c)$, I have nine market level simulated instruments for wages:

1. the EITC ATR: $\{\tau^{\mathsf{ATR}}_{\tilde{d}st}\}$
2. the portion of $\tilde{d}$ workers with positive EITC: $\{z^{\mathsf{Sh}}_{\tilde{d}st}\}$
3. the mean change in ETIC amount for $\tilde{d}$: $\{z^{\mathsf{dE}}_{\tilde{d}st}\}$

4,5. two EITC ATR approximation averages: $\{\bar{\tau}_{g_{(\cdot)}(\tilde{d})st}\}$

6,7. two positive EITC approximation averages: $\{\bar{z}^{\mathsf{Sh}}_{g_1(\tilde{d})st}, \bar{z}^{\mathsf{Sh}}_{g_2(\tilde{d})st}\}$

8,9. two mean changes in expected real EITC amounts approximation averages: $\{\bar{z}^{\mathsf{dE}}_{g_1(\tilde{d})st}, \bar{z}^{\mathsf{dE}}_{g_2(\tilde{d})st}\}$.

Based on the identification arguments above, I condition on the demographic specific simulated EITC ATR, share with EITC, and average change in EITC: $\{\tau_{ecst}, z^{\mathsf{sf}}_{ecst}, z^{\mathsf{dE}}_{ecst}\}$.

A.3.2.2 Labor Substitution Instruments

The labor substitution elasticity depends on the relative wage, $\ln[w_{est}/w_{e_0st}]$. My main specification uses a just identified model using the 'relative EITC ATRs' to instrument for relative wages:

$$\tau_{(\tilde{e},e_0)st} = \frac{\tau_{\tilde{e}st}}{\tau_{e_0st}}. \tag{A.54}$$

I also construct relative share of EITC claimants and the relative change in real EITC amounts to estimate an overidentified model. For the substitution elasticity, I only use the education based averages because, when I create the relative variables for the regressions, I match workers based on age so the age-group leave-out averages are absorbed into other fixed effects.

A.3.3 Comparison with Traditional Approaches

Here, I quickly describe the issues using more traditional approaches in the EITC literature to estimating relevant parameters when allowing worker heterogeneity and general equilibrium effects.

A.3.3.1 Labor Supply Difference in Difference

Previous authors have estimated labor supply responses using difference-in-difference style assumptions for unmarried women with and without children – see Eissa and Liebman (1996); Hotz et al. (2002) for an early example and a review of the empirical literature list of examples. This assumption supposes that these workers face similar market forces, such as being perfectly substitutable conditional on age and education (and experience), so that in a narrow window around

EITC expansions the only change between these workers is the difference in EITC policy effects. Such assumptions lead to expecting "parallel trends" before the reform and using the post-reform dynamics of women without children to form a counterfactual baseline for women with children.

To see the implications of these assumptions, consider the following model, where $\tau_{e,c,t} = 0$ if $t = 0$ and $\tau_{e,c,t} = 0$ if $c = 0$:

$$\mathsf{E}[l^S_{ect}] = \beta_0 + \beta_{e,c}(w_{e,t} + \tau_{e,c,t}) + \lambda_e \tag{A.55}$$

$$\implies \mathsf{E}[l^S_{ec,1}] - \mathsf{E}[l^S_{ec,0}] = \beta_{e,c}(w_{e,1} - w_{e,0} + \tau_{e,c,1} - \tau_{e,c,0}) \tag{A.56}$$

$$\implies \left(\mathsf{E}[l^S_{e,1,1}] - \mathsf{E}[l^S_{e,1,0}]\right) - \left(\mathsf{E}[l^S_{e,0,1}] - \mathsf{E}[l^S_{e,0,0}]\right) =$$

$$\underbrace{(w_{e,1} - w_{e,0})}_{\text{Incidence Effects}} \cdot \underbrace{(\beta_{e,1} - \beta_{e,0})}_{\text{Elasticity Differences}} + \underbrace{\beta_{e,1}\tau_{e,c,1}}_{\text{ATET}}. \tag{A.57}$$

If one assumes that wages are fixed, $(w_{e,1} - w_{e,0}) = 0$, then the DiD estimates the ATET with no additional assumptions about behavioral responses to wages. If one allows for wage changes (via exogenous changes or incidence effects), then one needs to assume that the wage responsiveness of workers with and without children is equivalent; i.e., $(\beta_{e,1} - \beta_{e,0}) = 0$. This latter restriction is testable in the data with an appropriate empirical strategy.

Without either assumption, then the DiD estimate of the ATET is biases in an unknown direction unless one knows the parameters $\{\beta_{e,1}, \beta_{e,0}\}$, in which case estimation is not necessary. My approach allows for heterogeneous labor supply elasticities and uses wage and EITC variation across states and demographic groups to estimate the elasticities.

A.3.3.2 Log Wage Difference in Difference

The empirical literature on the EITC has not focused much on wage effects, due to typically assuming fixed wages. Leigh (2010) regresses log wages at the individual level on the maximum state EITC amount, but does not report incidence parameters directly.

To see how this fits with the incidence model, suppose we observe wages and tax rates for skill level e across states s and years t. The incidence results imply the following equation, where

$\tau_{est} = \Psi_{est} = 0$ if $t = 0$:

$$\mathsf{E}[w_{est}] = \gamma_0 + \gamma_e \tau_{est} + \lambda_s + \Psi_{est} \tag{A.58}$$

$$\implies \mathsf{E}[w_{es1}] - \mathsf{E}[w_{es0}] = \gamma_e \tau_{es1} + \Psi_{es1} \tag{A.59}$$

$$\implies (\mathsf{E}[w_{e11}] - \mathsf{E}[w_{e10}]) - (\mathsf{E}[w_{e01}] - \mathsf{E}[w_{e-0}])$$

$$\gamma_e(\tau_{e11} - \tau_{e01}) + \underbrace{\Psi_{e11} - \Psi_{e01}}_{\text{GE Bias}}. \tag{A.60}$$

Unless one can control for GE spillovers or knows when they are negligible, then, even within a skill group, spillovers create a GE bias. If we compare across skill groups, $e \in \{0, 1\}$, in the same state where we know $\tau_{est} = 0$ for $e = 0$, then we still get GE bias unless skill group $e = 0$ has no exposure to skill group $e = 1$: $\gamma_1 \tau_{1st} + \Psi_{1s1} - \Psi_{0s1}$. However, if skill group $e = 0$ has no GE exposure, then we cannot trust that this is a valid control group. My approach deals with this GE bias by adding structural assumptions about labor demand and estimating labor market elasticities that compose the GE spillovers based on the incidence model.

A.4 Additional Estimation Results

In Table A.7, I provide additional elasticity estimates for labor supply. These specifications differ on five dimensions: method, weighting, sample, IVs, and dependent variable. The table displays the KP rk Wald F, a cluster robust Cragg-Donald statistic for first stage strength, the number of observations, and simple averages of the estimates elasticities.

A larger elasticity for unmarried women with children ('treated' workers) implies that that the spillover effect will be larger on the 'untreated' workers. A larger elasticity for untreated workers implies that spillovers will be larger on the treated workers.

The first line, model 0, is the baseline estimates used in the main text: I use two-step efficient GMM, weighted by the number of wage observations in a cell, using cells with at least five observations, using the baseline set of simulated tax instruments, as discussed in Appendix A.3.1.

The rest of models 1-14 vary some aspect of the empirical specification. Models 1,2 use more observations in the estimations by allowing sparser cells, which makes the elasticities more inelastic. Model 3 estimates the elasticities using two-stage least squares method, which tends to make the estimates more elastic. Models 4,5 use inverse wage variance weighting and no-weights respectively, which tend to make the empirical instrument strength weaker and thus larger elasticities.[15]

In models 6-9, I use different subsets of elasticities, which does not have a large effect on the estimated elasticities but does affect instrument strength. Because I am interacting the endogenous variable with demographic indicators, this is similar to estimating a non-linear model, so in models 10, 11 I use a control function approach. Model 10 uses a linear control function (first stage residual) approach while model 11 uses a cubic polynomial of the control function, but both estimates are effectively the same.

Models 12-15 estimate the elasticities in separate regressions based on parental and marriage status but using the same regression specification. The estimates for women with children are similar to baseline, but the estimates for women without children are much less elastic. Model 16 estimates the OLS relationship and finds near zero of negative labor supply elasticities, potentially due to the simultaneity bias that leads to use the instrumental variables method.

Finally, Models 17-20 use the (log) total number of workers in the labor force as the dependent variable. This measure is more coarse than the hours-per-worker variable that I use but is potentially subject to less measurement error. Because the hours based elasticities include the extensive and any potential intensive margin effects, the supply based elasticities are smaller. See that:

$$\mathsf{d}h_i\ell_i = \mathsf{d}h_i\ell_i + h_i\mathsf{d}\ell_i + \mathsf{d}h_i\mathsf{d}\ell_i \tag{A.61}$$

$$\implies \varepsilon^L = \mu^h + \upsilon^\ell + \xi^{h\cdot\ell}. \tag{A.62}$$

Panel (C) in the table shows estimates of υ^ℓ while the parameter used in the main text and Panels (A) and (B) are ε^L.

In Table A.8, I display alternative estimations for the labor substitution parameter. These

[15]Inverse wage variance weighting would be appropriate with measurement error in wages (Borjas et al., 2012) while unweighted treats sparser cells equally as cells with many observations, which cause bias if there is more measurement error in smaller cells.

Table A.7: Additional Elasticity Specifications
Average within Demographic Groups

Model	Method	Weighting	Sample	IVs	Obs	KP rk F Stat	Total	Unmarried No Children	Unmarried w/ Children	Married No Children	Married w/ Children
(A)	Log Total Hours per Person: Baseline Elasticities used in Main Results										
0	GMM	Wage Obs	5,5	All	47339	40	0.74	0.84	1.04	0.50	0.57
(B)	Log Total Hours per Person										
1	GMM	Wage Obs	0,0	All	67,182	29	0.62	0.76	0.88	0.42	0.42
2	GMM	Wage Obs	3,3	All	57,379	33	0.71	0.79	0.99	0.50	0.55
3	2sls	Wage Obs	5,5	All	47,339	40	0.64	1.03	0.94	0.23	0.36
4	GMM	Inv W sd	5,5	All	47,339	16	1.00	1.16	1.23	0.93	0.66
5	GMM	Unwt	5,5	All	47,381	16	0.79	0.92	0.99	0.68	0.60
6	GMM	Wage Obs	5,5	Age	47,339	12	0.65	0.84	1.06	0.33	0.38
7	GMM	Wage Obs	5,5	Edu	47,339	25	0.78	0.87	1.08	0.52	0.64
8	GMM	Wage Obs	5,5	ATR	47,339	8	0.81	1.19	1.12	0.51	0.43
9	GMM	Wage Obs	5,5	Lite	47,339	21	0.56	0.82	1.00	0.12	0.32
10	CF Linear	Wage Obs	5,5	All	47,339	40	0.68	0.75	0.79	0.65	0.53
11	CF Poly	Wage Obs	5,5	All	47,339	40	0.69	0.76	0.80	0.66	0.54
12	GMM	Wage Obs	K0,M0	All	13,433	14	0.28	0.28	–	–	–
13	GMM	Wage Obs	K0,M1	All	13,623	18	0.58	–	0.58	–	–
14	GMM	Wage Obs	K1,M0	All	7,768	8	0.65	–	–	0.65	–
15	GMM	Wage Obs	K1,M1	All	12,515	16	0.54	–	–	–	0.54
16	OLS	Wage Obs	5,5	–	47,339	–	0.10	0.16	0.21	0.05	-0.05
(C)	Log Total Labor Supply										
17	GMM	Wage Obs	5,5	All	47,339	40	0.46	0.55	0.7	0.21	0.37
18	GMM	Wage Obs	0,0	All	67,178	29	0.53	0.69	0.72	0.3	0.4
19	GMM	Wage Obs	3,3	All	57,379	33	0.5	0.63	0.72	0.26	0.41
20	GMM	Inv W sd	5,5	All	47,339	16	0.67	0.8	0.9	0.41	0.56
21	GMM	Unwt	5,5	All	47,428	16	0.62	0.71	0.76	0.53	0.48
22	OLS	Wage Obs	5,5	–	47,339	–	0.03	0.07	0.11	-0.02	-0.04

Unmarried women not in school full time between the age of 20-55; CPS ORG samples 1990-2000. All regressions same controls as Table 1.2 the main text. I consider combinations of estimation methods (GMM, 2SLS, OLS, Control functions), weighting (by number of wage observations, inverse log wage variance, unweighted), different sample selections ($(\#_a, \#_b)$ refers to $\#_a$ observations in demographic-state-year cell and $\#_b$ wage observations in a skill-state-year cell; (K#, M#) refers to being a parent (K1) or not (K0) and being married (M1) or not (M0)), and of instruments (Age/Edu uses only spillover IVs based on Age/Edu, Tax only uses EITC ATR IVs, Lite uses only EITC ATR and Share w/ EITC IVs – see Section A.3.1).

specifications differ on five dimensions: method / FEs, weighting, sample, IVs, and dependent variable. The table also displays the number of observations and the KP rk Wald F, a cluster robust Cragg-Donald statistic.

Broadly, the overidentified models have lower first stage statistics and the estimates tend to be smaller in magnitude (towards zero). Additionally, the Employment based estimates of ρ tend to be larger than the Hours per Worker specification. This could be for two reasons. Given that $\rho = \mathsf{d}\ln[L_1/L_0]/\mathsf{d}\ln[w_1/w_0]$, either the numerator is larger or the denominator is smaller.

Approximately and using an equilibrium relationship with the supply functions, we can write this as $\rho \approx \frac{\mu_1^h+\upsilon_1^\ell+\xi_1^{h\cdot\ell}}{\mu_0^h+\upsilon_0^\ell+\xi_0^{h\cdot\ell}}$. If $\frac{\mu_1^h+\upsilon_1^\ell+\xi_1^{h\cdot\ell}}{\mu_0^h+\upsilon_0^\ell+\xi_0^{h\cdot\ell}} < \frac{\upsilon_1^\ell}{\upsilon_0^\ell}$, then this implies that the relative hours response is lower for the lower skill workers than the higher skill workers. Another possibility is that new entrant low skill workers work fewer hours than the incumbent workers, so $\xi_0 < 0$.

As pointed out in the main text, the choice of FEs has a first order effect on the estimated elasticity. The baseline specification includes a fixed effect that is the interaction of education and age group indicator variables with year indicators, d_{et}, which is different than the labor supply specification that includes a fixed effect for education, age, marriage status, and parental status indicator interactions without year.[16] I add the year interactions based on the assumed parametric relationship:

$$\frac{L_t^A}{L_t^B} = \left(\frac{w_t^A/\vartheta_t^A}{w_t^B/\vartheta_t^B}\right)^\rho \implies \ln[L_t^A/L_t^B] = \rho\left(\ln[w_t^A/w_t^B] - \ln[\vartheta_t^A/\vartheta_t^B]\right). \tag{A.63}$$

I drop the marriage interaction because this absorbs too much variation.

To see how these choices affect the estimates, models 5-8 use alternative FEs. Models 5,7 use the interaction of education, age group, and marriage indicators, and the estimate seems similar to the main specification except the empirical instrument strength has gone down by an order of magnitude. Models 6,8 interact the above with year indicators, and this appears to raise instrument strength (although still less than baseline) but the estimates make less sense. For example, model 6 has a positive substitution elasticity (statistically indistinguishable from zero); although, model 8

[16]Dropping parental status is done because the substitution elasticity is estimated at the 'skill' level rather than demographic level.

is negative yet about a fourth as large in magnitude. Given that the first stage F statistic goes down, I interpret this as the FEs absorbing needed variation in the instrument.

Table A.8: Additional Elasticity Specifications
Average within Demographic Groups

Model	Method	Weighting	Sample	IVs	Obs	KP rk F Stat	ρ Hours per Worker	ρ Employment
(A)	Baseline in Main Results							
0	2sls	Wage Obs	5	JI-ATR	19,501	67.26	-1.81	-1.75
(B)	Just Identified							
1	2sls	Wage Obs	0	JI-ATR	29,604	63.66	-2.15	-2.00
2	2sls	Wage Obs	3	JI-ATR	25,773	63.61	-2.08	-1.93
3	2sls	Inv W sd	5	JI-ATR	19,501	47.91	-1.57	-1.40
4	2sls	Unwt	5	JI-ATR	19,501	58.03	-1.00	-0.71
5	2sls, FEs 1	Wage Obs	5	JI-ATR	19,501	6.54	-2.15	-3.41
6	2sls, FEs 2	Wage Obs	5	JI-ATR	19,501	21.89	0.15	-0.52
7	2sls, FEs 1	Wage Obs	3	JI-ATR	29,604	5.81	-4.29	-5.34
8	2sls, FEs 2	Wage Obs	3	JI-ATR	29,604	24.05	-0.55	-1.06
9	2sls	Wage Obs	5	JI-Pos	19,501	3.09	-1.83	-2.11
(C)	Over Identified							
10	GMM	Wage Obs	5	OvID	19,501	13.76	-1.57	-1.85
11	2sls	Wage Obs	5	OvID	19,501	13.76	-1.67	-2.06
12	GMM	Wage Obs	0	OvID	29,604	13.93	-2.30	-2.47
13	GMM	Wage Obs	3	OvID	25,773	13.46	-2.18	-2.35
14	GMM	Inv W sd	5	OvID	19,501	6.86	-1.54	-1.99
15	GMM	Unwt	5	OvID	19,501	8.74	-0.61	-0.54
(D)	OLS and Alternate Variable Constructions							
16	OLS	Wage Obs	5	–	19,501	–	0.06	0.01
17	2sls, alt 1	Wage Obs	5	JI-ATR	19,903	59.23	-1.81	-1.73
18	2sls, alt 2	Wage Obs	5	JI-ATR	12,288	88.8	-2.24	-2.20
19	2sls, alt 3	Wage Obs	5	JI-ATR	17,182	81.05	-1.97	-1.97
20	2sls, alt 4	Wage Obs	5	JI-ATR	5,239	61.23	-3.24	-3.19

Unmarried women not in school full time between the age of 20-55; CPS ORG samples 1990-2000. All regressions same controls as Table 1.3 the main text. I consider combinations of estimation methods (GMM, 2SLS, OLS; fixed effects and variable constructions), weighting (by number of wage observations, inverse log wage variance, unweighted), different sample selections ((#) refers to minimum value of the mean number of skill-state-year observations for the numerator and denominator group), and of instruments (Just Identifies using relative EITC ATRs or Share w/ EITC or Overidentified – see Section A.3.1).

A.4.1 Difference in Difference Regressions

To complement the model implied labor supply effects, I estimate a simple difference in difference specification. I use the 1990-1996 ASEC samples for the OBRA expansion and the 2006-2012 samples for the ARRA expansion. I regress an indicator for labor force participation during the previous year on an post indicator (1994-1996 and 2010-2012) times a parental status indicator. I include state-year indicators and demographic group indicators that interact age, education, marriage, parental status. I use robust standard errors clustered at the demographic group level and weight the regressions using the ASEC supplement weights.

In typical EITC DiD studies, one compares unmarried women with no qualifying children to those with qualifying children (Eissa and Liebman, 1996; Eissa and Hoynes, 2004; Bastian, forthcoming). One rationale for this is that unmarried workers who do not work definitely do not receive EITC benefits and these workers are thought to work in similar labor markets. As long as there is no other parental specific time-varying labor market changes around EITC expansions, then this should estimate the average treatment effect on the treated which is a measure of the direct labor supply effects of the EITC. Because the ARRA expansion was most generous specifically for workers with three or more qualifying child, I include two additional specifications. In column (3), I compare workers with no qualifying children to workers with three qualifying children. In column (4), I compare workers with one or two qualifying children to workers with three qualifying children.

Table A.9: EITC Difference-in-Difference Results

	OBRA	ARRA		
	(1)	(2)	(3)	(4)
Post × Parent Status	0.039	0.010	-0.006	-0.011
	(0.010)	(0.007)	(0.014)	(0.013)
Sub-Sample	-	-	$C \in \{0, 3\}$	$C \in \{1, 2, 3\}$
Obs	78,549	119,082	82,826	43,379
Clusters	64	64	64	32

Unmarried women not in school full time between the age of 20-55. All data from March CPS, ASEC samples, 1990-1996 & 2006-2012. All regressions include state-year indicators and demographic group indicators, as in the main text.

A.5 Additional Incidence Results

A.5.1 Individual Level Effects of 1993 Expansion

In Table A.10 I report individual level results rather than aggregate as in the main text. These results show how an individual's EITC amount is affected by incidence and behavioral responses. The change in the EITC is the naive change that holds all labor supply and wages constant. In Panel (A), unmarried mothers get roughly \$417 in expanded EITC but lose roughly a fourth of that amount due to wage incidence. For unmarried mothers, wage spillovers are less important, at roughly 21% of the wage effect, primarily because the direct effects dominate. For married mothers spillovers are 152% of the wage effect, while for women without children spillovers are only 8.4% of the wage effect.

Table A.10: Incidence Results: Individual Effects of 1993 Expansion

EDU	wL (a)	ChEITC (b)	$dw^{PE}L$ (c)	$dw^{GE}L$ (d)	$(dw^{GE}-dw^{PE})L$ (e)	$\frac{dw^{GE}L}{d^{PE}L}-1$ (f)	$\frac{dw^{PE}L}{\text{ChEITC}}$ (g)	$\frac{dw^{GE}L}{\text{ChEITC}}$ (h)
(A)				Unmarried Mothers				
LessHS	10,059	417	-117	-114	2.40	-3.4%	-28.6%	-29.3%
HS	17,637	314	-75	-70	4.70	-7.6%	-13.2%	-14.2%
SomeCol	18,259	260	-46	-41	4.90	-13.6%	-9.7%	-10.8%
BA+	30,936	99	-12	-3	9.30	-81.6%	-0.3%	-1.0%
Total	19,055	273	-60	-54	5.20	-20.8%	-11.7%	-12.7%
(B)				Married Mothers				
LessHS	10,796.2	162	-2.50	0.10	2.60	42.9%	-0.0%	-0.4%
HS	15,367.4	56	1.40	5.50	4.10	75.4%	0.2%	0.1%
SomeCol	19,334.3	35	5.30	10.50	5.20	118.7%	0.4%	0.2%
BA+	31,027.4	10	3.40	12.80	9.40	317.1%	0.1%	0.0%
Total	20,513.8	45	2.80	8.60	5.80	152.0%	0.2%	0.0%
(C)				Women without Children				
LessHS	11,196.2	20	-44	-42	2.60	-8.6%	-8.4%	-8.6%
HS	16,967.1	9	-26	-22	4.40	65.1%	-1.1%	-1.2%
SomeCol	18,859.6	4	-20	-15	4.90	45.3%	-0.7%	-0.8%
BA+	30,888.3	2	-5.30	3.80	9.10	-94.6%	-0.0%	-0.0%
Total	20,880.4	7	-20	-15	5.70	8.4%	-1.3%	-1.4%

All items are average across workers, weighted by hours × sample weights. All data from 1994 March CPS, Women from Tax Units Baseline labor supply elasticities in table 1.2 and $\rho = -1.8$.

A.5.2 EITC vs NIT

In Table A.11, I present an EITC vs Negative Income Tax (NIT) simulation results using the labor supply elasticities from Table 1.2. This exercise compares the main specification of Rothstein (2010), as presented in Table 5, with the general equilibrium effects this paper describes.

In the table below, the 'Rothstein' specification replicates the first column of Table 5 of Rothstein (2010) using my incidence sample (where differences are described in Appendix A.2). For these columns, I use a homogeneous labor supply elasticity of $\varepsilon^L = 0.75$ and the labor substitution elasticity $\rho = -0.3$. The values closely correspond to the values in Rothstein. For example, I calculate a labor effect of \$0.13 for the EITC and $-$\$0.18 for the NIT while Rothstein calculates \$0.09 and $-$\$0.16, respectively.

The next set of columns (D-G) use the estimated labor supply elasticities from Table 1.2 but use the same $\rho = -0.3$. The heterogeneous labor supply elasticity changes the labor supply shocks, which amplifies and attenuates different labor market effects. For example, the EITC wage effects are $-$\$0.42 in column (B) but are only $-$\$0.29 in column (D).

The last set of columns (H-K) use the estimated labor supply elasticities and substitution elasticity from Table 1.2, $\rho = -1.8$. This has a pronounced effect on the PE labor market effects but less on the GE effects. For example, the EITC wage effects are $-$\$0.42 in column (B) but are now only $-$\$0.12 in column (H) but for columns (F) and (J) the effects much closer at $-$\$0.04 and $-$\$0.03.

One noteworthy point is that if Rothstein had used a general equilibrium analysis, then, comparing the differences in columns (D,E) to (F,G), the EITC would have fared far better. First, note that Rothstein primarily used net earnings and transfers with fixed taxes to compare the programs. I have provided the additional columns of net earnings that allow taxes to change (given a fixed average tax rate) and the change in welfare assuming the expansions are revenue neutral.

Evaluating the programs based on Rothstein's criteria, in PE the EITC does worse on both measures, but in GE the measures give a mixed signal. Using the net earnings allowing for tax changes, fares better in both PE and GE. The net earnings for the EITC are always positive while are

Table A.11: Incidence Results:
Aggregate Effects: All Women
Rothstein (2010) Replication & Extension

	Rothstein		$\rho = -0.3$				$\rho = -2.00$			
	"PE"		"PE"		GE		"PE"		GE	
Dollars	EITC	NIT	EITC	NIT	EITC	NIT	EITC	NIT	EITC	NIT
	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)	(J)	(K)
Intended	1.00	0.56	1.00	0.56	1.00	0.56	1.00	0.56	1.00	0.56
Labor	0.13	-0.18	0.09	-0.12	0.24	-0.35	0.22	-0.30	0.27	-0.37
Wage	-0.42	0.60	-0.29	0.42	-0.04	0.06	-0.12	0.17	-0.04	0.05
Gross Earnings	-0.30	0.42	-0.20	0.29	0.20	-0.28	0.10	-0.13	0.23	-0.32
Net Transfer, Fixed Taxes	0.58	1.50	0.71	1.42	0.96	1.06	0.88	1.17	0.96	1.05
Net Earn, Fixed Taxes	0.70	1.42	0.80	1.29	1.20	0.72	1.10	0.87	1.23	0.68
Net Earnings	0.12	-0.35	0.20	-0.46	0.57	-0.99	0.50	-0.87	0.63	-1.04
Fiscal Externality	-0.10	0.05	-0.09	0.04	-0.07	0.02	-0.09	0.04	-0.08	0.03

Units in table are changes in dollars of earnings summed across demographic groups. Note: $Z^G = w \cdot L,\ Z^N = (1-\tau) \cdot w \cdot L$. All data from 1993 March CPS, Women from Tax Units Labor supply elasticities in table 1.2, except 'Rothstein' which uses $\varepsilon^L = 0.75$ for all.

always negative for the NIT expansions. This is because the EITC expands production by bringing new workers into the labor force while the NIT decreases production by having workers leave. For some workers, the NIT drives wages up which causes this group to pay more in taxes, which can cause net earnings to decrease.

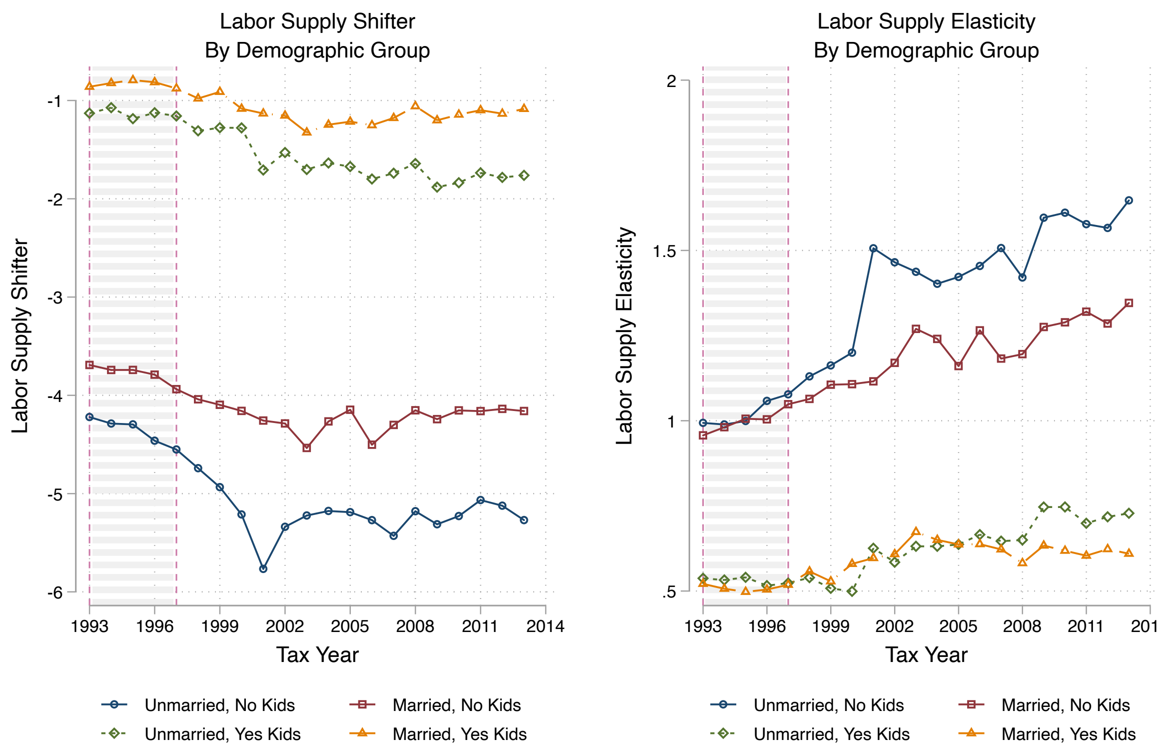
Finally, the welfare changes are always negative for the EITC and either positive or negative for the NIT depending on the parameterization. A negative welfare change here implies that the government expenditure increases (the welfare measure is the 'fiscal externality' – see Section A.1.2.2). For the EITC, the government is spending more because it is paying entering workers more in EITC. For the NIT, the government is spending more because it is paying exiting workers not to work. Balancing these two different reasons for increased government expenditure is a normative question.

A.6 Structural Model Implied Parameters

Using the approach outlined in Section 1.9, I back-out the structural parameters and calculate the model implied elasticities for the out-of-sample period. In Figure A.1 I plot the model implied average labor shifters and average supply elasticity by marriage and parental status over time.

The labor shifters appear to trend downward over time for unmarried women but constant for married women. This implies that the utility cost of labor supply is weakly increasing for unmarried women. For all groups, the elasticities are increasing since the late 1990's. Given equation 1.32, this is largely due to roughly stagnant real net wage growth and declining labor force participation in the 2000's. Together, for unmarried women this implies that the per dollar effectiveness of the EITC relative to the early 1990's is ambiguous, but should be more effective for married women.

Figure A.1: Model Implied Parameters



Supply shifter based on equation 1.33; elasticity based on equation 1.32; parameter β^d recovered from tax years 1993-1997 and estimated elasticities from Table 1.2 and tax and transfer inclusive real net wage.

APPENDIX B

APPENDIX TO CHAPTER TWO

B.1 Additional Data Sources

In Table B.1 I list additional information about State EITC returns and expenditures. Most of this information comes from annual state tax expenditure reports. Some values are estimates, some are listed as exact data, and others are not described in the reports. Several reports state that EITC claims are a high quality data item compared with other items in the reports.

Table B.1: State EITC Returns and Amounts Sources

State	Year	URL	Notes
CA	2018	http://www.dof.ca.gov/Forecasting/Economics/Tax_Expenditure_Reports/documents/Tax_ExpenditureReport_2019-20_B.png	Forecast 1 billion in 2020
CO	2017	www.colorado.gov/pacific/sites/default/files/2019_Annual_Report_1.png	
CT	2018	portal.ct.gov/-/media/DRS/Research/annualreport/DRS-FY19-Annual-Report.png?la=en	
DC	TY 2020	cfo.dc.gov/node/1456456	Estimate
DE	FY 2020	finance.delaware.gov/financial-reports/tax-preference-report/	
HI	TY 2018	files.hawaii.gov/tax/stats/stats/act107_2017/act107_earnedincome_txcredit_2018.png	
IL	TY2017	www2.illinois.gov/rev/research/taxstats/IndIncomeStratifications/Documents/2017-IIT-1040ILReturn-Final.png	
IN	FY 2018	www.in.gov/sba/files/Tax%20Expenditure%20Report%20FY%202018-2021%20Final%20GW.png	Estimate
IA	FY 2018	tax.iowa.gov/sites/default/files/2019-08/Individual%20Income%20Tax%20Report%202017.png	Partial Estimate
KS	TY 2017	www.ksrevenue.org/png/ar19complete.png	
LA	FY 2018	lla.la.gov/PublicReports.nsf/8F85E9838E24E5308625831B00524FF5/$FILE/0001A8EC.png	
ME	FY 2018	www.maine.gov/revenue/research/tax_expenditure_report_17.png	Estimate
MD	FY 2018	dbm.maryland.gov/budget/Documents/operbudget/FiscalYear2018Tax%20ExpenditureReport.png	Includes Montgomery county
MA	FY 2018	www.mass.gov/doc/2020-tax-expenditure-budget/download	
MI	FY 2018	sigma.michigan.gov/EI360TransparencyApp/files/Tax%20Expenditure%20Reports/Tax%20Expenditure%20Report%202018.png	
MN	TY 2017	www.revenue.state.mn.us/minnesota-income-tax-statistics-county	Estimate
MT			Not Yet in Effect
NE	TY 2018	revenue.nebraska.gov/research/statistics/nebraska-statistics-income	Table F2
NJ	TY 2019	www.nj.gov/treasury/taxation/png/taxexpenditurereport2020.png	
NM	TY 2017	realfile.tax.newmexico.gov/ 2018%20NMTRD%20Tax%20Expenditure%20Report.png	
NY	TY 2018	www.tax.ny.gov/research/stats/stat_pit/earned_income_tax_credit/earned_income_tax_credit_analysis_of_credit_claims_open_data_short2.htm	NYS + NYC EITC
OH	TY 2018	www.tax.ohio.gov/tax_analysis/tax_data_series/individual_income/publications_tds_individual/Y1TY18.aspx	
OK	TY 2017	www.ok.gov/tax/documents/Tax%20Expenditure%20Report%202017-2018.png	
OR	TY 2017	www.oregon.gov/dor/programs/gov-research/Pages/research-personal.aspx	Returns are partial year
RI	TY 2018	digitalcommons.uri.edu/cgi/viewcontent.cgi?article=1774&context=srhonorsprog	Estimate
SC	TY 2018	dor.sc.gov/resources-site/publications/Publications/2018-2019_AnnualReport.png	
VT	TY 2018	tax.vermont.gov/sites/tax/files/documents/income_stats_2018_state.png	
VA	2019	www.tax.virginia.gov/sites/default/files/inline-files/2019-annual-report.png	
WI	TY 2018	www.revenue.wi.gov/Pages/RA/IIT-RefundableCredits.aspx	

Year descriptions are either Tax Year, Fiscal Year, or is ambiguous based on language of the state tax agency. I include when the agency declares that values are estimates, but this may not be comprehensive.

B.2 Additional Results

B.2.1 Alternate Specifications

In Table B.2 I report coefficient estimates for alternative specifications for log total federal EITC returns and employment for women with less than a high school degree, using the SBFE and SBRD:L specifications. In column (a), I reproduce the main results from Table 2.3. For column (b), I do not weight the regressions, which changes the interpretation from an individual policy effect to a county policy effect. For column (c), I omit the the state GDP control, which was included as the previous literature finds that state supplement rates are correlated with the variable (Leigh, 2010). Finally, column (d) adds county-specific linear-trends, which is the most aggressive specification.

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

B.2.2 State Border Regression Results

Table B.3 displays the predicted state supplement rates from the following regression:

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

where y is the state supplement rate for the implemented program, T_{sb} is the year the state supplemented is implemented for the border, $1[\text{Two-Sided}]$ is an indicator for an incumbent program is along the border, and D_t are year indicators. I include the year indicators to absorb the general positive trend in state supplement rates.

I use the predicted values rather than coefficients to highlight the difference in magnitude of the one- and two-sided borders over time and compared to each other. This is the same as displaying the coefficients $\{\gamma_v^a\}$ for the one-sided and $\{\gamma_v^a + \gamma_v^b\}$ for the two-sided borders. These are the values (and their clustered standard errors) plotted in Figure 2.2.b in the main text.

To plot the reaction function for Figure 2.2.c, I use only the state borders where there is an incumbent program and look at how the. incumbent program 'reacts' when its neighbor state implements a program. Table B.4 displays the coefficients from the following regression:

$$y_{sbt} = \alpha + \sum_{v \in \mathsf{V}} \gamma_v \cdot 1[t - T_{sb} = v] + \lambda_t + \lambda_s + u_{sbt}, \tag{B.2}$$

Table B.2: Alternate Specifications: Fed Returns and Employment

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

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

where y is the state supplement rate for the incumbent program, T_{sb} is the year the *new* state supplemented is implemented for the border, λ_t and λ_s are year and state FEs respectively. The year and state FEs absorb a general positive trend in state supplement rates by time and age of incumbent programs. Figure 2.2 (**c**) plots the coefficients $\{\gamma_v\}$ and their White standard errors.

Table B.3: State Supplement Rates by Border Status: One- vs Two-sided Borders

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

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

B.2.3 Event Study Regression Results

The following tables underlie the plots in Figure 2.4. Specifically, they are 'stacked' event studies of state EITC supplement introductions between 2000 and 2018. For each empirical design, SBFE

Table B.4: State Supplement Rates by Border Status: One- vs Two-sided Borders

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

White standard errors parentheses. Controls: year and state FEs. Event time is relative to the state implementation year, where the omitted base year is the year before implementation. Samples is all state-borders where the implementing states at least 10 years apart, the implemented supplement activates between 2000-2018, and the implementation is not reversed.

or SBRD, I present three samples: pooled, one-sided, and two-sided. The pooled sample includes all state borders with a state supplement introduced; the one-sided are only those state borders where there is no incumbent program one one side of the border; the two-sided are those where there is an incumbent program when the supplement is introduced.

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

Table B.5: Stacked Event Studies : Log EITC Returns

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

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

Table B.6: Stacked Event Studies : Log Employment: Women, LessHS

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

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

Table B.7: Stacked Event Studies : Log Avg Monthly Earnings: Women, LessHS

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

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

APPENDIX C

APPENDIX TO CHAPTER THREE

C.1 Propositions 1 and 2

Recall that $D_a = \frac{\mathrm{e}^{F(a,y-r(a),y)/\sigma_\epsilon}}{\sum_{a' \in \mathcal{A}} \{\mathrm{e}^{F(a',y-r(a'),y)/\sigma_\epsilon}\}} \cdot M$ from the main text using logit demand. Here, we switch to indexing buildings using j rather than a. To make notation easier, let $\alpha = \frac{\partial F(a,y-r,y)}{\partial r} < 0$ be the (negative) marginal utility of consumption, and set $\sigma_\epsilon = 1$.

C.1.1 Proposition 1

Binding zoning restrictions, by reducing quantities at a plot k, increase rents at that plot. The rest of Proposition 1 will follow as long as plots, as competing products, are strategic complements in pricing decisions.

Definition C.1.1. Strategic Complements: If the cross derivative of a given player's own payoff function with respect to her action and that a rival's action is positive, then the actions are strategic complements.

In our Bertrand oligopoly setting, rents are strategic complements if

$$\frac{\partial^2 \pi_j}{\partial r_j \partial r_k} = \frac{\partial \left[\partial D_j / \partial r_j\right]}{\partial r_k} \cdot \left(r_j - C_j(D_j)\right) + \frac{\partial D_j}{\partial r_j} \cdot \left(-\frac{\partial C_j}{\partial D_j}\frac{\partial D_j}{\partial r_k}\right) + \frac{\partial D_j}{\partial r_k} \geq 0. \tag{C.1}$$

Denote the derivative of marginal cost as $\frac{\partial C_j}{\partial D_j} := c_j$. When we apply Logit demand functions, this becomes:

$$\frac{\partial^2 \pi_j}{\partial r_j \partial r_k} = -\alpha^2 D_j D_k (1 - 2D_j)(r_j - C_j) - c_j \alpha D_j (1 - D_j) - \alpha D_j D_k \tag{C.2}$$

$$= \underbrace{-\alpha D_j D_k}_{>0} \left[\underbrace{\frac{D_j}{(1 - Dj)}}_{>0} + \underbrace{(-c_j \alpha D_j (1 - D_j))}_{>0 \text{ if } c_j > 0} \right]. \tag{C.3}$$

Note, we use the equilibrium relationship that $(r_j - C_j) = -r_j / \varepsilon_j$.

Thus, generally the strategic nature of pricing decisions is ambiguous. A sufficient condition for strategic complements in the logit case is that $c_j \geq 0 \forall j$. This is true with constant marginal costs or diseconomies of scale for the building. With decreasing marginal costs, the strategic complementary of pricing decisions is ambiguous and may vary between pairs of buildings.

If marginal cost is constant, then the rent increase could only be due to an increase in monopoly markups. With variable marginal cost, this the degree that the markup changes is ambiguous. Decreasing marginal costs would push the landowner to expand quantity supplied and travel further down the demand curve, which may lead to a smaller markup per unit but greater profit (and lower rent). On the other hand, increasing marginal costs attenuate the landowner's desire to expand keeping the landowner in a steeper part of the demand curve but with greater marginal costs eating into the markup.

If long as marginal cost is 'locally constant' in equilibrium (i.e., its change is 'small enough'), then we can say buildings are strategic complements in the logit case. Given strategic complements of price strategies, an increase in zoning constrained building k's rent will increase demand for unzoned building j, and increases the price at j accordingly.

If there is sorting; e.g., preference heterogeneity for building attributes, then the relationship is again theoretically ambiguous even with constant marginal cost. Within our Manhattan data, we explore this empirically in Section 3.8.

C.1.2 Proposition 2

A more detailed proof of Proposition 2 follows. First, we prove that when an landlord's parcel ownership concentration increases, the landlord increases the prices at all properties. We apply the framework of Nocke and Schutz (2018b) and Nocke and Schutz (2018a) to calculate the price effect by utilizing the ι-markup of the landlord. The authors use a nested-logit model, but we simplify the result removing the nesting structure.[1]

[1]These results also remove individual heterogeneity in renter preferences in order to take advantage of the IIA property.

We wish to show that in the logit case with non-decreasing marginal cost, $\frac{\partial r_j}{\partial s_f} > 0, \ \forall\ j \in f$, which proves the proposition. Below, we show this in the two product for intuition and then in the general case with arbitrary number of products.

C.1.3 Oligopolist Pricing Equation

First, we show that landowner f chooses a common markup (Nocke and Schutz, 2018a,b). Let each landlord solves the following joint-profit equation:

$$\max_{\{r_j\}_{j\in f}} \sum_{j\in f} r_j D_j - C_j(D_j). \tag{C.4}$$

Following the insight from Nocke and Schutz (2018b), the first order for each property satisfies:

$$\left(r_j - \frac{\partial C_j}{\partial D_j}\right) = \frac{-1}{\alpha} + \pi_f = \frac{-1}{\alpha(1-s_f)}. \tag{C.5}$$

We can rearrange C.5 to solve for rent:

$$r_j = \frac{\partial C_j}{\partial D_j} - \frac{1}{\alpha(1-s_f)} > 0, \tag{C.6}$$

where marginal cost is positive to yield an upward sloping supply curve. Denote marginal cost as $\frac{\partial C_j}{\partial D_j} = c_j$. We will assume that its derivative is positive: $\tilde{c}_j := \frac{\partial c_\ell}{\partial D_\ell} \geq 0, \ \forall \ell \in J$.[2]

C.1.4 Two Product Case

Recall again that under logit demand:

$$\frac{\partial D_j}{\partial r_j} = \alpha D_j(1-D_j) < 0 \tag{C.7}$$

$$\frac{\partial D_k}{\partial r_j} = -\alpha D_j D_k > 0 \tag{C.8}$$

[2] A micro-foundation is that the residential space production function is concave in inputs which implies that the cost function in convex in quantity; hence, marginal cost is non-decreasing in quantity.

Price Effects:

$$r_j = \frac{-1}{\alpha(1-s_f)} + c_j(D_j) \tag{C.9}$$

$$\implies \frac{\partial r_j}{\partial s_f} = \frac{-1}{\alpha(1-s_f)^2} + \frac{\partial c_j}{\partial D_j}\left(\frac{\partial D_j}{\partial r_j}\frac{\partial r_j}{\partial s_f} + \frac{\partial D_j}{\partial r_j}\frac{\partial r_k}{\partial s_f}\right) \tag{C.10}$$

by symmetry

$$\frac{\partial r_j}{\partial s_f} = \frac{\frac{-1}{\alpha(1-s_f)^2} + \frac{\partial c_j}{\partial D_j}\frac{\partial D_j}{\partial r_k}\left[\frac{\frac{-1}{\alpha(1-s_f)^2} + \frac{\partial c_k}{\partial D_k}\frac{\partial D_k}{\partial r_j}\frac{\partial r_j}{\partial s_f}}{\left(1-\frac{\partial c_k}{\partial D_k}\frac{\partial D_k}{\partial r_k}\right)}\right]}{\left(1-\frac{\partial c_j}{\partial D_j}\frac{\partial D_j}{\partial r_j}\right)} \tag{C.11}$$

$$= \frac{-1}{\alpha(1-s_f)^2}\left[\frac{1-\frac{\partial c_k}{\partial D_k}\frac{\partial D_k}{\partial r_k} + \frac{\partial c_j}{\partial D_j}\frac{\partial D_j}{\partial r_k}}{\left(1-\frac{\partial c_k}{\partial D_k}\frac{\partial D_k}{\partial r_k}\right)\left(1-\frac{\partial c_j}{\partial D_j}\frac{\partial D_j}{\partial r_j}\right) - \left(\frac{\partial c_j}{\partial D_j}\frac{\partial D_j}{\partial r_k}\right)\left(\frac{\partial c_k}{\partial D_k}\frac{\partial D_k}{\partial r_j}\right)}\right] \tag{C.12}$$

imposing Logit

$$= \frac{-1}{\alpha(1-s_f)^2}\left[\frac{1-\frac{\partial c_k}{\partial D_k}\frac{\partial D_k}{\partial r_k} + \frac{\partial c_j}{\partial D_j}\frac{\partial D_j}{\partial r_k}}{1-\frac{\partial c_k}{\partial D_k}\frac{\partial D_k}{\partial r_k} - \frac{\partial c_j}{\partial D_j}\frac{\partial D_j}{\partial r_j} - \frac{\partial c_j}{\partial D_j}\frac{\partial c_k}{\partial D_k}\frac{\partial D_k}{\partial r_j}\alpha(1-s_f)}\right] > 0 \tag{C.13}$$

C.1.5 General Product Case

Note that we have the following:

$$[r_i] = [\Gamma(s_f)\cdot 1_f] + [c_i(D_i)] \tag{C.14}$$

$$\mathsf{D}_{s_f} r = \left[\Gamma'(s_f)\cdot 1_f\right] + \mathsf{D}_D c \cdot \mathsf{D}_r D \cdot \mathsf{D}_{s_f} r \tag{C.15}$$

$$\implies \mathsf{D}_{s_f} r \cdot [\mathbb{I} - \mathsf{D}_D c \cdot \mathsf{D}_r D] = \left[\Gamma'(s_f)\cdot 1_f\right] \tag{C.16}$$

$$\implies \mathsf{D}_{s_f} r = [\mathbb{I} - \mathsf{D}_D c \cdot \mathsf{D}_r D]^{-1} \cdot \left[\Gamma'(s_f)\cdot 1_f\right] \tag{C.17}$$

C.1.5.1 Definitions and Lemmas

Definition C.1.2. Strictly (Row) Diagonally Dominant : for every row, i, the element along the diagonal, a_{ii}, is greater in magnitude than the sum of the magnitudes of each non-diagonal element in the row $a_{i,j},\ j \neq i$. That is, $|a_{i,i}| > \sum_{j \neq i} |a_{i,j}|$.

Definition C.1.3. $\mathtt{Z}$-matrix : a matrix whose off-diagonal entries are less than or equal to zero.

Definition C.1.4. $\mathtt{M}$-matrix : a $\mathtt{Z}$-matrix where every real eigenvalue of A is positive.

Lemma 1. *If A is a $\mathtt{Z}$-matrix that is strictly diagonally dominant, then A is an $\mathtt{M}$-matrix by* Gershgorin Circle Theorem.

Lemma 2. *If A is an $\mathtt{M}$-matrix with positive diagonals and negative off diagonals, then $B = A^{\neg 1}$ is monotone positive; i.e., $b_{ij} > 0,\ \forall\, i, j$; proof in Fan 1958.*

C.1.5.2 General Case Proof

We need to show that the lemma holds and that the vector $B \cdot \Gamma'(s)$ is a monotone positive vector.
Let $[\mathbb{I} - \mathsf{D}_D c \cdot \mathsf{D}_r D] = A$.
First, see that A is (a) a $\mathtt{Z}$-matrix that is (b) Strictly (Row) Diagonally Dominant :

(a) for each row, using logit demand, we have

$$a_{i,i} = 1 - \tilde{c}_i \alpha D_i (1 - D_i) > 0 \tag{C.18}$$

$$a_{i,j} = \tilde{c}_i \alpha D_i D_j < 0 \tag{C.19}$$

(b) plug into definition of (row) diagonally dominant

$$\implies 1 + \tilde{c}_i |\alpha| D_i (1 - D_i) > \sum_{j \in f \setminus i} \tilde{c}_i |\alpha| D_i D_j = \tilde{c}_i |\alpha| D_i \sum_{j \in f \setminus i} D_j \tag{C.20}$$

$$\implies 1 + \tilde{c}_i |\alpha| D_i > \tilde{c}_i |\alpha| D_i \cdot s_f. \tag{C.21}$$

Thus A satisfies lemma 2, so B is a monotone positive matrix.
Second, $\Gamma'(s_f) = \frac{\mathsf{d}}{\mathsf{d}s_f} \frac{-1}{\alpha(1-s_f)} = \frac{-1}{\alpha(1-s_f)^2} > 0$.

Thus as $B \cdot \Gamma'(s_f)$ is a series of multiplication and addition of positive numbers, so $\mathsf{D}_{s_f} r$ must be a monotone positive vector.

C.2 Separate Developer and Landlord Decisions

The standard assumption in the urban literature is that a competitive construction sector purchases land to produce urban space that is then put on the rental market (or sold to initial owners). We have modeled the choice environment as landowners producing the urban space they provide to the rental market. In this section, we show that under the assumption of competitive construction and the existence of owners of differentiated land that our model leads to the same allocation. This implies that the standard assumptions imply that urban space is constrained. We show this in the horizontal sorting case.

Consider a developer who as *already* purchased land from a land-owner and must now decide how much urban space to provide to the rental market. The construction firms are price takers in factors and space, but can make a quantity choice. We consider the dual builder's problem of maximizing location conditional profit or minimizing costs subject to a level of demand by choosing labor and capital:

$$\max_{k,h}\{r \cdot q_j(k,h) - ik - wh\} \iff \min_{k,h}\{ik + wh \text{ s.t. } q_j(k,h) = d_j(r)\}$$

Given that these are dual problems, they each yield the same solution. Let's consider the cost minimization problem's solution of a building cost $B_j(r, d_j(r))$. With free entry, $\pi_j = r \cdot d_j(r) - B_j(r, d_j(r)) \geq 0$. This provides the builder's solution if the builder buys the right to develop location $j \in J$. The builder will develop a plan for each $j \in J$ and seeks to purchase land from land-owners.

Now, we must consider how land-owners set the price of land, r_j. Clearly, $r_j = \pi_j$, else another developer would bid up the price. This creates an open bid auction for each location, so the land price must also be bid up to the highest potential location profit, which is the monopoly location profit. Suppose a builder decides to set rent at cost and provide enough space to clear the market, then this builder must bid $\pi^{ce} = 0$. Another builder decides to reduce space and increase rent to clear the market, and so bids $\pi^m > 0$. The land-owner will choose the second bidder.

Here, free entry into the construction sector creates the incentives to engage in monopolistic behavior in the rental market when there is downward sloping demand. If urban space was viewed as homogeneous by renters, then developers would not be able to adjust market rents and space and make profits since all renters would have the same willingness to pay.

C.3 Detailed Construction of Samples

Here, we discuss the exact steps in sample construction. Recall, the samples we use in the paper are as following:

- 2008-2015 NYC: Ownership matched, unconstrained;
- 2010 Manhattan: IV, Estimation, Unconstrained, New Unconstrained.

C.3.1 2008-2015 NYC

We begin with all buildings in NYC, and then drop buildings based on:

1. missing location information, plots that are under construction, vacant, or are parks;
2. residential area is zero, there are zero residential units, or market values equal zero;
3. plots where the building is not classified as a private rental building (i.e., we drop owner occupied single family residences, condominium and cooperative buildings, 100% publicly owned buildings, any remaining commercially classified buildings, buildings designated as land-marks);
4. missing building characteristic information;
5. building has less than four units.

Next, we link this sample to the MDRC files that link reported building owners to shareholders using the BBL building identifiers. We then test if the reported building owner name matched the MDRC owner name (the owning entity, not shareholders) using a fuzzy string matching algorithm.

This results in a match rate of roughly 80% for each year. We drop buildings that do not match.[3] Using this matched group, we then calculate HHI and leave-out HHI measures.

Finally, we arrive at our HHI Estimation sample by dropping buildings that

1. have over 10% of units rent stabilized;
2. are zoning constrained;
3. are mixed-use.

This yields the same that is in Table 3.2.

In Table C.1 we present summary statistics for the HHI data.

C.3.2 2010 Manhattan

We begin with all buildings in Manhattan, and then drop buildings based on:

1. missing location information, plots that are under construction, vacant, or are parks;
2. residential area is zero, there are zero residential units, or market values equal zero;
3. plots where the building is not classified as a private rental building (i.e., we drop owner occupied single family residences, condominium and cooperative buildings, 100% publicly owned buildings, any remaining commercially classified buildings, buildings designated as land-marks);
4. missing building characteristic information;
5. building has less than four units.

To arrive at the estimation sample, we drop buildings where

1. there is positive commercial building area;
2. the census tracts has fewer than 3 remaining buildings;

This set of buildings constitutes the estimation sample on which we estimate the model.

[3]We believe matching failures happen primarily for two reasons. First, there does not seem to be oversight of the ownership registrations so misspellings are common. Second, the MDRC is a snap-shot that does not save information across years or transactions, so it is possible that a building owner changes and it is not recorded when we have access to the files.

Table C.1: Summary Stats:
2008-2015 NYC Unconstrained Rental Buildings

	Bronx	Brooklyn	Manhattan	Queens
	Tract Level			
$\mathsf{HHI}_{g,t}$	0.24	0.21	0.22	0.33
	Building Level			
Owner Share in Tract	11%	5%	8%	3%
Leave-Out HHI in Tract	0.13	0.07	0.11	0.06
Median Monthly Rent	$1,046	$961	$1,813	$925
Median Rent by Median Income	25%	23%	43%	22%
Median Monthly Land Value per Unit	$205	$250	$2,270	$222
Res.Units per Building	33.5	15.5	25.9	11.4
Years Since Construction	81	83	88	72
Years Since Renovation	46	65	36	69
log(Distance CBD)	2.36	1.41	1.53	1.75
log(Distance Subway)	-1.53	-1.69	-1.95	-1.60
Avg Unit Sqft	1004	954	1,031	901
Buildings	1,792	7,621	2,531	1,773

Note: Building data from PLUTO, NPV, FAR, MDRC files. Census tract HHI defined using shares in equation 3.8. Owner share in tract is building level average. Leave-out building HHI defined using adjusted shares in equation 3.9. All dollar values nominal, 2008-2015. Median income in 2010 for NYC is $ 50,711, used for all years. Building data from PLUTO, NPV, and FAR files. Monthly rental income is building income divided by total units divided by 12. Median income in 2010 for NYC is $ 50,711. Monthly land value per unit is [Land Value / (12 x Residential Units)]. Years since construction and renovation equal 2010 minus the construction year and most recent major renovation year. Geodesic distances are in log miles based on building (lat,lon) coordinates. Avg Unit Sqft is total building area divided by total units.

We drop buildings with commercial area – mixed use buildings – because we cannot be sure that we area measuring average residential rents as we cannot separate commercial and tenant income sources. As noted earlier, this is not the same as treating these buildings as outside goods for the model. Utility parameters are identified under the assumption that the parameters do not depend on whether the building has commercial space.[4]

We arrive at the 2010 Unconstrained Manhattan samples by dropping buildings that

[4]Unreported monte carlo tests show that under the assumptions of the model, parameters remain unbiased. At worst, we believe the model is less efficiently estimated due to smaller samples.

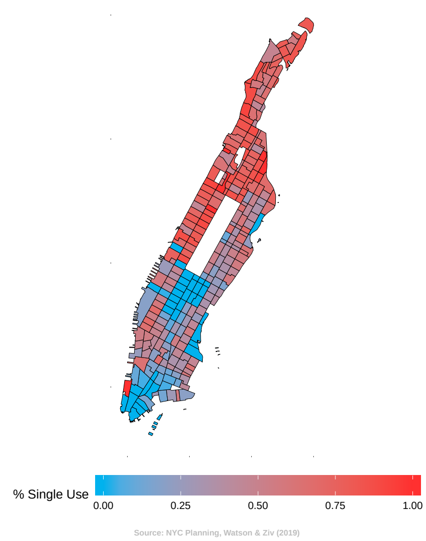
1. have over 10% of units rent stabilized;
2. are zoning constrained;
3. are mixed-use.

Finally, the 2010 New Unconstrained Manhattan / NYC sample subsets this by dropping buildings built before 2000. Summary statistics for the 2010 Manhattan samples are in Table 3.1.

C.3.3 Spatial Distribution of Single Use, Zoning Constrained, & Rent Control

In Figures C.1 and C.2, we plot the spatial distribution of building use status, zoning constrained status, and rent control status. We define a building as being mixed use if we observe positive commercial space in the building; else, single use. Commercial space includes retail space, office space, or (for a minority of buildings) industrial space. For mixed use buildings, we cannot differentiate commercial versus residential sources of building income.

Figure C.1: Distribution of Building Use in Manhattan



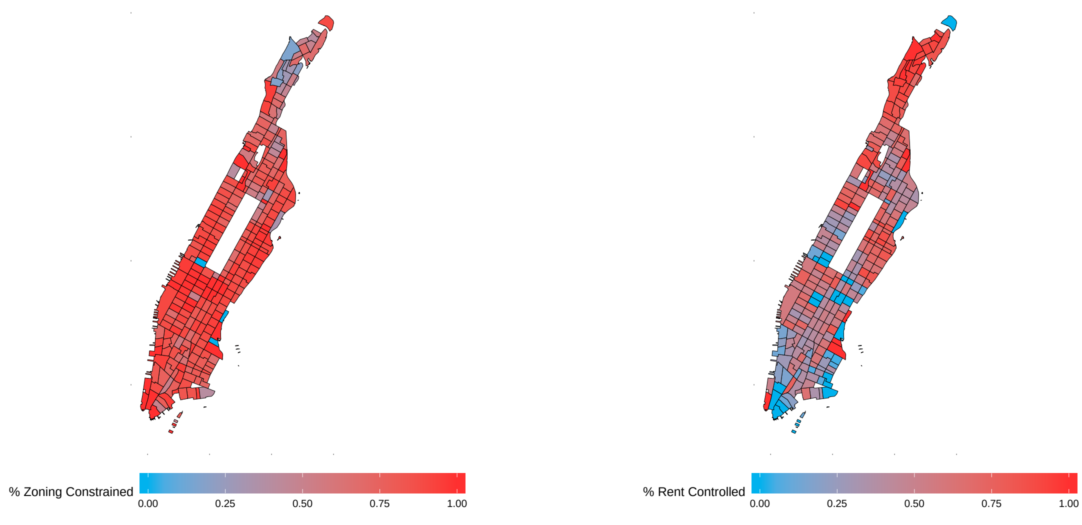
5

For figure C.2, a building is considered zoning constrained if the landlord could not legally add another unit at the minimum legally allowed area without affecting existing building units. Within our data we able to observe that whether a building's Floor Area Ration (FAR_j) is below its maximum allowable FAR (MaxFAR_j). A building can be below its MaxFAR but still zoning constrained if (MaxFAR_j) $-$ FAR_j) is less than the minimum allowable unit FAR, meaning a landlord cannot legally add an additional unit. Thus, a building is zoning constrained if (1) $(\text{MaxFAR}_j) - \text{FAR}_j) \leq 0$ or (2) $(\text{MaxFAR}_j) - \text{FAR}_j) \leq$ (Legal Min Unit FAR). We find that while 80% of rental buildings are zoning constrained only 30% are constrained due to (1).[6] This potentially implies that developers incorporate zoning constraints, which if binding would limit revenues, by building larger units that may attract higher income renters.

Finally, in figure C.2, we plot the spatial distribution of rent controlled buildings. We define rent controlled status by whether a building is on the 2012 NYC Department of Homes and Community Building Registration File. A building is on this list if the building has at least one unit that is rent controlled or rent stabilized. Being rent controlled implies that a landlord is not in complete control of unit pricing, so to some extent the landlord is constrained.

Figure C.2: Distribution of Zoning Constraints and Rent Stabilization in Manhattan

(a) Zoning Constraints (b) Rent Stabilization



Note: Panel (a) plots by Census tract the percent of buildings that are zoning constrained. Panel (b) plots, by Census tract, the percent of buildings that are rent stabilized. The data is 2010 Manhattan residential buildings with 4+ units. Zoning constrained is defined as building being legally not allowed to add one minimum size residential unit based on floor-area-ratios. A building is rent stabilized if more than 10% of building units are rent stabilized.

[6]For single-use buildings this is 81.7% and 34.2% and for mixed-use buildings this is 79.2% and 25.8%, respectively.

C.4 HHI and Ownership Matching

C.4.1 Ownership Matching

Here we describe how we match buildings to owner groups. This procedure is necessary because a large portion of reported rental building owners are a corporate entity that is itself owned a holding company.[7] Thus the reported ownership structure underestimates the degree of common ownership. The NYC Department of Housing Preservation and Development (HPD) requires that building owners register each building with multiple dwellings (or inhabited by non-family members) and compiles this registration list to create the Multiple Dwelling Registry and Contacts (MDRC). Importantly, the MDRC assigns a unique ID to each building-owner pair and for each owner lists the names of the main shareholders of the corporate owner or partnership. Building owners must re-register annually so the list updates annually. Thus we have a list of buildings with their corporate owner names and shareholder names.[8]

However, we face two data challenges in matching buildings to owners using the MDRC. First, we only have MDRC lists for three years: 2012, 2015, and 2020. Second, the MDRC does not link buildings by common owners. We deal with each in turn.

To create a building owner panel, we append the three MDRC annual files together and ‘back-fill’ the ownership from MDRC information for missing years. That is, if we observe a building-owner pair for year 2020, then we assume the owner is the same from 2020, 2019, 2018, and so on.[9] We then merge this with our DOF/PLUTO building year panel of rental buildings. Finally, we use a text matching procedure to ensure that the reported building corporate owner matches the MDRC corporate owner name.[10] Table C.2 reports the match rate for the main four boroughs by year used

[7]We speak loosely with the terms ’corporate entity’ and ’holding company’; some building owners are literally a corporation while others are limited liability companies, sole proprietorship, partnerships, or cooperatives.

[8]We arrange the shareholder names based on frequency. For example, if name A is associated with 5 buildings and name B with 4 buildings, then for any set of buildings with both names $\{A, B\}$ we designate name A as the primary name.

[9]We find that the 2015 file matches better to years 2016 and 2017 than back-filling the 2020 file, so we extend the 2015 file two years as well as back fill 2014 and 2013.

[10]We use the Stata command `matchit` with a threshold of 0.5.

in the rent sample.

Table C.2: Match Rate Across Boroughs

	BK	BX	MN	QN
2008	0.79	0.82	0.81	0.80
2009	0.80	0.83	0.83	0.81
2010	0.83	0.86	0.86	0.84
2011	0.83	0.87	0.87	0.84
2012	0.84	0.89	0.87	0.85
2013	0.85	0.88	0.87	0.85
2014	0.84	0.89	0.87	0.84
2015	0.84	0.88	0.87	0.84

Note: 2008-2015 NYC residential buildings with 4+ units. Data from DOF, PLUTO, MDRC files. Match rate between reported owner from PLUTO & FAR and MDRC owner name.

To find all buildings that have common shareholders, we again perform a text matching procedure. We perform this procedure for each tract-year pair in the four main boroughs of NYC for three sets of shareholder names. The first is matching the primary shareholder, the second is matching the primary and secondary shareholders, and the third is matching across all shareholders. Using only the first shareholder name is the most conservative measure of common ownership and is the one with the least expected errors.[11] For any building that does not match to the MDRC, we use the reported ownername (usually a corporate entity) and require an exact string match within the tract-year.[12]

To get a sense of the scale of the issue. For Manhattan rental buildings, we find that the average number of distinct owner groups ('landlords') in a tract-year are 48.6 using the reported ownership structure and 34.8 using the MDRC matched ownership structure. For the same set of buildings, we find that within a census tract the average landlord owns 3 buildings when we use the reported ownership structure and 4.3 buildings when we use the MDRC matched ownership structure Table C.3 reports these values by year for Manhattan and the other three major boroughs.

[11]We again use the Stata command `matchit` but increase the match threshold to 0.55 for primary name matching and to 0.6 for the multi-name matching. As the length of a string increases, the fuzzy text matching procedure is more likely to find false-positive matches.

[12]We use an exact matching because our fuzzy string matching procedure cannot tell the difference between corporate names of the form 555 Street LLC and 554 Street LLC.

Table C.3: Difference Between Reported and MDRCC Common Ownership

	Manhattan				Brooklyn, Bronx, Queens			
	Distinct Owners		Avg Bld per Owner		Distinct Owners		Avg Bld per Owner	
	MDRC	Reported	MDRC	Reported	MDRC	Reported	MDRC	Reported
2008	34.2	46.9	4.3	3.0	20.9	24.4	2.5	2.1
2009	34.6	47.8	4.3	3.1	21.1	24.7	2.5	2.1
2010	34.8	48.1	4.2	3.1	21.3	25	2.5	2.1
2011	35.0	48.5	4.3	3.0	21.5	25.2	2.5	2.1
2012	35.3	49.2	4.3	3.0	21.6	25.4	2.5	2.1
2013	35.3	49.4	4.4	3.0	21.8	25.6	2.5	2.1
2014	34.7	49.4	4.3	3.0	21.6	25.7	2.5	2.1
2015	34.8	49.4	4.2	3	21.6	25.7	2.5	2.1

Note: 2008-2015 NYC residential buildings with 4+ units. Data from DOF, PLUTO, MDRC files. Comparison between reported owners in PLUTO & FAR versus MDRC files. Owners matched within tract-years.

C.4.2 Additional HHI Results

In this section, we probe robustness to our results in Section 3.5 using two alternative specifications. First, we replace the leave-one-out HHI variable $HHI_{f(j),g,t}$, which calculates for each building, the concentration index at the tract level excluding the building's landowner's own buildings, with the tract-level variable $HHI_{g,t}$, which more simply calculates the total tract-level concentration. Results are largely similar to our main specification, although the point estimates are slightly attenuated.

Second, we explore an alternative specification where price-per-square-foot rather than total rent is the building-level outcome variable. Accordingly, in this specification, total square feet is no longer a control. Results are broadly slimiar to our main specification.

Table C.4: The Relationship Between Aggregate Ownership Concentration and Prices

	(1)	(2)	(3)	(4)	(5)	(6)
			ln[Average $r_{j,g,t}$]			
			Panel (A): Manhattan			
$\ln[\mathrm{HHI}_{g,t}]$	-0.012	0.161	0.075	0.009	0.162	0.075
	(0.032)	(0.080)	(0.076)	(0.038)	(0.076)	(0.076)
$\ln[s_{g,t}^{f(j)}]$				-0.028	0.002	-0.013
				(0.026)	(0.025)	(0.027)
Year FEs	Y	Y	Y	Y	Y	Y
Tract FEs	N	Y	N	N	Y	N
Building FEs	N	N	Y	N	N	Y
Observations	2,519	2,504	2,393	2,519	2,504	2,393
R^2	0.29	0.63	0.75	0.29	0.63	0.75
			Panel (B): Bronx, Brooklyn, Manhattan, Queens			
$\ln[\mathrm{HHI}_{g,t}]$	0.053	0.092	0.076	0.047	0.094	0.079
	(0.016)	(0.076)	(0.039)	(0.019)	(0.076)	(0.039)
$\ln[s_{g,t}^{f(j)}]$				0.007	-0.005	-0.038
				(0.014)	(0.013)	(0.014)
Borough-year FEs	Y	N	N	Y	N	N
Tract and year FEs	N	Y	N	N	Y	N
Building and year FEs	N	N	Y	N	N	Y
Observations	13,669	13,592	12,758	13,669	13,592	12,758
R^2	0.4	0.64	0.77	0.40	0.64	0.77

Note: The table replicates the results of Table 3.2 using tract-level HHI measures $HHI_{g,t}$, instead of the leave-one-out HHI, $HHI_{f(j),g,t}$. Otherwise, controls and specifications match Table 3.2. Standard errors clustered two ways by Census tract and year.

C.5 Detailed Construction of Average Building Rent

Recovering building average unit rents is a key feature of this analysis that relies on three facts. First, by law, the DOF assesses rental buildings based on their income generation. For single-use, residential rental buildings, this corresponds to the rent paid to landlords. For mixed-use rental buildings, we cannot separate the source of income between commercial and residental tenants.

Table C.5: The Relationship Between Ownership Concentration and Price per Square Foot

	(1)	(2)	(3)	(4)	(5)	(6)
	$\ln[(\text{Building } r_{j,g,t})/(\text{Building Square Feet})]$					
	Panel (A): Manhattan					
$\ln[\text{HHI}_{f(j),g,t}]$	-0.049	0.210	0.130	-0.012	0.206	0.158
	(0.038)	(0.097)	(0.094)	(0.050)	(0.094)	(0.098)
$\ln[s_{g,t}^{f(j)}]$				-0.046	-0.006	-0.015
				(0.033)	(0.025)	(0.037)
Year FEs	Y	Y	Y	Y	Y	Y
Tract FEs	N	Y	N	N	Y	N
Building FEs	N	N	Y	N	N	Y
Observations	2,517	2,502	2,392	2,517	2,502	2,392
R^2	0.27	0.65	0.74	0.28	0.65	0.75
	Panel (B): Bronx, Brooklyn, Manhattan, Queens					
$\ln[\text{HHI}_{f(j),g,t}]$	0.035	0.163	0.139	0.036	0.164	0.133
	(0.023)	(0.072)	(0.050)	(0.023)	(0.069)	(0.050)
$\ln[s_{g,t}^{f(j)}]$				-0.002	0.001	-0.035
				(0.017)	(0.014)	(0.018)
Borough-year FEs	Y	N	N	Y	N	N
Tract and year FEs	N	Y	N	N	Y	N
Building and year FEs	N	N	Y	N	N	Y
Observations	13,646	13,572	12,738	13,646	13,572	12,738
R^2	0.28	0.59	0.72	0.28	0.59	0.73

Note: The table replicates the results of Table 3.2 using rent per square foot as the dependent variable and omitting the total square foot variable as a control. Otherwise, controls and specifications match Table 3.2. Standard errors clustered two ways by Census tract and year.

This leads to our sample restriction of single-use buildings in our estimations.

Second, we use the web-scraped NPV data. We believe the NPV data is high quality because it is based on communications with owners who have a financial stake in ensuring the information is correct. However, because we rely on a third party's efforts in web-scraping, we must deal with the fact that the third party did not collect information on all buildings. Primarily, the web-scraped data does not include any building with 4 or 5 units and is randomly missing others.

To remedy this, we rely on the third fact. The DOF uses building income data in its assessment process to derive "market value" which is then used for property taxes. Specifically, the DOF calculates market value using the following formula:

$$\text{MarketValue}_j = \text{GIM}_j \cdot \mathsf{Avg}\,(\text{Annual Rent})_j \cdot \text{units}_j, \tag{C.22}$$

where the Gross Income Multiplier (GIM) is determined by the DOF based on the building's market value per square foot and its location.

Since we observe market value for all buildings in the FAR dataset, we can use the buildings that overlap the NPV data to backout the the function $\text{GIM}_j = \text{G}(\frac{MV_j}{SQFT_j}, \text{Units} \geq 10, \text{borough}, \text{year})$. We estimate the GIM function via the following:

1. For the matched set, divide market value by income to recover GIM_j;
2. Calculate market value by square feet (mvsqft);
3. By borough and year, calculate the 50-point quantiles of mvsqft;
4. By borough, year, and large building status (units ≥ 10), find the average GIM_j – $\mathsf{Avg}(\text{GIM} \mid \text{B,Y,U>10})$;
5. For the set of buildings that are not in the matched set, calculate $\frac{MV_j}{\mathsf{Avg}(\text{GIM}|\text{B,Y,U>10})} = \hat{Y}_j$.

We use the reported value Y_j for the matched buildings and $\hat{Y}_j$ for the unmatched buildings.

C.5.1 Additional Information

The income data is ultimately sourced from the Real Property Income and Expense (RPIE) statements that all income generating property owners are required to file annually and face financial penalties for not filing. Nevertheless, not all property owners will file this report. If an owner does not file, the DOF has the right to assign a market value based on its best judgement. In addition, the DOF documentation says that they will adjust report amounts that seem extreme; e.g., a building reporting high costs and no income in an area where other buildings are report incomes above costs.

Without access to the RPIE statements, it is not possible to determine which properties have been adjusted.

The DOF Assessment Guidelines show how Income and Market Value relate to each other and how one can be directly inferred using the other. In the table below, we describe the DOF mapping that goes from observed income to market value: $G: Y \times SqFt \rightarrow M$.

Table C.6: Example Mapping of Market Value to Income

y	GIM_{Low}	GIM_{High}	m	Y_j
$[y_1, y_2]$	$\frac{m_1}{y_1}$	$\frac{m_2}{y_2}$	$[m_1, m_2]$	$= MV_j \cdot \frac{y_2}{m_2}$
$[y_2, y_3]$	-	$\frac{m_3}{y_3}$	$[m_2, m_3]$	$= MV_j \cdot \frac{y_3}{m_3}$
$[y_3, y_4]$	-	$\frac{m_4}{y_4}$	$[m_3, m_4]$	$= MV_j \cdot \frac{y_4}{m_4}$

Note: This table provides a simplified example of the Gross Income Multiplier (GIM) method used by the NY DOF that we utilize to infer building income from observed building market value. For 80% of our multi-year sample, we observe both market value and income, which we use to estimate the GIM for the remaining properties, as described in the main text.

C.5.1.1 Robustness of Calculations

We can check the robustness of our calculations by using an auxilary dataset by the DOF, the Condo/Coop Comparable Rental Income data. By law, condominium buildings must be valued for tax purposes as-if they were rental buildings. To accomplish this, the DOF matches condominiums with rental properties and calculates and expected, market value and income of the condominiums. They publish these comparisons and include the rental building income and market value used in the comparisons. Thus, we are able to check our results for the matched buildings. Our values are nearly identical except for inconsistent rounding behavior on the part of the NYC DOF, typically in the owner's favor.[13]

[13]For Manhattan, we are able to check against 1,883 rental buildings, and we find 83 buildings where the absolute difference between our assigned GIM and the empirical ratio of market value to income is greater than 0.1; this represents an error rate around 4% of buildings. Again, these errors are due to inconsistent behavior by the NYC DOF.

C.6 BLP Inversion Step

For intuition, if we omit the random coefficients, then the model becomes a standard logit specification using grouped data. Berry (1994) shows that the mean utility can be solved for in closed form as:

$$\ln[s_j] - \ln[s_0] = \delta_j + X_j\beta + \alpha r_j. \tag{C.23}$$

One can use a linear 2SLS specification to estimate $\{\alpha, \beta\}$.

With random coefficients, the above does not work. However, BLP show that the following is a contraction mapping algorithm guaranteed to converge:

$$\mu_j^{r+1} = \mu_j^r + \left(\ln[s_j] - \ln[D_j(\mu_j^r;\theta)]\right) \ ,\forall j. \tag{C.24}$$

When $\|\mu_j^{r+1} - \mu_j^r\|_\infty \approx 0$ the algorithm has converged.[14] For the nested logit case, Grigolon and Verboven (2014) show that the following modification is also a contraction mapping and necessary:

$$\mu_j^{r+1} = \mu_j^r + \left(\ln[s_j] - (1-\rho)\ln[D_j(\mu_j^r;\theta)]\right) \ ,\forall j. \tag{C.25}$$

Once μ is recovered, then we can use the model's moment conditions to estimate $\{\beta, \alpha, \gamma\}$.

C.7 Instrument Construction

We use "Quadratic Differentiation Instruments," based on Gandhi and Houde (2018), with a spatial radius, as in Bayer et al. (2004, 2007). For the Nested Logit specifications, we create within nest differentiation instruments that exclude rivals in the same Census block-group. These instruments are meant to be *an* approximation to the optimal instruments in the sense of Amemiya (1977) and Chamberlain (1987).[15]

The 'true' optimal instruments are based on the partial derivative of the structural error term:

$$Z^{\mathsf{opt}} = \mathsf{Var}(\delta_j)^{-1} \cdot \mathsf{E}\left[\frac{\partial \delta_j}{\partial \beta} \quad \frac{\partial \delta_j}{\partial \alpha} \quad \frac{\partial \delta_j}{\partial \sigma} \middle| Z\right]. \tag{C.26}$$

[14] We use a tolerance of 10^{-12}, and we always start the algorithm with the linear specification mean value.

[15] Somewhat more formally they are a finite-order basis-function approximation to the optimal instruments.

This has exactly as many moments as parameters, so is exactly identified and no iterative weighting matrix is necessary.

To calculate this object, one must take a stand on the conditional distribution of the structural error, solve the Bertrand pricing problem, back out model-implied structural errors, and then calculate the derivatives. In a major methodological advancement, Conlon and Gortmaker (2019) describe how, given an initial set of estimates, one can calculate this object relatively quickly for most problems. Their `pyblp` software automates most of these steps with various options; however, this is not possible in our problem. Because we do not accurately observe prices for mixed-use buildings, which is roughly half of the choice set, we cannot credibly solve the Bertrand pricing problem.[16] Even conditional on obtaining the true parameter vector, our implied substitution between buildings will be biased up or down based on whether commercial rents are greater or less than residential rents in those buildings, which will bias the calculated 'optimal instrument.'

Nevertheless, Gandhi and Houde (2018) show that the optimal instruments can be approximated, in any dataset, by symmetric functions of the differences in building level covariates without needing to solve the Bertrand pricing problem. Their results formalize the intuition of the more traditional "BLP Instruments" that mark-ups are shifted by utilizing the 'product-space-distance' between products, where more isolated products as more immune to price shocks. However, there are still many choices of potential finite basis functions that can be used.

The authors suggest two 'flavors' for practitioners. First, they propose "Quadratic Differentiation Instruments" (DQ):

$$Z_{hj}^{\mathrm{DQ}} = \sum_{k \in \{K(j)\}} (x_{hk} - x_{hj})^2, \tag{C.27}$$

where $K(j)$ is a set of rivals for plot j. This is the set that we use in the main text.

Second, they propose "Local Differentiation Instruments":

$$Z_{hj}^{\mathrm{DL}} = \sum_{k \in \{K(j)\}} 1\left[\, |x_{hk} - x_{hj}| < \mathsf{sd}(X_h) \,\right], \tag{C.28}$$

[16] In addition, with rent control and zoning constraints, we would need to solve a constrained Bertrand pricing problem, which is not coded in `pyblp`.

where $\mathsf{sd}(X_h)$ is the empirical standard deviation of variable X_h. In unreported results, we find that these instruments have less strength relative to the DQ instruments; although, they do still find elastic results. These results are available upon request.

To deal with endogeneity of prices (or any covariate), the authors recommend using a predicted price using plausibly exogenous variation, such as the following additional example:

$$Z^{\text{DQ}}_{r,j} = \sum_{k \in \{K(j)\}} \left(\mathsf{E}[r_k \mid X, W] - \mathsf{E}[r_j \mid X, W] \right)^2, \tag{C.29}$$

where $\mathsf{E}[r_k \mid X = x_k, W = w_k]$ is from a first stage regression on all exogenous information, (X, W), where W are any variables excluded from the utility function.[17]

C.7.1 BLP-F Statistic

To assess the validity and *ability* of our instruments in identifying demand parameters, we report the 'first stage' statistics of our instruments, as advised in Armstrong (2016). We report a robust first stage F statistic of the linear regression of building rents on the model controls and instruments and the BLP-F statistic as devised in Armstrong (2014).

The robust F statistic has the virtue that it is robust to heteroskedasticity but cannot discern between the cases when excluded instruments are correlated with rents but "the researcher imposes a model that leads to product characteristics having an asymptotically negligible effect on markups (Armstrong, 2014)." The BLP-F statistic is based on the 'concentration parameter' and is designed to have power in cases when the usual F statistic would falsely reject a null hypothesis of no identification.[18]

The BLP-F statistic is a post-estimation procedure calculated in five steps. First, regress price on all model controls and instruments and then save the residual, $\dot{r}_j$. Second, calculate the sample

[17]Note, Gandhi and Houde (2018) specify W as any already available instrument, which Conlon and Gortmaker (2019) interpret to include $\{Z^{\text{DQ}}_{hj}\}_{h \in H}$ for the building $X's$. Currently, we do not use $\{Z^{\text{DQ}}_{hj}\}_{h \in H}$ as part of W, so that X are building characteristics in the utility function and W is land value from the NYC DOF.

[18]If $y = X\beta + u$, then the concentration parameter is defined as $\mathsf{Var}(X\beta)/\mathsf{Var}(u)$.

variance of the residual. Third, regress the model-implied markup, $mu_j = -D_j/[\partial D_j/\partial r_j]$, and instruments on the included model controls, and save the residuals: $\{\dot{mu}_j, \dot{Z}_j\}$. Fourth, regress $\dot{mu}_j$ on $\dot{Z}_j$ and save the predicted values, $\hat{\dot{mu}}_j$. Finally, calculate the BLP-F statistic as the following, where k is the number of instruments:

$$\mathsf{F}^{\mathsf{BLP}} := \frac{\mathsf{Var}(\hat{\dot{mu}}_j)}{\mathsf{Var}(\dot{r}_j)} \cdot \frac{J-k}{k}. \tag{C.30}$$

Critical values of the BLP-F statistic do not exist. However, as this is based on an standard F statistic, one could rely on 'rules of thumb' in that a statistic should be greater than some number, such as 10 or 25.

C.8 Additional Estimation Details

To aid our estimation, we follow most modern practices in estimating demand parameters. Many of these are based on advice found in Nevo (2000) (*N*), Knittel and Metaxoglou (2014) (*KM*), and Conlon and Gortmaker (2020) (*CG*).

`First`, we scale all $Z = (X, Z^{(x)}, Z^{(r)})$ variables by their empirical standard deviations to put their variances on the same order of magnitude. As in Brunner et al. (2017), we find this alleviates most model convergence issues.

`Second`, we use an 'overflow safe' method of calculating market shares which gives some protection when a solver inadvertently uses a parameter vector that is far from the true vector, as described in section 3.4 of *CG*.

`Third`, for the inversion step we always use the Berry (1994) logit inversion as the starting value, we use an accelerated fixed point algorithm, called `SQUAREM`, as described in section 3.2 of *CG*, and we use a fixed tolerance of $\|\mu^{s+1} - \mu^s\|_\infty < 10^{-12}$. *KM* show a loose or variable tolerance can cause catastrophic error propagation from the inversion step to the GMM estimates to the gradient, which can veer the optimization algorithm far off course.

`Fourth`, we use supply the analytical gradients of the GMM objective function using a gradient based solver, as described in *N* and benchmarked by *KM* and *CG*. This not only speeds up computation relative to gradient-free or approximated gradients but is also more reliable.

`Finally`, for technical and theoretical reasons we do *not* include a supply side for the model in estimation. Our main theoretical reasons are that we do not know enough about the marginal cost function for rental buildings nor do we wish to fully model the zoning and rent control constraints for a landlord. Brushing theoretical concerns aside, the analytical derivative of the supply moments effectively requires storing a $J \times J \times J$ three-dimensional matrix (where $J = 9,484$) in computer memory, which is not feasible using even for many super computers. We believe the primary empirical benefit of a supply moment would be to increase precision and ensure elastic demand. However, as the majority of our results do not suffer from either problem – see appendix C.9 – we do not think the supply side is necessary for the model's estimation.

C.9 Additional Estimation Results

Our 2010 Manhattan demand estimation is estimated on a single cross section of data. To probe the robustness of this, we expand the dataset to include the Bronx, Brooklyn, and Queens.

We reinterpret the model as now having four separate markets–the boroughs–within NYC. To do this we also now assume the outside good is composed of small building (1-3 units) rental market. Otherwise, we use the same conceptual sample of single-use, residential buildings to estimate demand. One computational change is that given the size of the new demand estimation problem, we only use a two-step GMM procedure rather than the iterated procedure in the main text.

Below we provide summary statistics for this sample as well as results. We find that the results are almost identical for Manhattan as in the main text. However, we find that the outer-boroughs have lower markup shares of rent.

Summary statistics for the 2010 NYC samples is in Table C.7.

C.10 Total Derivative of Monopoly Pricing Rule

The monopoly pricing rule is

$$r_j^\star = mc_j - \frac{D(r_j^\star; \{r_k^\star\}_k)}{m(r_j^\star; \{r_k^\star\}_k))}. \tag{C.31}$$

Table C.7: Summary Stats:
2010 NYC Rental Buildings

	IV	Estimation	Unconstrained	New Unc.
Res.Units per Building	17.9	15.3	10.4	14.1
Households per Building	17.0	14.6	9.9	13.2
Vacancy Rate	5%	5%	5%	6%
Percent Mixed-Use	13%	0%	0%	0%
Percent Rent Stabilized	46%	45%	0%	0%
Percent Zoning Constrained	76%	79%	0%	0%
Median Monthly Rent*	–	$1,028	$1,328	$1,637
Median Rent by Median Income*	–	33%	43%	52%
Median Monthly Land Value per Unit	$4,783	$4,134	$7,659	$4,260
Years Since Construction	84	82	79	3.8
Years Since Renovation	65	67	62	3.8
log(Distance CBD)	1.63	1.71	1.53	1.58
log(Distance Subway)	-1.67	-1.63	-1.61	-1.59
Avg Unit Sqft	813	817	1,033	1,294
Buildings	73,145	53,321	5,215	505

Note: Building data from PLUTO, NPV, FAR, MDRC files. Households allocated based on building units and 2010 Decennial Census and American Community Survey. Median income in 2010 at borough level from 2010 ACS. Vacancy rate is one minus the total households in building divided by total building units. A building is mixed-use if the building has positive commercial area. A building is considered rent stabilized if more than 10% of units are rent stabilized. A building is zoning constrained if the building would not be allowed to create an additional unit based on building floor-area-ratios and minimum unit area requirements. A building is 'new' if it is was built in or after 2000. Geodesic distances are in log miles based on building (lat,lon) coordinates. Monthly land value per unit is [Land Value / (12 x Residential Units)]. (*) – Rent data is only available for single use buildings

Totally differentiating this function, we get

$$\mathrm{d}r_j = \mathrm{d}mc_j - \left[\frac{\frac{\partial D_j}{\partial r_j}\mathrm{d}r_j + \sum_{k\in\mathcal{Z}} \frac{\partial D_j}{\partial r_k}\mathrm{d}r_k}{\frac{\partial D_j}{\partial r_j}} - \frac{\frac{\partial^2 D_j}{\partial r_j^2}\mathrm{d}r_j + \sum_{k\in\mathcal{Z}} \frac{\partial^2 D_j}{\partial r_j \partial r_k}\mathrm{d}r_k}{\left(\frac{\partial D_j}{\partial r_j}\right)^2} D_j \right] \tag{C.32}$$

$$= \mathrm{d}mc_j - \left(1 - \frac{\frac{\partial^2 D_j}{\partial r_j^2} D_j}{\left(\frac{\partial D_j}{\partial r_j}\right)^2} \right) \mathrm{d}r_j - \sum_{k\in\{\mathcal{Z}\}} \left\{ \left(\frac{\frac{\partial D_j}{\partial r_k}}{\frac{\partial D_j}{\partial r_j}} - \frac{\frac{\partial^2 D_j}{\partial r_j \partial r_k} D_j}{\left(\frac{\partial D_j}{\partial r_j}\right)^2} \right) \mathrm{d}r_k \right\} \tag{C.33}$$

To arrive at equation 3.19, we set $\mathsf{d}mc_j = 0$, solve C.33 for $\mathsf{d}r_j^{\mathsf{cf}}$, and then manipulate the equation to arrive at an elasticity form. A useful equivalence is the following: $\frac{\partial[\partial D_j/\partial r_j]}{\partial r_k}\frac{r_k}{\partial D_j/\partial r_j} = \frac{\partial \varepsilon_j}{\partial r_k}\frac{r_k}{\varepsilon_j} + \frac{\partial D_j}{\partial r_k}\frac{r_k}{D_j}$.

With preference heterogeneity – i.e., random coefficients – then the expression has no closed form solution, but is easily calculated with our estimated parameters and Monte Carlo integration. For intuition, if there were no individual agent heterogeneity in preferences, then

$$\mathsf{d}r_j = (1 - D_j)\mathsf{d}mc_j + \frac{D_j}{(1 - D_j)} \sum_{k \in \mathcal{Z}} \{D_k \mathsf{d}r_k\} \tag{C.34}$$

$$= (1 - D_j)\mathsf{d}mc_j + \frac{D_j}{(1 - D_j)} \mathsf{Avg}_D(\mathsf{d}r_k). \tag{C.35}$$

Without a full model of building costs, we cannot calculate $\mathsf{d}mc_j$, so we cannot calculate the true partial equilibrium change in unconstrained prices. Under the assumption of (locally) constant marginal costs, then our measure *equals* the partial equilibrium change in rental prices. Under the assumption of strictly increasing marginal costs, then $\mathsf{d}mc_j < 0$, so our measure would be the lower bound of the *magnitude* of the rent change. Without additional assumptions, our measure calculates the partial equilibrium change in the monopoly mark-up of unconstrained buildings due to a zoning-shock.

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