The Unintended Consequences of Encouraging Work: Tax Incidence and the EITC

Jesse Rothstein[1]
Princeton University

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[1]Industrial Relations Section, Firestone Library, Princeton, NJ 08544. E-mail: jrothst@princeton.edu. Earlier versions of this paper circulated under the title "The Mid-1990s EITC Expansion: Aggregate Labor Supply Effects and Economic Incidence." I am grateful to Jared Bernstein, David Card, Anne Case, Nada Eissa, Hank Farber, Bo Honore, Alan Krueger, Thomas Lemieux, Cecilia Rouse, Max Sawicky, and numerous seminar participants for helpful discussions, and the Princeton Industrial Relations Section and Center for Economic Policy Studies for financial support. Nina Badgaiyan provided excellent research assistance.

Abstract

The EITC is designed to encourage work. But EITC-induced increases in labor supply may drive wages down, shifting the intended transfer toward employers and hurting non-EITC low-skill workers. I exploit variation across family types and skill levels to identify the effect of a large EITC expansion in the mid 1990s. Ceteris paribus, low-skill single mothers keep only \$0.70 of every dollar they receive. Employers of low-skill labor capture \$0.72, \$0.30 from single mothers plus \$0.43 from ineligible workers whose after-tax incomes fall when the EITC is expanded. The net transfer to low-skill workers is less than \$0.28 per dollar spent.

1 Introduction

Most means-tested income redistribution programs impose high effective tax rates on earned income, thereby discouraging potential recipients from working. In recent decades, policy changes in the United States have shifted the incentives toward encouraging work via the imposition of time limits and work requirements on welfare recipients and via repeated expansions of the Earned Income Tax Credit (EITC), which by 2000 was 70% larger than traditional welfare (Hotz and Scholz, 2003).

The EITC is often seen as an implementation of a Negative Income Tax, or NIT (Friedman, 1962), but its central feature distinguishes it. Where an NIT would be available even to non-workers, only families with earned income can receive the EITC. Moffitt (2003a) notes that the hybrid system formed by the combination of the EITC and traditional welfare amounts to a sort of an NIT. However, the two programs are not often seen as part of a unified policy, and EITC expansions have not typically been accompanied by increases in the welfare benefit.

An important intended effect of the EITC is to increase labor supply among eligible workers.[1] But the literature on tax incidence (reviewed by Kotlikoff and Summers 1987 and Fullerton and Metcalf 2002) emphasizes that taxes may influence the equilibrium price of the good being taxed. In the standard model, EITC-induced labor supply increases lead to lower wages, allowing employers to capture a portion of the intended transfer. Moreover, because EITC recipients (primarily single mothers) compete in the same labor markets as others who are ineligible for the credit, wage declines extend to many workers who do not receive offsetting EITC payments. These unintended transfers limit the EITC's capacity to redistribute income to the poor.

Endogenizing the wage thus reduces the attractiveness of the EITC. But the practical importance of incidence effects is unclear. Tax changes are rarely large enough to have detectable effects on prices, so estimates of the incidence of income taxes have been few and far between. Fullerton and Metcalf (2002) note that most analyses of the distribution of tax burdens (e.g., Pechman and Okner, 1974; Pechman, 1985) assume that workers bear the full weight of income taxes, though they point out that "this assumption has never been tested" (p. 29).

I use a substantial expansion of the EITC between 1993 and 1995 to estimate the EITC's incidence. I focus on the female labor market, and particularly on single women.

[1] Eissa and Hoynes (2006) review the literature on the EITC's labor supply effects, while Robins (1985) summarizes experimental studies of the NIT. Saez (2002b) argues that an EITC-like structure is better than an NIT when the extensive margin elasticity of labor supply is sufficiently large. Like other optimal tax models, Saez's analysis takes the wage as exogenous.

Approximately 13% of working women–all with children, mostly unskilled, and disproportionately unmarried–could claim the credit in 1993. Changes in program parameters over the next two years induced mean absolute changes in marginal and average tax rates for these women of 9.2% and 8.0%, respectively.[2] The resulting labor supply shock provides variation that can be used to estimate the credit's incidence.

One contribution of this paper is to extend the traditional partial equilibrium tax incidence framework to allow for heterogeneity in tax rates. The traditional model treats labor as an undifferentiated input to production and assumes a single tax rate that applies to all wage income. Neither is accurate: Most taxes differentiate between types of families and, in effect, between workers of different skills. Both of these features provide useful variation. I derive simple expressions relating changes in tax rates to changes in the quantity and price of types of labor that are distinguished by the skill level and the tax treatment. Targeted earnings subsidies produce unintended transfers from both targeted and similarly-skilled ineligible workers to their employers. The transfer to employers is largest when labor supply is elastic and demand is inelastic; it is paid primarily by targeted workers only when there are few ineligible workers with skills similar to those of subsidy recipients.

There are two important empirical hurdles. First, I require measures of the change in the quantity and price of labor at each skill level. I identify skill levels as points in the cross-sectional wage distribution.[3] Shifts in the observed wage distribution reflect changes in both the skill composition of labor (i.e. in relative supply) and the wage earned by workers of each skill. I use a semiparametric "re-weighting" strategy, proposed by DiNardo, Fortin and Lemieux (1996, hereafter DFL), to estimate the price component.[4] I also demonstrate that the weights used in this estimator can be interpreted as measures of the change in labor supply at each skill level.

The supply elasticity is identified by within-skill comparisons between EITC eligible and ineligible women. Single mothers' labor supply increased substantially in the mid-1990s relative to single women without children.[5] The change is concentrated among

[2]Each figure is computed over women aged 16-64 in the 1994 March Current Population Survey who worked at least one week in 1993 and were either the reference person or the wife of the reference person for their household. The sample and the tax simulation are discussed below.

[3]I argue below that this is preferable to a simpler strategy that focuses on cells defined based on observable characteristics (e.g., education), as in Leigh (2007), when the focus is the estimation of labor demand rather than supply.

[4]The key assumption is that changes in the skill distribution are fully accounted for by changes in workers' observed characteristics. This is common in the inequality literature (see, e.g., Autor et al., 2005a; Lemieux, 2006). I discuss this assumption at length below, and present evidence (based on wage changes for groups facing small tax rate shocks and exhibiting small changes in observable selection) that selection on unobservables is not an important source of bias in my results.

[5]A small credit for childless families was added during the mid-1990s. I account for this credit in

women with two or more children and characteristics associated with low earnings, mirroring the distribution of changes in the EITC earnings subsidy and associated average tax rates. The entire change in labor supply occurs through increased participation, with essentially no change in weekly hours conditional on working. The estimates imply that the wage elasticity of individual labor supply is about 0.7 on the extensive margin and zero on the intensive margin, both in close agreement with earlier estimates.

The second hurdle arises in the identification of the effect of this shock on wages. Only between-skill comparisons are informative about labor demand. Thus, even when the reweighting procedure gives accurate estimates of changes in wages at each skill, the effects of tax changes may be confounded by shifts in the relative demand for high- and low-skill labor. Indeed, I find that low-skill workers' relative wages rose over the mid-1990s, even as their labor supply increased. A leading explanation is a shift in the relative demand for low-skill labor, either cyclical or due to changes in the production technology (i.e., skill-biased technical change, or SBTC). Both factors favored low-skill workers during the mid-1990s, as the unemployment rate fell steadily during this period – relative employment conditions for low-skill labor are pro-cyclical; see Hoynes (2000) – and research on SBTC indicates that it favored low-skill labor from about 1987 onward (Autor et al., 2005a).

To reduce bias from demand shifts, I adopt a difference-in-differences strategy, comparing the rate of change in skill-level wages across two periods with plausibly similar demand shifts but different shocks to labor supply. I use the period from 1988 to 1992 as my counterfactual. Autor et al.'s (2005a) results indicate that the form and rate of technical change was approximately constant from 1987 through the mid 1990s. I find that the relative price of low-skill labor increased even more quickly during the years prior to the EITC expansion than subsequent to it, indicating an outward shift in the demand for low-skill labor that masks the effect of the EITC. My preferred specification allows for unrestricted trends in the demand for labor at each skill level as well as a skill-biased "tilting" of these trends over time. I find that demand is downward sloping but relatively inelastic, with an elasticity around -0.3. This is precisely the elasticity that Hamermesh (1993, p. 135) characterizes as a "best guess."

Business cycle trends were sharply different in my two comparison periods. The national unemployment rate rose by two percentage points between 1988 and 1992, then fell by almost exactly the same amount between 1992 and 1996. Thus, identification of the demand elasticity based on the comparison between the recessionary 1988-1992 period

my analysis. As discussed below, it was too small to have meaningful impacts on the labor market, and women without children can be treated as effectively ineligible.

and the expansionary 1992-1996 period (and a tax-driven expansion of low-skill labor supply in the latter) likely leads me to understate the decline in low-skilled workers' relative wages that occurred as a result of the EITC expansion and to overstate the absolute demand elasticity.[6]

I conclude by computing the incidence implied by my estimated elasticities and by the distribution of EITC-eligible and -ineligible women across skill groups. The results indicate that the EITC produces extremely large unintended transfers. A dollar of EITC spending produces transfers of \$0.70 to the intended recipients and \$0.72 to employers of low-skill labor, with the excess \$0.43 coming from EITC-ineligible workers. Eligible women's after-tax incomes, which incorporate changes in earnings from supply shifts as well as changes in wages, rise by \$1.21, while those of ineligible women fall by \$0.73.

These precise effects depend on my estimated elasticities, but the qualitative results do not. Even with quite elastic demand, unintended transfers are substantial relative to the amount spent on the EITC. Transfers to employers of low-skill labor, and away from ineligible low-skill workers, are necessary consequences of the EITC's design. Using my elasticity estimates, the net transfer to low-skill workers amounts to less than 1/3 of government outlays, and total after-tax incomes rise by only \$0.47 for every dollar spent. This reduces the attractiveness of the EITC relative to the NIT (which with the same elasticity parameters produces small transfers *from* employers to their workers) and to other income support policies, like traditional welfare, that do not encourage work.

The EITC program is described in Section 2. Section 3 develops the incidence model with skill and tax rate heterogeneity. Section 4 describes the empirical implementation, and Section 5 develops a strategy for distinguishing changes in the price and quantity of labor at each skill level in repeated cross sections. Section 6 describes the data and presents simple regression estimates of changes in the return to skill surrounding the EITC expansion. Section 7 presents results, first describing changes in labor supply and wages, and then using several specifications to relate these to the underlying elasticity parameters. Section 8 explores several alternative specifications intended to identify potential biases in the main estimates. Section 9 presents the incidence calculation.

[6] The only other period in recent history which matches the mid-1990s on both cyclical and SBTC dimensions is the late 1990s. There were continued changes in EITC and welfare policy that induced continuing expansions of low-skill labor supply during this period, however, so the difference-in-differences in labor supply is approximately zero and the demand elasticity cannot be identified. I have also explored estimates that try to difference out business cycle effects by comparing to the expansionary 1983-1986 period. This comparison yields a positive effect of exogenous labor supply expansions on wages; evidently the bias from changes in SBTC between the mid-1980s and mid-1990s is larger than that from cyclical differences in my preferred strategy.

2 The EITC Program

The EITC is a refundable tax credit that depends on a family's total earnings according to a four-segment schedule.[7] Four parameters define the credit: A phase-in rate $\tau_1 < 0$, a maximum credit C, an income level p at which the credit begins to phase out, and a phase-out rate $\tau_2 > 0$. If a family's earned income is y, the credit is:

$$c = \begin{cases} \tau_1 y & \text{if } y \leq -C/\tau_1 & \text{(phase-in range)} \\ C & \text{if } C/\tau_1 < y \leq p & \text{(plateau)} \\ C - \tau_2 (y - p) & \text{if } p < y \leq p + C/\tau_2 & \text{(phase-out)} \\ 0 & \text{if } y > p + C/\tau_2 & \text{(ineligible).} \end{cases} \tag{1}$$

The parameters vary with the number of children but not with marital status or the number of workers. Appendix Table 1 presents the program parameters for the years 1983-2001, in 1992 dollars. I focus on the 1993-1996 expansion, in which maximum real credits (C) grew by 38% for one-child families and by 117% for families with two or more children but the kink points ($-C/\tau_1$, p, and $p+C/\tau_2$) were essentially stable. A very small credit was also added for childless families. Figure 1 displays the credit as a function of real annual earnings and number of children in 1992 and 1996.

Liebman (1998) discusses the labor supply incentives created by the EITC. Increases in τ_2 and C both raise virtual income (the zero-hours intercept of the relevant linear segment of the budget constraint in hours-consumption space). Income and substitution effects thus reinforce each other in the phase-out range, creating incentives to reduce labor supply. In the plateau region, the substitution effect is zero but the income effect is negative. In the phase-in range, however, marginal tax rates (MTRs) are negative and substitution effects would imply increased labor supply.

This supposes that labor supply decisions are made continuously. Given the concentrated distribution of annual hours – 74% of women who work at all in a year work at least 48 weeks, and 51% work between 38 and 42 hours per week[8] – it seems likely that the participation decision is often discrete. If so, average tax rates (ATRs) on a woman's potential earnings may be more important than MTRs. The EITC produces negative ATRs for all primary earners with potential earnings below $p + C/\tau_2$, so should have induced increased participation, at least from single parents.

Estimates of the EITC's labor supply effects overwhelmingly place them on the ex-

[7]EITC take-up rates are estimated at 80% or more (Scholz, 1994; Kopczuk and Pop-Eleches, 2007).

[8]Among single mothers with a high school education or less, a group that is quite likely to be eligible for the EITC, 64% work full-year and 55% work full time. The source is the 1993 & 1994 March Current Population Survey; the exact sample is decribed below.

tensive margin, with labor force participation elasticities with respect to net income between 0.69 and 1.16 (Eissa and Hoynes, 2006; Hotz and Scholz, 2003). This means that the EITC unambiguously expands single mothers' participation (Meyer and Rosenbaum, 2001; Grogger, 2002; Dickert et al., 1995; Keane and Moffitt, 1998), though Eissa and Hoynes (2004) find reductions in participation in a subset of married women for whom the EITC creates positive ATRs. By contrast, there is little evidence of effects on hours worked conditional on participation (Eissa and Liebman, 1996; Saez, 2002a).

The mid-1990s EITC expansion was nearly coincident with major reforms to the cash welfare system, which also attempted to push low-skill single mothers into the labor force.[9] This might have two effects on my analysis. First, it means that my measures of changes in tax rates – which do not incorporate effective tax rates produced by non-tax transfers – fail to fully capture the changes in the incentives that women faced. This may lead me to understate the labor supply elasticity.[10] A second effect is that welfare reform may have induced changes in the skill distribution in the labor force, as women who left welfare to enter employment likely have lower skill than observably-similar women who were never on welfare. As I discuss below, this would bias my estimates of changes in equilibrium wages. Importantly, however, this effect of welfare reform should be concentrated among single mothers. Measures of wage changes among single women should be free from bias. As I note below, the EITC's effects on wages can be identified from this group alone.

In most states, welfare reform was implemented in late 1996 and 1997, though some states had waivers from the federal government that permitted changes as early as 1992. I present a specification test below that excludes states and time periods potentially affected by welfare reform; this yields very similar estimates to those obtained from my main sample.

[9] There were also changes in food stamps and SSI (Daly and Burkhauser, 2003), and increases in the value of Medicaid coverage (Meyer and Rosenbaum, 2001) over the mid-1990s. Both served to increase the effective tax rate on labor force participation, offsetting perhaps 30-50% of the ATR reductions created by the EITC expansion.

[10] Welfare caseloads declined sharply in 1996 and even more in 1997. This could be a result of the EITC expansion as much as of welfare reform, as both raised the returns to paid employment relative to welfare receipt. Moffitt (2003b) reviews welfare reform, caseload trends, and state waivers. For contrasting estimates of the effect of reform on caseloads, see Wallace and Blank (1999) and Figlio and Ziliak (1999).

3 A Simple Tax Incidence Model

A simple partial-equilibrium tax incidence model[11] begins with constant-elasticity supply and demand functions for homogenous labor:

$$L^S(w) = \alpha(w(1-\tau))^\sigma \text{ and } L^D(w) = \beta w^\rho, \tag{2}$$

where w is the pre-tax wage, $w(1-\tau)$ is the after-tax (take home) wage, and $\rho < 0 < \sigma$. The equilibrium pre-tax wage and quantity are

$$w^* = \left(\beta\alpha^{-1}(1-\tau)^{-\sigma}\right)^{\frac{1}{\sigma-\rho}} \text{ and } L^* = \left(\beta^\sigma\alpha^{-\rho}(1-\tau)^{-\sigma\rho}\right)^{\frac{1}{\sigma-\rho}}. \tag{3}$$

Thus, employers bear a share $\frac{\sigma}{\sigma-\rho}$ of taxes–$d\ln w^* = \frac{-\sigma}{\sigma-\rho}d\ln(1-\tau) \approx \frac{\sigma}{\sigma-\rho}d\tau$–and workers bear the remaining $\frac{-\rho}{\sigma-\rho}$ share. The employer share is largest when supply is elastic (σ is large) and demand inelastic ($|\rho|$ is small).

Both σ and ρ can be computed from changes in wages and quantities in response to a change in the tax rate.[12] This only works, however, if there are no shocks to the parameters of (2). Most importantly, there can be no changes in the production level beyond those resulting from the tax change itself.

A generalization of the textbook model that allows for heterogeneity of worker skill and for tax rates that differ both across skills and across similarly skilled workers from different family types (e.g., married and single workers, with and without children) permits more flexible identification strategies. Across-family type variation in tax rates faced by workers of the same skill allows robust estimation of the supply elasticity, while across-skill-group variation identifies the demand elasticity even in the presence of unmodeled shocks to aggregate demand.

Begin with a Constant Elasticity of Substitution (CES) production function:

$$Y = \left(L^\phi + cK^\phi\right)^{\lambda/\phi}, \text{ with } \phi < 1. \tag{4}$$

[11] A full general equilibrium analysis of tax incidence would incorporate changes in the prices of final goods. These will tend to offset changes in wages. However, this offset will not be confined to the groups affected by targeted taxes so long as there is trade between groups. Consequently, although a full analysis of this is beyond the scope of this paper, general equilibrium effects are unlikely to be important to the distributional analysis.

[12] This is in contrast to the usual rule that identification of both supply and demand requires instruments for each. The key is that the instrument–the change in tax rates–is a direct change in price, so the "first stage" coefficient is itself informative about the parameters: For any $(L,\ w)$ on the untaxed supply curve, the taxed supply curve passes through $(L,\ w/(1-\tau))$.

Let total effective labor supply L be itself a composite of supply in S skill groups:

(5) $$L = \left(\sum_{s=1}^{S} b_s L_s^{\alpha} \right)^{1/\alpha}, \ \alpha < 1.$$

Cost minimization implies a set of labor demand functions of the form

(6) $$L_s^D = \psi \beta_s w_s^{\rho},$$

where w_s is the pre-tax wage for skill-s workers, $\rho = \frac{1}{\alpha - 1} < 0$, $\beta_s = b_s^{-\rho}$, and $\psi = \psi(w_1, w_2, \ldots, w_S)$ is a real-valued parameter determined by the level of aggregate demand. Note that w_t enters the expressions for L_s, $s \neq t$, only through ψ.[13]

Individuals supply labor with elasticity σ. Taxes may vary both across skill and, among workers of the same skill, across demographic groups (indexed by g). Because all workers of the same skill are substitutes in production, however, the pre-tax wage varies only across s. The after-tax wage for workers of skill s from group g is thus $w_s (1 - \tau_{sg})$, and the supply of type (s, g) labor is

(7) $$L_{sg}^S = \alpha \left(w_s \left(1 - \tau_{sg} \right) \right)^{\sigma} N_{sg},$$

where N_{sg} is the number of potential workers. Differentiating (7) and the inverse demand implied by (6), holding N_{sg} fixed, we obtain

$$\begin{aligned} &\text{(8a)} & d \ln L_{sg} &\approx \sigma d \ln w_s - \sigma d\tau_{sg} \\ &\text{(8b)} & d \ln w_s &= \rho^{-1} \left(-d \ln \psi - d \ln \beta_s + d \ln L_s \right). \end{aligned}$$

Approximating $d \ln L_s \approx L_s^{-1} \sum_k L_{sk} d \ln L_{sk}$ and neglecting to solve for the effects of $d\tau$ on the economy-wide production level ψ, we obtain the quasi-reduced form

$$\begin{aligned} &\text{(9a)} & d \ln w_s &\approx \frac{1}{\sigma - \rho} d \ln \psi + \frac{1}{\sigma - \rho} d \ln \beta_s + \frac{\sigma}{\sigma - \rho} d\bar{\tau}_s \\ &\text{(9b)} & d \ln L_{sg} &\approx \frac{\sigma}{\sigma - \rho} d \ln \psi + \frac{\sigma}{\sigma - \rho} d \ln \beta_s + \frac{\sigma^2}{\sigma - \rho} d\bar{\tau}_s - \sigma d\tau_{sg}, \end{aligned}$$

[13] Teulings (1995, 2005) models job assignment when adjacent skill levels are more substitutable than are those far apart in the skill distribution. The relevant labor supply for wages (in, e.g., the inverse version of 6) is then the local average around s, with more weight on points closer to s. As discussed below, I use kernel regressions to estimate the mean tax change at each skill level as a locally-weighted average. Up to the choice of kernel, these are exact analogues to the local averages that show up in Teulings' model.

where $d\bar{\tau}_s = L_s^{-1} \sum_k L_{sk} d\tau_{sk}$ is the tax rate change for the average skill-s worker. (9b) indicates that the supply of skill-s workers from group g increases with the across-group mean tax rate of skill-s workers but decreases with the own-group tax rate. (9a) says that wages rise with the across-group mean tax rate but not with the own-group rate conditional on this average. One implication is that a cut in one group's tax rate ($d\tau_{sg} < 0$) will raise after-tax earnings, $w_s (1 - \tau_{sg})$, in that group but will reduce after-tax earnings for similarly-skilled workers from of other g groups. By contrast, if there is no across-g variation in tax rates, then equations (9a) and (9b) reduce to the expressions for the homogeneous labor model.

By (9a), employers bear a share $\frac{\sigma}{\sigma-\rho}$ of the change in average taxes, just as in the homogenous labor model. The distribution of the worker share between groups depends on the weight of $d\tau_{sg}$ in $d\bar{\tau}_s$, which reflects the extent to which taxed and untaxed workers participate in the same labor markets. If the groups' skill distributions are distinct, the full transfer to/from employers comes from the taxed group, whose wages fall by $\frac{\sigma}{\sigma-\rho} d\tau_{sg}$. As the untreated group's share of the skill-s labor market rises, however, $|d\bar{\tau}_s|$ shrinks relative to $|d\tau_{sg}|$ for the targeted groups and the transfer to employers is funded through smaller hourly wage reductions spread across more workers, an increasing share of whom are not directly treated by the tax change.

4 Sources of Identification

A tax change that varies with both skill and demographic group permits estimation of supply and demand elasticities without assumptions on aggregate demand.[14] But different sources of variation are available to identify the two. A robust estimate of σ exploits within-skill, across-demographic group variation in $d\tau_{sg}$, using skill group fixed effects (in changes) to absorb the $d\ln\beta_s$ and $d\bar{\tau}_s$ terms in (9b). ρ is identified only from across-skill variation in $d\bar{\tau}_s$. It can be estimated via the inverse demand function (8b), in which changes in tax rates can be used to generate exogenous variation in dL_s, or by solving from the coefficients of the reduced form equations (9a) and (9b).

In either approach, a correlation between tax changes ($d\bar{\tau}$) and skill-biased demand shifts ($d\ln\beta_s$) will create bias. Most tax changes are targeted to particular parts of the

[14] An important issue concerns the lags with which supply and wages respond to the tax change. I focus on relatively short-run responses, examining data from 1995-1997 for the effects of tax changes phased in between 1993 and 1996. The long-run supply elasticity may well be larger than that seen in the short run. Evidence below suggests that the demand side also adjusts slowly, and that wages do not fall sufficiently in the short run to absorb all of the women who respond to the EITC by entering the labor force. If wage responses to supply shocks are larger in the long run than in the short run, I may understate the share of the EITC captured by employers.

income distribution, so are correlated with any shifts in the relative demand for high- and low-skill labor.[15] If the rate of technical change can be assumed constant across adjacent periods that saw differential changes in tax rate policy, it can be differenced away. Adding a t subscript to index periods (with $t = 0,\ 1$), (8b) becomes

$$d \ln w_{st} = \rho^{-1} \left(-d \ln \psi_t - d \ln \beta_{st} + d \ln L_{st} \right). \tag{10}$$

If $d \ln \beta_{s1} - d \ln \beta_{s0}$ is uncorrelated with $d\bar{\tau}_{s1} - d\bar{\tau}_{s0}$, ρ^{-1} can be consistently estimated via an instrumental variables regression of $d \ln w_{st}$ on $d \ln L_{st}$, using $d \ln \bar{\tau}_{st}$ as an instrument and including fixed effects for each time period and each skill level to absorb $d \ln \psi_t$ and the trend component of $d \ln \beta_{st}$, respectively. Identification comes from differential changes in $\bar{\tau}_s$ over the two periods.

In principle, the incidence framework in Section 3 could be extended to the individual level, allowing for heterogeneity in tax rates and nonlinear tax schedules.[16] Equations (9a) and (9b) make clear, however, that the key parameters for tax incidence calculations are the average tax shocks to cells defined at the s-g and s levels. Thus, only tax rates that vary importantly across cells and that influence aggregate labor supply in each cell are useful for identification. Accordingly, I focus on simple summary measures, the marginal tax rate (MTR) and the average tax rate (ATR), the latter defined as the difference between the family's tax bill with and without a woman's earnings as a share of those earnings. These can be seen as proxies for labor supply incentives on the intensive and extensive margins, respectively.[17] As (9a) and (9b) indicate, it is the relative importance of these measures in reduced-form wage and labor supply equations that identifies the parameters of interest.

Because both MTRs and ATRs depend on total annual earnings, the change in tax rates for workers at skill s is endogenous to unobserved determinants of changes in either labor supply or hourly wages. An exogenous component of the tax change can be isolated

[15] Autor et al. (2005a,b), among others, have argued that skill-biased technical change (SBTC) explains an important part of changes in wage inequality, though others (Card and DiNardo, 2002; Lemieux, 2006) disagree. SBTC would change the returns to skill, potentially confounding the effects of the EITC. Autor et al. (2005a) find that SBTC worked against low-skill workers in the 1980s, lowering their wages, but that the relative demand for low-skill labor has been shifting upward steadily since around 1987.

[16] A long literature considers the estimation of labor supply responses to nonlinear tax schedules. See, e.g., Hausman (1985); Moffitt (1990) and Hoynes (1996).

[17] Analysts often pair MTRs with virtual income, the zero-earnings intercept of the relevant linear segment of the budget constraint. Conditional on the MTR, variation in women's virtual income derives either from the husband's earnings or from non-labor income. Neither is highly correlated with skill as defined below and, as a result the across-cell variation in average virtual income has a very low signal-to-noise ratio. I exclude virtual income from the analyses below, though specifications that include it yield very similar MTR and ATR coefficients.

by focusing only on changes in rates due to changes in the tax schedule, holding hour and wage distributions constant. These serve as "simulated instruments" for actual tax rate changes (Auten and Carroll, 1999; Gruber and Saez, 2002; Leigh, 2007).

5 Separating Prices and Quantities

The key feature of "skill" in the incidence model is that workers of the same skill are perfect substitutes while those of different skills are not. One option for empirical implementation is to use use relatively crude skill groupings, dividing workers by, say, education and age. This is perfectly suitable for labor supply analyses (see, e.g., Blundell et al., 1998; Meyer and Rosenbaum, 2001), as the tax shock that determines the average labor supply response in any cell is the average tax rate change in that cell. This approach is less suitable for analyses of wage effects, however, as workers in different observables-based cells compete for the same jobs.[18] This means that the average change in labor supply among a cell's members does not equal the labor supply shock to the average member's labor market. Tax and labor supply changes are smoothed across cells relative to the shocks that are relevant for prices, creating non-classical measurement error. Inverse labor demand specifications that use cell-level averages will likely overestimate $|\rho^{-1}|$, leading to understatement of $|\rho|$ and overstatement of the employer share of the tax burden.

I do not rely on observed characteristics to identify worker skill. Rather, I assume that two workers who earn the same wage have the same skill, and that these workers are freely substitutable even if they have different education or are in different occupations (Teulings, 1995, 2005). This assumption is of course not literally true, but may be seen as a reasonable approximation, particularly in the low-skill labor market. Under this assumption, the tax shock that is relevant to a worker's labor market is the average tax rate change across workers earning the same hourly wage.[19]

With panel data, it would be straightforward to measure the change in each worker's labor supply and wage during the period spanning a tax change. Because the available panel data sets are too small to provide adequate precision, however, I work instead with repeated cross sections. This requires a strategy for distinguishing between changes in

[18] Leigh (2007) defines cells based on worker characteristics (e.g., state of residence or education and age). Gruber (1994) and Gruber and Krueger (1991) also leverage geographic variation in tax regimes, while Kubik (2004) exploits variation in median wages across occupations. Identification of wage effects requires that there be little potential for substitution across cells.

[19] As developed below, my approach correctly matches wage and tax changes at the same skill level, at the cost of perhaps smoothing supply responses across cells. Although this should not introduce bias, as labor supply is an endogenous variable in all specifications, the cell-based strategy is probably preferable for analysis of labor supply responses.

the skill composition of the labor force and changes in equilibrium wages for workers at different skills. If the EITC attracts low-skill women to enter employment, this will shift the distribution downward even with no change in any individual worker's wage; the distribution will be further shifted if the additional supply reduces the equilibrium price of low-skill labor. To distinguish these, I adapt DiNardo et al.'s (1996) reweighting strategy to balance the skill distribution in pre- and post-tax-reform cross sections.[20] If this can be accomplished, changes in the mapping from skill to wages can be tracked by following fixed percentiles of the (reweighted) wage distribution, and the weights used can be seen as measures of composition changes.

Two assumptions are required. First, the *ranking* of the wages paid to different skill groups is preserved over time; while the relative wages may vary, a higher-skill worker always earns a higher wage than a lower-skill worker. Second, selection into the labor force is based on observables; conditional on these, there are no changes in the distribution of unobserved skill in the labor market.

Let $w_{st} = \Lambda_t(s)$ be the (log) wage for a skill-s worker at time t, where $\Lambda_t(\cdot)$ is the (strictly increasing) wage schedule that maps skills to prices. For notational simplicity, I suppress g subscripts, though the DFL decomposition is carried out independently for each g group. Let $F_t(s)$ and $G_t(w)$ be the cumulative distributions of skill and wages, respectively, among workers. At any time the fraction of workers with skill below s must equal the fraction with wages below w_{st}: $F_t(s) = G_t(\Lambda_t(s)) = G_t(w_{st})$.

The change in the wage for skill-s labor between $t = 0$ and $t = 1$ is:

$$\Delta w_s \equiv w_{s1} - w_{s0} = \Lambda_1(s) - \Lambda_0(s) = G_1^{-1}(F_1(s)) - G_0^{-1}(F_0(s)). \tag{11}$$

If the skill distribution were fixed ($F_0 = F_1 = F$), changes in the wage distribution would necessarily reflect changes in the wage schedule, and Δw_s could be estimated as the change in the pth percentile ($p = F(s)$) wage:[21]

$$\Delta w_s = G_1^{-1}(p) - G_0^{-1}(p). \tag{12}$$

With changes in the skill distribution, the pth percentile will not correspond to the same skill level over time, and the right side of (12) will confound changes in F_t with changes in

[20] See also Johnston and DiNardo (1997, Section 11.4.2). Lee (1999) applies the DFL approach to study the impact of changing real minimum wages. DFL-style reweighting can be seen as a form of propensity score matching (see, e.g., Hirano et al., 2003).

[21] In experimental contexts, where the skill distribution may be assumed the same in treatment and control groups, Abadie et al. (2002) and Bitler et al. (2005) refer to Δw_s as the "quantile treatment effect."

Λ_t. Under selection on observables, however, it is sufficient to construct samples in which the distribution of observable skill correlates is balanced over time. This is accomplished by re-weighting the period-1 data. I demonstrate below, moreover, that the reweighting factors themselves can be used to measure the change in labor supply at each skill level.

Let X be a vector of observable characteristics, and assume that the conditional skill distribution is time-invariant: $F_t(s \mid X) = F(s \mid X)$. Let $\lambda_t(X)$ be the labor-supply-weighted density of X at time t. We can write the unconditional skill and wage distributions as

$$F_t(s) = \int F_t(s \mid X)\lambda_t(X)\,dX = \int F(s \mid X)\lambda_t(X)\,dX \tag{13}$$

and

$$G_t(w) = F_t\left(\Lambda_t^{-1}(w)\right) = \int F\left(\Lambda_t^{-1}(w) \mid X\right)\lambda_t(X)\,dX. \tag{14}$$

There are two time-varying components in (14), the inverse wage schedule, Λ_t^{-1}, and the distribution of X, λ_t. Let $\tilde{G}_1(w)$ be the counterfactual wage distribution had the period-1 wage schedule applied with labor supply as in period 0. Then

$$\begin{aligned}\widetilde{G}_1(w) &= \int F\left(\Lambda_1^{-1}(w) \mid X\right)\lambda_0(X)\,dX \\ &= \int F\left(\Lambda_1^{-1}(w) \mid X\right)\lambda_1(X)\frac{\lambda_0(X)}{\lambda_1(X)}dX.\end{aligned} \tag{15}$$

This is identical to $G_1(w)$ but for the weighting factor $\theta(X) \equiv \lambda_0(X)/\lambda_1(X)$. This can be written as

$$\theta(X) = \frac{\lambda_0(X) + \lambda_1(X)}{\lambda_1(X)} - 1, \tag{16}$$

where the first term is the inverse of the propensity score for appearing in period 1.[22]

Differences between the actual period-0 wage distribution and the counterfactual distribution, by assumption, derive solely from changes in the wage schedule:

$$\widetilde{G}_1(w) - G_0(w) = \int \left[F\left(\Lambda_1^{-1}(w) \mid X\right) - F\left(\Lambda_0^{-1}(w) \mid X\right)\right]\lambda_0(X)\,dX. \tag{17}$$

This expression can be inverted to indicate the change in wages at any skill level. The

[22] I assume for notational simplicity that sample sizes are the same in each period.

wage change for skill s is then

$$\Delta w_s = \tilde{G}_1^{-1}\left(F_0\left(s\right)\right) - G_0^{-1}\left(F_0\left(s\right)\right) = \tilde{G}_1^{-1}\left(F_0\left(s\right)\right) - w_{s0}. \tag{18}$$

The proportional change in the skill density between periods 0 and 1 is

$$\begin{aligned}
\Delta L_s &\equiv \frac{f_1\left(s\right) - f_0\left(s\right)}{f_0\left(s\right)} \\
&= \frac{\int f\left(s \mid X\right)\lambda_1\left(X\right)dX - \int f\left(s \mid X\right)\lambda_0\left(X\right)dX}{\int f\left(s \mid X\right)\lambda_0\left(X\right)dX} \\
&= \frac{\int f\left(s \mid X\right)\left[{}^1\!/\!_{\theta(X)} - 1\right]\lambda_0\left(X\right)dX}{\int f\left(s \mid X\right)\lambda_0\left(X\right)dX} = E_0\left[\frac{1}{\theta\left(X\right)} - 1 \mid s\right],
\end{aligned} \tag{19}$$

where $f_t\left(s\right)$ is the time-t density of s and the notation E_0 indicates that the expectation is over the period-0 conditional distribution of X.

The DFL procedure is easily extended to examine changes in labor supply at several margins. The change in supply due to exogenous changes in population demographic characteristics can be obtained by estimating the propensity score from the full period-0 and period-1 samples, regardless of labor supply. This yields a reweighting factor θ_i^{pop} for each period-1 observation. Given this, changes in labor force participation decisions can be examined by comparing the subsample of labor force participants in $t = 0$ and reweighted $t = 1$ data, using a propensity score computed from these data to construct θ_i^{lfp}. Further iterations yield θs for employment conditional on participation (based on data weighted by $\theta_i^{pop} * \theta_i^{lfp}$) and for hours conditional on employment (using $\theta_i^{pop} * \theta_i^{lfp} * \theta_i^{emp}$). Each θ generates a distinct ΔL_s that describes changes in labor supply at the corresponding margin. The product of the four θs matches the hours-weighted skill distribution across periods, as needed for computation of Δw_s via (18).[23]

The required selection-on-observables assumption is unattractive, and its failure will lead to mis-measurement of wage changes. Recall from above, however, that wage changes at each skill level are the same for all demographic groups. Thus, dependence on the selection-on-observables assumption can be lessened by measuring wage changes from groups that saw small changes in tax rates and, consequently, small changes in observed skill distributions.

[23] A version that carries out the reweighting in a single step yields similar estimates of Δw_s.

6 Data

My primary source for measures of wages and labor supply is the Current Population Survey (CPS) Outgoing Rotation Groups (ORGs), with observations on hourly wages and hours worked in the previous week for three to five thousand female workers each month. I assemble a pre-reform sample by pooling data from the 1992 and 1993 ORG files, and I pool data from September 1995 through August 1997 for the post-reform sample. When I use the pre-reform period to difference out demand shifts, I use the 1988 and 1989 ORG files for the beginning of this period.[24] In each case, the sample consists of women aged 16-64 in primary families. I exclude the self employed, observations with hourly wages (in real January 1992 dollars) below $1 or above $100, and observations with allocated wages. Although allocation rates are substantially higher in the post-reform sample than in the pre-reform sample, the change does not appear to vary with EITC exposure.

Table 1 presents summary statistics for the ORG sample. The first panel presents statistics for the full sample and for subgroups defined by marital status and the presence of children. Average ages rose by three to six months over the three year period considered here, and average education rose by about 0.2 years.[25] Because wages increase with age and education, this caused reductions in the relative labor supply of low-skill workers and increases in mean wages, independent of any behavioral changes. The second panel presents mean characteristics of workers, weighted by the number of weekly hours. Mean ages and education levels again increased in most groups but not among single mothers, for whom population shifts were offset by relative increases in supply from younger and less educated single mothers. The final row presents mean log hourly wages. Changes are fairly small in each group, with increases among married women and single childless women and a decline among single women with two or more children, again consistent with a compositional shift toward lower-skill women.

The ORG files do not report annual income, so do not permit simulation of the tax rate. I use TAXSIM (Feenberg and Coutts, 1993) to simulate the total federal and state tax burden and the marginal tax rate for each woman in the Annual Demographic Survey (i.e., the March CPS).[26] I use the 1993 and 1994 samples (describing the 1992 and 1993

[24]Each household appears in the ORG twice, at an interval of one year. To ensure a sample of unduplicated respondents, I use only observations in their 8th month in sample (i.e. the second ORG appearance) from 1992 and only those in their 4th month (the first ORG appearance) from 1989 and September 1996-August 1997.

[25]There are two sources of this shift. First, the population age distribution changed somewhat, and less-educated older individuals were replaced by younger cohorts with higher average education. Second, in 1994 and 1995 the Bureau of Labor Statistics introduced new sampling weights based on 2000 rather than on 1990 Census data.

[26]In complex families, the tax filing unit can be difficult to identify. I assume that tax filing units consist

tax years) for the pre-reform tax computation and the 1996 and 1997 samples (describing 1995 and 1996) for the post-reform computation. A working woman's ATR is computed as ${}^{\tau(y_i)-\tau(0)}/_{y_i}$, where $\tau(y)$ is the family's tax liability when the woman's earnings are y and y_i are her actual earnings. Ideally, my measures would include the effective tax rates created by the phase-out of transfer programs – Medicare, SSI, welfare, etc. – as family earnings rise, but these are difficult to measure with any accuracy. My omission of these programs may lead me to mis-measure changes in tax rates over the mid-1990s. I present a specification check below that suggests that the most important contemporaneous change, welfare reform, does not create major bias in my estimates.

As noted above, my analysis of tax responses is at the level of the demographic and skill group. I consider six demographic groups defined by the intersection of marital status (married and single) and number of children (zero, one, and two or more). For each group in each period, I use the March data to estimate kernel regressions of working women's tax rates on their hourly wages. The estimated mean tax rates are then used for contemporaneous ORG observations of the same family type and wage level. Because wages are measured more accurately in the ORG than in the March data – where the hourly wage is computed as the ratio of annual earnings to annual hours worked – there is some misclassification in this matching. As we shall see, the identification of tax effects relies on gross differences between low-, middle-, and high-skill workers, so is not likely to be very sensitive to this measurement error.

6.1 Mean tax changes by skill level

Although taxes are functions of annual earnings, in practice there is a strong relationship between the hourly wage and EITC eligibility. Columns 8, 9, and 10 of Appendix Table 1 list the wage rates at which a full-time, full-year breadwinner would reach the plateau, the beginning of the phase-out range, and the exhaustion of the credit. The first of these is below the federal minimum wage (shown in Column 11) in every schedule. The wage needed to reach the phase-out range is generally only a bit above the minimum. Two-income families reach the phase-out even more easily, and are ineligible for the EITC at all if both parents work full time at wages a bit above the minimum.

The dotted lines in Figures 2 (MTRs) and 3 (ATRs) show the change in mean tax rates – including the EITC and all other federal and state taxes but not the employer

of a woman, her husband (if present), and any resident children (under 18) of either. I drop members of subfamilies, except children who could be claimed by the head of the household (e.g., her grandchildren). I do not count resident children aged 18-24, though they can count toward EITC eligibility under some circumstances. As in the ORG data I discard duplicate March observations on the same households.

share of payroll taxes – among workers at the same real wage level between 1992/3 and 1995/6. This is a poor measure of the tax shock to skill-specific labor markets, as the same real wage may correspond to different skill levels at different times. The figures also show two methods of adjusting for this. First, I use the estimates developed below of the change in wage schedules between 1992/3 and 1995/7 to identify the wage level paid in the latter period to workers of the same skill as those who earned wage w in 1992/3. For example, my estimates indicate that a single, childless woman whose skills were valued at $5 per hour in 1992 would have earned $5.21 (in 1992 dollars) in 1996; the solid lines in figures thus compare mean MTRs and ATRs of $5 workers in 1992 to those of women earning $5.21 in 1996.

Observed tax rate changes are endogenous to changes in the quantity and price of labor at each skill level, as unobserved shocks to annual earnings may lead to changes in tax rates. I form simulated instruments for the changes in tax rates by comparing rates obtained by applying 1995 tax schedules to the observed 1992/3 data with the actual 1992/3 tax rates.[27] These are shown by dashed lines in Figures 2 and 3.[28]

The various methods yield very similar estimates of tax changes, both within and between groups. Figure 2 shows that mean MTRs fell for single mothers with two or more children and very low pre-expansion wages but rose for those with wages between about $5 and $12. This corresponds to the segments of the credit: Workers at wages around $5 are typically in the phase-in range, while those at higher wages are more likely to be in the phase-out. Among single women with just one child and, to a much lesser extent, among married mothers, mean MTRs rose at all wages below $12.[29] Mean ATRs fell for single working mothers at all wages below about $11 (Figure 3), with the largest declines at the lowest wages and in multiple-child families. Low-wage married mothers saw slight increases in mean ATRs, as the EITC's phase out taxes secondary workers' earnings. Despite the extension of a very small EITC to women without children, these groups show essentially no change in average ATRs and MTRs.

[27] To incorporate bracket creep, the tendency for rates to change as inflation and real wage growth shift workers into higher tax brackets, I inflate 1992/3 earnings by the CPI plus 1% per year before simulating the counterfactual schedule.

[28] I treat marital status and number of children as exogenous to the EITC. Although the credit rules create incentives to remain single and to have more children, there is little evidence for sizable effects on these margins (Baughman and Dickert-Conlin, 2006; Dickert-Conlin and Houser, 2002).

[29] A proportionate expansion of the EITC would have reduced MTRs for very low-wage single women with one child. This is offset by a slight leftward shift in the C/τ_1 kink point for this group (Figure 1), which moved some high-hour workers from the phase-in range to the plateau and raised their MTRs.

6.2 Difference-in-differences and DDDD estimates

Some simple regressions illustrate the variation that I exploit. Assuming that ATRs are more important determinants of labor supply than MTRs–this assumption is supported by the existing literature and by the results below–Figure 3 indicates that low-skill single women with children were "treated" by the EITC expansion, relative to higher-skill, married, or childless women. We should therefore expect increased labor supply among these women, and reduced wages for all low-skill women.

I begin by estimating a flexible specification for log hourly wages using the pre-reform data. I use the resulting coefficients to form a "skill index"–predicted wage–for each observation in both pre- and post-reform data, which I standardize to have zero mean and unit variance. I then estimate a regression of labor supply on the skill index; indicators for being unmarried, for having children, and for appearing in the post-reform data; and all two-, three-, and four-way interactions of these variables. Because low-skilled single women with children in the post-reform period were "treated" with reduced ATRs, the expected labor supply response to the EITC would produce a negative coefficient on the four-way interaction.

Column 1 of Table 2 presents marginal effects from a probit model for labor force participation, estimated on the ORG data. Estimates are negative and significant for both one- and two-or-more-child families, though the two-child estimate is substantially larger in magnitude. This is exactly the pattern we would expect if participation responds to tax incentives. Column 2 presents a model for employment. The estimates have the same signs and the same relative magnitudes, though both are smaller than in Column 1 (and only the two-child coefficient is significant). Evidently, only a portion of the participation impact of the reform is reflected in increased employment. Column 3 presents a linear model for weekly hours conditional on employment. There is no indication of an impact here. Column 4 combines the previous two outcomes, modeling total weekly hours per person. The participation effect dominates here and the point estimate for two-child families is consistent with shifts solely between non-employment and near full-time employment (-0.885/-0.028=31.6 hours per worker), though the estimate is imprecise.

Column 5 presents a model for log hourly wages. Recall that the EITC should have had the same effect on the wages of ineligible women as it did on those of eligible women in the same labor market, so the four-way interaction coefficients should be zero. One is significantly negative, a result that disappears in the more sophisticated analysis below. I also show the two-way skill-time interaction and the three-way interaction of these with marital status. The former should capture any incidence effects if all women compete in the same labor market, while the latter is more relevant if single women are in a distinct market. Both coefficients are zero, indicating no wage changes of the form that incidence models would imply. Again, these results are not robust to more careful controls for changes in the skill composition of labor and in labor demand.

7 Results

7.1 Decomposing labor supply and wage changes

I begin by applying the DFL reweighting strategy to distinguish changes in labor supply from changes in the wage schedule. Pooling data from 1992/3 and 1995/7, I estimate propensity scores for appearing in the later data. The propensity score model is estimated by probit, and includes the full interaction of four education dummies and eight potential experience categories; separate intercepts, linear education terms, and quadratic potential experience terms for whites, blacks, Hispanics, and Asians; the number of children under age six; state fixed effects; and indicators for residence in a metropolitan area and in a central city. All are fully interacted with marital status and indicators for zero, one, and two or more children. As noted earlier, I compute a sequence of propensity scores, weighting by different measures of labor supply. I use each to compute the corresponding weights $\hat{\theta}_i$. I then estimate ΔL_s via family-type-specific kernel regressions (using an Epanechnikov kernel and a bandwidth of 0.03 log points) of $\left(\hat{\theta}_i^{-1} - 1\right)$ on w_i using period-0 data.[30]

Demographic shifts produce changes in the skill composition of labor supply that are exogenous to tax rates. I begin by "cleaning out" purely demographic changes, reweighting the full CPS samples – irrespective of labor force participation – to balance the distribution of observables over time. With balanced population samples, I can consider changes in labor supply decisions, beginning with labor force participation. As described above, I use labor force participants from the 1992/3 and 1995/7 samples to estimate a probit model for appearance in the latter period, then use the fitted values to compute reweighting factors. Figure 4 graphs estimated changes in labor force participation, ΔL_{gs}^{lfp}, against the 1992/3 wage, w_{s0}. The shaded areas also show 90% pointwise confidence intervals. Participation rates rose for all groups and all wages, but not evenly. There are large skill-biased changes in participation among single mothers, with dramatic increases at the bottom of the skill distribution. These increases are largest for women with two or

[30] Inference is conducted via bootstrap with 600 replications. I select bootstrap samples (with replacement) from the ORG data and re-estimate the propensity score on each bootstrap sample. I construct pointwise 90% confidence intervals for $\Delta \hat{w}_s$ and $\Delta \hat{L}_s$ as the range between the 5th and 95th percentiles of the distribution of estimates across bootstrap replications..

more children, among whom participation rates grew by as much as forty percent.

After again reweighting the data to balance the observable characteristics of labor force participants, I estimate a third propensity score to describe changes in employment conditional on participation. Figure 5 shows that conditional employment rates declined among low-skill women of all six demographic groups and particularly among single mothers. Some of the new labor force participants from Figure 4 evidently transitioned into unemployment rather than into employment. Changes in conditional employment are smaller than those in participation, so unconditional employment (not shown) has a pattern like that for participation though with a smaller scale.

Finally, Figure 6 displays intensive-margin changes in usual weekly hours conditional on employment.[31] There is no indication of large changes on this margin, either across demographic groups or across skill levels within groups. Thus, when I combine the three reweighting factors to obtain the change in total weekly hours per woman (Figure 7), the extensive margin changes dominate. Hours rose substantially among low-skill single women with two or more children, rose by a smaller amount among one-child single mothers, and declined somewhat for low-skill married mothers (this is driven by the unemployment effect discussed earlier). This pattern of changes is exactly what we might have predicted from the EITC expansion, which created large incentives for low-skill single mothers, particularly those with two children, to enter the labor force.[32]

The reweighting procedure provides informative measures of labor supply changes, but its primary value is to permit measurement of changes in the price of skill. Changes in the wage schedule are graphed as solid lines in Figure 8, with the 1992/3 log wage on the horizontal axis. The shaded area shows confidence intervals. For comparison, the dashed line indicates the change in the wage distribution unadjusted for composition changes. This is merely a Q-Q plot for log wages, flattened by subtracting the $t = 0$ log wage from the $t = 1$ series to make changes more visible.

To illustrate the computation, consider the 10th percentile of the single childless women's wage distribution in 1992/3, \$4.86. The 10th percentile wage for this group in 1995/7 is \$5.14, and $\ln(5.14) - \ln(4.86) = 0.056$, so $(4.86,\ 0.056)$ is one point on the dashed line in the upper left panel of Figure 8. There were increases between 1992/3 and 1995/7 in the share of single childless women's labor coming from high-skill women,

[31] I perform two additional reweighting steps: self employment rates conditional on employment, before the hours analysis, and wage allocation rates, after the hours analysis. Changes on these margins are uncorrelated with tax rate changes (see Table 4, below).

[32] Eissa and Hoynes (2004) find that low-skill married mothers with husbands whose earnings placed the family in the phase-out range reduced their labor force participation in response to the 1993 EITC expansion. Only a small fraction of working married women faced this incentive, however, so their response does not have detectable effects on the aggregate supply of labor.

driven primarily by population aging. This pushed fixed skill points downward in the distribution, so the skill level that was at the 10th percentile of the 1992/3 distribution was lower in the distribution in 1995/7. The reweighted 1995/7 data adjust for this. In these data, the 10th percentile wage was \$5.05, so the skill group whose labor sold for \$4.86 in 1992/3 saw that price rise by 3.8% by 1995/7, not the 5.6% indicated by the raw change in the wage distribution, and the solid line passes through (4.86, 0.038).

The naive estimates indicate rising inequality among married women, and particularly among married women with children. This derives to a large extent from demographic changes rather than from shifts in the wage schedule. Composition-corrected wage changes for married women are approximately zero, except that the returns to very high skills increased among married women with two or more children.[33] For single women – whose labor market was most affected by the EITC expansion – wages rose or were stable at the bottom of the distribution (initial wages below about \$6.50), fell in an absolute or at least relative sense in the middle, and were roughly stable at the top. There is little evidence of across-the-board reductions in relative or absolute wages for mid- and low-skill women, as would be expected if the increased labor supply from single mothers in these skill groups bid down market wages and if there were no countervailing shifts in demand. Indeed, the most prominent change is the increase in wages at the bottom of the distribution relative to the middle. Finally, note that wage schedule changes are generally similar for single women with and without children, suggesting that the former estimates are not greatly biased by selection on unobservables.

A potential explanation for the rise in low-skill workers' wages is that the positive shock to relative labor supply produced by the EITC was overwhelmed by a technical change-driven increase in relative demand. Indeed, recent evidence suggests that technical change during the mid-1990s favored low-skill over medium-skill labor (Autor et al., 2005a). That evidence indicates that the rate of technical change was approximately constant from around 1987 through the late 1990s. If so, it may be possible to difference out the effects of demand shifts. I repeat the DFL procedure using 1988-89 and 1992-93 data to eliminate the effect of compositional shifts during the earlier period, then compute the change in wages at skill levels corresponding to points in the 1992-3 wage distribution. Wage patterns over this earlier period are qualitatively similar to those in Figure 8, with rising relative wages for the lowest-skilled workers. This lends support to the SBTC explanation. There were no large changes in labor supply in the earlier period.

[33] The pattern here – composition-corrected changes resembling those produced by changes in observables and diverging from those seen in other groups – seems consistent with changes in the distribution of unobservables. As this group is not central to my analysis, which focuses on low-skill single women, I do not pursue it further.

7.2 Within-skill analyses of labor supply responses

Having estimated the change in the supply and price of labor at each skill level and in each demographic group, I can now relate these changes to the changes in mean tax rates shown in Figures 2 and 3. I create a data set consisting of the estimates of ΔL_{gs} and Δw_{gs} for each of the six family structure groups at skill levels corresponding to half percentiles of the 1992/3 wage distribution, and merge these to estimates of $\Delta \tau_{gs}$ obtained from the March data for the corresponding demographic group and wage level.[34]

I begin by modeling supply in isolation, comparing identically-skilled workers across family types. Equation (9b) suggests that an appropriate specification is

$$\Delta L_{gs} = A_g + B(s) + \Delta \tau_{gs} * C + \varepsilon_{gs}, \tag{20}$$

where ΔL_{gs} is the change in labor supply at skill s from family type g and $\Delta \tau_{gs}$ is the change in mean tax rates. $B(s)$ is a skill level control, and must be sufficiently flexible to absorb any across-skill influences on supply, including the $d\bar{\tau}_s$ and $d\ln \beta_s$ effects from equation (9b).

Table 3 presents estimates of (20).[35] The dependent variable in each specification is the change in per capita weekly hours worked. I use data from all six family structure groups in Panel A and from only single women in Panel B, and I consider three tax measures: the marginal tax rate (MTR) in "Model 1," the average tax rate (ATR) in "Model 2," and both together in "Model 3." I report only the C coefficients, which estimate -1 times the supply elasticity with respect to either the marginal (MTR) or average (ATR) wage. I use three specifications for $B(s)$. It is omitted in column 1. In column 2, $B(s)$ is a simple linear control for the log wage in the base year. In column 3, I include fixed effects for each skill level, thereby removing all restrictions on $B(s)$. In this specification, C is identified only from within-skill, across-demographic-group variation in tax rate changes. Finally, in Columns 4-6 I instrument for the the change in tax rates with the simulated instrument, the predicted change from pre-reform data.

When the ATR and MTR are entered separately in OLS specifications using all women, each is significant. The estimated elasticity of supply with respect to the marginal wage is around 0.3, and the elasticity with respect to the average wage is 0.57. When the two tax

[34] I discard the top and bottom 3 percentiles, leaving 187 skill groups. I discuss in Section 8 the possibility of serial correlation between observations in adjacent skill groups.

[35] As above, I use the bootstrap for inference, re-estimating the DFL reweighting factors and regression (20) on each of 600 bootstrap draws from the underlying microdata. I compute standard errors as 0.74 times the interquartile range of the coefficients across draws. This is robust to tail behavior – which is a problem in some of the IV specifications, where one or two replications yield first stage coefficients very near to zero – and estimates the standard deviation if the distribution is normal.

variables are entered together, however, the MTR effect disappears–the point estimates fall to approximately zero, and are insignificant–while the ATR effect remains. The ATR effect is moreover robust to instrumentation and to the exclusion of married women, while the estimated MTR effects are small and insignificant in these specifications.

The estimates in Table 3 thus indicate sizable supply elasticities with respect to the average wage, but no effect of marginal wages on hours worked. To probe further, I use the successive propensity score models described in Section 7.1 to examine the various margins of labor supply separately. Table 4 presents specifications similar to those in Table 3, Panel B, Column 6. The first panel presents results for each margin of the DFL decomposition: Population size (row 1), labor force participation (2), employment if participating (3), non-self-employment if employed (4), hours if employed (5), and non-allocated, valid wages (6). The patterns match those seen in Figures 4 through 7: MTRs have essentially no effect on any margin of labor supply. ATR effects on participation are significant and large, indicating an elasticity with respect to the average wage around 1.1-1.2. By contrast, the coefficient for employment conditional on participation is positive, suggesting that employment rates (i.e., one minus the unemployment rate) have an elasticity of about -0.35 with respect to a woman's average wage. Neither ATRs nor MTRs affect hours conditional on employment, rates of self employment, or wage allocation.

The second panel presents several interesting combinations of the labor supply measures. The overall employment elasticity (row 7) combines the participation response and the smaller, offsetting employment response, for a net ATR effect around -0.6. Hours per person (row 8) have a slightly larger response. On the other hand, this is somewhat offset by demographic shifts, so the ATR effect on total hours worked (row 9) at each skill level is smaller and insignificant.

The results thus appear to indicate that responses are exclusively on the extensive margin, consistent with the strong effect of ATRs rather than MTRs. Because only one week's labor supply is observed in the ORG data, however, they cannot distinguish true extensive margin responses from intensive responses that take the form of changes in weeks worked per year. To examine this issue, Rows 10 and 11 show estimates for changes in the probability of annual labor force participation and in annual hours conditional on participation, both computed from the March data. The March sample is smaller than the ORG and the estimates are imprecise. However, the results are at least consistent with a response that is solely on the margin of annual participation.

Finally, row 12 presents models in which the dependent variable is the log hourly wage. Recall that the specifications in this table include skill fixed effects while demand is in theory constant across demographic groups at the same skill level, so the coefficients should be zero. On the other hand, if new labor force entrants drawn in by the EITC expansion are selected on unobservables, this would bias the estimated wage changes for EITC-affected groups and would likely yield a non-zero coefficient in these models. Both ATR and MTR coefficients here are almost exactly zero.

7.3 Across-skill analyses of wage changes

I now return to the two equation system, (9), and I exclude skill fixed effects in favor of the across-group mean tax rate changes. I compute $\Delta\bar{\tau}_s$ as the across-g average of the change in tax rates, $\Delta\tau_{gs}$, with groups weighted by their shares of pre-expansion skill-s labor supply. I explore two constructions of this average: over all six groups (married and single by zero, one, and two or more children) and only over the three groups of single women. The first is appropriate if all women compete in the same labor market, while the second is better if single and married women participate in distinct labor markets. (This possibility is suggested by the series in Figure 8, which indicate divergences between married and single women's wages between 1992 and 1996.) I estimate specifications of the form:

$$\begin{aligned}
&(21\text{a}) \qquad \Delta L_{gs} = A_g^L + w_{s0} * B^L + \Delta\tau_{gs} * C^L + \Delta\bar{\tau}_s * D^L + u_{gs}^L \text{ and} \\
&(21\text{b}) \qquad \Delta w_{gs} = A_g^w + w_{s0} * B^w + \qquad + \Delta\bar{\tau}_s * D^w + u_{gs}^w \ ,
\end{aligned}$$

where A_g^L and A_g^w represent demographic group fixed effects and the w_{s0} controls are included to absorb any SBTC or cyclical factors that are linear in the base log wage. As in (9a), the own-group change in taxes is excluded from the wage equation (21b).[36]

The first column of Table 5 reports the correspondence between the coefficients of equations (21a) and (21b) and the underlying elasticity parameters. I include two additional regressors measuring purely demographic change in the labor force, which is gradually aging and becoming more educated. Even in the absence of behavioral changes, the supply of high-skill labor would have risen relative to low-skill labor. A straightforward extension of the earlier model incorporates shifts in labor supply coming from demographic changes: Exogenous supply increases drive down the wage and thereby reduce endogenous supply.[37]

[36] I have also estimated specifications that include this term, which should have absorbed biases in my measure of Δw_{gs} coming from differential changes in unobservables for single mothers. Its coefficient was always small and insignificantly different from zero, and other coefficients were largely unaffected.

[37] Whether purely demographic changes in the skill composition of the labor force have the same impacts on equilibrium wages as do behavioral shifts in labor supply behavior depends on many factors, including the openness of the U.S. economy and life-cycle patterns in consumption. Specifications that constrain the population effects to zero give nearly identical results.

Columns 2 and 3 present estimates of (21a) and (21b). I use as dependent variables the changes in labor supply and wages for single women, stacking observations for the zero-child, one-child, and two-or-more-children groups. Following the results in Tables 3 and 4, I focus on ATRs, instrumenting for $\Delta\tau_{gs}$ with the simulated change in the s-g cell and for $\Delta\bar{\tau}_s$ with the average of this across g. In Column 2, I treat single women as a distinct labor market, and average ATRs only across single women. In Column 3, I instead assume that all women participate in the same labor market, and I include married women's tax rates in the ATR average.

The own-group tax rate coefficients for labor supply are similar to those in Table 3. Population coefficients are also reasonable, though imprecisely estimated. The coefficients on the across-group average ATR, however, take the wrong signs in both the labor supply and wage equations and in both specifications, and the wage effects are significant. Skill groups whose mean ATRs fell the most saw increased wages and expanded labor supply among EITC-ineligible women.

This result suggests that tax effects are confounded by skill-biased demand shifts. An exogenous increase in the relative demand for low-skill labor would have raised wages at skill levels where $\Delta\bar{\tau}_s$ is negative, potentially producing the observed pattern of coefficients. As noted earlier, by augmenting the sample with data on wage and labor supply changes between 1988 and 1992, I can add skill-group fixed effects to (21a) and (21b). These absorb any technical change that proceeded at the same rate over the two windows.[38] I also include an interaction of time with the 1992 log wage, to allow for "tilting" in the form of technical change over time or for cyclical effects. Tax effects are identified from nonlinearities in the across-skill relationship between log wages and the second difference of (simulated) tax rates.

Estimates are reported in Columns 4 and 5 of Table 5. These are much more reasonable, though imprecise. In particular, we now see the expected positive coefficient on the mean ATR in the wage equation, though this is only significant in Column 5. The mean ATR coefficients in the labor supply equation are not clearly positive, but positive values are well within the confidence intervals.[39]

[38] There are two potential confounding factors here: The 1988-92 and 1992-96 periods come at different points in the business cycle, and the federal minimum wage was raised in 1990 and 1991. To avoid the effects of the latter, I exclude skill levels whose 1992/3 wages were below \$4.38, the peak real value of the minimum wage over this period, from the stacked analysis. Business cycle effects are harder to deal with, as there is no expansionary period other than the 1992-1996 period that had similar SBTC trends but no policy-induced labor supply shocks. Relative demand for low-skill labor is generally thought to be pro-cyclical (Hoynes, 2000). This suggests the comparison of relative wage changes for low-skill workers in 1988-1992 and 1992-1996 will tend to indicate overly elastic demand.

[39] The elasticity parameters can be recovered from the coefficients in Table 5 via optimal minimum distance (Abowd and Card, 1989), with three overidentifying restrictions. Estimates are quite noisy,

7.4 Inverse demand specifications

A more direct method of estimating the demand elasticity is via an inverse demand equation. This also permits me to examine the possibility that selection-on-unobservables leads to mismeasurement of wage changes in groups that were directly affected by the EITC expansion. I estimate specifications of the form

$$\Delta w_{gs} = D_g + w_{s0} * E + \Delta L_{gs} * F + \Delta L_s * G + v_{gs}. \tag{22}$$

Here, ΔL_s is the proportional change in skill-s labor, averaged across demographic groups. When ΔL_s is instrumented by the simulated change in tax rates, G estimates the inverse of the demand elasticity, ρ^{-1}. The same potential biases arise here as in Table 5. First, wage changes (Δw_{gs}) may be mis-measured if the selection-on-observables assumption is violated. Second, the estimated inverse demand elasticity will be confounded by any shifts in labor demand that are correlated across skill groups with the simulated instrument. To investigate the former issue, I distinguish between measures of Δw_{gs} for groups directly affected by the EITC expansion, among whom there were large composition changes, and for those not directly affected, among whom the observables composition of labor supply did not change meaningfully. For the latter, I again use the 1988-1992 period to attempt to difference out demand shifts.

If the various groups are indeed perfect substitutes in production, Δw_{gs} should not vary across g groups. I do not impose this constraint in the DFL decomposition, so empirically $\Delta \hat{w}_{gs}$ varies slightly with g. Inclusion of ΔL_{gs} (instrumented with $\Delta \tau_{gs}$) in (22) can be seen as a test of the selection-on-observables assumption. A non-zero estimated F coefficient would indicate that bias in $\Delta \hat{w}_{gs}$ is correlated with the tax change across s-g cells.

Table 6 presents estimates. The first two columns stack observations on wage changes between 1992 and 1996 for all three groups of single women. In the first, the key regressor is the change in labor supply among single women, the appropriate measure of the supply shock if single women form a distinct labor market. The estimate indicates a sizable, positive wage response to the EITC-induced increase in labor supply; the effect of a 1% increase in supply is to increase wages by 1.21% (s.e. 0.54%). As in Table 5, this is counter to the theoretical prediction that supply increases will drive prices downward. Column 2 replaces the average supply change among single women with that among all women, as would be appropriate if all participate in the same labor market. The coefficient is much larger than that in Column 1, but it is imprecisely estimated; there is again no evidence

however, and the restrictions are rejected in each specification.

that measured wage changes in each group are correlated with own-group tax rates.

There is no indication in Columns 1 and 2 that selection-on-unobservables biases wage measurements, as both of the own-group labor supply coefficients are approximately zero. As another check, however, Columns 3 and 4 use only single, childless women's wage changes for the dependent variable. There were no notable changes in the distribution of observed characteristics in this group, and the selection-on-observables assumption is quite plausible here. The coefficient on the own-group change in labor supply is constrained to zero in these columns, as there is no instrument for this group's supply. We still see large positive coefficients on the all-single-women and all-women changes in labor supply. The final row of the table converts the estimates of the coefficient G to a demand elasticity, $\rho = G^{-1}$. This is positive in each of the first four columns, significantly so in Columns 1 and 3.

The estimates in Columns 1-4 of Table 6, like those in Table 5, appear consistent with a positive shock to the relative demand for low-skill labor. I can again attempt to remove this effect by adding data on wage changes between 1988 and 1992. As before, the identifying assumption is that SBTC was approximately constant over the two periods, and is therefore absorbed by skill fixed effects. As in the earlier specifications, I also include a time-specific linear term in the base log wage to allow for tilting of demand trends over time. The augmented $\hat{G}$ estimates (Columns 5 and 6 of Table 6) take on the correct sign, indicating that a 1% increase in the quantity of labor supplied reduces wages by 3.6% (if single women constitute the labor market) or 2.7% (if the market includes married women). These coefficients imply small but negative demand elasticities, -0.28 in Column 5 and -0.37 (insignificant) in Column 6. This aligns closely with Hamermesh's (1993) "best guess" for the constant-output demand elasticity of -0.3. However, my estimates imply much less elastic demand than do those that are identified from immigration-induced shocks to the supply of low-skill labor, where the implied demand elasticities range from -2.5 or -3 (Borjas, 2003) to negative infinity (i.e. perfectly elastic) (Card, 2005).

8 Robustness

I discuss in this section several possible biases that have not yet been addressed. I begin with two that do not seem able to account for the pattern of results presented so far. First, the minimum wage might have constrained wages from falling for the lowest-skill workers, perhaps producing the increase in relative wages observed at the bottom of the skill distribution between 1992/3 and 1995/7. Attribution of this to the minimum wage requires that the minimum have extremely large spillover effects: Figure 8 indicates that the decline in wages starts above around \$6 per hour, in a period when the minimum wage hovered around \$4.25. A similar pattern is seen between 1988 and 1992. Relative wages rose at the bottom of the distribution, but this increase extended well beyond the plausible reach of the minimum wage. It seems more plausible that the increase is attributable to shifts in demand than to legislated wage floors.

Second, wages may be misleading about changes in worker compensation. Most EITC-eligible women saw their marginal tax rates increase; although this does not seem to have affected labor supply, it should have raised the value of untaxed work amenities (e.g. flexible schedules) or benefits (health or child care) relative to cash wages, perhaps leading to changes in the composition of compensation. This would be consistent with the decline in relative wages between 1992 and 1996 among mid-skill mothers, as these are the women whose MTRs rose the most. It is not consistent, however, with the similar wage responses for childless single women, for whom MTRs did not change dramatically.[40]

Table 7 presents estimates that investigate two other potential sources of bias. The first is serial correlation. I have treated each half percentile of the pre-expansion female wage distribution as a separate labor market. This is a strong assumption – shocks to the labor market at one skill level probably spill over into the markets for labor at adjacent skill levels.[41] To allow for this, I re-estimated my preferred specifications for labor supply and demand on subsamples consisting of more widely spaced skill groups. Row 1 of Table 7 repeats the preferred estimates from the earlier tables (Model 1 from Table 3, Column 6, Panel B for supply and Column 5 of Table 6 for demand). Rows 2 and 3 restrict the sample to observations corresponding to every 2 or 3.5 percentiles.[42] This has essentially no effect on the estimated elasticities or their standard errors.

A second issue is the near coincidence of the EITC expansion and major reform of the U.S. welfare system, aimed largely at moving recipients into paid employment. I can investigate the effect of welfare reform on my analyses by attempting to exclude from the sample observations that were affected. I drop all observations from 14 states that implemented welfare reform before late 1996, as well as all observations from any state from the 1997 ORG files–leaving October 1995 through December 1996 as the "post"

[40] One might imagine that employers are constrained in the variety of compensation packages they can offer, and that a shift in one group's preferences pulls the average package in that direction. Unfortunately, I not aware of data on non-wage compensation with sufficient detail to permit evaluation of this sort of hypothesis.

[41] Serial correlation could also arise from sampling error in estimated wage and labor supply changes. The bootstrap standard errors should correctly account for this, but cannot account for "true" spillovers.

[42] The base specification uses 187 skill groups. These correspond to 199 half percentiles of the 1992 wage distribution, excluding the top and bottom three percentiles. (The demand analyses exclude additional groups at the bottom of the distribution that might have been affected by the minimum wage between 1988 and 1992.) Rows 2 and 3 use 47 and 27 skill groups, respectively.

period–then reestimate labor supply and wage responses on the shrunken data set.[43] Row 4 of Table 7 presents the resulting elasticity estimates. These are noisier than those from the main sample, but point estimates are similar and if anything indicate a larger (more negative) demand elasticity.

The final column of Table 7 shows the employer share of tax incidence that is implied by the estimates in the earlier columns. Rows 1 through 3 show that employers bear approximately 3/4 of any taxes (and capture the same share of subsidies). Row 4 implies a smaller but still substantial employer share. I discuss the implications of these estimates for the distributional effects of the EITC below.

9 Discussion

This paper has used the labor supply shock induced by the mid-1990s EITC expansion to investigate the labor market effects of income taxes. I extend the traditional incidence model to allow for multiple skill groups and tax schedules, and derive the implications of targeted taxes for wages and labor supply. The extended model shows promise for the empirical evaluation of the incidence of federal taxes, which are otherwise difficult to study given their uniformity across space and their spillover effects on untaxed participants in the same labor markets as those facing the tax.

The EITC provides useful variation, as it creates high effective tax rates targeted at low-skill workers. Previous work has demonstrated sizable labor supply responses to the EITC expansion. Tax induced supply shocks are the key to identification of tax incidence, and it is difficult to imagine another plausible policy change that would produce shocks larger than those created by the EITC in the low-wage labor market. I extend methods proposed by DiNardo et al. (1996) to obtain semiparametric estimates of wage and labor supply changes among women of different skills, then explore several strategies for estimating their relationship to changes in the tax schedule.

The clearest result is that the EITC expansion led to increased labor force participation of low-skill single mothers. Consistent with earlier evidence, women appear to have responded to their average tax rates rather than to marginal rates. Implied supply

[43] The excluded states received major waivers to impose time limits or stricter-than-usual work requirements before September 1996: Arizona, California, Connecticut, Illinois, Iowa, Massachusetts, Montana, North Carolina, South Dakota, Utah, Virginia, Vermont, Washington, and Wisconsin (Bitler et al., 2005; Crouse, 1999). This strategy would not work if welfare reform in the remaining states had effects on labor supply before it was implemented. One component of the reform package was lifetime time limits on welfare receipt. In models with job search or hysteresis, the expectation of future limits can lead to anticipatory changes in labor supply. Nevertheless, it seems likely that the most important effects of welfare reform occurred after its implementation.

elasticities are reasonable, in the 0.5 - 1.0 range. Taking the employment elasticity of 0.6 from row 7 of Table 4, changes in average tax rates account for 26% of the 4.1 percentage point increase between 1992/3 and 1995/7 in the weekly employment rate of single mothers relative to single childless women.[44]

The more novel contribution of the paper is its analysis of wage responses. Wages rose between 1992 and 1996 for the skill groups experiencing the largest supply shocks. Taken literally, this implies a large positive demand elasticity. Further investigation indicates that this result reflects the confounding effects of contemporaneous skill-biased changes in the production technology. A specification that allows for a constant rate of technical change between 1988 and 1996 yields negative but relatively inelastic demand. If anything, this elasticity is still somewhat downward biased by my inability to control for business cycle effects. The evidence thus suggests that the EITC expansion put substantial downward pressure on low-skill wages, but that this was offset by demand shifts favoring low-skill workers.[45]

The EITCs incidence depends on the supply and demand elasticities and on the overlap of eligible and ineligible groups' skill distributions. I simulate an expansion of the EITC that is large enough to increase total payments by $1, distributed across single mothers of various skills in proportion to the mid-1990s expansion. I assume that the supply elasticity is 0.73 with respect to average tax rates and zero with respect to marginal rates, and that the elasticity of demand for single women's labor is -0.28.

Table 8 presents the distributional effects. Row 1 shows the tax credits. Rows 2A-2D show the resulting changes in earnings, decomposed into components due to changes in supply, changes in wages, and a residual component: $\Delta(wL) = L * \Delta w + w * \Delta L + (\Delta L)(\Delta w)$.

The credit induces single mothers to enter the labor force, driving down wages and, in turn, reducing the labor force participation of single women with and without children. Employers of low-skill workers capture a substantial portion of the credit via reduced wage bills, 72 cents per dollar of EITC spending with the parameters used here.[46] Equally

[44] This is on top of a 15% share deriving from changes in the demographic composition of the workforce. These shares are nearly identical to those estimated by Meyer and Rosenbaum (2001). Grogger (2002) estimates a very similar EITC share of changes in single mothers' weeks worked between 1993 and 1999. Looney (2005) estimates a larger EITC effect, though because his total change is also larger he assigns the EITC a smaller share.

[45] The result from Figure 5 and Table 4 that the unemployment rate of low-skill women rose between 1992/3 and 1995/7 appears difficult to square with a positive shock to demand. It seems most easily interpreted as the result of labor market frictions: If the employers do not immediately adjust to labor supply increases by adding jobs, short-run unemployment will result. This will be concentrated among the new entrants, as in Figure 5, but some may spill over to other groups as job changers search longer for openings.

[46] These parameters may not be exactly right, of course, and confidence intervals for this calculation

interesting is the incidence of this transfer on eligible and ineligible workers. All single women at the same skill level see the same change in their hourly wages, but because ineligible childless women form the bulk of the low-skill labor market, they also bear most of the resulting burden (row 2B). While eligible women's supply rises, that of ineligible women falls as a consequence of the reduction in wages. The net change in total labor supply is positive but small (row 2A), only 0.20 as compared with the 0.73 that would obtain with fixed wages. Total earnings fall by just over half as much as is spent on the program (row 2D), with larger reductions among childless women and moderate increases among women with children. All of these effects, of course, are concentrated among low-wage workers.

Row 3 of the table presents the total transfer, equal to the sum of tax credits and changes in wage rates. The transfer to the EITC's intended beneficiaries is \$0.70 per dollar spent on the program, slightly less than the \$0.72 transfer to employers. The cost to ineligible low-skill workers is a substantial \$0.43. The net transfer to workers is only \$0.28.

Row 4 presents changes in after-tax income, adding to the transfers changes in earnings resulting from changes in labor supply. Earnings changes magnify the pure transfers. After-tax income of women with children rises by \$1.21 for every dollar spent on the program, but that of childless women falls by \$0.73, and the net increase in workers' after-tax income is less than half of the amount spent on the credit.

Given the possible confounding of EITC-induced wage effects with other sources of changes in relative demand for low-skill labor, the elasticities used for this calculation may be incorrect. I argued above that the likely bias was toward overstatement of $|\rho|$, which would imply that employers capture an even larger share of the credit. If demand is more elastic than I have estimated, however, the employer share is smaller than 72%, but likely remains substantial. For example, if the true demand elasticity is -1.5 – outside of the [0, 1] range that Hamermesh (1993) claims is plausible – the employer share is only 33%. This continues to be borne to a large extent by uncovered workers, however. The net transfer from women without children is \$0.20 per dollar of EITC spending, while that to women with children is \$0.87.

The calculations discussed here focus on the flow of dollars rather than on welfare. They also ignore the taxes that would be needed to finance an EITC or other transfer. Finally, they ignore preexisting distortions to the low-skill labor market. Since many single

would be quite wide. The EITC's transfer to employers would disappear, however, only if labor supply were perfectly inelastic or demand perfectly elastic. If, as the results here indicate, the elasticity of supply is large relative to that of demand, the transfer from low-wage workers to their employers is substantial.

mothers face positive net effective tax rates that more than offset the EITC's negative rate, EITC expansions produce first-order reductions in deadweight loss. Nevertheless, the EITC is seen by policymakers more as an income transfer program than as a correction for labor market distortions. It transfers much less money to low-skill workers than the statutory distribution would indicate. With my elasticity estimates, the net transfer to single mothers amounts to only about two thirds of what is intended, and that to single workers as a whole is less than one third of the amount spent on the program. Even if the policymaker attaches zero value to single mothers' leisure, and so cares only about total after-tax incomes, the EITC transfers less than half as much per dollar spent as would a targeted lump-sum transfer.[47] Although perfect targeting might be infeasible, a parallel simulation of a similarly-sized NIT that phases out linearly with income–which would be at least as easy to administer as the EITC–indicates that it would have minimal effects on labor supply and earnings, and would therefore be approximately as cost effective as the infeasible transfer. The wage effects of the EITC make it less attractive relative to other income support mechanisms than it first appears.

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Figure 1. EITC Schedule, 1992 and 1996 by number of children

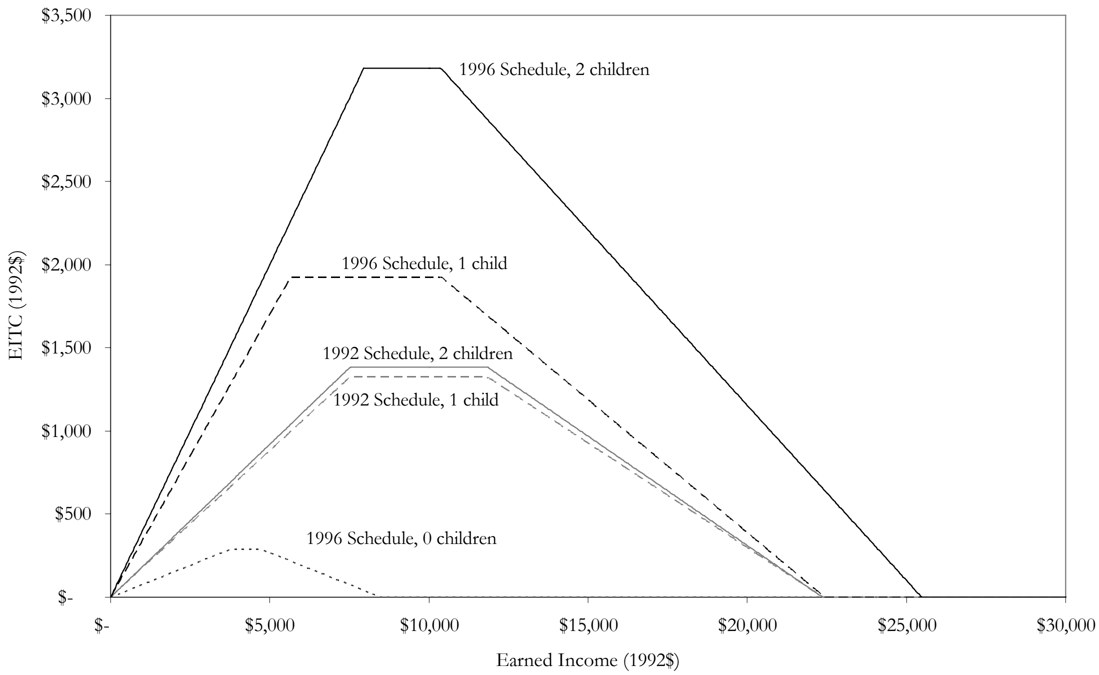
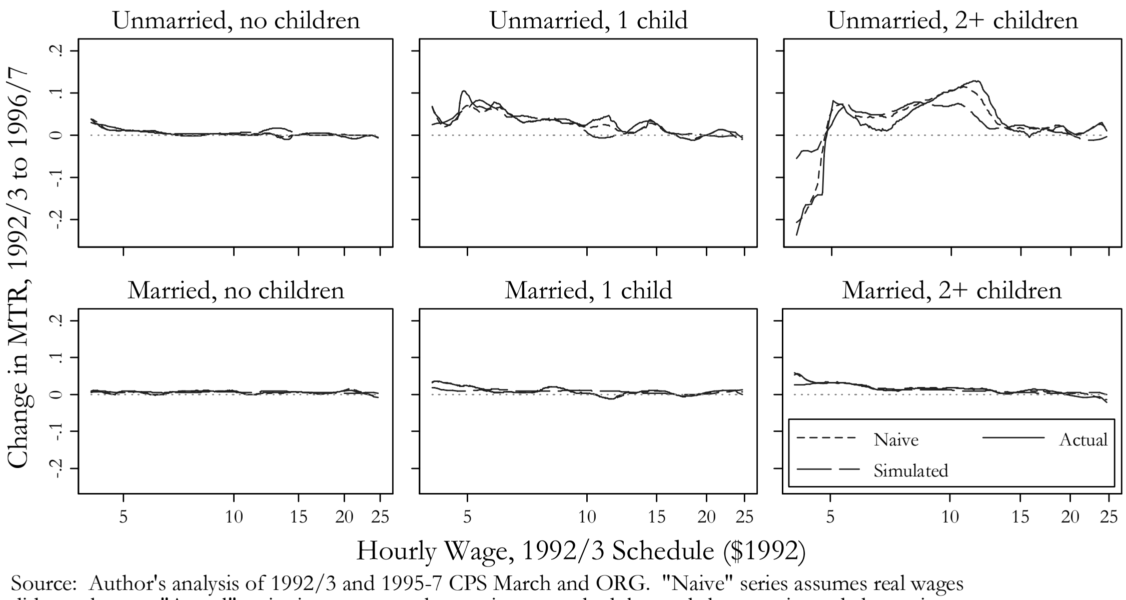
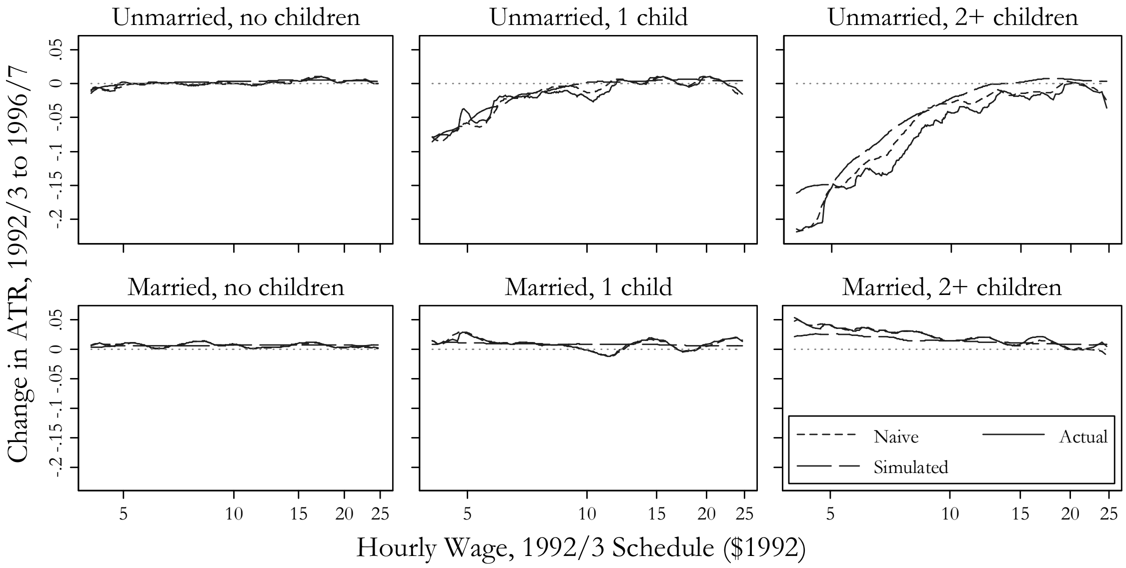


Figure 2.
Change in mean MTR among families with working women, by skill and group



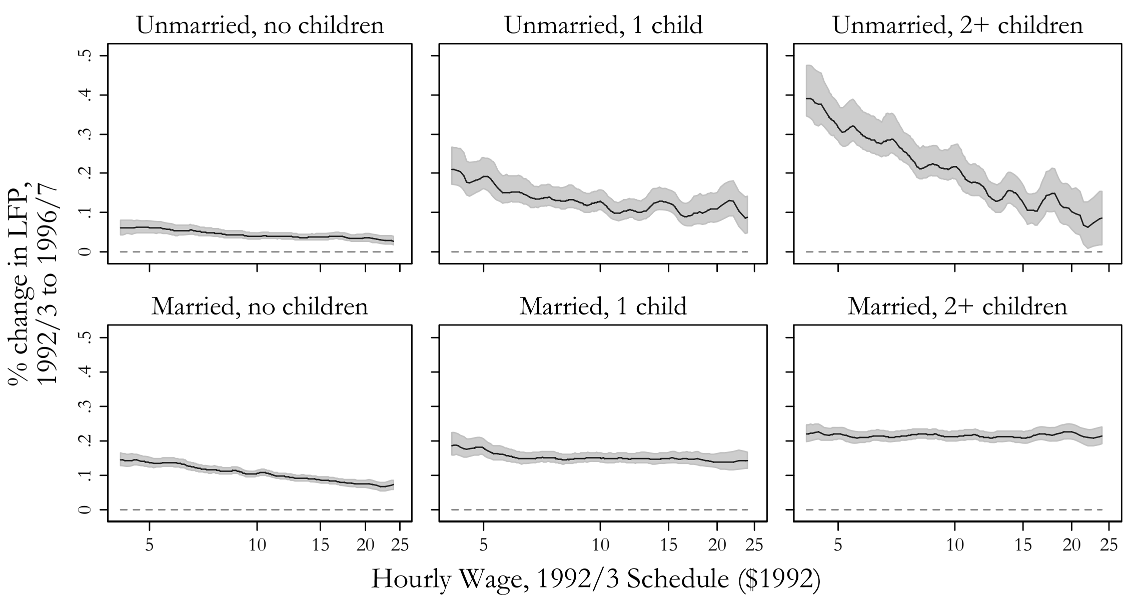
Source: Author's analysis of 1992/3 and 1995-7 CPS March and ORG. "Naive" series assumes real wages did not change. "Actual" series incorporates changes in wage schedules, and shows estimated change in mean MTR for women in the skill group that earned wage w in 1992/3. "Simulated" series is predicted change in mean taxes from pre-period data, holding labor supply constant and assuming real wages grow 1% per year.

Figure 3.
Change in mean ATR among families with working women, by skill and group



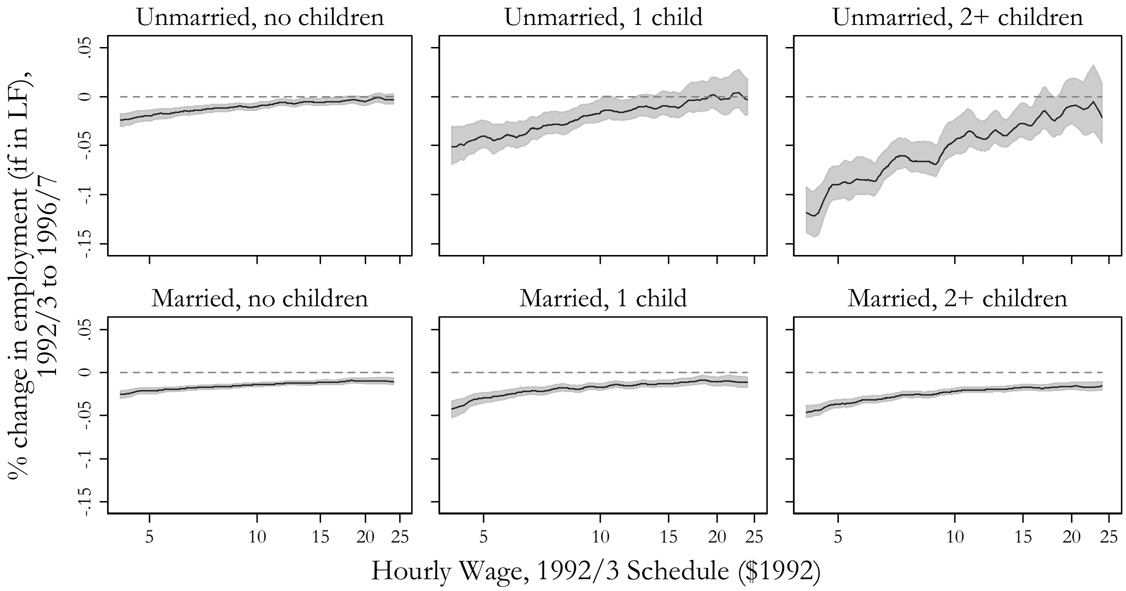
Source: Author's analysis of 1992/3 and 1995-7 CPS March and ORG. "Naive" series assumes real wages did not change. "Actual" series incorporates changes in wage schedules, and shows estimated change in mean ATR for women in the skill group that earned wage w in 1992/3. "Simulated" series is predicted change in mean taxes from pre-period data, holding labor supply constant and assuming real wages grow 1% per year.

Figure 4.
Change in labor force participation, women, by skill and group



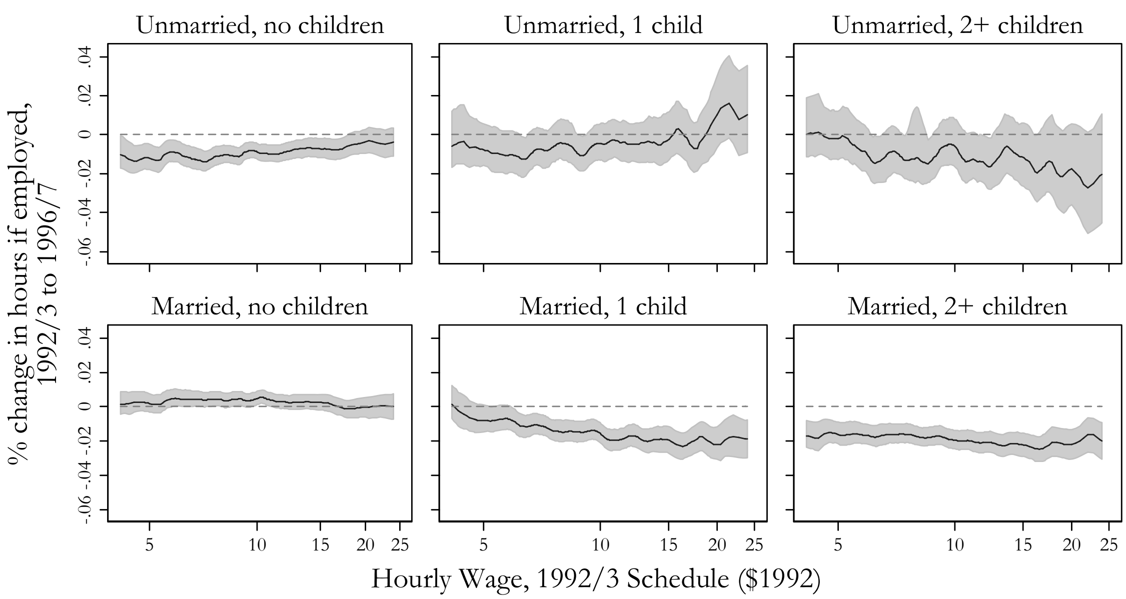
Source: Author's analysis of 1992/3 and 1995-7 CPS ORG. 90% confidence intervals, indicated by shaded regions, are computed by sampling the underlying microdata (with replacement) and feeding bootstrap samples through the DFL algorithm; see text for details.

Figure 5.
Change in employment rate (of those in LF),
women, by skill and group



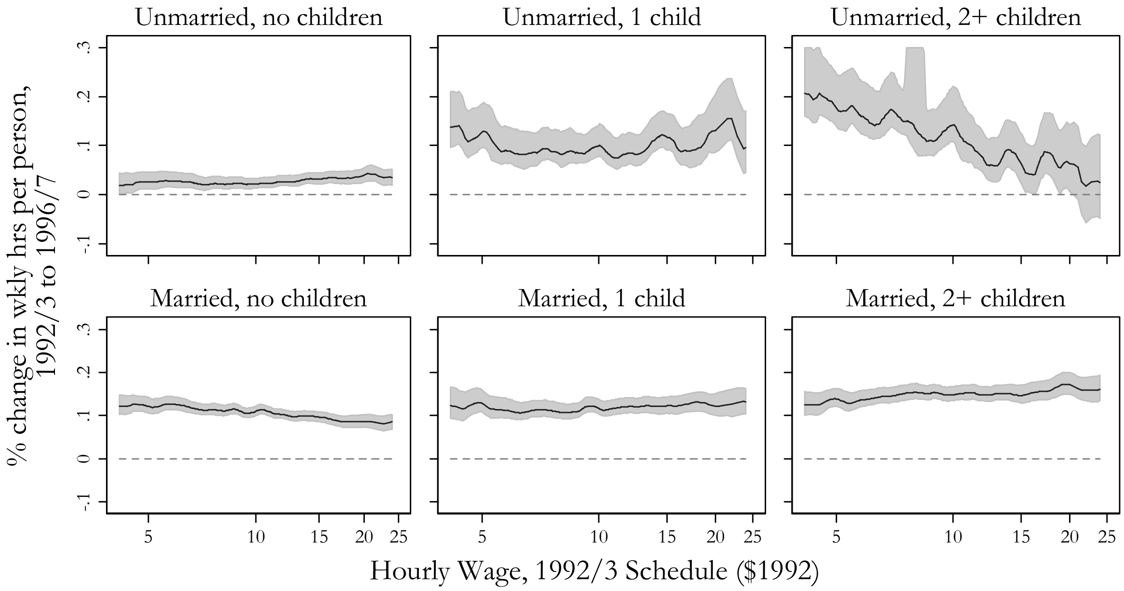
Source: Author's analysis of 1992/3 and 1995-7 CPS ORG. 90% confidence intervals, indicated by shaded regions, are computed by sampling the underlying microdata (with replacement) and feeding bootstrap samples through the DFL algorithm; see text for details.

Figure 6.
Change in usual hours if employed,
women, by skill and group



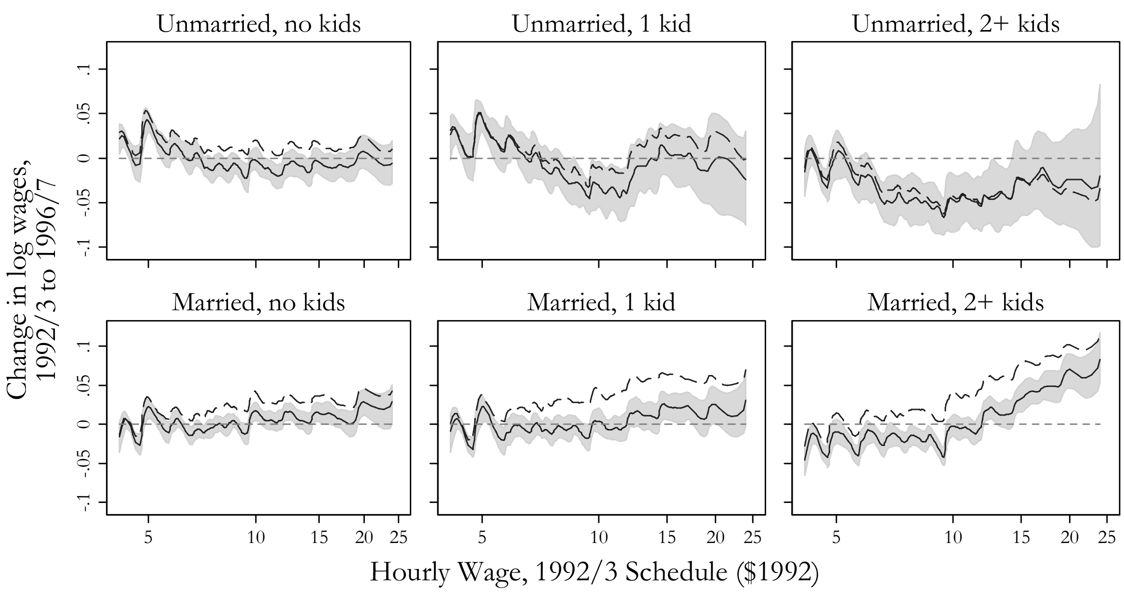
Source: Author's analysis of 1992/3 and 1995-7 CPS ORG. 90% confidence intervals, indicated by shaded regions, are computed by sampling the underlying microdata (with replacement) and feeding bootstrap samples through the DFL algorithm; see text for details.

Figure 7.
Change in total weekly hours per person, women, by skill and group



Source: Author's analysis of 1992/3 and 1995-7 CPS ORG. 90% confidence intervals, indicated by shaded regions, are computed by sampling the underlying microdata (with replacement) and feeding bootstrap samples through the DFL algorithm; see text for details.

Figure 8.
Change in log wages, women, by skill and group



Source: Author's analysis of 1992/3 and 1995-7 CPS ORG. 90% confidence intervals, indicated by shaded regions, are computed by sampling the underlying microdata (with replacement) and feeding the bootstrap samples through the DFL algorithm; see text for details. Dashed lines show naive point estimates (but not C.I.s) of the difference between corresponding percentiles of the raw log wage distributions, ignoring changes in composition.

Table 1. Summary statistics before and after the EITC expansion

	All women			Married		Single					
						Zero children		One child		Two+ children	
	Pre	Post	Change	Pre	Change	Pre	Change	Pre	Change	Pre	Change
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Population											
N (unweighted)	166,715	143,504	-23,211	118,891	-17,717	30,005	-3,234	8,444	-978	9,375	-1,282
Share of pop.	100%	100%		71%	-0.8%	18%	0.7%	5%	0.1%	6%	0.0%
Black	0.12	0.12	0.002	0.07	0.001	0.17	**0.008**	0.30	-0.002	0.37	-0.015
	(0.32)	(0.33)	[0.001]	(0.25)	[0.001]	(0.38)	[0.004]	(0.46)	[0.008]	(0.48)	[0.008]
Hispanic	0.09	0.09	**0.007**	0.08	**0.007**	0.07	0.003	0.10	**0.020**	0.16	0.009
	(0.28)	(0.29)	[0.001]	(0.28)	[0.001]	(0.25)	[0.002]	(0.30)	[0.006]	(0.36)	[0.006]
Age	40.8	41.2	**0.45**	41.0	**0.50**	43.7	0.22	36.0	0.24	33.4	**0.54**
	(11.6)	(11.4)	[0.05]	(11.3)	[0.05]	(13.0)	[0.12]	(9.2)	[0.17]	(7.0)	[0.12]
Education	12.8	13.0	**0.19**	12.9	**0.19**	13.0	**0.18**	12.4	**0.17**	11.9	**0.23**
	(2.6)	(2.6)	[0.01]	(1.5)	[0.01]	(2.8)	[0.03]	(2.3)	[0.04]	(2.4)	[0.04]
Employed	0.61	0.69	**0.073**	0.59	**0.081**	0.72	**0.031**	0.66	**0.073**	0.50	**0.103**
	(0.49)	(0.46)	[0.002]	(0.49)	[0.002]	(0.45)	[0.004]	(0.47)	[0.008]	(0.50)	[0.008]
Hours / week (0 if	21.3	23.6	**2.32**	19.8	**2.53**	27.1	**1.02**	24.2	**2.34**	18.0	**3.12**
not employed)	(19.9)	(19.6)	[0.08]	(19.6)	[0.09]	(19.7)	[0.18]	(19.5)	[0.34]	(19.7)	[0.33]
Workforce (Hours weighted)											
Share of workforce	100%	100%		66%	-0.1%	23%	-0.7%	6%	0.2%	5%	0.4%
Black	0.12	0.11	-0.001	0.08	-0.003	0.14	0.002	0.27	-0.012	0.30	-0.006
	(0.32)	(0.32)	[0.002]	(0.27)	[0.002]	(0.35)	[0.005]	(0.44)	[0.011]	(0.46)	[0.012]
Hispanic	0.07	0.08	**0.008**	0.07	**0.007**	0.05	0.004	0.09	**0.021**	0.12	0.010
	(0.26)	(0.27)	[0.002]	(0.26)	[0.002]	(0.23)	[0.003]	(0.28)	[0.008]	(0.33)	[0.009]
Age	39.2	39.6	**0.46**	39.2	**0.57**	40.7	**0.55**	36.5	-0.02	34.9	-0.17
	(10.4)	(10.4)	[0.06]	(10.1)	[0.07]	(12.1)	[0.16]	(8.2)	[0.21]	(6.3)	[0.17]
Education	13.3	13.5	**0.17**	13.3	**0.19**	13.7	**0.15**	12.9	**0.20**	12.6	0.09
	(2.4)	(2.4)	[0.01]	(2.3)	[0.02]	(2.5)	[0.03]	(2.1)	[0.05]	(2.4)	[0.06]
log(wage)	2.23	2.25	**0.020**	2.23	**0.028**	2.30	**0.017**	2.14	0.012	2.08	**-0.025**
	(0.51)	(0.52)	[0.003]	(0.50)	[0.004]	(0.53)	[0.007]	(0.51)	[0.013]	(0.50)	[0.013]

Notes: Standard deviations in parentheses; heteroskedasticity-robust standard errors for changes in square brackets. Bold changes are significant at the 5% level.

Table 2. Simple regression estimates of the change in low-wage single mothers' labor supply and wages

Dependent variable:	In labor force	Employed	Weekly hrs. if employed	Weekly hrs. per person	ln(real wage)
	(1)	(2)	(3)	(4)	(5)
Model	**Probit**	**Probit**	**OLS**	**OLS**	**OLS**
Single*one kid*post-1994*skill index	**-0.029**	-0.015	0.230	-0.385	0.005
	(0.013)	(0.013)	(0.388)	(0.444)	(0.015)
Single*two kids*post-1994*skill index	**-0.037**	**-0.028**	-0.187	**-0.885**	**-0.036**
	(0.012)	(0.012)	(0.404)	(0.406)	(0.015)
Post-1994*skill index					0.001
					(0.005)
Single*post-1994*skill index					0.000
					(0.008)
N	310,219	310,219	202,282	310,219	148,209

Notes: Robust standard errors in parentheses; bold coefficients are significantly different from zero at the 5% level. All models include controls for a rich vector of observable characteristics (see page 19); main effects for single, for 1 and 2 or more children, for post-1994, and for a standardized predicted skill index (computed as the fitted value from a pre-1994 regression of log wages on observable characteristics using only employed women); and all two- and three-way interactions of the marital status, children, time, and wage index variables. Probit columns report marginal effects. Sample derives from the 1992, 1993, 1995, 1996, and 1997 CPS ORG; see text for details.

Table 3. Estimates of tax effects on labor supply, measured as weekly hours worked

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All women						
Model 1						
Change in ATR	**-0.57**	**-0.56**	**-0.57**	**-0.65**	**-0.63**	**-0.62**
	(0.18)	(0.18)	(0.19)	(0.21)	(0.21)	(0.21)
(Married) x (# of kids) dummies	y	y	y	y	y	y
Base log wage	n	y	n	n	y	n
Skill group fixed effects	n	n	y	n	n	y
Model 2						
Change in MTR	**-0.27**	**-0.30**	**-0.31**	-0.13	-0.21	-0.22
	(0.12)	(0.11)	(0.11)	(0.16)	(0.16)	(0.17)
Model 3						
Change in ATR	**-0.56**	**-0.53**	**-0.54**	**-0.65**	**-0.60**	**-0.58**
	(0.19)	(0.19)	(0.20)	(0.23)	(0.24)	(0.24)
Change in MTR	-0.02	-0.05	-0.05	0.00	-0.06	-0.08
	(0.12)	(0.12)	(0.12)	(0.18)	(0.18)	(0.19)
Panel B: Single women						
Model 1						
Change in ATR	**-0.61**	**-0.72**	**-0.72**	**-0.64**	**-0.77**	**-0.73**
	(0.21)	(0.23)	(0.24)	(0.22)	(0.24)	(0.25)
Model 2						
Change in MTR	-0.25	-0.25	-0.24	-0.04	-0.09	-0.04
	(0.13)	(0.13)	(0.13)	(0.16)	(0.17)	(0.18)
Model 3						
Change in ATR	**-0.62**	**-0.79**	**-0.79**	**-0.65**	**-0.85**	**-0.80**
	(0.21)	(0.26)	(0.26)	(0.23)	(0.28)	(0.29)
Change in MTR	0.00	0.08	0.09	0.02	0.11	0.11
	(0.13)	(0.12)	(0.13)	(0.17)	(0.18)	(0.19)

Notes: Dependent variable is the percentage change in labor supply (weekly hours) per woman. Observations are 187 skill by six (Panel A) or three (Panel B) demographic groups, yielding N=1,122 or 561. Standard errors (in parentheses) are estimated as 0.74 times the interquartile range of 600 bootstrap replications, each sampling from the underlying CPS data and re-estimating the DFL model and then the labor supply regressions. Bold coefficients exceed 1.96 standard errors (in absolute value). IV estimates instrument for the actual change in mean tax rates with that predicted by applying the post-period tax schedule to pre-period observations.

Table 4. IV estimates of reduced-form tax effects on labor supply at various margins

	ATR only	MTR only	Both together	
			ATR	**MTR**
	(1)	**(2)**	**(3)**	**(4)**
Sequential breakdown of labor supply				
(1) Population size	0.33	0.20	0.23	0.16
	(0.24)	(0.15)	(0.28)	(0.18)
(2) Labor force participation	**-1.13**	-0.11	**-1.21**	0.11
	(0.20)	(0.14)	(0.23)	(0.15)
(3) Employment conditional on LFP	**0.35**	0.02	**0.38**	-0.05
	(0.07)	(0.05)	(0.08)	(0.06)
(4) Formal sector (not SE) conditional on employment	-0.04	0.04	-0.08	0.06
	(0.07)	(0.05)	(0.09)	(0.06)
(5) Hours conditional on non-SE employment	-0.09	-0.04	-0.07	-0.03
	(0.06)	(0.04)	(0.08)	(0.05)
(6) Non-allocation of wages conditional on hours	-0.12	-0.04	-0.11	-0.02
	(0.11)	(0.08)	(0.14)	(0.09)
Interesting combinations				
(7) Employment-population ratio (=2+3)	**-0.60**	-0.04	**-0.66**	0.09
	(0.21)	(0.15)	(0.24)	(0.16)
(8) Total hours per person (=2+3+4+5)	**-0.73**	-0.04	**-0.80**	0.11
	(0.25)	(0.18)	(0.29)	(0.19)
(9) Total hours worked (=1+2+3+4+5)	-0.36	0.20	-0.56	0.30
	(0.38)	(0.26)	(0.45)	(0.29)
March CPS data				
(10) Annual labor force participation	**-0.55**	0.17	**-0.75**	0.31
	(0.23)	(0.14)	(0.26)	(0.16)
(11) Annual hours worked if ever in labor force	-0.32	0.10	-0.44	0.18
	(0.20)	(0.18)	(0.24)	(0.21)
Wages				
(12) Hourly wages	-0.03	-0.02	-0.02	-0.02
	(0.15)	(0.11)	(0.21)	(0.13)

Notes: Specifications are identical to those in Panel B, Column 6 of Table 3, but the dependent variable varies across rows. All are measured as percentage changes, so coefficients are interpretable as the negative of elasticities with respect to marginal/average wages. Standard errors are in parentheses; bold coefficients exceed 1.96 standard errors (in absolute value).

Table 5. Reduced form models for labor supply and wages

	Predicted	1992-1996 changes		1988-1992 & 1992-1996 changes	
		Single women	**All women**	**Single women**	**All women**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Dependent variable: Change in labor supply, group g, skill s					
Change in ATR within group	$-\sigma$	**-0.73**	**-0.74**	**-1.06**	**-1.06**
		(0.25)	(0.25)	(0.23)	(0.23)
Change in avg. ATR (across groups)	$\frac{\sigma^2}{\sigma - \rho}$	-0.24	-1.02	0.00	-0.26
		(0.32)	(1.26)	(1.02)	(2.02)
Change in population size (avg. across groups)	$\frac{-\sigma}{\sigma - \rho}$	-0.10	-0.16	-0.20	-0.12
		(0.21)	(0.37)	(0.31)	(0.63)
Group dummies		y	y	y	y
Base log wage		y	y	n	n
Skill dummies		n	n	y	y
1992-1996 indicator		n	n	y	y
Base log wage * 1992-1996		n	n	y	y
Dependent variable: Change in wage, group g, skill s					
Change in avg. ATR (across groups)	$\frac{\sigma}{\sigma - \rho}$	**-1.26**	**-5.14**	2.45	**4.78**
		(0.39)	(1.75)	(1.33)	(2.28)
Change in population size (avg. across groups)	$\frac{-1}{\sigma - \rho}$	-0.38	-0.52	-0.22	-1.14
		(0.29)	(0.63)	(0.26)	(0.70)

Notes: All specifications use labor supply and wage observations for single women with zero, one, and two or more children. All are estimated by IV, using the pre-reform prediction of the change in tax rates as an instrument for the observed change. Standard errors (in parentheses) are estimated as 0.74 times the interquartile range of 600 bootstrap replications, each sampling from the underlying CPS data and re-estimating the DFL model. Bold coefficients exceed 1.96 standard errors (in absolute value).

Table 6. Inverse labor demand
Dependent variable is the change in log wages at skill s in demographic goup g

Wage measures	**All single women**		**Single, childless women**			
Sample	**1992-1996**		**1992-1996**		**1988-1992 & 1992-1996**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Change in hours per person within demographic group	0.06	0.00				
	(0.23)	(0.22)				
Change in hours per person, avg. among single women	**1.21**		**1.04**		**-3.59**	
	(0.54)		(0.52)		(1.82)	
Change in hours per person, avg. among all women		4.91		3.36		-2.67
		(2.90)		(2.15)		(1.48)
Additional controls						
Group dummies	y	y	n	n	n	n
Base log wage	y	y	y	y	n	n
Skill dummies	n	n	n	n	y	y
1992-1996 indicator	n	n	n	n	y	y
Base log wage * 1992-1996	n	n	n	n	y	y
Implied elasticity of demand	**0.83**	0.20	**0.96**	0.30	**-0.28**	-0.37
	(0.37)	(0.12)	(0.48)	(0.19)	(0.14)	(0.21)

Notes: Changes in hours are treated as endogenous; instruments are the pre-reform prediction of the mean change in ATRs in the relevant group. All models include controls for the change in the population size of the groups over which labor supply and wage changes are measured. Standard errors are estimated as 0.74 times the interquartile range of 600 bootstrap replications, each sampling from the underlying CPS data and re-estimating the DFL model. Bold coefficients exceed 1.96 standard errors (in absolute value). Elasticities are computed from the coefficient on the average change in labor supply among single/all women, and their standard errors via the delta method.

Table 7. Alternate specificatons for elasticity parameters

	Supply	Demand	Employer share of tax incidence
	σ	ϱ	$\frac{\sigma}{\sigma - \varrho}$
	(1)	(2)	(3)
(1) Base specifications	**0.73**	**-0.28**	0.72
	(0.25)	(0.14)	
(2) Using only every 2nd percentile (47 skill groups)	**0.72**	**-0.24**	0.75
	(0.25)	(0.12)	
(3) Using only every 3.5th percentile (27 skill groups)	**0.73**	**-0.20**	0.79
	(0.24)	(0.08)	
(4) Pre-TANF sample	**0.62**	-0.53	0.54
	(0.28)	(0.36)	

Notes: "Base specification" in column 1, row 1 is that used in Model 1 of Table 3, Column 6, Panel B. Base specification in column 2 is that used in Table 6, column 5. Remaining rows use fewer skill groups (rows 2 and 3) or use labor supply and wage changes estimated from a CPS sample that excludes observations potentially affected by welfare reform (row 4). Standard errors in parentheses; bold coefficients exceed 1.96 standard deviations (in absolute value).

Table 8. Incidence of $1 in transfers to single mothers via the EITC

	Eligible (with children)	Ineligible (no children)	Total
	(1)	(2)	(3)
(1) Tax credits	+ 1.00	--	+ 1.00
(2) Labor market effects			
(2A) Change in labor supply	+ 0.51	- 0.31	+ 0.20
(2B) Change in wages (at old labor supply)	- 0.30	- 0.43	- 0.72
(2C) Residual (interaction)	- 0.01	+ 0.01	- 0.00
(2D) Change in earnings (=2A + 2B + 2C)	+ 0.21	- 0.73	- 0.53
(3) Total transfer (=1 + 2B)	+ 0.70	- 0.43	+ 0.28
(4) Total change in after-tax income (=1+2D)	+ 1.21	- 0.73	+ 0.47

Notes: I simulate a credit expansion for single mothers with total payments equal to $1. Simulation is based on σ=0.73, ϱ=-0.28, a distinct labor market for single women, and the observed distribution of single women with and without children across skill levels.

Appendix Table 1. EITC parameters, 1987-2001 (in constant 1992 dollars)

Year	Marginal tax rates: Phase-in	Marginal tax rates: Phase-out	Max. credit	Kink points: 1) Phase-in to plateau	Kink points: 2) Plateau to phase-out	Kink points: 3) Phase-out to no credit	Hourly wage for FT, FY worker to hit: Kink 1	Kink 2	Kink 3	Federal minimum wage
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
One-child families										
1983	-10.0%	12.5%	$704	$7,043	$8,452	$14,086	$3.52	$4.23	$7.04	$4.72
1984	-10.0%	12.5%	$675	$6,752	$8,102	$13,503	$3.38	$4.05	$6.75	$4.52
1985	-14.0%	12.2%	$717	$6,520	$8,475	$14,343	$3.26	$4.24	$7.17	$4.37
1986	-14.0%	12.2%	$704	$6,401	$8,321	$14,081	$3.20	$4.16	$7.04	$4.29
1987	-14.0%	10.0%	$1,051	$7,509	$8,546	$19,059	$3.75	$4.27	$9.53	$4.14
1988	-14.0%	10.0%	$1,037	$7,400	$11,670	$22,031	$3.70	$5.83	$11.02	$3.97
1989	-14.0%	10.0%	$1,030	$7,354	$11,586	$21,882	$3.68	$5.79	$10.94	$3.79
1990	-14.0%	10.0%	$1,023	$7,310	$11,518	$21,752	$3.66	$5.76	$10.88	$4.08
1991	-16.7%	11.9%	$1,228	$7,355	$11,589	$21,890	$3.68	$5.79	$10.94	$4.38
1992	-17.6%	12.6%	$1,324	$7,520	$11,840	$22,370	$3.76	$5.92	$11.19	$4.25
1993	-18.5%	13.2%	$1,392	$7,525	$11,845	$22,380	$3.76	$5.92	$11.19	$4.13
1994	-26.3%	16.0%	$1,929	$7,337	$10,414	$22,489	$3.67	$5.21	$11.24	$4.02
1995	-34.0%	16.0%	$1,928	$5,671	$10,394	$22,459	$2.84	$5.20	$11.23	$3.91
1996	-34.0%	16.0%	$1,924	$5,660	$10,382	$22,425	$2.83	$5.19	$11.21	$4.25
1997	-34.0%	16.0%	$1,932	$5,682	$10,429	$22,509	$2.84	$5.21	$11.25	$4.50
1998	-34.0%	16.0%	$1,955	$5,750	$10,553	$22,786	$2.87	$5.28	$11.39	$4.43
1999	-34.0%	16.0%	$1,947	$5,727	$10,493	$22,677	$2.86	$5.25	$11.34	$4.34
2000	-34.0%	16.0%	$1,917	$5,638	$10,339	$22,335	$2.82	$5.17	$11.17	$4.20
2001	-34.0%	16.0%	$1,923	$5,656	$10,370	$22,404	$2.83	$5.19	$11.20	$4.08
Two or more children										
1983-1990: Same as one child										
1991	-17.3%	12.4%	$1,272	$7,355	$11,589	$21,890	$3.68	$5.79	$10.94	$4.38
1992	-18.4%	13.1%	$1,384	$7,520	$11,840	$22,370	$3.76	$5.92	$11.19	$4.25
1993	-19.5%	13.9%	$1,467	$7,525	$11,845	$22,380	$3.76	$5.92	$11.19	$4.13
1994	-30.0%	17.7%	$2,393	$7,976	$10,414	$23,948	$3.99	$5.21	$11.97	$4.02
1995	-36.0%	20.2%	$2,863	$7,954	$10,394	$24,555	$3.98	$5.20	$12.28	$3.91
1996	-40.0%	21.1%	$3,180	$7,949	$10,382	$25,480	$3.97	$5.19	$12.74	$4.25
1997	-40.0%	21.1%	$3,196	$7,990	$10,429	$25,604	$3.99	$5.21	$12.80	$4.50
1998	-40.0%	21.1%	$3,233	$8,082	$10,553	$25,904	$4.04	$5.28	$12.95	$4.43
1999	-40.0%	21.1%	$3,214	$8,034	$10,493	$25,753	$4.02	$5.25	$12.88	$4.34
2000	-40.0%	21.1%	$3,168	$7,919	$10,339	$25,381	$3.96	$5.17	$12.69	$4.20
2001	-40.0%	21.1%	$3,175	$7,938	$10,370	$25,454	$3.97	$5.19	$12.73	$4.08
Families without children										
1983-1993: No credit										
1994	-7.7%	7.7%	$290	$3,787	$4,733	$8,520	$1.89	$2.37	$4.26	$4.02
1995	-7.7%	7.7%	$289	$3,774	$4,723	$8,497	$1.89	$2.36	$4.25	$3.91
1996	-7.7%	7.7%	$289	$3,774	$4,721	$8,495	$1.89	$2.36	$4.25	$4.25
1997	-7.7%	7.7%	$290	$3,794	$4,747	$8,540	$1.90	$2.37	$4.27	$4.50
1998	-7.7%	7.7%	$294	$3,839	$4,794	$8,633	$1.92	$2.40	$4.32	$4.43
1999	-7.7%	7.7%	$292	$3,815	$4,775	$8,590	$1.91	$2.39	$4.29	$4.34
2000	-7.7%	7.7%	$288	$3,756	$4,701	$8,457	$1.88	$2.35	$4.23	$4.20
2001	-7.7%	7.7%	$288	$3,771	$4,714	$8,483	$1.89	$2.36	$4.24	$4.08